AESTIMATIO

Critical Reviews in the History of Science

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Preface

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

Aestimatio has come along very nicely in the years since its inception in 2004. To mark the occasion of its 10th anniversary and, more practically, to accommodate as best we can the ever increasing number of items to be published in each volume, we have given the journal a facelift which, we hope, readers will find attractive and easily legible.

As before, *Aestimatio* will remain available online free of charge at http://www.ircps.org/aestimatio, where one will now find not only links to the reviews *separatim* but also to the completed volumes. Those interested in printed copy may secure volumes 1–8 from Gorgias Press (go to helpdesk@gorgiaspress.com). Volume 9 and all subsequent volumes will be available through a print-on-demand service (go to http://www.ircps.org/aestimatio) that was first announced in volume 9.

We also continue to distribute volumes electronically through EBSCO and to register their contents in both the Directory of Open Access Journals and the Standard Periodical Directory.

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Cross-Cultural Scientific Exchanges in the Eastern Mediterranean, 1560–1660 by Avner Ben-Zaken

Baltimore: Johns Hopkins University Press, 2010. Pp. xx + 246. ISBN 978–0–8018–9476–3. Cloth $60.00*

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Avner Ben-Zaken a jugé utile d'en informer le lecteur dans l'introduction du livre sous recension : c'est en découvrant pendant ses études supérieures des témoins proches-orientaux de la diffusion du copernicianisme qu'il a choisi son thème de recherches – les échanges scientifiques interculturels entre les pays riverains de la Méditerranée orientale, durant la période 1560–1660 [4]. Cette indication autobiographique incite le recenseur à rappeler le parcours intellectuel dont le présent livre prétend être, non pas l'aboutissement, mais un jalon. Depuis la préparation de sa thèse [2004a], l'auteur a en effet publié cinq articles consacrés peu ou prou à la même thématique, à la même tranche chronologique et à la même zone géographique [2002, 2004b, 2004c, 2009a, 2009b]. Mais les deux derniers parus, en dépit de leur date de publication, prolongent en fait le livre sous examen. En outre, et toujours en 2010, Avner Ben-Zaken a publié un second livre, qui aborde la thématique en question mais sous l'angle particulier de l'autodidactisme en élargissant la zone géographique et la période chronologique de l'enquête. Pendant près d'une décennie et en une demi-douzaine de publications, l'auteur s'en est donc tenu à l'objet historique sur lequel son attention avait été attirée au cours de ses années d'étudiant.

* [Edd.] This is a revision of a review by Max Lejbowicz that was posted on 27 February 2013 and then withdrawn at his request on March 2. The aim in this revision is to provide an accounting of Ben-Zaken's book that does justice to the full range of the diverse sources on which the author draws in making his case. The Editors apologize for any inconvenience that this may cause readers of *Aestimatio*.

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Les travaux d'Avner Ben-Zaken se caractérisent autant par leur objet que par leur manière de le traiter. Ils s'attachent à des péripéties propres à la micro-histoire culturelle. En cinq chapitres rendus quasiment autonomes par les faits qui y sont réunis et interprétés, les *Cross-Cultural Scientific Exchanges in the Eastern Mediterranean* traitent du thème qu'ils annoncent en explorant les tenants et les aboutissants d'événements culturels ponctuels. Nettement circonscris, ces épisodes concourent au même but : décrire l'imbrication des cultures retenues, celle de l'Europe latine et celles du Proche-Orient arabe, persan et turc, en prenant pour fil directeur les réalisations scientifiques, et plus précisément, astronomiques de ces diverses cultures.

Plus précisément encore, chaque chapitre explore « a marginal textual object, written mostly by marginal figures in the history of science » [7]. C'est ainsi que sont traités successivement : dans le 1er chapitre, les mésaventures, sur fond apocalyptique, de Taqī al-Dīn, l'initiateur de l'éphémère observatoire d'Istanbul (1577–1581), lequel est rapproché d'Uraniborg, fondé en 1576, par un Tycho Brahe qui s'essaye aux prédictions astrologiques, tout comme son pendant ottoman ; dans le 2^{e} chapitre, les pérégrinations orientales, de 1614 à 1626, de Pietro della Valle en quête de la version primitive du livre de Job qui réconcilierait les Écritures Saintes et l'astronomie copernicienne ; dans le 3^{e} chapitre, la quête du Juif crétois, Joseph Solomon Delmedigo (1591–1655), un élève de Galilée, qui cherche dans le karaïsme – une dissidence du judaïsme rabbinique – le moyen de concilier la tradition mosaïque avec le nouvelle physique ; dans le chapitre 4^{e}, sur fond cette fois de querelles entre les tenants du parlementarisme, acquis au copernicianisme, et les monarchistes attachés aux traditions anglicanes, les recherches menées par l'orientaliste anglais, John Greaves, soutenu par des prélats de premier plan, d'une langue et d'une unité de mesure qui seraient primordiales et normatives par nature (le triomphe du parlementarisme marque l'arrêt de ce genre de prospections au profit d'une « universal language for natural philosophy based on the universality of nature » [138]) ; et dans le 5^{e} et dernier chapitre, enfin, la traduction en turc, en 1660, par Ibrāhīm Efendi al-Zigetvārī Tezkirecī, un scribe de l'armée ottomane, de la *Nouvelle théorie des planètes* rédigée en 1635 par le cosmographe du roi de France Louis XIII, Noël Duret, qui infléchit l'original vers une problématique soufie.

Cet ensemble de chapitres est précédé par une introduction au titre évocateur « Incommensurable Cultures ? », et suivi par une conclusion, titrée de manière non moins évocatrice, « From 'Incommensurability of Cultures' to Mutually Embraced Zones ». Est-ce que le lecteur, même sensible à l'œcuménisme final, est convaincu par la démonstration menée dans les chapitres intermédiaires ?

L'ouvrage, qui, on l'a vu, insiste sur l'importance des théories astronomiques témoigne trop souvent d'une méconnaissance des données de base de l'astronomie. Que penser de :

> As the centuries pass, the celestial pole moves gradually through the stars in his circle, at a rate of 0.7 degrees per century, and completes on revolution every twenty-six thousand years [112] ?

L'avancée de 0.7 degré par siècle est reprise à la page 113 : la valeur donnée n'est donc pas une coquille. Une simple multiplication montre qu'animé d'un mouvement d'une telle vitesse, le pôle céleste n'a parcouru en 26 000 ans qu'à peine plus de la moitié de la distance annoncée ($260 \times 0.7 = 182$), puisque – faut-il le rappeler ? – un cercle contient 360 degrés.

Une meilleure connaissance de l'astronomie médiévale aurait certainement permis une présentation plus rigoureuse des faits retenus et des idées défendues. Or pour évoquer la traduction déficiente de l'*Almageste* de Claude Ptolémée par Gérard de Crémone au XIIe siècle, Avner Ben-Zaken renvoie [107n5] à une étude de Marshall Clagett, qui, certes, a fait date dans l'histoire des sciences médiévales, mais consacrée à Euclide et centré sur un autre traducteur arabo-latin du XIIe siècle, comme le titre l'indique expressément, « The Medieval Latin Translation from the Arabic of the *Elements* of Euclid, with Special Emphasis on the Versions of Adelard of Bath » [Clagett 1953] – de surcroît un traducteur antérieur d'une génération à Gérard de Crémone. Comme il se doit, cette étude fondatrice ne contient aucune allusion à l'astronome alexandrin. Tout ce passe comme si l'œuvre de ce grand érudit, Paul Kunitzsch, était passée par pertes et profits [1974] et comme si Gérard de Crémone, ce traducteur particulièrement fécond, n'avait pas fait l'objet d'études spécifiques [Lemay 1978 ; Pizzamiglio 1992 ; Burnett 2001, pour ne retenir que les plus notables]. Tout se passe enfin comme si des travaux n'avaient pas été consacrés au latin des traductions arabo-latines médiévales des textes astronomiques [Poulle 1987 ; Lorch 1990], alors que leur connaissance permettrait d'évaluer plus justement la critique que les arabisants du XVI^e^ siècle leur adressent.

Avner Ben-Zaken écrit [27, 34] que les deux plus anciens observatoires en terre d'Islam, sont ceux de Marāgha au XIII^e siècle et de Samarcande au XV^e. L'ouvrage qu'il cite et qui, sur la question, fait autorité [Sayili 1960], ne dit pourtant pas la même chose. Les deux observatoires en question n'y sont présentés qu'aux chapitres 6 et 8, dans un ensemble qui en compte 12. C'est dire qu'ils sont précédés par de nombreux autres. L'un des plus marquants, sans être le plus accompli, est sans doute celui érigé à l'initiative du calife al-Maʾmūn (813–833), pour accompagner le puissant mouvement de traduction gréco-arabe qu'il avait également suscité. Sans cette exploration continue du ciel et sans les perfectionnements qu'elle implique au double plan pratique et théorique, l'engouement des Latins pour l'astronomie arabe dès le XI^e siècle, et surtout à partir du XII^e, est proprement inintelligible.

Avner Ben-Zaken étudie très en détail la fameuse enluminure extraite de la chronique persane versifiée d' ᶜAlāʾ al-Dīn al-Manṣūr, *Shāhinshāhnāma/Le livre du roi des rois* (i. d. de Murād III ; v. 1581) qui rehausse le manuscrit Istanbul University Library, F. 1404. Elle est censée reproduire certains des instruments d'inspiration européenne dont l'observatoire d'Istanbul disposait. Que ne s'était-il pas intéressé, pour mieux connaître le niveau technique atteint par Taqī al-Dīn, à l'un des manuscrits de la bibliothèque de l'observatoire de Kandilli qui contient un petit *zīj* probablement autographe ? [King, 1993 ; 1998, XV.248–249].[1] Toujours en rapport avec cette enluminure, Avner Ben-Zaken commente les deux sphères qu'elle représente en les rapprochant de deux autres, l'une terrestre, l'autre céleste, apparues lors d'une vente chez Christie's en 1991 : elles auraient été fabriquées à Anvers en 1579 et, comme en témoigne l'inscription de l'une d'elles, étaient destinées au sultan Murād III. Or David King [1998, 94], pense qu'elles ont été fabriquées à Duisbourg et qu'elles n'ont jamais quittées l'Europe. L'avis d'un tel spécialiste demanderait au moins d'être pris en considération avant de proposer une interprétation.

La technique astrologique, si importante dans l'astronomie médiévale et renaissante, n'est guère mieux traitée. En reprenant la traduction anglaise d'un passage du traité qu'Abū Maᶜšar a consacré à l'histoire des religions et des dynasties, Avner Ben-Zaken croit devoir préciser ce qu'il faut entendre par « cardines » ; il s'agirait de l'adjectif promu au rang de substantif par l'ellipse d'un nom déterminé, soit « cardinal points » [35]. Il lui aurait suffi de lire les com-

[1] « Zīj » est le mot arabe désignant des tables astronomiques. Le livre de David A. King est pourtant cité dans la bibliographie d'Avner Ben-Zaken.

mentaires des éditeurs du livre en question pour s'apercevoir que le nom déterminé est tout autre [Yamamoto and Burnett 2000, 1.579] : considéré comme adjectif, « cardines » qualifie, dans ce contexte, non pas un point mais une ligne, celle que découpent quatre des douze maisons (anglais : « places ») à une date précise sur la sphère locale : les $1^{\text{ère}}$ et 4^{e} (pour l'arc nocturne) et les 7^{e} et 10^{e} (pour l'arc diurne). Abū Maᶜšar ne parle donc pas du simple passage des planètes à l'un des quatre points cardinaux mais de leur présence dans l'une des quatre parties bien délimitées de la sphère locale, les maisons cardinales.

Force est aussi de constater que la connaissance de la langue arabe par l'auteur n'est pas toujours satisfaisante. Il traduit, sans enquête préalable, le mot arabe « idrāk », abondamment utilisé dans le chapitre 5, par l'anglais « apperception » ; français « aperception ». En fait, le mot appartient au vocabulaire d'Avicenne. Il est généralement traduit en français pas « perception » [Goichon 1984, 83–84 ; Sebti 2000, 18 ; Marcotte 2006] ! Comment admettre tout de go que le même mot arabe puisse renvoyer, à la fois, à une opération psychologique et à la conscience de cette opération ? De même, la proximité sémantique d'« idrāk » avec « arṣād » [151] mériterait d'être mieux documentée. Enfin, la juxtaposition dans une même page [161] d'une référence à *Tahāfut al-falāsifah* d'al-Ghazzālī traduit par « The Incoherence of Philosophers » et à *Ḥikmat al-ishrāq* de Suhrawardī traduit par « The Philosophy of Illumination » concrétise un étrange flottement terminologique, qui court d'ailleurs tout au long de l'ouvrage. Sans doute que, pour le traité de Suhrawardī, Avner Ben-Zaken se contente de reprendre le titre de la traduction de John Walbridge et Hossein Ziai [1999], qui est effectivement citée dans la bibliographie [225]. Mais n'est-ce pas commettre un gros contre-sens que de laisser penser qu'en arabe classique, « falsafa » (« la philosophie ») et « ḥikmat » (« la sagesse ») sont de simples synonymes ? Est-ce qu'al-Ghazzālī a fustigé l'incohérence des sages ? Une mise au point lexicographique s'imposait pour affiner les analyses proposées –, mise au point d'autant plus facile à rédiger qu'elle a déjà été faite, et très bien faite [Jolivet 1991].

Avner Ben-Zaken se laisse parfois entraîner par les sortilèges de l'occultisme et de la cabale. De retour de ses voyages au Proche-Orient et arrivé à Amsterdam, Joseph Solomon Delmedigo se laisse convaincre d'écrire un traité qui réconcilierait le judaïsme et la nouvelle physique. Il le titre « Élim ». Avner Ben-Zaken voit dans ce nom, dont il est question dans Exode 15.27 :

a utopian biblical locale where, according to cabalists, the revelation of the secrets of nature had commenced and had spread to the whole universe [78]

a utopian oasis in the desert [90]

En fait, ce nom est également utilisé dans le livre des Nombres 33 :9–10 et, d'après François Michel Du Buit [2002], auteur d'une *Géographie de la Terre Sainte* [1958, *non vidi*], il renverrait soit à l'oasis de Garandel si l'on suit l'Exode, soit à l'oasis de Bir Qatia si l'on suit les Nombres. Il s'agit donc, pour le bibliste, de la mythification d'un lieu incertain, non d'un lieu mythique à proprement parler. Une fois rappelé cet acquis de l'exégèse, il serait intéressant de le confronter aux conceptions des cabalistes du XVII^e^ siècle : c'est dans le maintien de ce genre d'écart que s'opère le travail de l'historien.

Tout au long de son livre, Avner Ben-Zaken fait preuve d'un réel talent de conteur. Il lui reste maintenant à le soumettre plus strictement aux réquisits de la méthode historique et plus spécialement de l'histoire des sciences – si, du moins, il veut que soit prise au sérieux sa manière de concevoir les *Cross-Cultural Scientific Exchanges*.

In addition to the problems which Ben-Zaken had with technical elements of astronomy and astrology, as well as with lexicography, there are numerous methodological, philological, and factual issues that mar his storytelling. The term 'truth' is certainly problematic and not well appreciated among current writers in the humanities. But there is the obligation for any author who wishes to establish his or her credentials as an academic historian (of science) to verify to the best of her or his capacity that he or she represents the primary sources correctly. This means that translations marked as her or his own enterprise must not be taken from someone else. Moreover, a translation should reflect the original text in such a manner that it can be recognized by some other academic reader. Indeed, a story should be retold in accordance with its original structure and sense, and factual information must be reliable and verifiable. Ben-Zaken violates all these points in different ways and to varying degrees of gravity.

The chapter on Taqī al-Dīn rests in essential points of Ben-Zaken's ruminations on a faulty English translation of a technical Arabic text by the Ottoman scholar produced in 1966 by Sevim Tekeli, without Ben-Zaken's admitting his dependence on this translation.[2] While Ben-Zaken appropriated many outright mistakes and modernizing interpretations from Tekeli's translation, his interpretation of Taqī al-Dīn's work as evidence for the scholar's adherence to hermetic and millenarian beliefs is his own. A short and relatively simply extract indicates the various kinds of shortcomings of Ben-Zaken's and Tekeli's treatment of the Arabic text.

Tekeli translated the passage in question by:

> It asks aid from two sections of sciences, the mathematics and natural sciences. As we come to the mathematics. [*sic*] It uses branches of algebra, geometry, the science of surveying, dinamics [*sic*], mechanics and the science of balances. As for the natural sciences. [*sic*] They are the sciences of talisman, chemistry. Nevertheless it needs intelligence and the ability of freedom of action and skill about manufacture, as the art of goldsmith and carpentary [*sic*], making string, yellow brick-work. [Tekeli 1966, 142].

Ben-Zaken ignores the last sentence of Tekeli's version and writes:

> [The art of building clocks] relies on two sections of sciences, mathematics and natural philosophy. As for mathematics, it uses fields of algebra, geometry, science of surveying, dynamics, and the science of balances. As for natural philosophy, it requires knowledge in the art of talismans, magic, and alchemy. Both require a high ability of direct intuitive perception, power of imagination. [18]

My strictly literal translation, which includes an indication of a word that I find difficult to render meaningfully, is as follows:

> Its support (comes) from two parts of philosophy (*ḥikma*), the mathematical and the natural. As for the mathematical (part), to it belongs number theory, geometry, the science of proportion, the science of surveying, the science of moving automata (*ḥiyal*), the science of pulling weights, and the science of the balances. As for the natural (part), to it belongs the science of the talismans, the science of incantations, and the science of alchemy. Notwithstanding, one needs much comprehension, power of speculation [?],[3] and excellence in many crafts such as the craft of goldsmithing, blacksmithing, carpentry, tinsmithing, stringing and (glazing) gold colored tiles.

[2] Compare pages 18ff. with Tekeli 1966, 215–323 (Arabic text), 139–212 (English translation).

[3] *taṣarruf*. This word means usually something else, e.g., free disposal, action, free movement, etc. [Wehr and Cowan 1979, 598–599].

As these three renderings of this passage from Taqī al-Dīn indicate, several expressions are given meanings that they did not have in the 16th century or they are omitted:

- 'ḥikma' stands for philosophy in a broad sense or knowledge but does not signify science;
- 'ᶜilm al-ᶜadad' does not mean algebra, which is 'ᶜilm al-jabr wa'l-muqābala' in Arabic, but number theory;
- 'ᶜilm al-ḥiyal al-mutaḥarrika' does not mean dynamics but refers to automata. Dynamics did not exist as a specific branch of knowledge with its proper name.

In addition, like Tekeli, Ben-Zaken does not translate

- 'ᶜilm al-nisba', which designates the fourth of the theoretical mathematical sciences or theoretical music, i.e., the theory of proportion.

Further, in deviating from her, he also forgets to translate

- 'ᶜilm jarr al-athqāl', which means the science of pulling weights.

The translation of 'idrāk'—meaning, among other things: achievement, accomplishment; perception, discernment, awareness, consciousness; comprehension, understanding, grasp; reason, intelligence; sexual maturity, puberty, etc. [see Wehr, and Cowan 1979, 323]—as 'direct intuitive perception' results from Ben-Zaken's belief that this word must always be understood in its more or less specific meaning within Suhravardī's philosophy of illumination. The translation of 'taṣarruf' by 'imagination' fits the text well but not the word, which has no relationship whatsoever to imagination or fantasy. Such a translation suggests an emendation on Ben-Zaken's part which he ought to have announced in order to clarify his reading of the Arabic text. There is, unfortunately, no such note.

On the same page, Ben-Zaken claims:

> He tells us that, when young, 'he used to study the books of other mathematicians....I inspected texts in common use, the *Spherica* of Theodosius, the *Elements* of Euclid, the book *On Equilibrium of Planes* of Archimedes [*sic*], and the books of arts, which have the precise works and texts on mechanics.'

The Arabic text is much longer than revealed in Ben-Zaken's direct quotation. It does not, however, mention the titles of the Greek books in their standardized manner, though Tekeli's English translation does [1966, 139, 215]. Particularly revealing is the reference to Archimedes' *On the Equilib-*

rium of Planes, which was not known under this title in Arabic at all. In my literal translation, the Arabic text says:

> Thus, in the time of childhood [or: youth], I was very much in love with the science of the constructions [of clocks and] strong in reading the books of the rest of the mathematical sciences until I had certain [knowledge] of the shadow and the optical instruments in practice and in theory. I revealed the secrets in regard to their figures and their lines fundamentally and decisively. I looked into the common use [or: circulation] of the treatises on constructing [clocks], the Theodosian spheres, the Euclidean theorems, and the Archimedean polyhedra [?],[4] the books on automata (*al-ḥiyal*) of the most subtle construction, and the treatises on the science of the steelyard (*al-qarasṭūn*), the balance, and the pulling of weights, [and] the like thoroughly investigated in this art [or: branch (*al-fann*)], from the basics to the utmost degree and from the letters to the conclusions [or: results]. Praise be to God.

Ben-Zaken continues:

> Although he could get hold of such classics in their Arabic translations and commentaries, Taqi al-Din tells us that, for mechanics, he relied on sources *from other religions*, that he *gathered their useful fruits*, and that *no one in the Islamic world has come to terms with such knowledge*....In a later portion of the book, he explains that knowledge of clock-making had for some time been obtained by rote, and he states his motive for writing the book to document ideas that might fall into oblivion. [18–19]

The passages in italics show where Ben-Zaken has closely followed Tekeli's translation [1966, 140–141], which, as I will show, is faulty. Moreover, Ben-Zaken's last sentence is a summary based on an a misinterpretation: the Arabic text claims only that the author had written his work against the explicit orders of scholars of the religious sciences.

In my literal translation, the entire passage goes as follows:

> What keeps arriving in these regions from their instruments, in particular, what belongs to the art (or: craftsmanship) of the people [or: tribes] of the Alans, the Magyars [*scil.* Hungarians], the French, and the Germans, is of utmost certainty and accuracy and of extreme beauty and illustration in addition to the fact that [their] instruments are plated with much gold, [although they can be procured] for a small price....During the period of my being in the service of the lord of the dynasty,...the Grand Vizier,...Excellency Aḥmad Pāshā...I regarded attentively his treasury [or: library] which was [filled] with those instruments of different

[4] *al-tasaṭṭuḥāt al-arshimīdisiyya.*

> constructions and [reflected on] which benefits they contained that [could] not be obtained with the astrolabe nor the quadrant. I [pondered] that truth of reflection and speculative thinking and was at home in its novel domains [as well as] previous thoughts. Nonetheless, I continued to discuss with the well-versed masters of this science from the remaining creeds and I harvested its beneficial fruits [that are] easy to use and of [great] variety until what [they contained] as drawings had been extracted by me and all of their principles on which they [rest], whether evident or hidden, [had become] clear to me. Thus, it came to me from this[5] that it [never] was assembled by a single [person] of those who occupy themselves with this art [or: discipline] in the domains of Islam and [that never] anyone of the elites or the masses had become renowned for it. I came to understand that it was impossible for me [to obtain] the approval [or: agreement] by the *sharᶜiyya* sciences, that the determination that was lamented upon (*bukiyathā*) [should] not be directed towards it in what remains of the life[6] and that I [should] forget what I had achieved in it [so that] its trace [becomes] forgotten, obliterated, effaced, and extinct.

The list of such problems can be extended without much effort for this chapter and renders Ben-Zaken's claims about where, how, and for which purposes Taqī al-Dīn acquired his knowledge of mechanical clocks unreliable.

Ben-Zaken's misrepresentations are not limited to Arabic texts. They can also be detected in his discussions of Ottoman Turkish, German, Persian, Italian, and even English texts. For example, in the first case, Ben-Zaken describes an Ottoman miniature and its content as follows:

> This miniature depicts star-like prophetic verses hanging from the sky above Sultan Murād III, heralding the rise and fall of the rules of the previous sultans. [38]

The three names inscribed in the medallions are those of Grand viziers, not sultans: (Koca) Sinan Pasha (1580–1582; the first vizierate), (Şemsi) Ahmad Pasha (1579–1580) and (Sokullu) Mehmed Pasha (1565–1579). The texts added outside of the medallions give the Hijra dates for their respective vizierate, not any fancy 'star-like prophetic verses'.

A second instance concerns the German Protestant Salomon Schweigger (1551–1622), who visited the Ottoman Empire as the clerical member of a Habsburg embassy to the Ottoman court (1578–1581), not as a Habsburg

[5] I.e., I understood.
[6] I.e. for the rest of my life.

envoy as Ben-Zaken claims [24]. Ben-Zaken admits that Schweigger's report on Taqī al-Dīn is written in 'an unsympathetic tone', calling the Arab scholar 'a worthless astronomer', which is demonstrably contrary to the evidence that we possess in the latter's works. Neither Schweigger's prejudices nor his obvious incompetence when it comes to judging Taqī al-Dīn's scholarly 'worthiness' and his character—he slanders him as an 'artless charlatan, unholy rogue' [24]—moved Ben-Zaken to reflect on the 'trustworthiness' of his main witness for other 'exciting facets' of his story about Taqī al-Dīn and the observatory.

Taqī al-Dīn could not have acquired his knowledge in his own society nor could he have carried out his observations: so much was unquestionably clear to Schweigger. Hence, the German cleric proposed that the Arab scholar acquired his knowledge of Greek astronomical writings in Arabic by some 'secretly held Jew' [see 24]. Ben-Zaken, however, wants to go further. He claims on the basis of information from George Saliba [see 178n64] that 'Taqī al-Dīn knew Italian and was exposed, somehow, to Italian culture' [25]. The reference in footnote 64 is to Ambrogio da Calepino's (1435–1511) first Latin (1502), later multilingual, dictionary. Italian was only one of its languages. French was another one. Thanks to Hüseyin Sen (Utrecht), I have obtained a copy of this manuscript note. In contrast to what Ben-Zaken writes, there is no clear evidence that Taqī al-Dīn understood Italian, let alone that he had been exposed somehow to Italian culture. Neither did the Arab scholar note 'that it would be better to consult Italian sources and dictionaries' [25, ref. to n64] or 'that he read about Ptolemy in the dictionary of the multilingual Ambrogio da Calepino' [178n64]. Rather, he wrote:

واما المجسطي فمعناه الاعظم في لفتهم. هكذا قراءته في كتاب امروز طالبينو.

My literal translation:

> As for 'Almagest', it means 'The Greatest' in their scrolls.[7] I read this in the book of Amrūz Ṭāl-bīnū.

The hyphen indicates a short vowel that is not marked in the note. We are free to choose any of the three possibilities: 'a', 'i', or 'u'. In any event, we can accept that Taqī al-Dīn received some philological information from a

[7] Or: rolls, packages, turnabout. I think that it is a spelling mistake for 'لغتهم', which means 'their language'.

book by Ambrogio da Calepino, which will have been one of the various print versions of his dictionary. However, Taqī al-Dīn used the French form of the Italian author's first name and did so in a manner that seems to reflect a spoken rather than a written version. Thus, it is unlikely that he knew Italian and had been exposed to Italian culture. It is even doubtful that he had read the entry in its French version himself.

Not satisfied with his embellishment of Saliba's report, Ben-Zaken extends a storyline suggested by Schweigger. In this account, Taqī al-Dīn is cast as a prisoner captured by Italian pirates on one of his two sea voyages from Alexandria to Istanbul (1549–1552; 1560s), who spent some time as a slave in the household of an unknown Italian mathematician from whom he learned mathematics, astronomy, and further skills [25–27]. In order to boost his tale, Ben-Zaken cites three stories of three other captives. The only one of the three who was undoubtedly involved in sharing knowledge from one side of the Mediterranean to the other was the captive and temporary convert to Catholicism, Leo Africanus [25]. In his case, though, the transfer went from Arabic to Latin. Thus, he is not very convincing support for Ben-Zaken's story about Taqī al-Dīn. The second example is the Ottoman judge Muṣṭafā Efendi who was captured by the Hospitaller knights and imprisoned at Malta for two years before his ransom payment arrived and brought him back home [25]. According to his own description of the years of his captivity, he was never involved in any exchange of knowledge. Finally, the third example is the so-called Hajji Ahmet, the 'probable author' 'of a world map that was printed in Venice in 1560 and delivered to the Islamic world' [26]:

> [He] tells a story of woes, according to which he was a suffering captive in Italy. We learn, further, that he requested that his Muslim brothers purchase the map so that the income might be used to set him free. [26]

Footnote 72 [179] links these claims to a paper by Jerry Brotton [2000], though without giving a specific page number. This is not surprising since the text printed by Brotton contradicts both points made by Ben-Zaken, despite Brotton's own mistakes that make clear that he too was not able to read the quoted title of the map or its text in its original language, which he claimed was Arabic though it is Ottoman Turkish in both cases.

Brotton relied primarily on V. L. Ménage's analysis of the map, which proved that both map and text were full of linguistic, geographical, and historical mistakes that no educated Islamic scholar, the *persona* imputed to Hajji

Ahmet, would have committed. Ménage had reasonably concluded that the map and text had been concocted by people in Venice, and he presented archival documents from 1568 linking the printed sheet to two translators of Ottoman Turkish of the Venetian Republic—the well known Michele Mambre (or: Membrè) and Cambi, about whom almost nothing is known—and to the Venetian publisher of the map, Marc' Antonio Giustinian [Ménage 1958]. Brotton, however, writes about the map and its Ottoman Turkish text as though he were able to understand it, and as though it was he who had discovered the map's fabrication. Brotton thus aimed to deceive his readers about the language of the map and its texts as well as about the identification of some of the Venetians involved in its fabrication. In following Brotton, Ben-Zaken has not only ignored newly published studies by Antonio Fabris [1989] and Ben Arbel [2002] that counter Brotton's claims, he has also proved incapable of correctly summarizing Brotton's repetition of Ménage's results.

Brotton's incomplete citation of Menage's text is as follows:

> In the name of God, the Merciful, the Compassionate: O ye wise and O ye learned, the blessings of God be upon you! Be it known unto you that I...Hajji Ahmed from the City of Tunis.... became, through the decree of revolving destiny, a captive in Europe [Firengistan].[8] There I was bought by one of the Frankish lords, a good and learned man, so that I never lacked freedom to perform my religious duties or failed to fulfill them according to the rule and prescription of Islam; and thanks to the learning I had acquired the people here treated me with all honour and respect. Now the people of these countries have drawn and produced this presentation of the world according to the teaching of the philosophers of old, Plato, Socrates, Abu'l-Fida and the great Lokman and have in this map written down and communicated fully, according to the

[8] Surprisingly, the text left out by Brotton from Ménage's translation [1958, 107–108] is as follows:

(1) this poor, humble and feeble creature, who stands in need of the mercy of his Generous Lord,

(2) had from my childhood followed the *dānishmend*-course in the *medrese* of the city of Fez in the Maghrib. Over a long period I devoted most of my life to the zealous and persistent pursuit of learning and wisdom and an honourable name, but after I had acquired the desired,

At the end of this text, Brotton also left out this sentence:

(3) Thus it is my hope that I may be delivered with glory and honour from Firengistān and that the Self-Sufficient God may bring about His servant's return safe and sound to the lands of Islam.

> demands of science and logical arrangement, the facts concerning the Heavens and the surface of the Earth, in order that those who peruse it, of low and high degree, may draw great benefit from it. I therefore, on seeing this really excellent and important work, and realizing that it was of value and essential to all Moslems and their rulers, translated it systematically from the language and script of the Franks into the Moslem script; and they undertook to grant me my manumission as the reward of my labour. But I swear by the Mighty and Gracious God that the troubles and trials that I underwent before bringing it to this form are beyond description. However, praise be to God, Who has granted us understanding and solicitude for others, for by means of this valuable work I have become the instrument for benefiting all the Moslems. [Brotton 2000, 35–36].

None of the examples given by Ben-Zaken thus confirms that Taqī al-Dīn had been a captive in Italy or that he had learned his sciences as well as technical knowledge there or that he was instrumental, as Ben-Zaken claims in a further twist of the story, in the printing of (a 13th-century) Arabic version of Euclid's *Elements* by the Medici Press in Rome [25]. This twist is the result of Ben-Zaken's misunderstanding of Schweigger's German text and his ignorance of the year when the Medici Press published this Arabic text. The volume appeared in 1594, after Taqī al-Dīn had been dead for nine years. Ferdinando I de Medici had acquired the manuscript for the Press in a collection of more than 100 in 1586 from the former Jacobite Patriarch Ignazio Niᶜmat-Allāh Aṣfar of Mardin, who had taken refuge in Rome, i.e., a year after Taqī al-Dīn's demise. There was, then, no chance that he could have been involved in preparing the manuscript for print.[9] Furthermore, Schweigger never claimed that the Arab scholar was involved in this project in Rome. Instead, Schweigger was of the opinion that Taqī al-Dīn had translated the works of Ptolemy, Euclid, Proclus and other famous astronomers into Arabic:

> er bracht zu wegen Ptolemei/Euclidis/Procli/vnd andrer berühmbter Astronomorum Schrifften in Arabischer Sprach/. [Schweigger 1639, 91]

But this is utter nonsense.

Similarly serious mistakes can be found in Ben-Zaken's representation of Pietro della Valle's Persian text on Tychonic astronomy (composed in Goa in 1623–1624) and its Italian translation (executed in Rome in 1631). Some of

9 http://www.iranica.com/articles/italy-viii-persian-manuscripts-2.

them concern codicological aspects, i.e., the material appearance of the manuscript/s. Others concern the text more specifically. He writes on page 46:

> Della Valle's handwriting in the manuscript letter to al-Lārī appears in a column of Italian and a column of poor Persian, but also includes phrases and terms in Arabic, Ottoman-Turkish and Latin.

Given that Ben-Zaken here describes the text as consisting of two columns, one in Italian and the other one in Persian, and speaks of della Valle's handwriting, I had thought that he was referring to the language and vocabulary of MS Cittá del Vaticano, Biblioteca Apostolica, Persiano 9. This manuscript, however, does not contain Ottoman-Turkish or Latin words. That it contains Arabic words is not surprising given the substantial number of Arabic terms that make up medieval and early modern literary as well as philosophical and scientific Persian. But should he be referring to the Persian treatise and its Italian translation, then their description as columns is unfortunate, since the respective texts cover opposite pages and only the Italian translation was undeniably written by della Valle himself. Again, the Persian text quite appropriately contains Arabic but no Ottoman or Latin phrases and terms, except for a number of Latin forms of personal names like Khrīstufurus Burrūs (Christophorus Borrus), Tīkhūn Brāhah (Tychon Brahe), Pavlus (Paulus), Khrīsustumus (Chrysosthomos), Qusmus Midīqī (Cosimus Medici), Pinayda (Pineda), and (at the end of the text, but not in the marginalia) Kāliliyūs (Galileus), and Kaplarūs (Keplerus) as well as two technical terms in Latin and one Turkish term that was standard in Safavid administrative geography for 'province'.

The Latin terms are 'spīrah' and 'āpūkālīpsis' in the Persian text [ff. 12b, 27b] and in the Italian translation, they are 'spira' and 'apocalisse' [ff. 12a, 27a]. Della Valle explicates 'spīrah' by its Portuguese equivalent 'parafuso' and supplies an image of a spiral.[10] In addition, there is an Italian word, 'dūkā' ('duke') [f. 19b].[11] Della Valle added that he used the Portuguese 'parafuso'/'parafūsu' so that some traveler not knowing Latin but Portuguese who came through Lar might be able to translate the word into Persian [ff. 14a–b]. The Turkish term, not surprising for a dynasty whose vernacular was a Turkish dialect, is 'ūlkah' ('ülke') [f. 19b]. In sum, della Valle's Persian is not at all that poor, though it is certainly not on par with the sophisticated literary language of

10 'Spira' and 'parafuso' appear also on ff. 12a–b, 16a–b, 17a–b.

11 Compare the Italian translation on f. 19a.

the time nor even the language of contemporary scientific texts in Persian. In effect, this manuscript provides us with clues about della Valle's lack of familiarity with these two forms of cultural communication and, hence, with the literary and scientific texts even after his stay of six years in Safavid Iran. Still, it *is* a fascinating witness to spoken early modern Persian and of the kind of language that Catholic visitors of Iran were interested in and able to learn.

Ben-Zaken's subsequent claim

> ...but certain autobiographical insertions on the margins of the introduction and the concluding sections introduce the possibility that the Copernican cosmology, based on the Galilean discoveries, might be a better world system. [47–48]

cannot be supported on the basis of the two extant manuscript copies. The only insertions to be found in the margins are either corrections that the copyist added in rectifying his own mistakes or additions of words that he could not read before. A few free spaces are left indicating there might have been more problems that could not be solved. Granted, in the third of his four chapters, della Valle explicitly refers to Kepler, Galileo, and Grand Duke Cosimo II. He calls Kepler 'another astronomer, ...an observer of the *qayṣar* [i.e., Rudolph]' and 'famous among the Franks', and Galilei 'a famous observer in the province of Toscana, a province in our country Italy'; and he reports that the newly discovered 'four or five little planets which rotate around Jupiter' were named *midīqī* in honor of the Grand Duke. Given these clear statements about Galilei and Kepler, Ben-Zaken's mistaken description of the material properties of the manuscript and its text seems strangely unnecessary, though it is in line with his methods for telling the story of Taqī al-Dīn. Characteristic of Ben-Zaken's working practice is also the absence of any folio numbers in the manuscript that would specify where the alleged 'autobiographical insertions in the margins of the introduction and the concluding sections' occur [see 183n1].

Neither can Ben-Zaken's claim that della Valle 'mentions "the end of Kepler's life"' [185n42] be found in either the Persian text or its Italian translation. The reference made in this footnote is to ff. 2a–b where della Valle speaks only about Borri. The only time that della Valle speaks of Kepler is on ff. 21a–b:

هفتمی بقول منجمی دیگر کَپْلَرُسْ نام که هم اورا راصد قیصر ودر میان فرنگیان
مشهور است اینچنین است

> La settima per detto d'un altro astrologo chiamato Keplero, che'esso ancora è Mathematico di Cesare, e famoso tra gli Europei, es in tal guise.

Neither the Persian nor the Italian wording leaves any doubt that della Valle speaks of Kepler as being among the living. Ben-Zaken's erroneous claim turns out to be the result of his misinterpreting a passage on f. 4b because he surprisingly misreads a clearly written, standard Islamic formula indicating that a person mentioned by a name, here Rudolph II, had deceased: 'ghafara llāhu lahu' ('May God pardon him'). The passage is about Brahe and his work, not Kepler. The Italian translation (f. 4a) adds after the emperor's name equally clearly: 'che Dio gli perdoni'.

Three quotations from della Valle's Persian astronomy center on the Book of Job. In discussing them, Ben-Zaken speculates that della Valle used the Augustinian Diego de Zúñiga's (1536–1597) commentary on this book [61, 63]. While it cannot be excluded that della Valle read this work in Goa (which Ben-Zaken should have explored in the light of archival documents available in Goa and the collection of books in the Goan library formerly belonging to various missionary orders), della Valle does refer three times explicitly to the commentary by the Jesuit Juan de Pineda [ff. 23b,8; 24b, 14; 25b,3], something that Ben-Zaken has overlooked. As Ben-Zaken would have it:

> This is the abstract of the book of Christopher Borrus, which I translate. It has made me content, and I also agree with it. But, certain verses of Job the prophet raise a little doubt. The Book of Job was translated to Latin and was in the hands of observant believers, but the real Book of Job the prophet is in the language of Hebrew and Chaldean. [61]

> One should look for the original Book of Job in the original language. Therefore one should look for the saying of Job in the original language and what power of benefit his saying has. So if one would look at the original piece that is the statement of Job himself, that is good! But if it is the statement of God to Job then it is a command of God and we cannot say anything against it. [63]

> ...we do not have the Book of Job in Hebrew and Chaldean that could point out the cosmological truth. With God's will these original texts would someday resurface from the treasury of the basement of the Vatican. ...for the time being we could avoid relying on a sole source like the Vulgate [by] consulting commentaries on the Book of Job in Hebrew and Chaldean. [64]

But, on comparison with the Persian original and its Italian translation, it becomes clear that Ben-Zaken has misunderstood the Persian as well as the

Italian texts. To aid the reader, I provide della Valle's Italian translation of the Persian original followed by my own:

> Questo é il Compendio del Trattado del Pré Cristoforo Borro, che il pouero ha tradotto (cioè io ha tradotto) al quale queste due parole aggiungiamo che la detta opinione al pouero (cioè a me) piace assai: solo quella parola, che è nel libro di Job Profeta, da un poco di dubbio; perche Pineda, che ha scritto l'esplicatione di Job, la sua esplicatione l'ha fatta in lingua Latina. e sopra'l Libro di Job nelle medesima lingua Latina interpretato, nella traduttione di Lui che é riceuuta della Chiesa dei Latini cioè della Congregatione de' Fideli di lingua Latina; ma il Libro originale di Job Profeta in lingua Ebrea/e Chaldea è scritto. Per la qual cosa bisogna uedere quella parola nella sua lingua originale che forza, e che proprietà hà, e se la dichiaration di Pineda, conforme a quella uiene a proposito. Se la parola originale de detto Profeta è capace di questo significato, bene: ma se nò, la parola del Profeta Job è parola di Dio, et è di fede; non possiamo dir contra quella. ...il detto libro di Job Profeta in lingua Ebrea et Chaldea non habbiamo, per potere alla certezza di questo arriuare: ma sarà, piacendó a Dio, nel paese nostro Roma la grandissima, che è palagio di scienze; e como è Sede di Pietro, che fù capo della fede, e capo de' dodici Apostoli: et è sede del Successor di lui, e Vicario della Presenza di Giesù, che è il Papa, di là vien fuori ogni esplication de' libri della fede, che sia riceuuta dalla Chiesa vniversale. Là dunque meglio la certezza di questa opinione co'i libri Ebrei i Chaldei comprenderemo: non da noi solamente, ma co'l conseglio di molti sauij che in lingua Ebrea e Chaldea siano assai dotti.[f. 25a]
>
> This is an abbreviation of the treatise by Father Christophorus Borrus, which [this] poor man has translated. After it, we have added these two [statements], the opinion of which pleases [this] poor man. Only that [statement] which [is in the Book] of the Prophet Job gives a little doubt, because Pineda, who wrote the interpretation of Job, made his own interpretation in Latin. Furthermore (*bālāyī*), it was translated into the very same Latin language in his translation, which was accepted by the Church of the Latins, i.e., the community of the believers [who speak] Latin. However, the original Book of the Prophet Job was written in Hebrew and Chaldean language [*sic*]. Therefore, it is necessary to see what that [statement] is in its original proper language, which power and which property it has, and if Pineda's explanation may be [shown] to agree with it.
>
> If the original [statement] of the said prophet admits this meaning, fine. If not, [then] the Prophet Job's [statement] is a divine word and a Holy text (*naṣṣ*).[12] We cannot speak differently [than] it. ...we do not have the said book of the Prophet Job in Hebrew and Chaldean language [*sic*] so that we might arrive at the truth

[12] I.e., the text of the Qur'an.

> of this statement. But, if God wills it, all interpretation accepted in the Catholic Church of the books of faith will come from our country Rome the Great, which is the abode of knowledge, and because [it is] the throne of Peter, who was the head of faith and the head of the twelve apostles, and the throne of His deputy, (*khalīfe*)[13] that is, the deputy of Holy Jesus (*Ḥadharat-i ᶜĪsā*), who is the Pope. Then, we will discover the truth of this opinion in the Hebrew and Chaldean books there, not by us alone but with the advice of many scholars who are very learned in the Hebrew and Chaldean language [*sic*]. [f. 25b]

The problems posed by Ben-Zaken's stories are not limited to mistaken, misappropriated, or fanciful translations. Similar mistakes exist in regard to simple historical statements concerning dates, meetings, exchanged materials, or royal titles. Two examples will suffice to show this.

First, the caption on page 58 reads: 'Figure 13. The Pythagorean itinerary of Pietro della Valle, as illustrated in his journal'. The map shown is from Pierre Du Val (1619–1683), a French geographer, who began his career in 1662. He produced this map on the basis of the French translation of della Valle's *Viaggi* that was published in Paris in 1664. It was not part of the Rome edition of 1650, as Ben-Zaken maintains on the basis of a Houghton Library copy to which this map was added before the frontispiece. Neither was it part of his 'journal', i.e., his *diario*, which contains no maps of this kind, only sketches of local vistas, so to speak. Ben-Zaken's idea of a 'Pythagorean itinerary' of della Valle is as farfetched as his claim that the Italian traveler was on a relentless search for an ur-text of the Book of Job.

Next, we have the following amazing description of where and how della Valle first met the Italian Jesuit Christofero Borri at Goa:

> They stayed in the same monastery and met for the first time at a midday meal. They exchanged views about the various Eastern cultures they had explored. Borrus bragged of how he had impressed the Chinese *literati* by making accurate astronomical predictions, thus convincing them to convert to Christianity. In response, della Vella mentioned meeting a brilliant Persian astronomer Mullah Zayyn [*sic*][14] al-Dīn al-Lārī, who had firmly rejected the possibility of conversion. Borrus then offered to use the same approach that had proved successful in China: to send a translation of his book on the Tychonic system to al-Lārī, with the hope of convincing him that the advanced state of European astronomy

13 I.e., caliph.

14 This is not a typo on Ben-Zaken's part but his persistently mistaken transliteration of the Arabic word.

> resulted from religious superiority. Quickly agreeing, the two men—della Valle, trained in classical and Near Eastern languages, and Borrus, skilled in astronomy, cartography, and mathematics—worked to translate into Persian a short Latin work by Borrus on the Tychonic system. [47]

The primary sources available for evaluating this impressively detailed account are:

(1) della Valle's Persian treatise (with his Italian translation) on Borri's summary of Brahe' astronomy,
(2) various editions of della Valle's printed letters,
(3) della Valle's unpublished diary,
(4) a newly recovered manuscript of della Valle's journey that was auctioned on 13 November 2008 by Sotheby's,
(5) della Valle's unpublished letters and his notebooks compiled in Rome as well as
(6) three Latin versions of Borri's account of Cochinchina and Borri's later book on astronomy *De tribus coelis*.

Borri's works do not mention della Valle at all. Della Valle mentions Borri in the Persian treatise with Italian translation, in the printed letters, in a few of the original letters, but not in his diary.

According to the description of della Valle's autograph that was auctioned by Sotheby's, its text does not seem to mention Borri, although I cannot guarantee this since the three pages published by Sotheby's in its description of Lot 81 are not in a readable resolution and my efforts to contact the new owner through Sotheby's have so far been unsuccessful.[15] These pages do, however, bear on the difficult issue of dating the arrival of either of the two men at Goa, since della Valle explicitly states that he arrived on 8 April 1623 and wrote his notes about Goa on May 13 of the same year. According to Olga Dror, he met Borri for the first time on 10 April 1623, i.e., two days after his arrival [2006, 41]. But she was not in possession of any more specific information about the where, the when, and the what of their meeting. In contrast, she knew that Borri had lived several years in Cochinchina, the southern part of what was later to become Vietnam, and one year on Macao, but never in China. She also determined that his knowledge of the local language was so limited that he could not engage in a sophisticated debate with anybody, let alone

[15] http://www.sothebys.com/app/live/lot/LotDetail.jsp?lot_id=159488098.

a member of the Chinese *literati* who did not speak this language either, and that he did not understand the two main local religions, Buddhism and Daoism, very well. Furthermore, in his account of his time in Cochinchina, Borri remained vague about his personal role in the conversions of the locals, which other Jesuit sources contribute primarily or solely to a second missionary working there in this period [Dror 2006, 31–32, 37–39].

Thus, Ben-Zaken's story of the encounter between della Valle and Borri is at odds with the picture of the Jesuit and his activities in East Asia that is discernible in the extant sources. His mistaken claim about Borri's presence in China may be the result of a faulty inference from the stated intention of the Jesuit Society to send its member to a Chinese mission and his (mis)reading of della Valle's Persian treatise or its Italian translation:

اولکهٔ چین رسیده در بلدی نزدیک چین که اورا کوچینچین یا کاچوچین مگویند چند سال اقامت داشت.[16]

> ...et arriuato in fin a Cina, in un certo paese uicino a Cina, che lo chiamano Cocincina, o Caciocina. [MS Città del Vaticano, Biblioteca Apostolica, Persiano 9, f 2b, Italian translation f 2][17]

Unfortunately, the mistakes committed by Ben-Zaken are not exhausted by these examples. As we have emphasized, *Cross-Cultural Exchanges* may well qualify as a good narrative. Regrettably, it is nowhere near so successful as history.

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[16] The text adds a superfluous ' و ' before ' یا '.

[17] For the change of plans of the Jesuit Mission regarding Borri, see Dror 2006, 31–32.

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A Response to Laywine on Hagel, *Ancient Greek Music*★

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I am sad that as intelligent a reviewer as Alison Laywine was so deeply dissatisfied with my book. On the one hand she has convinced me that there are issues which deserve her harsh criticism, even if part of her disappointment (I hope) might perhaps be ascribed to the fact that the title misled the renowned Kantian scholar to expect an introduction to the study of ancient music, which she apparently came to embrace only recently. On the other hand, I find my argument so severely misrepresented both overall and sometimes in detail that I decided to write this response, in the hope that in this way the readers of *Aestimatio* will know better what to expect to find in the book.

To begin, Laywine could not locate 'a single, self-contained, coherent statement of [my] overall motivations' [168] and therefore embarks upon construing one on the basis of my doctoral thesis, published a decade earlier. Turning the volume around, she might instead have found the statement, 'This book endeavors to pinpoint the relations between musical, and especially instrumental, practice and the evolving conceptions of pitch systems.' This, I think, covers it nicely. I will give one example of the internal cohesion of the argument which Laywine is missing. Towards the end of my book, the necessary ingredients are collected (I trust) for tuning a lyre to the same pitch (within a semitone) and the same intervals (within a small fraction of a tone) as ancient lyres had been in the second century, in order to accompany a tune that had actually been composed for such a lyre. To accomplish this we need to understand the pitch range of the cithara [ch. 2] and how the tunings reported by Ptolemy relate to the keys of the notation [ch. 4] in comparison with other treatises [ch. 3], which in turn presumes a diachronic

★ See http://www.ircps.org/aestimatio/9/124-170.

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understanding of the evolution of this notation [ch. 1]. On the other hand, it is necessary to understand how the tunings support the specific 'modes' used at the same time [ch. 6], and it is a useful corroboration to see that the reported theatre resonators were in best accordance with music of just this type [ch. 7]. Finally, it is necessary to single out musical documents that were citharoedic [ch. 8] as opposed to *aulos*-accompanied pieces [ch. 9].

None of that was part of my thesis, which focused instead on modulation in making a case *inter alia* for complex, rapid, and remote modulation in the 'Delphic Paean' by Athenaeus. Inevitably, as my book talks about pitch-systems and the development of instruments, modulation is also a recurrent topic there. For this reason, Laywine lightly dismisses the requirement of entangling herself within the interwoven strands of the argument, which builds on concurrent evidence from archaeology, the texts, and the scores, sometimes with the help of statistical testing. Instead, she decides that the topic of modulation would provide a chance for upsetting the whole building with a single blow. Consequently, most of her review is dedicated to countering my arguments for remote modulation in the Delphic Paean [Hagel 2000] and the Ashmolean Papyri [Hagel 2010]. Take Hagel's speculative interpretation of these away, she implies, and the whole edifice collapses. This approach, I am afraid, is sadly flawed from the outset. It is true that my argument throughout assumes the widespread practice of modulation—but quite ordinary modulation between neighboring keys, which is well attested both in the texts and the extant scores, and whose existence nobody (including Laywine herself, as it seems for all practical intents) has ever denied. In contrast, what Laywine tries to refute with considerable knowledge and verve is the presence of remote modulation in the two mentioned fragments, modulation between keys that may even be located opposite each other in the circle of fifths. If the Ashmolean fragments are better explained in another way, I am quite happy to renounce whatever wrong I have said in the 13 pages dedicated to them. In the structure of my argument, however, the interpretation of these fragments does not form the basis of far-reaching conclusions. Laywine has mistaken a twig for the root, pruning for chopping down.

Admittedly, if Laywine is right, my book from 2000 is more seriously compromised. I will, therefore, take the liberty to address a couple of her arguments in detail (which is not to imply that I stand by every idea that I expressed back then). First of all, I wonder how Laywine incorporates within her general rejection of remote modulation in antiquity the fact that the treatises explicitly acknowledge it as part of musical practice.[1] If we take the texts at face value, the discussion about whether anything of the kind is going on in the Delphic Paean or the Ashmolean fragments in particular is of little interest for the general question: it was going on elsewhere, then.

Even so, let us review my claims about the second part of the Paean. Laywine first objects on a very general level that any suggestion about what went on 'inside Athenaeus' head' is pure speculation. Certainly so, but then any musical or textual interpretation is speculation—including Laywine's claims about what 'Hagel thinks' [136], 'believes' [127], 'must be thinking' [158] or 'apparently takes' [153] (the last is the most wrong). I fully agree with her that the whole dispute can ultimately be reduced to the 'question whether we have any good reason to describe the melody this way' [145].[2] I also agree that drawing parallels to the 'modern Western' musical tradition may be a useful tool for understanding what is going on. However, I think that Laywine's parallels do not adequately reflect important aspects of what is happening on the surface of the ancient melody and, therefore, ignore crucial points of my argument. My interpretation was based, firstly, on the fact that the 'odd' note (O) is embedded within the *Gebrauchsskala* of its context by providing the 'link note' (B) that ties it to the rest *via* the circle of fifths, though this note does not belong to the melodic repertoire otherwise but

1 E.g., Aristides Quintilianus, *De mus.* 1.11 [Winnington-Ingram 1963, 22.15–16]:

> All sorts of modulations take place between the keys, according to each one of the intervals, both the composite and the incomposite ones...their forms and structural cohesion can be perceived in terms of modulation from a note by a tone or a semitone, and generally by any interval, odd or even, downwards or upwards. They establish common ground between tetrachords: sometimes these differ by a semitone, sometimes by a tone, sometimes by larger intervals....

Modulation 'by a semitone' is equivalent to modulation across almost (if applied to a whole system) or exactly (if applied to tetrachords) half the circle of fifths, the most remote modulations of key possible in both the ancient and the modern Western system.

2 However, Laywine later takes a much more uncompromising stance [149]: even if it could be shown that only a single melodic interpretation is possible, this would still not reveal the composer's intentions because all the facts may be side-effects of an unknown intention. This is, of course, true and I capitulate before such a degree of philosophical rigor. No, we cannot know anything for sure!

appears only once—which I took to be a plausible signal of the composer's (playful) awareness of harmonic relations. Secondly, I have argued that the melodic and rhythmical structure indicates how the notes are arranged within different scale fragments that are well known from theory and the scores alike, and which an ancient listener would have 'recognized' just as we 'recognize' a major chord when its notes are played in succession. In contrast, Laywine relies on a notion of 'coherence' that is tied to individual notes rather than musical context in a way that strikes me as hardly useful. In particular, she believes that since all the other notes are present in the Phrygian scale—unfortunately she forgets the link note B—and since this is the basic key of the section, it would always be 'more natural' to interpret them as '—well, er, uhmm—Phrygian notes' [145] and to consider the 'odd' one out as—well, er, uhmm—odd ('exharmonic' [148]).[3] This is, I think, tantamount to arguing that if a piece is, by and large, in C major but for a while introduces an f♯ instead of f, it would be more natural to perceive the key as still C major rather than as a modulation to G major, because all the other notes are still the same.[4] Perhaps it depends on how long the 'while' must be in order to speak about modulation? At any rate, even the uncontested parts of Athenaeus' piece show, by means of the notation particular to specific keys, how quickly one can switch to another one and back. But that does not seem to be the issue anyway: Laywine would apparently agree about neighboring keys such as C and G. The preceding example is merely to show that her

3 Laywine tries to conceal the desperation behind the designation as 'exharmonic' by ascribing it the potential function of providing a semitone stop below a structurally important 'fixed' note, M [148]. This idea hardly stands exposure to the facts of the melody, where M frequently leads over to an emphasized O (emphasized by length and/or repetition), while the opposite is never the case. M, therefore, does not gain prominence from the presence of O. On the contrary, it lends prominence to it, just as is demanded within the framework of my interpretation of the notes in question as a couple of πυκνά a semitone apart where O is the 'fixed' note of the lower one. Where the higher πυκνόν emerges, its lowest note M is given weight in a similar way by reaching it from the note above.

4 Rephrasing Laywine [147f]:

> Too much is the same for it to be likely that g, a note that has by now so solidly established itself in our musical insight as being the dominant of C major, could be understood as the tonic of G major even be it in the company of f♯, and even if f♯ appear to be the leading note to a G major chord: g–b–d.

general argument, if applied generally, seems to entail absurd conclusions. Therefore, everything reduces to the question of melodic usage.

Here then is my own 'modern' parallel to what Athenaeus achieves (though drawing parallels is ultimately hampered by the fact that in the ancient chromatic genus notes may become harmonically ambiguous more easily than in our ubiquitous diatonic). Suppose the following:

> A piece's first movement starts off in C major with a brief introduction of an f♯ towards its end. The second movement uses f♯ on a regular basis (for instance in the context of g–b–d), while f is also present from time to time. Suddenly, a single odd g♯ appears. A bit later, there is another g♯ but one preceded by a c♯—not really that odd, after all. However, c♯ never turns up again. Instead, g♯ becomes really prominent, especially in the sequence e–g♯–b (upwards and downwards), which alternates with f–a–c (also upwards and downwards). No fewer than nine of such 'triads' are found in close succession...

Looking at such a score, I would yield to speculation and say that the composer intended to switch/modulate between two major chords a semitone apart, which theory might term 'E major' and 'F major'.[5] Following Laywine's argument, I suppose that she would prefer to label the whole second movement as G major with modulation to D major and an exharmonic g♯. I leave it to the reader to decide on the basis of the Paean's melody which kind of description appears more plausible.

But again, all this is peripheral for the project of the book under review, concerning which I have concluded my general plea above. All I have to add are a few details in which I find that either the evidence or my arguments

[5] Laywine repeatedly implies that I have claimed 'that we are really in Hyperiastian' [144] or even that somebody might have 'heard a modulation from Iastian to Hyperiastian'. This seems to be a misunderstanding. Actually, where I have used the name 'Hyperiastian', it is always enclosed in quotation marks, and 'Iastian' I used only as a means of clarifying the structure. Moreover, I have pointed out that in Aristoxenian terminology, which may have been more relevant at the time of the composition, the posited remote modulation would take place between 'high Mixolydian' and 'low Mixolydian', a terminology that 'may' have played a role [Hagel 2000, 73]. The intention of modulation by a semitone, however, is independent from the question whether the composer would have had a name for the keys. I only argue for the former.

are misrepresented. First of all, I am not aware of ever having assumed or argued that equal temperament played a role in ancient music-making [e.g., 127n2, 144, 153]. Above all, no actual scale or set of scales in a particular performance needed to be equally tempered: a 'Pythagorean' tuning, for instance, would satisfy all demands. However, Aristoxenus effectively maintained that the octave consists of 12 equal semitones, as was required to set up a full coherent system of modulating scales in theory; ancient notation basically reflects the assumption of a closed circle of fifths.

Related to this issue is Laywine's concern that some of my arguments 'would lead us to expect that the tonic chromatic would at least find special favor with Aristoxenus', while 'the surviving theoretical treatises do not seem to privilege the tonic chromatic' [165] over the alternatives of 'different shades of the chromatic, the enharmonic and its different shades, as well as the diatonic and its variants'. This appears to involve a twofold error. Firstly, my arguments by no means require the prominence of chromatic over diatonic or even enharmonic; rather, they entail the prominence, among diatonics, of a diatonic with semitones and tones (instead of three-quartertone intervals, and so on), and the prominence, among enharmonics, of a quartertone enharmonic. All this is warranted by the sources. Secondly, the notion that theorists do not favor the tonic chromatic over other shades of chromatic is plainly wrong. Among the Aristoxenian sources, some quote the tonic chromatic exclusively and all others, including Aristoxenus himself, treat it as the *typical* variant. Similarly, practically all the non-Aristoxenians who describe intervals in terms of ratios chose the numbers in a such way that they can only reflect the 'tonic chromatic'—from Archytas on, who derives the 'chromatic note' by means of a whole tone, through Eratosthenes and Didymus and the whole 'Timaeus' tradition up until Roman times. Ptolemy is the only one who also provides for a 'tense' version (which he gives as the citharoedic standard in the higher range; in the lower range, however, his χρωματική is still an exact whole tone above the bottom note).

Concerning my treatment of the ancient scores, Laywine [154n17] implies that my sole motive for assuming that the bulk of the notes in Pöhlmann and West 2001, no. 5 is restricted to a fourth is the fact that they are so in no. 6. In fact, no less than 69 out of the 71 preserved notes of no. 5 fall within the fourth in question. To most people, a percentage of 97% might warrant my designation as 'the major part'. Laywine also seems to insinuate that the seemingly crowded notes within the fourth in no. 6 might belong to different pieces after all. A glance at the fragments shows that this cannot be the case: ΥΠΜΛ are ubiquitously coupled with either T or N, and the latter two also appear in close context.

Against the 'rush to judgment' by Pöhlmann and West, Laywine defends an 'Arabic' interpretation of the Ashmolean scales by observing that her intervals are 'vastly easier to sing than the weird and horrible seventh diminished by an enharmonic δίεϲιϲ in what survives of the Orestes fragment' [160]. Actually, the quoted interval is not part of the melodic line at all but occurs between vocal notes and what has been taken as instrumental notes of disputed purpose (perhaps only to give the accompanist an idea of the intended 'harmonization'?). The 'horrible' intervals, therefore, likely do not indicate successiveness at all. In any case, there is hardly a question of them having been sung. Laywine subsequently proposes understanding the modulation not as one of key but of genus, finding 'nothing in Hagel's analysis that would exclude the possibility'. No wonder, since I have also suggested that, e.g., on page 267 (compare the synopsis on page 271).

Unfortunately, Aristides Quintilianus' 'Wing Diagram', which Laywine cites as a source for ancient notation [135n5], is not preserved in the manuscripts; perhaps she relies on the reconstruction in Barker 1989 [428f]?

More problematic is Laywine's remark about 'the lyre and the cithara' as providing the context for the theory of interval-ratios: 'no great surprise here because string-lengths can be readily compared in terms of musical ratios' [166]. Actually, difference in string-length plays no role on Greek or Roman lyres; and even if it did, it would not warrant any sort of comparison.[6] But Laywine plays the *ʿoud*, which may well explain why she is much more inclined to considering microtonal variants within a single performance than I am: such a practice is intimately connected with instruments with a (fretless) fingerboard and the musical cultures where these play an important role. A lyre, however, has no fingerboard and comparatively few strings; therefore, its notes are too precious a resource to waste it on mere microtonal

[6] This is because comparison of length between strings of different length presupposes that similar portions of both sound the same pitch. This can be ensured on an experimental instrument (in ways outlined by Ptolemy) but not on any ancient lyre that we know of.

variation (which however played a role in differentiating the individual tunings). Admittedly, the case might be different for an expertly played *aulos*; however, I still cannot see why Aristoxenian theory would not have incorporated a 'modulation according to shade' alongside the other four types of melodic modulation if it was common in practice.[7]

Finally, it is of course mainly the fault of my user-unfriendly presentation if Laywine sometimes misses the essential connections of the argument.[8] As an example, she complains that the solution of the fundamental riddle expounded and allegedly solved in my chapter 1 'did nothing to advance later discussion in the book' [168]. Actually, it forms the basis for relating Ptolemy's work to the rest of ancient music. Has Laywine missed the point that Dorian eventually turns out to be, in some sense, Lydian (and not Hypolydian as she only quotes), and that this is essential for figuring out why a tuning that Ptolemy describes as instantiating the Dorian key would be called λύδια? Or has she failed to realize that the relation between Ptolemy's system of keys and the keys of notation had not been figured out before? At least, her review never mentions this topic, which I would have considered one of the book's major achievements. But since she never expresses doubts about this point either, I may perhaps console myself with the warming thought

7 Here we cannot really 'conjecture "til the cows come home" [157]. If ancient writers present a list of possible melodic modulations, evidently implying that it is exhaustive, this leaves little room for speculation that another one was 'discussed in treatises or parts of treatises that have been lost'. Nor is it really an option, at least not without specifying a possible motive, to have Aristoxenus exclude from his theory a kind of modulation which was part of late classical music, which was reflected in notation, and which could be described within his framework straightforwardly («πέμπτη δὲ κατὰ χρόαν ὅταν μενόντων τῶν δυνάμεων καὶ τοῦ γένους κινῆται τὰ διαστήματα»). I appreciate Laywine's caution concerning an argument from silence; but sometimes general scepticism may be dissipated by greater familiarity with the evidence.

8 Not always is Laywine herself a model of helpfulness. When she informs us that my portrayal of the presence of lots of notes within a narrow range (such as six of them within a fourth) as a sign of sophistication is 'simply false' [155], simpler minds like mine may crave an explanation or an example of non-sophisticated music with comparable characteristics. Perhaps, though, we have different ideas of 'sophistication', which I do not necessarily consider as laudatory and would probably not apply to the great melodies of three notes that she cites, even though I would almost certainly agree that they are great. My fault then, as the non-native speaker.

that, at the end of the day, even a harsh critic accepted crucial points of my argument, even if I cannot be sure whether she was aware of the fact.

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The Archimedes Palimpsest edited by Reviel Netz, William Noel, Natalie Tchernetska, and Nigel Wilson

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The book is an outcome of the project of reading with modern techniques the so-called 'Archimedean palimpsest' (= Codex C), a prayer-book or εὐχολόγιον retaining beneath its surface-text some writings of Archimedes and Hyperides as well as portions of a commentary on Aristotle's *Categories*. The Archimedean texts were identified and read for the first time when the Danish scholar J. L. Heiberg first inspected the palimpsest in Istanbul in summer of 1906; the non-Archimedean fragments have been identified in the earlier stages of this project. After Heiberg's travels to Istanbul, Codex C was stolen and underwent several vicissitudes until it was recovered and then acquired by an anonymous collector at an auction in 1998. It is now located with the owner.

The importance of the palimpsest can be understood immediately if one looks at the Archimedean manuscript tradition. This tradition, for most of the treatises, derives from three independent sources:

(a) the 10th century Codex C standing alone, the subject of the book under review;
(b) the lost Codex A, which can be reconstructed from a series of apographs of it that were made between *ca.* 1455 and 1 January 1544;
(c) the Latin translation of William of Moerbeke, achieved *ca.* 31 December 1269 and based partly on Codex A and, most importantly, partly on a further Archimedean exemplar, Codex B, last heard of in 1311.

Therefore, only Codex C has survived among the Archimedean manuscripts written before the 13th century, while the texts contained in Codex A and B can only be reconstructed by standard philological methods. What is more, the palimpsest is our unique source for two Archimedean treatises: the

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Stomachion (of which, however, only a short fragment remains) and the celebrated *Method*, addressed to the distinguished Alexandrian scholar Eratosthenes. Codex C contains also an almost complete Greek text of *On Floating Bodies*, which otherwise can only be read in William of Moerbeke's translation. It was on account of this new evidence that Heiberg published in 1910–1915 a critical edition of Archimedes' *Opera omnia* which replaced his earlier edition of 1880–1881 [cf. 1907].

The book under review is the 'official' outcome in print of the project of restoring, conserving, and reading the palimpsest with modern techniques, a project that was developed at the Walters Art Museum in Baltimore where the palimpsest was located until recently. The result of this renewed reading is a series of digitally-processed images of the leaves of the manuscript, which are stored on the palimpsest website [http://www.archimedespalimpsest.org/]. These images can be downloaded freely and are also reproduced in the book.

After an introduction by William Noel, who was apparently the driving force of the whole project, volume 1 is divided into five parts:

- a detailed codicological description of the manuscript;[1]
- the history of the codex from the making of the εὐχολόγιον to Heiberg's travels to Istanbul to read the Archimedean texts;[2]
- a monothematic section by Abigail Quandt on 'Conserving the Archimedes Palimpsest';
- descriptions of the image-processing and organization of the data mounted on the palimpsest website;[3] and
- a presentation of the texts.[4]

Volume 2 contains digitally-processed images of (almost) all leaves of the palimpsest (either recto or verso of a single folio in one single image) with

1 This part was collectively authored by 'Abigail Quandt and the editors', assisted by S. Lucà, S. Parenti, and J. Lowden.

2 In succession: 'The Making of the Euchologion' by A. Quandt, 'The Strange and Eventful History of the Archimedes Palimpsest' by J. Lowden, and '*Itinera Archimedea*: On Heiberg in Constantinople and Archimedes in Copenhagen' by E. Petersen.

3 In succession: 'Imaging and Image-Processing Techniques' by W. A. Christens-Barry, R. L. Easton, Jr., and K. T. Knox; 'Imaging with X-Ray Fluorescence' by U. Bergmann; and 'The Palimpsest Data Set' by D. Emery, A. Lee, and M. B. Toth.

4 In succession: 'The Palimpsest in Context' by N. Tchernetska and N. Wilson, and 'The Place of Codex C in Archimedes Scholarship' by R. Netz.

facing transcription. The order of the images is such as to provide a continuous reading of the works in the palimpsest; the foliation of Codex C being thereby perturbed, the reader can locate specific folios by resorting to the useful 'Concordance of Foliations' that closes volume 1. When the original folios are too damaged to produce readable images, these are replaced by Heiberg's photographs or, if none of these was available, by scans of his critical edition of Archimedes [!]. (In the latter case, I have been unable to find indications as to what the facing transcription corresponds to.)

The transcriptions have been carried out by a host of scholars. In the case of the Hyperides texts, the job was done well before the publication of the book.[5] The Archimedean writings were transcribed by Nigel Wilson and Reviel Netz.

The Archimedes Palimpsest has several merits: it presents all images in a handy format, though for more refined investigations the images stored on the website are better ('weighing in' at over 250 Mb each). Further, it collects in a single publication the transcriptions and an introduction to the non-Archimedean texts, explains in detail the image-processing techniques, and offers a most interesting exposition of the actions and techniques that were used to conserve the palimpsest. Most chapters of the book are pleasant to read and even entertaining. It is, however, less satisfying if one wishes to use it for scholarly purposes.

Let us say first that the only material of any value about the non-Archimedean texts is the transcriptions. The scanty and quite generic information on these texts presented in the section 'The Palimpsest in Context' (2½ pages on Hyperides, 3 pages on the commentary on Aristotle's *Categories*) does not even provide a full bibliographical record in the first case, and, in the second, consists in no more than an inconclusive discussion of authorship and some paleographical notes.

So let us then turn to Archimedes. I shall focus first on the 'diacritic and punctuation' section at 1.46–47. There are four pieces of information in it requiring comment.

(1) Regarding the presence of an 'unexplained abbreviation' in *Spiral Lines*, prop. 24, one reads that 'the required text is τριπλαςίων ἔςτω'.

[5] See the bibliography appended at the end of this review.

In fact, it is simply «τριπλασίων», as the transcription at 2.173 has it. The abbreviation is clearly visible even in the facing image: it is a «Γ» with a superimposed «π». The explanation is straightforward: in mathematical manuscripts, «Γ» (usually, «Γ̄») is the cardinal ‘three’, «Γʹ» is the ordinal ‘third’ or the aliquot part ‘1/3’, «Γ» with a superimposed «κ» stands for the adverb «τριάκις»,[6] «Γ» with a superimposed «π» stands for «τριπλασίων».[7]

(2) It is reported that the sign for «ἔστω» is ‘fairly rare but not totally unknown’. Hundreds of instances of it can be found in reading mathematical manuscripts [see also 40n13 below]. Where do we have to set the threshold for a sign’s being no longer ‘fairly rare’?

(3) A variant of the sign for «ἔσται» in the *Method*[8] is described as ‘a semicircle with two dots’. It is said to be ‘exceptionally rare’ on the grounds of evidence that we owe to G. Vitelli and dating to 1885.[9] More details would have been welcome, as some strokes of the palimpsest’s sign might no longer be visible and insensibly different variants of it are attested: four occurrences of one such variant occur in the first folio of Vat. gr. 218 [see Figure 1].

Figure 1. The sign for «ἔσται» in Vat. gr. 218, f. 1

In addition, it is questionable that what can be seen in the 250 Mb digital image can be termed without hesitation ‘a semicircle with two dots’: Heiberg read or guessed the standard sign for «ἐστι» (an oblique straight stroke with two dots in the same positions as the

[6] The Cod. Matrit. 4678 (Diophantus) offers many occurrences of this abbreviation, and for several numeral adverbs.

[7] Or «τριπλάσιος». On what grounds, then, did the editors choose the former? Of course, the fact that this is the reading of the other branch of the Archimedean tradition should not interfere with a transcription of Codex C.

[8] At f. 158r, col. 1, line 6 = 2.120 = Heiberg 1910–1915, 2.500.4.

[9] The sign can be found in Laur. Plut. 32.9 and is recorded in Allen 1889 and Cereteli 1904.

implied one for «ἔϲται») and I suspect that his reading should be retained.

(4) An abbreviation closing propositions 3 and 4 of the *Method* that looks like «ΟΙ» and apparently stands for the canonical «ὅπερ ἔδει δεῖξαι».[10] This is left unexplained: the authors recall, just to dismiss the connection, that the 'combination of the first and last letters of the words abbreviated reminds one of *nomina sacra*'. Yet the bar on f. 63v is quite distant from the underlying letters in comparison with the location it has when it marks denotative letters. Maybe the copyist only found in his exemplar, and misunderstood, the residual horizontal stroke of a superimposed «π», which was in fact a canonical abbreviation for «ὅπερ» [see, e.g., Ephrem's Euclid in Laur. Plut. 28.3] followed by some diacritical sign that he misread for an «I». Ending a proposition with a simple «ὅπερ» + sign[11] is not uncommon, as we gather from Pappus' Vat. gr. 218 [see Addendum, p. 44]

Turning from the 'diacritic and punctuation' section to the section on 'codex C and Archimedean scholarship', I give three examples of its unreliability, bearing respectively: on the treatment of the 'Archimedean scholarship' in question, on the evidence coming from the figures, and on that coming from the transcription.

First, Netz asserts that *On the Sphere and the Cylinder* 'is written in pure Koine dialect, no traces remaining of Doric' [1.277]. This is strictly speaking false, as already noted by Heiberg [1879, 69–70], since the word «τῆνοϲ» is Doric [f. 109v, col. 2, line 2 = 2.190 = Heiberg 1910–1915, 1.4.15]. Netz suggests that this treatise was originally written 'in (some version) of Doric, which then becomes koinicized in the milieu of Eutocius', and asserts that this is 'the *communis opinio*, to the extent that anyone other than [he] has opinions on the matter' [1.278]. Contrary to what Netz suggests, this really is a *communis opinio*, since it has been part of Archimedean scholarly folklore since Heiberg's 'Philologische Studien zu griechischen Mathematikern' [1883, 543–544]. Still, there are serious problems with this view. On the one hand, the Archimedean lemmata accompanying the Eutocean commentary *On*

[10] At ff. 63v, col. 2, line 30 = 2.84 = Heiberg 1910–1915, 2.454.7; 44v, col. 1, line 36 = 2.88, which was not read by Heiberg.

[11] On f. 44v of the palimpsest, the abbreviation is followed by the usual sign ':—'.

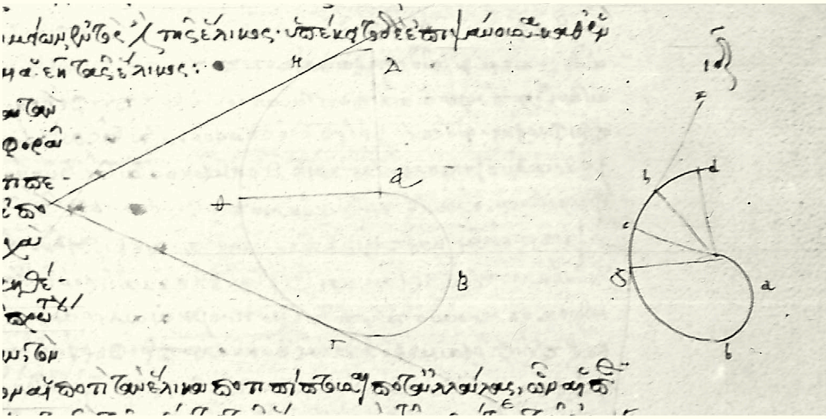

Figure 2. The diagram of *Spiral Lines* prop. 13 in Marc. gr. 305, f. 70r

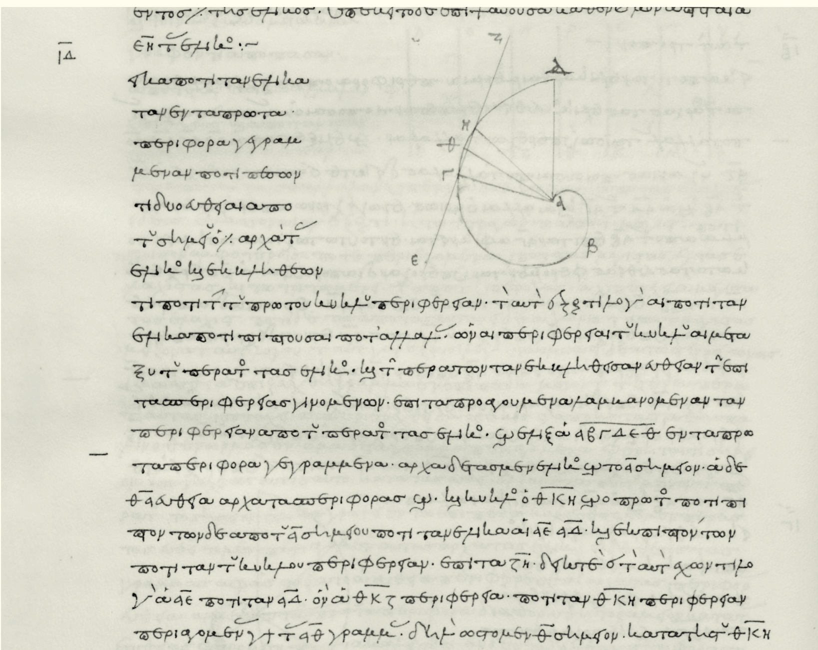

Figure 3. The diagram of *Spiral Lines* prop. 13 in Laur. Plut. 28.4, f. 79v

the Sphere and the Cylinder are in Koine; on the other hand, Eutocius himself asserts that he had recovered what he took to be a lost Archimedean appendix to *On the Sphere and the Cylinder* because it retained in part the author's 'beloved Doric dialect', and that he set out to rewrite it. Since a similar claim is not made concerning the main text of *On the Sphere and the Cylinder*, one may submit that the doricisms of this treatise were lost before Eutocius began his commentary on it but that he took it as obvious that the Archimedes should have written his treatise in Doric.

Second, the evidence from the figures is treated unreliably. Let us consider the nearly incredible 12-line paragraph at 1.284, inclusive of footnote 51. The aim is to show that '[a]ncient diagrams seem to wish to emphasize the impossibility of an impossible case' envisaged in a proof by *reductio*. A 'very clear example' is allegedly provided by the figure associated with *Spiral Lines* prop. 13 and said by Netz to be '[his] reconstruction' of a diagram representing as a broken straight line the impossible tangent at two points of a spiral. The figure presented by Netz cannot be termed a 'reconstruction' because it is attested exactly as it is reproduced, in Codex C (with the omission of the letters «E» and «Z») and in two apographs of Valla's lost Codex A, namely, in Marc. gr. 305, f. 70r [see Figure 2, left], and Par. gr. 2361, p. 204. What Netz omits to say is that the other two apographs of Codex A [Laur. Plut. 28.4, f. 79v, and Par. gr. 2360, f. 51r] and William of Moerbeke's translation in Vat. Ottob. lat. 1850,[12] which most probably derives from Codex A itself, have two figures different from the one just seen but similar to each other: these are reproduced from the first manuscript as Figure 3 and from the second as Figure 4 below.[13]

12 And in the margin of Marc. gr. 305, as we see again in Figure 2. Note that it is a figure with Latin lettering, identical with the one in the Vat. Ottob. gr. 1850; this phenomenon is unique in Marc. gr. 305.

13 In Figure 3, I have included also a stretch of text from *Spiral Lines* prop. 14 in order to show four consecutive occurrences of the sign for «ἔϲτω» discussed under point 2 above; these are all contained in the three lines centered on the horizontal stroke on the left margin. The reader can easily estimate by extrapolation how many occurrences of this sign are found in Laur. Plut. 28.4, written in an imitative script by Johannes Scutariotes in about 1491–1492 [Rollo 2012].

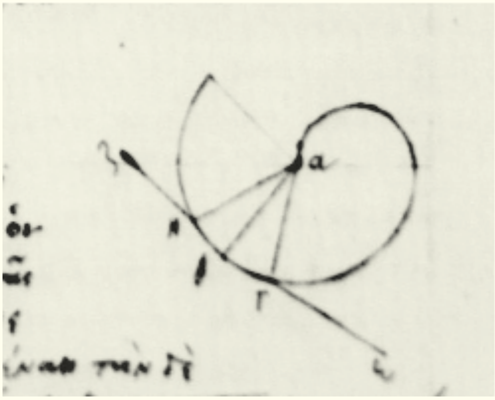

Figure 4. The diagram of *Spiral Lines* prop. 13 in Par. gr. 2360, f. 51r

It follows from this that Codex A had two figures, a 'weird' and a more 'regular' one. The 'regular' diagram was added, probably, in the margins at some stage of the tradition, simply because the former does not represent the 'impossible' configuration supposed in *Spiral Lines* prop. 13: the *reductio* proves that straight lines ZE and ΘA intersect each other somewhere between Θ and A, which is subsequently shown to be impossible. As a consequence, the 'weird' diagram does not even represent the 'impossible case': it is simply and plainly wrong. Furthermore, one might ask what is a 'weird' behavior of a straight line and what is a more 'regular'one. Netz expends a rhetorical question and an exclamation mark to highlight the 'contortions' that the (broken) line 'has to go through'! Well, just one 'contortion', the point of inflexion. Still, it is debatable which is the line that has had to go through more 'contortions', whether it is

- the one in Figure 2 (left)—recall that for a Greek geometer a broken straight line remains just a single, though broken, straight line,
- the one in Figure 3: a tangent that crosses a curve—quite an impossible object after all, or
- the one in Figure 4: the 'straight line' that has a curved portion, as it partly coincides with the spiral—this is Heiberg's figure.

But this is not the end of the story. It remains to read footnote 51; I quote it in full, inserting my comments in italics:

> The figure itself is identical in Codices A and C; [*This is false, as we have just seen.*] however, Codex A [*It should be 'Codex C'.*] omits the letters «E» and «Z» (once again we see an error in the mathematical execution [*What does this mean?*] of Codex C; not that Codex A is free of such mistakes). This diagram in Heiberg is not only geometrically different [*Of course, since he chose the other figure that is attested in the manuscripts.*] but also, nearly uniquely, contains a misprint: O for Θ. [*There is no misprint in Heiberg's edition: Netz apparently has in his hands the phototypeset reproduction made in 1972 of the 1913 volume. Such reproductions, as often happens, tend to fade out some details of the letters. In the reprint of 1972, the horizontal stroke of the «Θ», which features as it should in the original figure of 1913, has nearly disappeared, the outcome being an «O» with an irregular internal outline. It is easy to check this by looking at the same figure in Heiberg 1880–1881, 2.56, a complete scan of which is available online. Of course, Heiberg recycled the clichés of the diagrams from his first edition to the second.*]

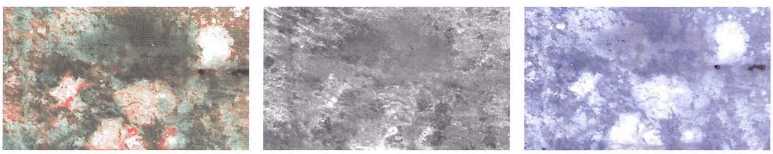

FIG. 11. Enhancements of the word *plethos* (multitude) in Codex C, fol. 177v + 172r 1.1, left: processed pseudocolor image, center: processed subtraction image, right: processed ultraviolet image

Figure 5

Third, the evidence from the transcribed text is treated unreliably. The entire interpretation of the *Stomachion*,[14] a work preserved only in the palimpsest, as dealing with combinatorics 'hangs on' reading a «πλῆθος» that Heiberg 'missed' [1.316n78]. Three images are adduced at 1.293 as evidence for there being such a word [see Figure 5]. I challenge anyone to see it. Heiberg had about 15 working days to read the palimpsest in Istanbul; he was granted no more than six hours a day—still, on the way back from his last journey, he wrote to a colleague of his in Copenhagen that, after all, it is dangerous to stare too long at letters: they tend to generate whole words. Staring at digital images apparently has the same effect. But there is more to the issue. Netz only says at 1.316n77 that the clause containing the crucial word must be corrected to accommodate for the presence of «πλῆθος» («ὀλίγον» for the palimpsest's «ὀλίγων»); the correction is tacitly included in the clause

[14] I urge the reader conversant with Italian to look at Morelli 2009.

when this is discussed at 1.293 but the 'official' transcription at 2.285, has «ὀλίγων»—thankfully.

The goal of the 'transcription' of the Greek text is 'to produce the best reconstruction possible of the reading in the codex as it existed in the tenth century'; therefore, it

> was made on the basis of images of Codex C, Heiberg's reading of the manuscript as they can be deduced from his critical edition..., and on the immediate textual context of the characters no longer visible. [2.vii]

How can this be called a 'transcription'? Any reading can be justified resting on such principles. These have also the harmless but disturbing consequence of making the authors encumber their apparatus with 100s of doricisms restored by Heiberg in his critical text of the still 'unkoinicized' treatises.[15] To give an extreme example, the apparatus to the transcription on 2.19 [ff. 14v + 19r] counts 113 items, 108 of which are pseudo-variants indicating restored doricisms: 27 «ποτί» instead of «πρός», 41 «τᾶς» instead of «τῆς», and so on. In the transcription of the inscriptions and subscriptions of the Archimedean treatises, there is also a mistake: one of the crosses surrounding the inscription of *On the Sphere and the Cylinder* at f. 109r, col. 2, is taken for an abbreviation of an article «τῆς», so that at 2.189, we read the ungrammatical title «ΑΡΧΙΜΗΔΟΥϹ (ΠΕΡΙ) Τ(ΗϹ) ϹΦΑΙΡΑϹ (ΚΑΙ) ΚΥΛΙ(Ν)ΔΡΟΥ».[16] Further, several figures are drawn incorrectly; in each case, the erroneous diagram quite appropriately counts as a separative variant with respect to the 'readings' attested in the tradition of the lost codex A, thereby enhancing the alleged divergence between A and C.[17]

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15 Heiberg listed all of these interventions at 1910–1915, 2.x–xviii.

16 I owe the example of the inscription of *On the Sphere and the Cylinder* to D'Alessandro and Napolitani 2012.

17 Recall that one of the disturbing features of Codex C is that its text quite often coincides with A's: as Heiberg put it, Codex C

> saepius, quam exspectaueris, cum A in erroribus conspirat, non modo in lacunis..., sed etiam in erroribus minoribus.... [1910–1915, 3.lxxxix]

For further details concerning the incorrectly reported figures, see again D'Alessandro and Napolitani 2012.

Providing a diplomatic transcription of what can be read now in the (digital images of the)[18] palimpsest would have served the needs of the scholarly community far better than this un-philological patchwork. Scholars seriously interested in the Archimedean palimpsest are advised to spend a night downloading the images from the website instead.

Addendum to item (4) on p. 38

A very similar abbreviation, followed by the canonical paragrapheme *dicolon + paragraphos*, can be found as the last sign of *Alm.* 2.2 in the most authoritative manuscripts of Ptolemy's treatise. It is located as follows: Par. gr. 2389, f. 28v [Figure 6], Vat. gr. 1594, f. 29v [Figure 7], Marc. gr. 313, f. 56v [Figure 8], Vat. gr. 184, f. 96r [Figure 9]. On the grounds of Vat. gr. 180, f. 27v, where one reads «ἐδει δειξαι» [Figure 10], Heiberg prints «ὅπερ ἔδει δεῖξαι» in his edition [1898–1903, 1.92.15 app.].

Figure 6. Par. gr. 2389, f. 28v

Figure 7. Vat. gr. 1594, f. 29v

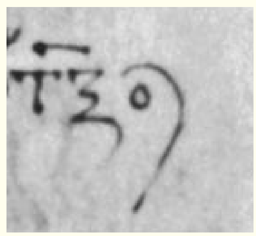

Figure 8. Marc. gr. 313, f. 56v

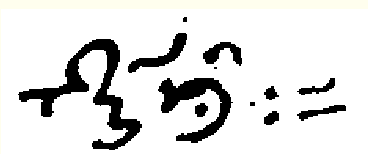

Figure 9. Vat. gr. 184, f. 96r

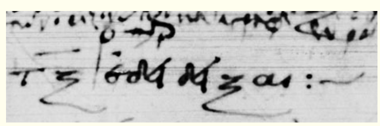

Figure 10. Vat. gr. 180, f. 27v

18 As said above, the low quality of the images printed in this book makes them useless for this purpose.

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Fra Mauro's Mappa Mundi and Fifteenth-Century Venice by Angelo Cattaneo

Terrarum Orbis 8. Turnhout, BE: Brepols, 2011. Pp. 470. ISBN 978–2–503–52378–1. Cloth €96.85

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Of all the known medieval and Renaissance maps, none is more famous than Fra Mauro's *Mappa Mundi* of 1449–1460. This large table-sized map, with beautiful and intricate illustrations, bristling with descriptive legends, is often used for illustrations in modern world-history textbooks. And yet, this is a map that defies categorization or full explanation. Historians for many generations have argued as to whether the map represented the end of medieval cartographic knowledge or the beginning of new cartographic understanding in the age of exploration; whether its use of vernacular (Venetian) was indicative of its parochial nature; and whether it was influential or ignored. Angelo Cattaneo, in his magisterial account of the life and times of this important artifact, is firmly convinced and convincing that Fra Mauro's map was embedded in his time and place, that it was more modern than medieval, that it was well appreciated and understood, and that it supplied an important step in the development of early modern cartography.

Fra Mauro, a *converso* monk in the monastery of San Michele di Murano in Venice, appears to have devoted much of his intellectual life to an understanding of world geography, with a large map of the world as its result. The map, created between 1449 and 1460 (there is some uncertainty as to the final date), depicts the whole known world (the *oecumene*), including Europe, Asia, and Africa. Cattaneo gives us a full description and understanding of Fra Mauro's work, as an image, as a text, and as a beautiful artifact.

The *Mappa Mundi* was a huge circular map designed in the first instance for hanging on the wall and it included over 300 legends, seven large ones in the four corners outside the map and many others within the map, describing places, people, and, most particularly, trade goods and potential. The outer

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legends presented Fra Mauro's cosmographical world, including translations from Aquinas and a beautiful rendition of Paradise. Cattaneo traces the sources of many of these legends, which include Marco Polo and Conti, and he identifies the artist responsible for the illustration of Paradise, Leonardo Bellini. From both the map and the legends, we see an author who views the world as complex but navigable, open for trade rather than conquest. He is a Venetian, after all. The *Mappa Mundi*, according to Cattaneo, is a complex and up-to-date scientific work, popularizing important natural philosophical debates and ideas.

Cattaneo illuminates the interaction within the *Mappa Mundi* of authoritative texts and modern travel narratives. Using Asia as an example, he demonstrates that Fra Mauro has read and understood Marco Polo's work but has not slavishly copied it. (Ramusio had believed that Fra Mauro was using a lost map of Polo's but this is clearly not the case.) Rather, Fra Mauro made use of what he saw as the most up-to-date information, using both Polo and the more recent account by Poggio Brocciolini of Nicolò de' Conti's Indian Ocean voyage, and correcting them as appropriate. Cattaneo shows that Fra Mauro drew on these travel narratives in four ways: by using toponyms, by paraphrasing passages especially with regards to trade routes, by creating images based on them, and by his own narrative style. Fra Mauro reworked these sources, correcting when he knew information from other sources. Essentially, Fra Mauro read these two authors as trade guides to the global spice trade. This may tell us how Venetians in general read these two great travel accounts and certainly shows that the *Mappa Mundi* had a practical mercantile thrust.

Part of the ongoing debate about Fra Mauro has been his use of vernacular. Does this show that he was unlettered? That the *Mappa Mundi* was designed for the less scholarly? Cattaneo argues that Fra Mauro was well educated, a humanist, and yet also scholastic—in other words, a man of his time. According to Cattaneo, Camaldolese monks at the time often used vernacular in order to reach a larger audience; and Fra Mauro fits into this pattern. He also shows that Fra Mauro had read a large number of scholarly works, citing 40 different works on the *Mappa Mundi* itself. He was one of the first to cite Strabo and relied heavily on Thomas Aquinas and the commentators on Aristotle. Although Fra Mauro tried to read everything in his field, Cattaneo argues that he should not be seen as a medieval encyclopedist since he wished to have his own view of the world, not just a complete one from the past. This can be seen, according to Cattaneo, by the many legends in which Fra Mauro states his own opinion. So, argues Cattaneo, we should view Fra Mauro as a modern man, in part a humanist (but still keen on ancient texts), not an old-fashioned encyclopedist, but someone participating in the changing intellectual world of 15th-century Venice.

Cattaneo takes on several other interesting discussions about this famous map in standalone chapters at the end of the book. He has an interesting chapter examining the cost of Fra Mauro's *Mappa Mundi* relative to similarly sized works of art of the period. His conclusion is that the *Mappa Mundi* was a luxury good but at the lower end of such goods in cost. A final chapter looks at the role played by this map in the 19th-century creation of the discipline of the history of cartography, arguing that this *Mappa Mundi* was an important artifact of study as scholars worked to develop this discipline.

Perhaps unavoidably in a book of this type, there are some problems. Cattaneo takes too defensive a position, arguing rather repetitively for the modernity of his subject. He occasionally sets up his opponents as more strident than they are and, therefore, his arguments are not always as subtle as they could be. The claim that Fra Mauro is the end of medievalism rather than the beginning of modernity is an old one and Cattaneo's more complex rendering of this 15th-century monk and his works could have stood well without the argumentative rhetoric. Further, it would have been better to have had a good concluding section rather than dissipate the argument in the final section on cost and historiography.

That said, Cattaneo's is a convincing case. Fra Mauro's work was an important contemporary intervention in the growing geographic and cartographic knowledge of the late 15th and early 16th centuries. The *Mappa Mundi* should be seen as an important dialogue between ancient and modern, humanistic care of older sources weighed with contemporary eyewitness accounts. The use of vernacular should be seen as important popularization of natural historical and philosophical ideas rather than as indicative of some monastic backwater. The world was poised for new discoveries and connections, and the lack of America on the map should not blind us to its importance for the European world of cartography and trade.

The Genesis of Science: How the Christian Middle Ages Launched the Scientific Revolution by James Hannam

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Shortlisted for the Royal Society Science Book Prize, James Hannam's *The Genesis of Science* is a work of incredible breadth, weaving a substantial pattern of medieval progress and scientific achievement. The argument is a significant one. Building on foundational studies of natural philosophy by scholars like Edward Grant, David C. Lindberg, and Lynn Thorndike, the book champions the idea that 'natural philosophy in the Middle Ages led to the achievements of modern science' [xxi]. Unlike its predecessors though, *The Genesis of Science* is written for a popular audience. Also, much of the book is an act of intellectual iconoclasm, combatively confronting the pervasive idea within pop intellectualism that the medieval world was scientifically backward. The book is directed at overthrowing the gross misperceptions, caricatures, and generalizations which depict a medieval world where 'there was no science worth mentioning' [xiv]. This metanarrative, Hannam explains, has been promulgated from the Renaissance to the present day as the dominant understanding of scientific progress. Recent books like John Gribbin's *The Scientists* continue to make claims that a figure like the 17th-century physician William Gilbert 'deserves the title of the first scientist' [2002, 68]. Hannam's book rakes at just this kind of misconception by tracing the history of the prejudice and demonstrating how in every medieval century scientific progress was made in logic, physics, mathematics, and technology.

The Genesis of Science combines a generalized survey of the history of ideas from the Fall of Rome in AD 476 to the trial of Galileo in the 17th century with a dose of historical revisionism in order to debunk the popular debasement of medieval thought. There are two very important and very different elements to *The Genesis of Science*: the historical content which is usually benign to

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the point of encyclopedic and Hannam's interpretation of the history, which is more challenging and speculative. While what Hannam has to say about the people, inventions, and discoveries in the medieval world mostly has been said before, his basic argument that this caused (or 'launched') the Scientific Revolution is highly original and deserving of further inquiry.

Taken together, the book surveys more than a dozen medieval thinkers in almost rapid-fire succession. The first eight chapters rehearse common topics, as Hannam steers his readers through the early and high Middle Ages with brief descriptions of the careers of Gerbert of Aurrillac (Pope Sylvester II) and his use of the abacus and astrolabe, and Anselm of Canterbury and his studies in logic. The 11th and the 12th centuries witnessed the advancements made by Adelard of Bath, Peter Abelard, and Peter Lombard. Then, the influx of Aristotelian philosophy—along with Aristotle's two key commentators Averroes and Thomas Aquinas—in the late 12th and 13th centuries brought about a major shift in the formulation of scientific categories and methods. Echoing the opinion of many medieval scholars, Hannam bemoans the neglected emphasis on logic and scholastic rationalism in many contemporary renderings of how science developed. For him, this Aristotelian framework laid the groundwork for modern structures of argumentation, rationality, and falsification.

Students of medieval thought will not find anything entirely novel in this extended summation. In fact, *The Genesis of Science* is highly reliant upon secondary literature for the bulk of its content, though it rarely engages with other scholars directly. Edward Grant's *God and Reason in the Middle Ages* demonstrates many of the same things about science before 1500 with a more nuanced critique of the modernist prejudice against pre-modern science [2001]. One could argue that this preliminary material, at least everything leading up to the 13th century, is tangential to the book's ultimate argument. On the other hand, the way in which Hannam has pulled these various individuals and their ideas together into a single narrative should be commended and appreciated.

In chapters 8–13, we have a better sense of Hannam's end goal, as he begins to set out exactly how the Middle Ages 'launched' the beginnings of modern science. The book positions thinkers like Roger Bacon and Robert Grosseteste as the real genesis of experimentation and scientific discovery (e.g., Bacon's ruminations on the possibilities of gunpowder, flying machines, and spectacles). Unfortunately, the book's discussion of Grosseteste falls considerably short of A. C. Crombie's study of Grosseteste's impact on later scientific thought [1971]. From here, Hannam moves through late medieval flashpoints of scientific insight in looking at the Oxford don Richard Wallingford's 14th-century mechanical clock, Thomas Bradwardine's work on an early version of logarithms, Richard Swineshead's positing of a mean speed theorem, and John Buridan's examination of momentum and his concept of 'impetus' [179]. All of these present inaugural moments of groundbreaking achievements in science that are usually credited to much later individuals.

In these chapters, the book also carefully notes the Catholic Church's relationship to science. Hannam challenges the perception that the medieval Church was anti-science. Hallmark events like the banning of Aristotle's books in Paris in the 13th-century, the execution of Cecco D'Ascoli, and the trial of Galileo often receive a disproportional amount of attention compared to the broader history. While the Church did strongly censor scientific endeavor, the number of these suppressions pales in comparison to the times when the Church housed, funded, and promoted medieval intellectual advancement. Furthermore, the limitations placed upon thinkers, Hannam insightfully argues, 'served a dual purpose' [97]. The limitations protected theology from rationalistic materialism, which was the Church's primary intent, and it also shielded the scientists themselves 'from those who wanted to see their activities further curtailed' [97]. For much of the medieval period, the Church acted as the defender and patron as well as the regulator of scientific pursuits. Moreover, the book is quick to stress the important role that religion played in the Scientific Revolution. Galileo, Brahe, Newton, and others did not shun religious categories and ideals. Instead, the scientists employed religious structures and motivations in their explorations. As Margaret Osler determines in her *Rethinking the Scientific Revolution*, religion often shaped the questions that science was asking as well as many of the assumptions that guided the groundbreaking discoveries of the 17th century [2000]. By and large, these thinkers sought to establish more certain reasons and explanations for absolute truths about the universe, providing stronger foundations for their religious beliefs.

The real culprit in the book is not religion but Renaissance humanism because of its demeaning view of the Middle Ages. In Hannam's opinion, the mark left by humanism on scientific thought is more negative than positive.

Figures like Desiderius Erasmus, he exclaims, 'almost managed to destroy 300 years of progress in natural philosophy', because humanism despised medieval logic and scholasticism [218]. However, it is here [chs 14–17] that Hannam's argument begins to reveal its own limitations. The book takes the first of several missteps in an effort to confute the popular myth of medieval science by conflating humanism and Protestantism. While they are different, Hannam contends that the more important fact is that the two both protested medieval science. He goes so far as to suggest that 'Protestant writers' like Locke and Hobbes refused 'to give an ounce of credit to Catholics', compounding the assault on the Middle Ages because of their religious prejudice [xv]. Here, Hannam seems to overlook the fact that Hobbes saw Rene Descartes, a devout Catholic, as a worthy opponent for debate over Descartes' theory of light. Likewise, Locke almost certainly borrowed from both Blaise Pascal and Descartes, as John Marshall has pointed out [1994, 138, 196]. Instead, what the *The Genesis of Science* portrays is a pattern of long, continuous progress in scientific thought—which is itself largely synthetic—until Renaissance humanism, followed closely by the Reformation, began demeaning the entire medieval tradition.

Another problematic issue is that the book struggles to communicate the sort of indisputable, direct links and associations between natural philosophers and the Scientific Revolution which are essential to substantiate the argument for causation. Its innovative and bold assertion about the launching of modern science seems to be the book's Achilles' heel. Certainly, it is enlightening to find out that Galileo's work on the mean speed theorem was likely borrowed in part from William of Heytesbury [338]. Also, the book notes that Buridan's mathematics were essential in the curriculum at the University of Paris well into the 16th century, indicating their continued influence over the early modern period [278]. However, these examples are few and far between. In fact, there is an unmistakable sense that the impact of the Middle Ages on the Scientific Revolution was slightly more indirect than Hannam would like to admit. This is exemplified in the book's assessment of Nicholas Copernicus' theory of the Earth's orbit, which is similar to insights found in works by Buridan and Nicholas of Cusa in the 14th century. Unfortunately, Hannam admits, while the three offer essentially 'the same argument' for planetary motion, we still do not know if Copernicus had 'direct access to Buridan's work' [278]. This kind of qualification places serious limitations on his causation thesis. There were certainly seeds planted in the 13th and 14th centuries that produced fruit later on. Medieval thinkers wrestled with many of the ideas that built the Scientific Revolution; but it remains uncertain, on the whole, how much was borrowed from these wrestlings and how much of the similarity was simply happenstance.

A final point of concern with this book is the somewhat bizarre and jarring statement toward the end:

> You could call any century from the twelfth to the twentieth a revolution in science, with our own century to end the sequence. The concept of the scientific revolution does nothing more than reinforce the error that before Copernicus nothing of any significance to science took place at all [350].

If this is the case, then what exactly is the book about? This comment exposes an unresolved tension for Hannam's overarching argument. What is the significance of the Scientific Revolution in Hannam's view? Was it merely an extension of the previous three or four centuries? Or was it something that the Middle Ages launched? Erasing the Scientific Revolution as a historical period devalues one of the more monumental socio-cultural paradigm shifts in Western science. Alongside the cultural shift of the Renaissance and the philosophical shift in the Enlightenment, science was being reoriented along a different axis, addressing questions from new vantage points and with new ideals. Over the course of the early modern period, science came to be seen no longer simply as a means of understanding the world. Science became a means of manipulating, altering, and reshaping nature to conform the world to human needs and purposes. In *The Scientific Revolution: A Historiographical Inquiry*, H. F. Cohen explains, 'The idea of the applicability of science is...one of the great novelties of the Scientific Revolution' [1994, 192]. The book radically reduces the innovative nature of the Scientific Revolution. In an effort to overthrow the misconception of a rebirth of learning and science from the backwardness of the Middle Ages, it seems that Hannam falls into the opposite trap of not recognizing any major transition at all. This relatively smooth narrative of progress from medieval to modern is unique and useful to a certain extent because it offers an alternative to the dominant perspective of a backward Middle Ages. By positing such a grand story, however, Hannam opens himself up to charges of creating his own kind of historical positivism, wherein the Middle Ages are positioned as just another step in the slow progress toward the present day. Such a characterization is something that *The Genesis of Science* cannot shake easily as the end goal always seems to be the modern world.

It is difficult to overlook the lack of nuance with which some of the material is handled and the book's attempt to prove a causal relationship ultimately does not quite hit the mark. However, *The Genesis of Science* is an important contribution to challenging the current misconceptions about medieval thought within pop intellectualism and such a counter-assault is long overdue. The fact that it is written as a popular history of science makes it a unique and valuable contribution to the discipline. The book provides an accessible, well-contextualized recitation of often unnamed and relatively unknown thinkers who are too easily forgotten. For his efforts to memorialize these individuals, Hannam should be praised. The analysis of Galileo's impact and importance [chs 19–20] is equally insightful and should be read as a germane summary of the events surrounding the astronomer's career. In general, the book is a piquant introduction to the intellectual world preceding the Scientific Revolution. Few readers will walk away being able to deny the ingenuity and variety of medieval science.

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A Passage to Infinity: Medieval Indian Mathematics from Kerala and Its Impact by George Gheverghese Joseph

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This ambitious work undertakes to address in just over 200 pages a very extensive set of topics concerning the so-called Kerala school, which first became known to European historians in the mid-19th century [2–3]. Its members were a remarkably brilliant and innovative group of mathematicians and astronomers active in the mid-second millennium AD in southwest India. They were responsible for, among other things, important results on infinite series and infinitesimal methods that were later rediscovered by European mathematicians investigating the 'new analysis' or calculus.

The book's chief goals are the following: to investigate and describe the mathematical genesis, technical practices, and major discoveries of the Kerala school; to explore its social origins and context as well as its relation to traditional knowledge systems in the region; and to analyze historiographic problems concerning modern historical views of second-millennium Indian mathematics in general and the Kerala school in particular, including recent hypotheses about possible scientific transmissions from Kerala to early modern Europe. The formidable task of covering this extensive ground is shared by several researchers, mostly collaborators in the UK Arts and Humanities Research Board's Research Project on Medieval Kerala Mathematics, whom the author credits in the acknowledgements and in the individual chapters where their contributions appear.

The first chapter is a short introduction outlining historiographic issues in the history of mathematics and the book's objectives. The second chapter, 'Social Origins of the Kerala School', includes research by M. Vijaylakshmy and V. M. Mallayya in a historical survey of intellectual traditions in medieval Kerala and biographical summaries of some central figures. Chapter 3, 'Mathematical Origins of the Kerala School', is chiefly focused on recapitulating the work

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of the early sixth-century astronomer/mathematician Āryabhaṭa I, among whose followers in astronomy the Kerala scientists are usually counted. 'Highlights of Kerala Mathematics and Astronomy' in chapter 4 briefly outlines some of their seminal discoveries, while chapter 5, 'Indian Trigonometry from Ancient Beginnings to Nīlakaṇṭha', which is based on research by V. M. Mallayya, summarizes trigonometric findings by medieval Indian mathematicians before the Kerala school and culminates in an extensive discussion of trigonometry in the works of the Kerala scholar Nīlakaṇṭha. This theme is continued in the next and longest chapter, 'Squaring the Circle', based on the work of Dr. Mallayya and the late K. V. Sarma, which elegantly outlines what is widely considered the crown jewel of Kerala mathematics, namely, the derivation of the Mādhava-Leibniz infinite series for the circumference of a circle and associated methods for the computation of arc-length and π. Chapter 7, 'Reaching for the Stars', surveys Kerala school work on power series for sine and cosine functions. The next two chapters, 'Changing Perspectives on Indian Mathematics' and 'Exploring Transmissions: A Case Study of Kerala Mathematics' are largely devoted to exploring the possibility of transmission of Kerala mathematics to Europe before the 19th century. They incorporate work by D. Almeida, U. Baldini, and A. Bala. Finally, a brief conclusion extends the investigation to general historiographic questions concerning transmission and innovation in mathematics, and their dependence on cultural context.

The compression of so much material into such a limited space has understandably produced some elisions and ambiguities. The frequent use of transliterated Sanskrit is a well-chosen compromise between reproducing Sanskrit in *nāgarī* script and using only translated technical terms; but it would have been more helpful to use a consistent transliteration scheme with a full range of diacritical marks. For instance, on pages 94–95, the name 'Vaṭeśvara' is spelled sometimes with an underdot indicating the retroflex 'ṭ' and sometimes without, but never rendered precisely with both accents. The alphanumeric encoding named after the Sanskrit consonants 'ka', 'ṭa', 'pa', and 'ya' is identified [e.g., 36, 217] as 'Katapyadi' instead of the more standard and intuitive 'kaṭapayādi'. The word 'śāstra' ('science', 'treatise') is spelled 'shastra' when separate but 'sastra' when compounded in, e.g., 'jyotisastra' ('astral science') [201: more precisely, 'jyotiḥśāstra'].

More confusing than these minor typographical glitches are the frequent allusions and assertions carelessly expressed or insufficiently explained. Readers unfamiliar with Roman Catholic religious orders, for example, may not immediately understand that the passing reference to the French scholar Marin Mersenne as a 'minim [*sic*] monk' [164] means that Mersenne was a member of the Minim Friars. The above-mentioned Vaṭeśvara does not appear in the book's rather hit-or-miss index or in the list of 'Major Personalities and Texts in Indian Mathematics' on page 12, although a section of chapter 5 is devoted to Vaṭeśvara's trigonometric work, described [95] as 'one of the most comprehensive and innovative achievements of early Indian trigonometry'. The preeminent sixth-century scientist Āryabhaṭa I is briefly stated to have 'attended the University of Nalanda' [42], i.e., the renowned medieval center of Buddhist learning in the Bihar region. This is an oft-repeated but ill-supported legend based on Āryabhaṭa's description of 'knowledge honored in Kusumapura', referring probably to the medieval urban center that is now Patna, close to but not identical with the Buddhist institution of Nalanda. His chief work, the *Āryabhaṭīya*, is called 'the premier Indian text to be read and commented on for at least another thousand years' in the realm of Indian mathematics [54], which oddly ignores the immense popularity and canonical status of the 12th-century *Līlāvatī* of Bhāskara II. Likewise, it is by no means certain that 'at the time of Āryabhaṭa, mathematics was rarely treated outside its astronomical context' [62]: the lack of surviving texts from this period makes it impossible to pronounce conclusively on the nature of textual genres in the Sanskrit exact sciences. Moreover, the author surely does not intend to claim that Āryabhaṭa was the first Indian mathematician to solve the problem of computing decimal place-value square roots, but that is the impression he produces by the claim that 'ever since Aryabhata devised a method to calculate square roots, Indian mathematicians could approximate' a trigonometric quantity by a rational number [66]. It is similarly confusing to assert that Indian mathematicians after Āryabhaṭa 'calculated sine values for any angle in radians' [59], when the units of length in question were actually equivalent to arc-minutes rather than radians. Other puzzling and potentially misleading remarks of this nature can be found throughout the book; most seem to spring from a hasty or clumsy attempt to squeeze rather complicated historical and mathematical information into an expository framework too small for it.

These flaws are regrettable because they risk obscuring the many valuable contributions contained in the volume. The detailed explanations in modern mathematical notation of various significant results found by Kerala mathematicians, particularly in chapters 5, 6 and 7, are especially helpful. So are the surveys of current research that tie in the work of the volume's contributors with that of fellow scholars. (To their detriment, however, the bibliography and notes omit any mention of the published research of the late David Pingree.) The discussion in chapter 2 of the social context within which the Kerala scholars worked is also commendably detailed, although much of the exposition in both the chapter's text and the notes suffers from a lack of specific supporting citations—a brief footnote at the start of the chapter does invoke recent joint articles by Joseph and other contributors as its general basis. The reader intrigued by the interesting descriptions of, for example, the family-run *Gurukula* educational institutions in Kerala [33] finds no sources cited there to guide the quest for more information. Despite these limitations, this material covers important ground and is well worth reading.

The topic that ultimately inspired the book's genesis, as the author notes on page 1, is a question of cross-cultural transmission: namely, 'the conjecture of the transmission of Kerala mathematics to Europe, with a view to informing the wider history of mathematics' [3]. To investigate this issue, the author and other members of the above-mentioned Project on Medieval Kerala Mathematics examined correspondence, reports, and Indian manuscripts in European archives with known or possible connections to 16th- and 17th-century Jesuit missionaries in South India who were rightly deemed the most likely candidates to supply a conduit for translation and transmission of scientific texts [179–185]. The inspection of this under-studied and historically important corpus is a laudable achievement, especially in light of the neglect of much of this material (some of it hitherto not even catalogued) by institutions and scholars in the lands where it currently resides.

Since a historically validated narrative of early modern European mathematicians borrowing core concepts of calculus from predecessors in Kerala would have made headlines in scholarship on the history of mathematics and beyond (while doubtless inspiring a surge of interest in the Indian mathematical tradition which is both well deserved and long overdue), it is hard to help feeling disappointed that this hypothesis ultimately came to nothing. As Joseph candidly observes, the sifting of the various archives 'has yielded no direct evidence of the conjectured transmission' [186]. He quotes the summing-up by fellow researcher Ugo Baldini in greater detail:

> Thus, unless new evidence is found and some basically new circumstance is established, the only possible deduction seems to be that not only no information exists on a Jesuit mathematician having managed to study some advanced Indian text (not to say to transmit it, or its content, to Europe), but no serious clue appears of a scientific interchange not purely superficial and more than occasional. [191]

Joseph, following the lead of another contributor (Arun Bala), then raises the question [192–193] whether a different type of transmission might have taken place without leaving documentary evidence:

> '...the Indian mathematical discoveries may have reached Europe as a set of practical computing rules rather than a body of mathematical discoveries'...if there was transmission of knowledge of infinite series to Europe, it was done indirectly through practical uses, with a truncated version being passed on from local craftsmen to their foreign counterparts (such as navigators) and then being reconstructed in Europe by the mathematically knowledgeable without being aware of its provenance.

This is certainly a very vague and speculative conjecture, as the author acknowledges. He proposes it for consideration not entirely on its own (still undetermined) merits but as part of a larger historiographic claim, namely, that the assumption 'of independent European discovery of some of the Kerala mathematics...as a default solution by most historians is debatable' [193]. In other words, he suggests that most historians discount the possibility of Indian influence on the early modern invention of calculus more on the basis of Eurocentric bias than as part of a consistent historiographic outlook. Noting that the renowned historian of ancient science Otto Neugebauer accepted certain combinations of plausible circumstantial evidence in the absence of direct evidence for inferring scientific transmission from one culture to another [162], Joseph argues that requiring documentary evidence to support the conjecture of a transmission of calculus concepts from Kerala to Europe is somewhat capricious and unfair:

> O'Leary uses an admixture of the Neugebauer and the van der Waerden paradigm to claim the Greek origin of Indian astronomy and mathematics....In these circumstances priority, communication routes and methodological similarities appear to establish a socially acceptable case for transmission from West to East. Despite these elements being in place, the case for transmission of Kerala mathematics to Europe seems to require stronger evidence. [163]

This implied accusation relies on some exaggerated or distorted arguments as well as some valid criticisms. It is certainly true that there was a great deal of Eurocentric bias in much 19th- and 20th-century scholarship and speculation concerning cross-cultural transmission of science. Moreover, it is also true that Indian mathematics remains much more under-studied and much more incompletely treated in scholarship on the history of mathematics than other mathematical traditions. We cannot assume from these facts, however, that Eurocentric bias is still dictating modern historians' attitudes towards conjectures about scientific transmission involving India. It is not true, for example, that such speculations as those of O'Leary in 1948 (much less those of Sédillot in 1875 or Bentley in 1823, justly deplored on pages 157–158) would be widely regarded as 'a socially acceptable case for transmission from West to East' among historians of science today.

Furthermore, the 'Neugebauer paradigm' for weighing circumstantial evidence of transmission obviously cannot apply in exactly the same way to well-documented historical developments in mathematics and science as it does to poorly documented ones. It is one thing for Neugebauer to argue, for example, that Euclid's so-called 'geometrical algebra', which has left no clear record of independent discovery in extant Greek sources, was probably ultimately influenced by related ideas in earlier Babylonian mathematics. It is quite another to argue that infinitesimal calculus, whose various stages of development in the hands of European mathematicians are very well attested in surviving texts, was probably influenced by related ideas in earlier Kerala works, despite the complete absence (so far) of detectable traces of Kerala material in the abundant textual record of early modern European mathematics. Both these examples involve the hypothesis of a scientific transmission from 'East' to 'West': Mesopotamia to Greece in the first case and Kerala to Europe in the second. The crucial difference between them is not a matter of Eurocentric bias but rather that in the former case there is virtually no documentary evidence supporting the alternative hypothesis of a completely independent rediscovery by the 'Western' mathematicians, whereas in the latter case there is a great deal of such evidence.

That said, it must be acknowledged that Joseph makes a very good point about the need for this sort of direct discussion of historiographic assumptions: 'The methodology underlying the testing of such claims and assessing the relevant evidence remains relatively undeveloped' [199]. Different historians will inevitably sometimes come to different conclusions about what qualifies as historically probable or historiographically sound. What matters more than unanimity is clarity about the reasoning and criteria employed to reach the different conclusions. In foregrounding this issue within the comparative history of mathematics, as well as in the contributions described above, *A Passage to Infinity* has performed a valuable service.

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Ancient Babylonian Medicine: Theory and Practice by Markham J. Geller

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The study of ancient Mesopotamian medicine has expanded dramatically over the past couple of decades. In addition to the publication of major editions and studies of cuneiform medical works, several monographs have appeared which study particular illnesses or methods of healing in Mesopotamia, and a number of conferences have been held and published exploring both Mesopotamian medicine itself, its role within wider cuneiform scholarly traditions, and its relationship with later, particularly Greek, medical traditions. The field even has its own journal, the *Journal des Médecines Cuneiformes*, which has appeared twice yearly since 2003. Mark Geller has been one of the scholars to play a key role in this growth of interest in Mesopotamian medicine. This makes it appropriate that he should be the first to attempt to write a general introduction to the subject which will be both accessible to the nonspecialist (which includes historians of medicine in other ancient cultures, Assyriologists who know little about the cuneiform scientific traditions, and even doctors and medical students who are interested in the ancient origins of their discipline) and at the same time makes a contribution to our understanding of medicine and scholarship more generally in Mesopotamia. The result, *Ancient Babylonian Medicine: Theory and Practice*, is in my opinion a huge success.

There are several approaches that could be utilized in writing a book of this kind. One would be to provide a survey of works in cuneiform that deal with aspects of medicine. Another would be to catalogue Babylonian designations of illnesses and their modern equivalents, and to compare Babylonian knowledge and treatment of these illnesses with modern knowledge and treatment. Geller takes a much more interesting approach. His aim is to try to understand what 'Babylonian medicine' is, both in its underlying theoretical framework and as a healing practice. As a consequence, the

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reader will not find in this book lists of illnesses or medical ingredients and their modern equivalents (which would be impossible anyway as in many cases the specific plant or mineral referred to cannot be identified), nor will he or she find a discussion of whether specific ingredients had identifiable medicinal properties as defined by modern science. Instead, Geller discusses issues such as the relationship between magic and physical methods of healing, who were the groups that provided medical care and what was their training, the role of medicine in society and who had access to medical care, and the interrelationships between cuneiform medical texts and what this tells us about how they were written and used. The book is much more rewarding as a result.

The book begins with an introduction providing background information on ancient Mesopotamia, the sources for studying Babylonian medicine, and the terminology of ancient medicine. Geller here also poses one of the central questions of the book: 'Is Babylonian medicine magic and is Babylonian magic medicine?' This question is a valid line of enquiry for the study of many traditions of medicine in ancient and even modern societies. It is particularly relevant to the study of Babylonian medicine because many illnesses were attributed to the action of gods or ghosts and methods of treatment often combined what we might consider 'magical' means such as incantations, amulets, and so on, with the administering of medicines made from plants, minerals, and the like. Indeed, the line of demarcation between magical and medical healing is even more blurred than just described. For example, we have examples of herbal or mineral medicines that are activated through magical means such as exposure to the light of a star.

In chapter 1, Geller raises the issue of whether Babylonian medicine is a science. For Geller, to qualify as a science there must be an underlying theory to a practice which is therefore not founded simply upon technological thinking. He lists three necessary conditions for the existence of a theory in an ancient context [12–13]: imagination (the idea that natural events are not just random but have an explicable structure), deductive logic, and observation. Geller then demonstrates the presence of all three of these conditions in Babylonian medicine. Some historians will certainly disagree with Geller's definition of what makes a practice 'science'—for example, I am uneasy with his rule of thumb [18] that the more mathematical a practice is, the more scientific it is—but Geller raises some interesting questions of how we should classify ancient knowledge.

Geller returns to the question of the relationship between medicine and magic in chapter 2, entitled 'Who Did What to Whom?'. Two professions were involved in healing in Mesopotamia: the *mašmaššu*, which Geller translates by 'exorcist' (because this is a term loaded with meaning in today's culture, some people, including myself, prefer more neutral translations such as 'ritual expert'), and the *asû*, conventionally translated by 'physician'.[1] These two professional titles suggest a strict division of labour in the practice of healing: the *mašmaššu* uses supernatural or magical means to aid the patient while the *asû* uses physical remedies such as medicines made from herbs and minerals—what might be thought of as a split between the spirit and the body. As Geller shows, however, the division of responsibilities between the *mašmaššu* and the *asû* is less clear cut: sometimes the *mašmaššu* would use physical means of healing and sometimes the *asû* would use magical means. Indeed, by the second half of the first millennium BC, the *asû* seems to have disappeared from the cuneiform record and both magical and physical means of healing are associated with the *mašmaššu*. Geller makes the interesting suggestion that because the *asû* was a layman whereas the *mašmaššu* was associated with the temple, and because most scholarly and administrative texts from the late period relate to the temple, the *asû* may simply have fallen under the radar of the cuneiform record and his profession may still have existed in wider society.

In chapter 3, Geller discusses the politics of medicine: legal codes, access to medicine, and the health of the king, as well as the Babylonian approach to public health issues such as epidemics. The Code of Hammurabi is informative for the role of the *asû* (physician) in the Old Babylonian period. Interestingly, the *asû* is one of only a very small number of professionals mentioned in the Code, highlighting the need to regulate the practice of medicine because of its impact upon society as a whole. Even more interestingly, the *mašmaššu* is not named in the Code, suggesting that during the Old Babylonian period there was a clear distinction between their two professions and that only the one dealing with the physical body was deemed to

[1] The situation is somewhat complicated by a third professional title 'āšipu', which also means 'exorcist'. Geller discusses whether the *mašmaššu* and the *āšipu* are synonymous on pages 48–50.

require control by law. The *asû* appears in two contexts in the Code. First, in laws concerning physical harm inflicted on an individual, the person who has caused that harm may be required to pay the *asû*'s bill. Secondly, malpractice by the *asû* was punishable either by acts such as cutting off the *asû*'s hand or by payment of silver.

Royal correspondence provides our main source of information about the relationship between the *asû* and *mašmaššu* as healers and the patient. Although any issue concerning the king inevitably differs from the experience of the rest of society—for example, the king could have multiple healers working either together or in competition to aid in his recovery from illness and to provide advice on day-to-day health matters, something that would be beyond the reach of all but a very small number of the elite of society—these letters provide an insight into the variety of ailments that the *asû* and *mašmaššu* were called on (and felt able) to treat.

In chapter 4, 'Medicine as Literature', Geller discusses the composition, copying, and reading of Babylonian medical texts. This chapter nicely links to chapters 6 and 7 which are concerned with the training of healers, the extent to which medical texts were part of this process, and the tradition of writing commentaries on medical texts. Geller's discussion of the commentary genre is particularly interesting as he makes a plausible argument that these texts provide insight into the process of the creation of medical knowledge. The discussion in these chapters has important consequences not only for the study of Babylonian medicine but also for our understanding of the processes of development and practice of many other genres of Babylonian scholarship.

Geller returns to the question of the relationship between medicine and magic in the final chapter of the book. He concludes that the disciplines of medicine and magic, and the individuals who practiced them, were distinct during most of Mesopotamian history. But these two practices were clearly complementary, magic providing the 'bedside manner', a psychological factor in the healing process that was just as important, perhaps often more important, than the administering of herbs and minerals which sometimes may in themselves have had little or no effect on the patient's recovery. In order to understand the Babylonian approach to healing, it is necessary to consider both aspects of the approach to treating a patient.

Ancient Babylonian Medicine is an important and fascinating book which not only provides a much needed introduction to the theory and practice of medicine in ancient Mesopotamia but also makes a significant contribution to the study of ancient Mesopotamian scholarship. It is clearly and elegantly written, nicely illustrated, and well produced. It is to be regretted, however, that the publishers have assigned such a high list price for the book, making it difficult to assign the text in undergraduate classes. We can but hope that the publishers will consider publishing a more reasonably priced softcover edition in the future.

A Response to Acerbi on Netz, Noel, Tchernetska, and Wilson edd. *The Archimedes Palimpsest*[★]

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In a notice concerning *The Archimedes Palimpsest*[1] published in *Aestimatio* 10 (2013) 34–46, Fabio Acerbi—attentive and erudite as ever—makes many comments, some of which I sympathize with. (Especially, when he notes the awkwardness of the choice to mark dialect variations—a choice that ended up over-burdening the apparatus; Heiberg's opposite choice put us in a bind). Putting aside several minor remarks, I respond to the two main substantive claims.

1. That the diagram set of *Spiral Lines* 13 in Codex A included not just an impossible figure but also another, normalized one

This claim, which has potential ramifications for the nature of the ancient diagram, is based on the evidence of copies BDEG. In fact, the normalized diagram in E is in a second hand, while those of BDG are each differently oriented. We are left with the evidence of EH4, each (in its original state) with just a radically impossible figure, as against BDG, each with just a normalized diagram—and each distinct. Note that:

(i) no copy contained the putative dual set of diagrams (both impossible and normalized),
(ii) this is in fact typical. In general, EH4 tend to copy automatically what we may reconstruct as the source in A while BG (and sometimes D) introduce variations based on their own mathematical understanding (so, for instance, with the arcs of the polygon in *Sphere and*

[★] See http://www.ircps.org/aestimatio/10/34-46.
[1] R. Netz, W. Noel, N. Tchernetska, and N. Wilson edd. Cambridge: Cambridge University Press, 2011. 2 vols. Pp. 700. ISBN 978–1–107–01684–2. Cloth £150, $240.00.

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*Cylinder*1, generally preserved by EH4 but sometimes ‘corrected’, in various ways, by BG and, occasionally, D. It would be a very thin reed indeed, to suggest that the arcs of the polygon could have been independently introduced by EH4).

The reconstruction of Codex A with only the radically impossible figure—identical to that of C—is, therefore, virtually certain.

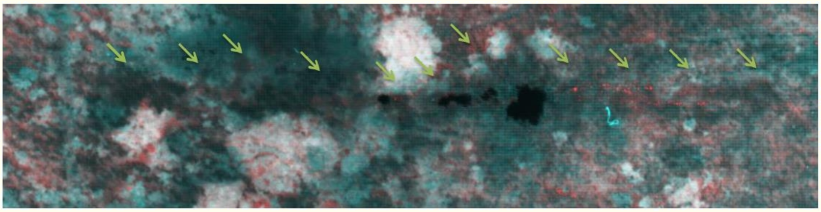

Figure 1

2. That the reading of «πλῆθος» in 177v col. 1.1 is baseless

I attach here a pseudo-color image [Figure 1] of the first 11 letters of the line, arrows pointing at the letters (curiously, the fifth letter is now best read in the original Heiberg image, for which the reader is referred to the dataset). The reading is as difficult as it gets in this Archimedes manuscript but it is not in serious doubt. Of course, the reading of such a text does require a more intensive familiarity with the particular document because a great deal of the work involves eliminating alternative letter forms: one needs to know the precise details of the particular script as well as the pattern of damage to such letter forms. In my experience with students, it takes several months to acquire such skills.

I applaud Acerbi for his insistence that textual publications should serve as the beginning, not end, of critical revision (I have after all contributed to a revision of no less than Heiberg). However, when critical attitude turns into unbounded skepticism, one loses precisely the advantages of the editors’ tacit knowledge—of their familiarity with the idiosyncrasies of a document. Far better would be to make one’s best effort to follow in the editors’ footsteps, while being willing to diverge from them.

That Acerbi did not make such an effort may be related to an undercurrent—which one would be disingenuous to ignore—of personal animus. I am reluctant to join this conversation and do so only because silence, under such circumstances, might be misconstrued. To prevent the future possibility of such misconstruals, it is best to make clear that this is my first, and last, response.

Aristoteles, Meteorologica*: Liber quartus. Translatio Henrici Aristippi* edited by Elisa Rubino

Aristoteles Latinus 10.1. Brussels: Brepols, 2010. Pp. lxvi + 75. ISBN 978–2–503–53472–5. Cloth €60.49

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Può dirsi nozione ampiamente diffusa che a partire dall'anno Mille conosciamo in Europa un risveglio culturale di vaste proporzioni. Soprattutto tramite il contatto con la vasta produzione intellettuale ed esegetica degli autori arabi si diffonde in Europa una nuova cultura, che subito si contrappone alle varie scuole imperanti nel continente europeo. Costantino Africano, legato anche alla tradizione scientifica della Scuola medica salernitana, ed in seguito Adelardo di Bath furono, tra gli altri, esponenti di un vasto lavoro che tendeva a riabilitare i risultati della scienza contrapponendoli alle chiacchiere ed assurde speculazioni, dove vagavano senza costrutto le scuole della Gallia. L'opera immensa di Adelardo contribuisce con le sue traduzioni ad un vasto rinnovamento della cultura dell'epoca. Fu Adelardo che fece conoscere in Occidente gli *Elementi* di Euclide ed il lavoro di al-Khuwarizmi che di fatto introdusse nella cultura europea le cifre arabe, e con queste una diversa concezione della matematica. Ma le opere che lo posero in aperta contrapposizione con la cosiddetta speculazione delle 'scuole della Gallia' furono il *De eodem et diverso* e le *Quaestiones naturales*, laddove Adelardo si confronta con l'atomismo antico e la *Fisica* di Aristotele, ancora ignoti alla sua epoca. Su queste basi egli rivendica la fiducia nella disciplina scientifica e nella ragione, motivi che saranno di grande importanza nello sviluppo scientifico successivo.

Non possono in questa sede essere taciuti quegli esponenti della scuola di Toledo che portarono a scoprire quelle parti della logica aristotelica ancora ignote: ci riferiamo alle traduzioni dei *Primi* e *Secondi analitici*, dei *Topici* e degli *Elenchi sofistici* (Giacomo da Venezia) ed al lavoro in particolare di Gerardo da Cremona su Aristotele e sui commentatori greci (*in primis* ovviamente Alessandro di Afrodisia).

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Un movimento molto ampio, che portò una profonda rivoluzione negli studi filosofico-scientifici. Non a caso in questo periodo si parlerà di una 'logica nuova' contrapposta alla *logica vetus* delle *Categorie* e del *De interpretazione* note nella traduzione di Boezio.[1]

Questo breve e scarno panorama che parte dall'anno Mille ci è parso quanto meno utile ad inquadrare un altro illustre traduttore: ci riferiamo ovviamente ad Enrico Aristippo che tradusse in latino il *Menone* ed il *Fedone* di Platone, il IV libro della *Meteorologia* di Aristotele, nonché la fondamentale edizione delle *Vite dei filosofi* di Diogene Laerzio. Il IV della *Meteorologia* di Aristotele ha nel medioevo, sia nella tradizione latina sia in quella araba, un luogo centrale negli studi fisici, per un periodo tanto lungo che, in piena epoca moderna, Bernardino Telesio si rifà esplicitamente alla *Meteorologia* aristotelica, ricorrendo alle due forze contrapposte, caldo e freddo, per delineare la sua concezione della natura.

Gli unici commentari greci del IV libro a noi pervenuti sono quelli di Alessandro di Afrodisia e di Olimpiodoro. Il primo ritiene il trattatello come una ricerca a sé stante e non la prosecuzione dei primi tre libri, mentre il secondo ritiene che sarebbe più opportuno collegarlo alla trattazione degli elementi del *De generatione et corruptione*.[2] L'opinione oggi prevalente è che questo libro sia da considerarsi una ricerca autonoma, non tenendo conto, ovviamente, di quanti lo ritengono spurio.

Quel che abbiamo brevemente esposto ci aiuta a valutare nella sua importanza la prima traduzione dal greco di Enrico Aristippo, che leggiamo ora nella pregevole edizione critica di Elisa Rubino. Il testo compare nella collana Aristoteles Latinus; esso è completato da una vasta introduzione e dall'indice delle corrispondenze latino-greche. Il lavoro della Rubino si segnala, fra l'altro, per la sua individuazione di una raccolta che costituisce una barriera non superabile da un punto di vista filologico. La raccolta è quella di Alfredo di Sareshel (Alfredo Anglico), che avrebbe messo insieme i primi tre libri dei *Meteorologica* (tradotti dall'arabo da Gerardo da Cremona), il quarto libro (tradotto dal greco da Aristippo), e il *De mineralibus* (tradotto dall'arabo dallo stesso Alfredo). In questo caso Rubino riprende e amplifica un'ipotesi avanzata da Gudrun Vuillemin-Diem [2008]. In particolare, Rubino sottolinea

1 Si veda Brams 2003.
2 Sui due commentari si vedano Natali 2002 e Viano 2002a.

[xxxix] come tutti i 96 codici della traduzione di Aristippo siano da ricondurre alla raccolta alfrediana, che, seguendo l'ipotesi più che verosimile di Vuillemin-Diem, chiama *Editio Alfrediana*.

Anche agli occhi di un non-filologo, quale il sottoscritto, la traduzione che leggiamo nell'edizione di Rubino si segnala per l'adesione al testo greco. Il testo di Enrico Aristippo segue con sufficiente fedeltà quello aristotelico, pur con le facilmente rilevabili diversità (ne sottolineiamo qualcuna) tra la lingua greca e quella latina. Esaminiamo i punti di maggiore interesse.

Il tema fondamentale, cui si è già accennato, non è, come nel *De generatione et corruptione*, quello della costituzione assoluta degli elementi formata dalle quattro qualità (*virtutes*/δυνάμεις) contrarie caldo, freddo, secco, umido; ma l'azione delle due qualità definite attive, caldo e freddo, su una materia definita dalle due qualità passive secca ed umida. E' da sottolineare che in questo quadro concettuale azione, di caldo e freddo, e passione, di secco ed umido, non sono intese in senso astratto, ma come realtà fisicamente determinate, come carne e fuoco oppure oro e ferro.

La generazione (*generatio*/γένεσις) delle cose è opera del caldo proprio e naturale (*proprie et secundum naturam*/οἰκεῖος καὶ κατὰ φύσιν) sul materiale costituito da umido e secco, la corruzione (*corruptio*/φθορά) avviene ad opera del freddo (è da tener presente che, in questa sede, il freddo è da intendersi come sopravvenuta mancanza del caldo naturale, come si ricava facilmente dalla morte dei corpi organici) [378b15 sgg.]. Sulla base di tali premesse Aristotele intende spiegare la formazione dei corpi naturali (sia organici sia inorganici) nonché le loro principali caratteristiche, quali flessibilità, plasticità, e così via.[3]

Il fenomeno centrale nel quarto della *Meteorologia* è certamente la cottura. Qui Aristippo sceglie di tradurre con «digestio», che però, come vedremo, è solo una sottospecie del più ampio cottura. Non appare verosimile che abbia tratto il termine dalle opere biologiche[4] se pensiamo alla loro pressocché nulla diffusione, quindi la possibile conclusione è che Aristippo lo abbia ricavato da 381b5, dove Aristotele paragona la cottura del cibo nel corpo a

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[3] Su queste caratteristiche si veda l'elenco completo al capitolo VIII. La traduzione di Aristippo in questo caso si discosta in più di un punto dall'originale.

[4] Cfr. ad esempio *De part. anim.* 650a5 sgg. dove la digestione vien detta un tipo di cottura simile alla lessatura (ἕψησις); si veda anche *De gen. anim.* 743a30 sgg.

qualcosa di simile alla lessatura (qui Aristippo translittera in «epsesis»). Ma, sottolinea Aristotele, molti di questi processi sono anonimi (letteralmente senza nome) e quindi numerosi altri vanno compresi nella stessa definizione. Poco oltre [380a18, ma anche 380b30] viene ribadito che non esistono nomi per ciascun processo, ma che essi vengono detti per metafora (e crediamo che il termine vada inteso in senso letterale, cioè trasposizione da un fenomeno all'altro), con una importante differenza: essi cioè sono da raggrupparsi sotto una stessa specie (qui Aristippo segue una versione del testo contenente una negazione, che non regge ad un semplice esame logico: *non secundum eandem ideam, metaphorisque*/κατὰ μὲν τὴν αὐτὴν ἰδέαν μεταφορᾶϲ δέ. Infatti Aristotele sta sostenendo che mancano i nomi rispettivi, ma questi fenomeni sono da intendersi dello stesso tipo. Altrimenti questa distinzione non avrebbe senso).[5]

La cottura si distingue in maturazione (*pepansis*/πέπανϲιϲ), lessatura (*epsesis*/ἕψηϲιϲ), arrostimento (*optesis*/ὄπτηϲιϲ); a questi processi corrispondono i rispettivi contrari, che sono: non-cottura (*indigestio*/ἀπεψία), divisa a sua volta in crudezza (*omotes*/ὠμότηϲ), semi-cottura (*molinsis*/μώλυνϲιϲ), bruciatura (*stateusis*/ϲτάτευϲιϲ). Tali fenomeni si producono sotto l'azione del caldo, e non genericamente il calore prodotto dal fuoco o da altro agente riscaldante,[6] bensì il caldo presente secondo natura (φυϲικόϲ), che è specifico, ovvero proprio (οἰκεῖοϲ), del corpo in cui avviene la trasformazione.[7] Il caldo produce questo effetto perché è un compimento (*completio*/τέλειωϲιϲ) delle qualità-fattori passivi, che sono la materia propria a ciascun corpo. Il contrario avviene nei casi di non-cottura, abbiamo in questi processi una

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5 Anche nel nostro comune linguaggio diciamo che l'argilla viene *cotta* nei forni, o che gli ascessi *maturano*: evidentemente qui non usiamo i nomi nel loro senso proprio, ma per metafora.

6 Come ad esempio il caso di bagni caldi per favorire la digestione.

7 Sul calore naturale si veda il passo di *de part. anim.* 648a25 sgg. dove viene sottolinata la differenza tra calore accidentale e calore proprio o essenziale. Sulla funzione del calore interno, o vitale, e la sua identificazione con lo *pneuma*, si veda Freudenthal 1995, 107 sgg. Lloyd 1996, 83 sgg., manifesta una serie di perplessità su questa definizione; ad esempio [96] si chiede perché mescolanze quali la fusione di due metalli non sia indicata come una cottura. La risposta si legge in *De gen. et corr.* I.10 [ma anche 322a15 sgg.], dove viene chiarito che in un certo senso (quello della fusione) i corpi mescolati rimangono identici, in un altro no, come nel nutrimento, dove il corpo digerito perde la sua forma, quindi la propria natura.

sproporzione di calore in difetto o mancanza, è il caso della semi-cottura o della crudezza, o in eccesso, è il caso della bruciatura.

Qui leggiamo il tentativo aristotelico di unificare i processi riguardanti i corpi più semplici in natura: infatti all'azione di caldo e freddo viene ricondotto tutto intero il loro processo di formazione (*conglutinantur corpora*/συνίσταται τὰ σώματα) [384b25], e tale processo riguarda i cosiddetti omeomeri (*omiomera corpora*/ὁμοιομερῆ σώματα)[8] sia nei corpi viventi sia nei minerali o i metalli. A questi proposito è utile una precisazione. «Omeomero» vuole indicare tutti quei corpi che sono strutturati in parti uguali (la parte è sinonima del tutto, dice anche Aristotele) e che costituiscono la seconda scala della materia in natura: e cioè gli elementi, i corpi omeomeri, quelli anomeomeri, ed infine i corpi viventi (si veda il cap. XII). Questi corpi, con le loro caratteristiche, sono, come abbiamo detto, prodotto dell'azione di caldo e freddo. Il punto è che Aristotele ha chiaramente in mente un modello di tipo biologico, e non meccanico (intendiamo qui meccanico nel senso del meccanicismo democriteo). In quest'ultimo caso si potrebbe paradossalmente pensare che carne ed osso possano essere prodotti anche all'esterno dei corpi viventi, come le pietre o l'argento.[9] A questa conclusione sembra condurre il testo, se preso alla lettera, quando troviamo [389b sgg.] che il processo di formazione di tutti i corpi omeomeri è prodotto dell'azione di caldo e freddo e dai movimenti da questi provocati, così come tutte le loro caratteristiche, quali elaticità, frantumabilità, cedevolezza e simili. In questo caso è utile tener presente, ripetiamo, che la prospettiva di Aristotele è di carattere biologico, infatti il discorso aristotelico precisa: per quanto riguarda la materia essi (gli omeomeri) derivano dagli elementi, riguardo all'essenza consiste nella definizione (Aristippo traduce «λόγος» con «causa»). Vediamo il passo [389b30 sgg]: *ut ex materia ex eis que dicta sunt, ut uero secundum substantiam in causa*/ὡς μὲν ἐξ ὕλης ἐκ τῶν εἰρημένων, ὡς δὲ κατ' οὐσίαν τῷ λόγῳ. Qui ricorre la ben nota distinzione tra materia e forma che in natura è da intendersi anche come fine (ἕνεκά του), perché, chiarisce

[8] E' interessante notare come qui Aristippo intenda correttamente «omeomero» come aggettivo e lo traslitteri. Lo sottolineiamo perché Lucrezio afferma invece che non è traducibile in latino e lo rende quindi con il sostantivo «ὁμοιομερία» [Diels e Kranz 1956, 59A44], dando origine così alla famosa tradizione «omeomerica» su Anassagora.

[9] Lo ritiene invece verosimile Gill 1997, 160.

Aristotele, il fine è meno chiaro nella carne e nell'osso, ed ancora meno nel fuoco e nell'acqua,[10] se prendiamo i due estremi la materia non è nient'altro che se stessa, e l'essenza è la definizione; gli intermedi sono analoghi a ciò che è più vicino ad uno dei due: quindi in natura abbiamo solo intermedi, cioè composti di materia e forma, come leggiamo all'inizio della *Fisica*.

Vediamo un caso esaminato in *De part. anim.* 649b22 sgg., quello del sangue. Il sangue è caldo perché il caldo fa parte della definizione del sangue (τὸ αἵματι εἶναι). Cioè esso è caldo per natura. Qui leggiamo un inciso illuminante: diremmo la stessa cosa dell'acqua calda se la potessimo indicare con un solo nome (εἰ ὀνόματί τινι cημαινόμενον), cioè se l'acqua fosse per natura calda, nella sua definizione sarebbe di fatto inclusa la nozione di calore. Ma proseguiamo nella lettura. Riguardo alla sua costituzione materiale (ὑποκείμενον) il sangue non è caldo (e qui siamo nell'orizzonte del libro IV), ne consegue che il sangue in una certo senso è caldo, in un altro no; e questo perché secondo la definizione (τῷ λόγῳ) ad esso appartiene il caldo, ma siccome il calore proviene dall'esterno (κατὰ πάθος *scil.* dal corpo vivente) il sangue in sé non è caldo. Questa apparente contraddizione si risolve ponendosi dal punto di vista aristotelico: il fatto è che il sangue, in quanto entra a far parte della costituzione del corpo vivente è per essenza caldo, ma se lo consideriamo separatamente da questo, come semplice omeomero, esso è evidentemente freddo. Quanto abbiamo qui esposto trova conferma nel nostro testo [389b10]: finché conserva la propria «φύcιc» il sangue (così come midollo e sperma) è caldo, quando si corrompe e si allontana dalla propria natura non lo è più.

Da questo punto di vista quindi gli omeomeri che fanno parte dei corpi viventi sono definibili soltanto in relazione all'organismo tutto e non in sé, al punto che le loro proprietà sotto un certo aspetto vi sono, sotto un altro no. Questo dato di fatto ripropone la differenza tra gli omeomeri definibili come organici e quelli inorganici come i metalli ed i minerali.

Quando passa quindi alla ricerca sulle parti omeomere che possiamo chiamare biologiche Aristotele mostra di essere consapevole che la formazione

[10] Quindi anche gli elementi hanno forma e fine e non sono pura materia, come vuole Gill [1997, 145]. Ma si veda Düring 1976, 594; a p. 603 leggiamo ancora: «Ogni cosa materialmente esistente possiede una forma non appena ha una qualità.»

del ferro non è spiegabile allo stesso modo della formazione delle ossa e della carne. Leggendo il capitolo 12 si trova evidente conferma di questo mutamento di prospettiva.

Il lavoro di Aristippo quindi, all'interno del *corpus* aristotelico, rappresenta un momento importante nel passaggio alle opere biologiche,[11] come mostra la chiusura del capitolo XII. Il testo ebbe poi grande diffusione nel Medioevo per l'interesse che attirava da parte degli studiosi di alchimia; ma qui il discorso si farebbe troppo ampio, per la copiosa letteratura sull'argomento, e forse fuori luogo nell'orizzonte che abbiamo scelto per questa breve analisi.

Qualche ultima considerazione sul testo proposto. La Rubino ha fornito agli studiosi un altro elemento per una considerazione più complessa dell'opera di Aristotele. Abbiamo accennato al problema degli omeomeri, perché il testo di Aristippo si pone in valida contrapposizione al commento di Simplicio, che contiene molti frammenti di Anassagora e che ha grande fortuna ancora nella critica contemporanea. Sarà quindi di grande utilità per chi si interessa di fisica aristotelica, come, più in generale, per chi si occupa di scienza nel Medioevo.

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11 Il carattere anticipatorio del lavoro è sottolineato da Furley 1983.

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Ce petit volume de la Bibliotheca Teubneriana contient l'édition du P. Lit. Lond. 165 (Brit. Libr. 137 = Mertens-Pack^3 [MP^3] 2339), traditionnellement dénommé « Anonyme de Londres » ou, en latin, « Anonymus Londin(i)ensis », qui représente le plus long papyrus grec médical connu à ce jour. Postérieure de plus d'un siècle à l'*editio princeps*, publiée à Berlin, par l'allemand H. Diels [1893b], l'édition de Daniela Manetti est le fruit d'un travail commencé il y a plus de vingt-cinq ans, avec la parution de ses « Note di lettura dell'*Anonimo Londinese*. Prolegomena ad una nuova edizione » [1986], et poursuivi dans une dizaine de publications, dont « Autografi e incompiuti. Il caso dell'*Anonimo Londinese* P. Lit. Lond. 165 » [1994], dans lequel la chercheuse démontre le caractère autographe du papyrus, ou encore le très récent article « *Anonymus Londiniensis de medicina* (P. Br. Libr. inv. 137) xvii 21–22. Collocazione di un frammento *incertae sedis* » [2009]. D. Manetti a en outre partiellement édité, traduit et commenté l'*Anonyme de Londres*, dans le Corpus dei Papiri Filosofici (CPF) : c'est le cas pour les col. ii 18–iii 7 et xxx 15–24 [CPF Stoïciens 3T], v 35–vii 40 [CPF Hippocrate 32T], v 36–37, vi 42–43, vii 37–40, vii 42–43 et viii 10–12 [CPF Aristote 37T], xi 23–43 [CPF Hippon de Crotone 1T], xiv 6–xviii 8 [CPF Platon 129T], xviii 8–xix 1 et xx 16–24 [CPF Philolaos 1T], xix 1–18 [CPF Hippocrate 28]), xxiii 42–xxiv 9 [CPF Aristote 22T], et xxxvii 32–46 [CPF Démocrite 7T]. Enfin, depuis la parution de la présente édition, elle a publié l'article « *Medicina more geometrico demonstrata.* Cassio Iatrosofista *Problemi* 1 » [2011], où elle souligne les affinités, dans l'argumentation dialectique, entre les *Problèmes* de Cassius Iatrosophiste (200–240 apr. J.-Chr.), et l'*Anonyme de Londres* ; elle y annonce également qu'une comparaison approfondie de ces deux textes fera l'objet de l'un de ses prochains travaux.

A côté de D. Manetti, et si l'on excepte les études relatives à la « question hippocratique » [voir Jouanna 1992, 89–91], plusieurs chercheurs se sont inté-

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ressés au « papyrus médical de Londres » ces trois dernières décennies : ainsi, D. Gourevitch [1989], T. Dorandi [1992], M.-H. Marganne [notamment 2004, 66–70 ; 2010], ou L. Del Corso [2008, 43–46], pour ne citer que ceux-là. Très récemment, la recette médicinale écrite au verso du papyrus a été l'objet d'un poster, que nous avons présenté au XXVI^e Congrès International de Papyrologie [2010b], puis, d'une édition, par I. Andorlini [2010]. Dans le cadre de notre mémoire de maîtrise, soutenu en juin 2010 à l'Université de Liège [2010a], nous avions effectué une édition critique du papyrus, précédée d'une introduction, et pourvue d'une traduction française – disponible en ligne depuis janvier 2011, sur le site du Centre de Documentation de Papyrologie Littéraire de l'Université de Liège[1]–, ainsi que d'un commentaire. Enfin, dans un article paru dans *Archiv für Papyrusforschung und verwandte Gebiete* [Ricciardetto 2012], nous avons tenté de montrer les liens étroits entre le texte médical du recto et la copie d'une lettre de Marc Antoine, l'un des triumvirs (*ca* 83–30 av. J.-Chr.), au Κοινόν des Grecs d'Asie, écrite au verso du papyrus, et non éditée par D. Manetti.

Comme il est d'usage dans la Bibliotheca Teubneriana, la préface de l'édition de D. Manetti est écrite en latin ; l'éditrice y fournit quelques données générales sur l'*Anonyme de Londres*. Les conditions d'acquisition du papyrus, en Égypte, en 1889, dans un lot comprenant notamment la *Constitution d'Athènes* d'Aristote [P. Lit. Lond. 108 = MP^3 163], et les *Mimes* d'Hérondas [P. Lit. Lond. 96 = MP^3 485], sont brièvement évoquées, et on pourra compléter utilement ces informations par la lecture de son article « Proposte di collocazione di due frammenti in PBrLibr inv. 137 (*Anonimo Londinese*) e nuove letture » [1997, 141–143]. D. Manetti fait provenir le papyrus du nome hermopolite.[2] Toutefois, à la suite de G. Bastianini [1996, 84] à propos de la provenance du fameux papyrus de la *Constitution d'Athènes*, on pourrait penser, prudemment, à la capitale du nome, Hermopolis, un centre de grande taille, avec un public nombreux et diversifié, plus à même de s'intéresser aux textes contenus dans ces papyrus que ne le ferait un petit village périphérique. À partir de critères paléographiques et textuels, elle date l'*Anonyme* du I^er siècle de notre ère ; pour plus de détails sur cette datation, on se référera à son article de 1994, mentionné ci-dessus.

[1] http ://promethee.philo.ulg.ac.be/cedopal/PDFs/Anonyme%20de%20Londres_janvier%202011.pdf.

[2] « de loco quodam in nomo Hermopolita cogitare possumus » [vii].

Les caractéristiques bibliologiques du papyrus sont abordées dans plusieurs paragraphes. De dimensions remarquables (336,5 x 23,5 cm), l'*Anonyme de Londres* est aujourd'hui divisé en onze cadres. Le recto conserve 39 colonnes d'écriture, tandis que le verso, en majeure partie blanc, comme il est d'usage pour un rouleau, contient trois textes brefs :

(a) deux renvois au verso,
(b) une recette médicinale,
(c) une copie d'une lettre de Marc Antoine.

On possède également une quarantaine de fragments, qui figurent en fin d'édition [97–100], du moins quand leur emplacement originel n'a pu être déterminé. Vingt-trois de ces fragments n'ont été découverts qu'en 1900, et publiés un an plus tard, par F. G. Kenyon et H. Diels [1901], qui avaient pu replacer correctement sept d'entre eux ; n'ayant pu retrouver les autres, pour lesquels on ne possède par ailleurs aucune photographie, D. Manetti a été contrainte de reproduire les transcriptions des premiers éditeurs [99–100]. L'éditrice décrit brièvement la constitution du rouleau, et son état actuel : il doit probablement manquer une à deux colonnes au début, tandis que la dernière n'est écrite qu'à moitié ; en outre, on a perdu une à plusieurs colonnes entre les colonnes ix et x, aujourd'hui très fragmentaires. Quant au nombre de lignes par colonne, comme le nombre de lettres par ligne, il augmente au fur et à mesure que l'on progresse dans le texte.

Trois « signes » de ponctuation sont récurrents dans l'*Anonyme* : l'espace blanc (*spatium vacuum*), la *paragraphos* et la *diplē obelismenē* ; ces deux derniers signes se situent dans l'interligne, et sont généralement combinés à l'*ekthesis* d'une à deux lettres dans la ligne suivante. Cependant, l'usage de l'*Anonyme* n'est pas constant, et les exceptions sont énumérées dans une note [ix n13]. Dans une autre note [n14], l'éditrice considère avec raison que le « point épais » identifié par H. Diels en xxxi 25, doit être une tache d'encre, tandis qu'elle ne dit mot de la signification du trait oblique (« / ») placé dans la marge gauche de xix 35. Quant aux traces d'encre, qui ne semblent pas être des lettres, situées entre les colonnes xiii 7–8 et xiv 7–8, il s'agit peut-être de traits ornementaux [voir xiv 7–8, dans l'apparat]. Enfin, dans la marge gauche de xxiv 43, le petit trait légèrement descendant, que l'éditrice ne sait expliquer (« nescio qua ratione, scripsit P »), est en réalité une fibre, d'après les vérifications que nous avons pu effectuer en autopsiant le papyrus, à la British Library de Londres, en mars 2012.

Les deux pages suivantes [x–xi] sont consacrées aux phénomènes linguistiques présents dans le texte de l'*Anonyme*, à savoir

(a) l'usage de l'iota adscrit, rarement omis par l'auteur,
(b) l'emploi inconstant du « ν » épenthétique,
(c) diverses « erreurs orthographiques »,[3] parfois corrigées par l'auteur, dont les plus fréquentes s'expliquent par l'iotacisme.

On trouve aussi de nombreuses abréviations [xi–xii], que l'éditrice distingue en « compendia » et en « abbreviationes ». Les premiers, très fréquents, peuvent être utilisés seuls, au milieu d'un mot, ou dans des composés : par exemple, le signe « / » pour « ἐϲτίν » ; « \\ » (et non « // », comme elle l'écrit) pour « εἰϲίν » ; « γ́ » pour « γ(άρ) » ; « κ̂ » pour « κ(ατά) » ;[4] « μ́ » pour « μ(εν) ».[5] Les seconds sont de deux types, soit par suspension d'une lettre,[6] ou par combinaison de deux lettres,[7] soit par insertion d'un petit trait horizontal au-dessus d'une lettre.[8] Ce trait sert également à noter les chiffres (« ᾱ, β̄, γ̄ »), afin de les distinguer des lettres. Fait remarquable, il arrive que le nom d'un médecin, Hérophile de Chalcédoine [xxxvi 47], et de deux sectes médicales, les Empiriques [xxxi 26] et les Érasistratéens [xxxvi 18], soit abrégé. Quant à l'abréviation que H. Diels avait lue « ν̣̂ » [ii 30], pour « Ν̣(εωτέροιϲ) », à savoir les Stoïciens, il faut en réalité la lire, à la suite de D. Manetti, comme « μ́ », pour la particule « μ(έν) ».

Selon l'éditrice, l'auteur du texte médical du papyrus doit être un érudit anonyme,[9] qui a puisé dans de nombreuses doctrines, plutôt qu'un étudiant en médecine, ou un scribe, comme le suggéraient H. Diels [1893a, 410–411] – pour lui, il s'agirait de « notes privées d'un étudiant en médecine, copiées à partir d'un modèle lacunaire et abîmé » –, et W. H. S. Jones [1947, 4], qui y voit des notes d'un jeune étudiant en médecine, « tirées d'une série de leçons d'un professeur, et recopiées ensuite par un scribe » [voir Manetti 1986, 58]. En effet, si l'on excepte la recette médicinale et la lettre de Marc Antoine, écrites, au verso, par deux mains différentes, une seule main a tracé l'*Anonyme*.

3 « errores orthographicos » [x].
4 Par exemple, « κ(ατα)χθέντεϲ », etc.
5 Par exemple, « μ(έν), λέγομ(εν) » ou « μ(έν)τοι », etc.
6 Par exemple, « πνευ$^{\mu}$ » pour « πνεῦμ(α) », etc.
7 Par exemple, « ατ » pour « αἰ(τία) » et variantes ; « Λ » pour « λό(γοϲ) » et variantes, etc.
8 Par exemple, « γεν̄ » pour « γέν(ηται) », etc.
9 « anonymus vir doctus » [xii].

En outre, l'auteur corrige régulièrement ses erreurs, n'hésite pas à modifier la structure syntaxique de ses phrases tout en écrivant, et intègre souvent des additions interlinéaires et marginales, qui enrichissent le contenu de l'exposé. Une fois, il fournit une seconde version, mieux articulée, d'un sujet – la définition et la classification des affections (πάθη) – considérant la première imparfaite [i 16–38 et i 39–iii 7 ; Manetti 1994, 52–53]. Le travail de l'érudit n'est cependant pas terminé, puisque, sans raison apparente, il s'interrompt au milieu de la trente-neuvième colonne, tandis que, en vii 37, il promet de discuter d'un point (ὧϲ προϊόντοϲ ἐπιδείξομ(ε̣ν̣) τ̣οῦ λό(γου)/comme nous le montrerons au cours de l'exposé), mais il n'y reviendra pas. Réunies, ces observations ont conduit D. Manetti à considérer l'*Anonyme de Londres* comme un « autographe », c'est-à-dire « l'œuvre d'un savant qui médite sur le texte qu'il est en train d'écrire » [Dorandi 2000, 59].

À notre avis, si l'hypothèse de l'autographe est convaincante, un texte raturé, inachevé, parfois confus, tel que l'est sans aucun doute l'*Anonyme de Londres*, ne s'identifie pas nécessairement au « brouillon » d'un ouvrage que l'on est en train de composer. On ne peut exclure qu'il se soit agi des notes d'une personne qui étudie la médecine – pas forcément d'un étudiant, comme le voulaient H. Diels et W. H. S. Jones –, ou, plutôt, qui s'exerce à la dialectique, peut-être en vue d'un examen ou d'un concours de médecine : sur ce point, voir notre article [Ricciardetto 2012], évoqué ci-dessus.

Le début du papyrus étant lacunaire, on n'a pas conservé le titre initial, si toutefois il était noté ; comme le texte s'interrompt brusquement, il n'y a pas de titre final. H. Diels [1893a, 407, et l'introduction à son édition, p. xvi] le restituait par « Ἰατρικά », ou « Ἰατρικὴ ϲυναγωγή », d'après l'intitulé d'une œuvre doxographique, perdue, attribuée à Aristote, mais qui, si l'on en croit Galien,[10] aurait été écrite par l'un de ses disciples, Ménon. Se fondant sur cette référence, et sur le fait qu'Aristote est cité à diverses reprises dans le papyrus, en particulier comme source doxographique, dans la deuxième section, F. G. Kenyon, dans un article antérieur à la première édition de l'*Anonyme* [1892, 238], puis, surtout, H. Diels, avaient estimé que Ménon devait être la véritable source de cette section, que l'*Anonyme* avait peut-être utilisée par l'intermédiaire des Ἀρέϲκοντα (en latin, *Placita*), perdus, d'Alexandre Philalèthe (*ca* 50 av. J.-Chr.–25 apr. J.-Chr.), l'auteur le plus récent

[10] Galien, *In Hipp. nat. hom. comment.* i 2 [CMG 9.1, 15.23–30 = Rose 1886, fr. 373].

cité dans le papyrus. Devenue au fil du temps *communis opinio*, le bien-fondé de cette hypothèse n'a été remis en question que dans un article de D. Manetti [1986, 59–64 ; voir également 1990, 220]. Selon la philologue italienne [1990, 220–222], l'hypothèse qu'Aristote lui-même ait été l'auteur de l'œuvre doxographique que l'*Anonyme* a connue directement, et, par suite, utilisée et adaptée à ses intérêts, ne doit pas être écartée ; en effet, dans des listes d'ouvrages hellénistiques [voir Moraux 1951, 110 et 186–193], ainsi que chez Diogène Laërce [*Vita* 5.25], il apparaît comme l'auteur d'un traité *De la médecine* (Ἰατρικά ou Περὶ ἰατρικῆς), tandis qu'Alexandre d'Aphrodise, *In librum de sensu commentarium 1* [Wendland 1901, 6.19], nous apprend qu'il avait projeté d'écrire un traité *De la maladie et de la santé*/Περὶ νόσου καὶ ὑγιείας.

Dans la présente édition, le titre adopté est également « Ἰατρικά », que l'éditrice traduit en latin « De medicina » (mais toujours « Iatrica » dans l'en-tête des pages impaires de l'édition proprement dite). Toutefois, on l'a dit, le titre n'a pas été préservé, et il n'est même pas sûr qu'à l'origine, il y en ait eu un, si, comme nous le pensons, il s'agissait d'un texte qui n'était pas destiné à l'édition, mais de notes.[11] Il paraît dès lors plus prudent de ne pas le restituer – ce qui ne remet pas en cause l'emploi, par l'auteur, d'une doxographie aristotélicienne –, ou, si on le restitue, de le noter entre crochets droits indiquant la restitution,[12] pour donner au lecteur une idée du contenu du papyrus.

Le reste de la préface est consacré au contenu de l'*Anonyme*, à sa doctrine et à ses sources, en particulier Platon et Aristote, qu'il connaît bien et cite avec déférence. Le texte comprend trois sections : lacunaire, la première section [col. i–iv], nosologique, est consacrée à la définition de la notion de « maladie ». La deuxième section [col. iv–xxi] concerne l'étiologie des maladies, selon au moins vingt philosophes et médecins, dont six sont totalement inconnus par ailleurs : Alcamène d'Abydos, Timothée de Métaponte, Abas, Ninyas l'Égyptien,[13] Thrasymaque de Sardes, Phasitas (que H. Diels déchiffrait Phasilas) de Ténédos. Quant au nom « Héracléodore » [ix 5] conjecturé par

11 Cf. Marganne 2007a, 106, et l'extrait de la préface du traité *Sur ses propres livres* de Galien.

12 [« ἰατρικά »], mais on pourrait également songer à [« ὅσα ἰατρικά »], d'après Aristote, *Problèmes* 1.1.

13 Sur le papyrus on ne lit que « ὁ δὲ Αἰγύπτιος Νινυ[» ; voir à ce propos les considérations de Marganne 2007b, 127.

Diels, l'éditrice ne l'a pas retenu, puisqu'on ne lit que « [..].κλεοδω̣[» sur le papyrus, ce qui permet d'autres restitutions. Cette section est introduite par un sous-titre au nominatif, écrit sur deux lignes [iv 18–19], et en *eisthesis* d'environ huit à neuf lettres par rapport au reste de la colonne. D. Manetti le déchiffre et le restitue de la manière suivante : « Α̣ἰ̣[τιο]λο̣γ̣ι̣κ̣ό̣ϲ | νόϲ̣ο̣ι̣ », ce qu'il faut vraisemblablement comprendre « Α̣ἰ̣[τιο]λο̣γ̣ι̣κ̣ὸ̣ϲ <λόγοϲ (?)>. | Νόϲ̣ο̣ι̣ »/« <Discussion> (ou <Discours>) étiologique. Maladies », dans lequel « νόϲοι » serait une variante de « αἰτιολογικόϲ ». De fait, le traité pseudogalénique *Le médecin. Introduction*, probablement contemporain de Galien, donne cet adjectif comme synonyme de « παθολογικόϲ ».[14]

Enfin, décrite en détail [xiv–xvii], la troisième section [col. xxi–xxxix] est physiologique. On mentionnera en particulier l'attribution à Hérophile d'un nouveau témoignage, grâce à une meilleure lecture de xxxvi 47, où le nom de ce dernier se trouve abrégé, à côté de celui d'Asclépiade, écrit en entier : sous le titre « Hérophile et Asclépiade : une relation scandaleuse ? », cette découverte a été exposée par D. Manetti lors d'un séminaire à l'Université de Liège, le 19 mars 2009. La présence du médecin de Chalcédoine, à côté d'Asclépiade, est d'importance, puisque la citation qui suit directement la mention de leur nom ne se rapporte plus seulement au médecin de Bithynie, comme le pensait H. Diels, mais aussi à Hérophile. Voici le texte de l'édition de D. Manetti, pour le passage concerné [xxxvi 47–50] :[15]

> κ̣[αὶ θ]α̣υ̣μ̣[α]ϲ̣το̣ὶ̣ καὶ Ἡρ(όφιλοϲ) καὶ Ἀϲκληπιάδηϲ | διά̣ τ[ι]νοϲ ὑπ̣ομνήϲεωϲ τοιαύτηϲ· ἡ φύϲιϲ – φ(αϲὶ) – | τ[ηρ]η̣τ̣ι̣κ̣ὴ κ[α]θέϲτηκεν τοῦ τε δικαίου καὶ | τ[ο]ῦ̣ ἀ̣[κ]ο̣{υ}λούθου
>
> [Comme, suivant ce qui est observable par la raison, et suivant la perception, différentes et variées sont les émanations qui proviennent de nous, ainsi aussi, suivant la perception et suivant ce qui est observable par la raison, (une différence) s'insinue en nous], ce dont *Hérophile et Asclépiade* se sont étonnés, à

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14 viii.1 :

> Μέρη ἰατρικῆϲ τὰ μὲν πρῶτά ἐϲτι πέντε, τό τε φυϲιολογικὸν καὶ τὸ αἰτιολογικὸν ἢ παθολογικὸν κτλ.
> Les parties premières de la médecine sont au nombre de cinq : la physiologie, *l'étiologie* ou *pathologie*, etc. [trad. Petit 2009].

15 Il faut noter que les deux premiers mots sont restitués par l'éditrice en note.

> travers un rappel de ce genre : « La nature », *disent-ils*,[16] « vigilante, préserve ce qui est juste et conséquent ».

Il en va de même en xxxix 5 :

> Et *ils disent* en outre : « Comme la nature veille à ce qui est juste, etc. »

On doit probablement rattacher la citation de ces deux médecins aux théories opposées (un « couple improbable », selon D. Manetti), à la mention, par l'*Anonyme*, d'un autre auteur, disciple d'Asclépiade, puis directeur de l'école hérophiléenne d'Asie mineure – une région directement concernée par la copie de la lettre de Marc Antoine, et qui constitue le lieu possible de rédaction de l'*Anonyme* –, Alexandre Philalèthe. En outre, selon D. Manetti, c'est peut-être aux Ἀρέϲκοντα de cet auteur, souvent cité en compagnie d'Asclépiade, que l'*Anonyme* aurait puisé ses informations sur le médecin de Bithynie.

L'édition proprement dite de l'*Anonyme de Londres* comprend le texte grec, accompagné de *testimonia* et d'un apparat critique, en latin. Lorsque le déchiffrement est trop incertain, D. Manetti indique dans l'apparat sa lecture du papyrus, et, parfois, d'autres lectures possibles ; on y trouve également des propositions de restitutions, qu'elle n'adopte pas, ainsi que toutes les lectures et conjectures que H. Diels et d'autres chercheurs ont émises, de la fin du XIX^e^ siècle à nos jours. À l'inverse de H. Diels, l'éditrice a normalisé l'orthographe, reléguant en note les « erreurs orthographiques », les lettres ou les mots supprimés par l'*Anonyme*, les signes indiquant l'addition interlinéaire (« ˋ...ˊ ») ou marginale (P^mg^), et les espaces blancs.

Les progrès réalisés depuis la première édition sont considérables.

(1) D. Manetti propose régulièrement des restitutions là où H. Diels avait renoncé à en faire. Par exemple, en ii 2, elle suggère « [ἀντ]ιδιαϲ[τέλλ]ε̣ϲθαι »/« opposer » [cf. i 19–20], contre « [..] ἰδίαϲ [.....]ά̣ϲθαι » chez H. Diels. En x 35, elle restitue « λ]απαρὰν̣ »/« gonflé, mou, flasque » – dont c'est la seule attestation dans le papyrus –, contre «]απαρα » chez Diels. En xxvi 16–19, elle édite

> καὶ γὰρ[17] τὰ θαν(άϲιμα) | τῶν [φαρμά]κ̣ων̣ ἐ̣ρ̣ο̣ῦ̣μ̣(εν) ˋτροφὴν̣ˊ, ἐπειδήπερ οἱ ὄρ̣τ̣υ|γεϲ ϲ[ι]τ̣ούμ̣ε̣νοι τὸ κώνειον τρέφουϲι | [το]ὺϲ ἀνθρώπουϲ

16 Diels éditait « φ(ηϲί) »/« dit-il ».

17 Il faut lire « γ(ὰρ) ».

> et en effet nous dirons que, parmi les drogues, les mortelles sont une nourriture, puisque les cailles, en en mangeant, nourrissent les hommes de ciguë[18]

contre

> ˋ[διὰ]ˊ τί γὰρ τὰ θαν(άϲιμα) τῶν [....]ν κα[..]εụοụ[..]ẹπι[.] ˋτ̣ρ̣ο̣φ̣ή̣ˊ, ε̣ἴ̣περ οἱ ο̣ἴϲ̣υ|πο̣ϲ [..]ουν̣[...]οι τὸ κώνειον τρέφουϲι | τ̣ο̣ὺ̣ϲ̣ ἀνθρώπουϲ

chez H. Diels.[19]

(2) L'éditrice choisit parfois les solutions que l'érudit allemand avait suggérées dans l'apparat. Par exemple, en viii 35, elle opte pour « Ἄ[β]α̣ϲ », que H. Diels avait proposé dans l'apparat, à côté de « Α[ἴ]α̣ϲ », ou « Ἀ[.]ο̣ϲ », mais, pour les lettres suivantes, mal conservées, elle renonce à sa transcription « δ[ὲ] ἰ[δίω]ϲ », et ne restitue rien ; peut-être faut-il y voir un ethnique, et comprendre, avec F. G. Kenyon, « δ' ὁ 'Ι....ϲ » (pour δ' « ὁ 'Ι[αϲεύϲ] », à savoir « d'Iasos », une ville de Carie), ou encore, selon elle, « δ' [ὁ] Ἴ[κιο]ϲ », c'est-à-dire « d'Ikos », une île de la mer Égée, au nord de l'Eubée [cf. Tite-Live, *Hist. rom.* 31.45].

(3) Pour des raisons paléographiques, ou de contenu, D. Manetti propose fréquemment de nouvelles restitutions, qui emportent souvent notre conviction : par exemple, en ii 9, « [ἣν παραλείπομ(εν)] » – il s'agit de la seule attestation de ce verbe dans le papyrus –, contre « [ἀλλ' ἐπὶ ἐκεῖνα ἴωμ(εν)] » chez Diels ; en vii 29, « τ[ὸ πάθοϲ τίκτει] » contre « τ[ίκτει] » ; en xiii 29, « μυξ̣[ῶ]ν̣ » (dans le sens de « narines »)[20] contre « μυ[κτήρων] » ; voir également xxvi 9, 14, xxx 28, etc.

(4) Aux lignes très lacunaires, où toute conjecture se révèle hasardeuse, l'éditrice préfère ne pas restituer, ou, alors, elle le fait prudemment, en note ; par exemple, en ix 20–44, elle laisse de côté toutes les réécritures de l'*editor princeps* à propos des théories d'Hérodicos[21] et de « Niny[as] ».

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18 Sur ce que l'on nomme le « coturnisme », à savoir une intoxication suite à l'ingestion de chair de caille, voir Amigues 2008, 100–102.

19 Pour la leçon « ὄρτυ|γεϲ »/« cailles », contre « οἴϲυ|ποϲ »/« suint » ou « graisse de la laine », chez H. Diels, voir Kotsia-Pantele 1989.

20 Cf. Radt 1977, fr. 89 (Sophocles) = Élien, *Nat. an.* 7.39, à propos des naseaux d'une biche.

21 H. Diels l'identifie à Hérodicos de Sélymbrie, un médecin contemporain ou légèrement postérieur à Hippocrate, mentionné à diverses reprises par Platon [*Prot.* 316e, *Phèdre* 227d, et *Républ.* 406a], mais voir désormais Manetti 2005.

(5) L'éditrice a corrigé ou abandonné diverses lectures de H. Diels. Par exemple, en iv 34, il faut lire « ἐκπονήϲῃ », et non « ἐκπέμπῃ » [voir également vii 20–21, xii 6, 26, xx 4, etc.]. En xxxvi 57–58 – désormais un *locus desperatus* –, au lieu de lire, comme H. Diels, « καιατῶν ἢ | καπν(ῶν) », à savoir « [des remèdes] à base de calament et de fumeterre », elle édite « καὶ †ατωντι|μαπατ(ων)† » (les autres lectures possibles sont énumérées dans l'apparat). En plus de contenir des citations d'auteurs hellénistiques, auxquels la philologie de la fin du XIXe siècle accordait en général peu d'intérêt, les dernières colonnes du papyrus, assez fragmentaires, ont en outre souffert de la hâte avec laquelle l'*editio princeps* avait été effectuée, et ont ainsi particulièrement bénéficié de la relecture minutieuse de D. Manetti.

(6) L'éditrice ne retient pas certaines corrections de H. Diels, qu'elle considère « non nécessaires » : voir, par exemple, i 11, où Diels avait inséré la particule « <μὲν> » entre « κείνηϲιν » (pour « κίνηϲιν ») et « πάντα ».

(7) En plusieurs endroits, son interprétation du texte est différente de celle de H. Diels, mais l'absence de traduction ne permet pas toujours d'en rendre compte aisément. Les lignes viii 14–17 en constituent un exemple. Voici le texte tel qu'il avait été édité par H. Diels en 1893b :

> ὅταν μ(ὲν) γὰρ[22] ἡ κεφαλὴ[23] ὑγιὴϲ | ἢ καὶ {[καθ]αρά} καθαρά, καὶ ἡ τροφὴ ἀπ' αὐ|τῆϲ προϲτίθεται[24] τῶι ὅλωι ϲώματι ⟦κἀὶ ὁ⟧ ὑγιαίνει τὸ ζῶιον,

que l'on traduit

> lorsque la tête est saine et pure, et que, de là, la nourriture est intégrée au corps tout entier, l'être vivant est sain.

H. Diels, suivant sur ce point F. G. Kenyon, considère donc la deuxième occurrence du mot « καθαρά » [l.15] comme une dittographie ; notons en outre que la lettre « ο », supprimée par l'*Anonyme*, doit sans aucun doute être lue comme « ō », à savoir « ο(ὕτωϲ) ». Cependant, la syntaxe d'une telle phrase pose problème : en effet, pour que celle-ci soit correcte, il faudrait, soit supprimer « καί » à la ligne 15, soit corriger l'indicatif présent « προϲτίθεται » du papyrus en un subjonctif (« προϲτίθηται »). C'est pourquoi, dans son édition, D. Manetti

[22] Il faut lire « γ(ὰρ) ».

[23] Pour Diels, il faut lire « κοιλία » ; correction « non nécessaire », selon Manetti.

[24] Il faut lire « π(ροϲ)τίθεται ».

propose une ponctuation différente, ne considérant pas la seconde occurrence du mot « καθαρά » comme une dittographie, mais jugeant les lettres « καιō », écrites après le mot « cώματι » [l.16], comme ayant été erronément supprimées par l'auteur (en réalité, pour D. Manetti, seul le « και » est supprimé par l'auteur, qui a exponctué les lettres, mais, comme H. Diels, nous pensons que le « ō » l'est également). La philologue italienne obtient donc le texte suivant :

> ὅταν μ(ὲν) γ(ὰρ) ἡ κεφαλὴ ὑγιὴc | ἦ̣ καὶ [κα]θαρά, καθαρὰ καὶ ἡ τροφὴ ἀπ' αὐ|τῆc π(ροc)τίθεται τῶι ὅλωι cώματι, καὶ ο(ὕτωc) | ὑγιαίνει τὸ ζῶιον

qu'il faut vraisemblablement traduire

> en effet, lorsque la tête est saine et pure, la nourriture, qui est également pure, est intégrée au corps tout entier à partir d'elle, et, ainsi, l'être vivant est sain.

Par cette nouvelle interprétation, la phrase est syntaxiquement correcte, et il n'est pas nécessaire de supprimer le second « καθαρά » ; on se demande toutefois ce qui a bien pu pousser l'auteur à supprimer les termes « καὶ ο(ὕτωc) ».

Mentionnons encore la correction proposée par W. S. Schubring [1952, 419n1], et rapportée par D. Manetti dans son apparat : celui-ci, en se fondant sur la lecture de H. Diels, qui considérait les trois premières lettres de la première occurrence du mot « καθαρά » comme lacunaires, suggérait non sans hésitation la correction « [ἡ νεί]α<ι>ρα »/ « le bas-ventre », à lire pour « [ἡ κοιλίη] ». Cette dernière solution fut finalement adoptée par E. Craik, dans son édition des *Lieux dans l'homme* d'Hippocrate [1998, 128].

(8) D. Manetti revient parfois aux solutions de H. Diels, et laisse de côté les hypothèses qu'elle avait un temps formulées dans d'autres publications : voir, par exemple, vii 1–2, où elle édite « λέ]||γ̣ει δι[», comme Diels, tandis qu'en Manetti 1996, 296, se fondant sur un passage, proche du nôtre, de *Nature de l'homme* 2 [Jouanna 1975, 170.3–7], elle avait édité

> λέ]||γ̣ει δι[ττῶc γί(νεc)θ(αι) τὰc νόcουc ἢ ὑπὸ τ(ῶν) ἐκτὸc]
> il dit que les maladies surviennent de deux manières, soit de ce qui est extérieur, etc.

On note ci-dessous quelques divergences dans notre lecture du papyrus, que nous avons pu examiner autoptiquement. Ainsi, en iii 44–45, pour des raisons de taille de lacune, nous restituons « ἀρ]|[ρ]ω[cτί]α »/« faiblesse », plutôt que « ἀρ]|[ρ]ώ̣[cτημ]α »/« infirmité ».[25] En iv 10, nous lisons sur le papyrus « πά̣θος̣ »[26] un accusatif, sujet de la proposition infinitive complément du verbe « cυμβέβηκεν̣ » – plutôt que « πάθο̣υ̣ς̣ », comme l'éditent H. Diels et D. Manetti –, ce qui donne [iv 7–10] :

> Εἰρῆcθαι δὲ τὸ̣ πάθοc | cυμβέβηκεν̣ [ἀπὸ] π̣α̣ρ̣α̣κολουθ̣οῦντοc | [ἢ] ἀ̣π̣ὸ̣ τ̣ό̣π̣ου· ἀπὸ μ(ὲν) γ(ὰρ) π̣α̣ρ̣α̣[κολουθ]ο̣ῦντοc | πά̣θος̣ ε̣ἰ̣ρ̣ῆ̣c̣θ̣α̣ι̣ τὸν π̣[υρε]τ̣ὸ̣ν̣ κτλ.
>
> Il se trouve que l'affection est dénommée d'après le symptôme qui l'accompagne, ou d'après sa localisation. En effet, est dénommée « affection » d'après le symptôme qui l'accompagne : la fièvre, etc.

En xiv 37, on distingue peut-être «] καί », plutôt que « ἐ]π̣ε̣ί », que restituent H. Diels et D. Manetti. En xviii 7, nous ne distinguons pas le trait descendant, situé dans la marge gauche, signalé en note par D. Manetti ; il s'agit vraisemblablement d'un trou dans le support. En xxviii 13, entre « μ[(ὲν)] » et « καὶ δ⟦ ~~ιοτ~~ ⟧ιὰ τὰc », on distingue les traces de deux lettres, non remarquées par D. Manetti, qui correspondent peut-être à l'abréviation « ΓΙ », à savoir l'une des formes du verbe « γί(νομαι) » ; si cette lecture se confirme, la correction « <(ἐcτιν)> » ne s'avérerait plus nécessaire. En voici le texte grec [xxviii 12–14], tel que nous le comprenons :

> τ̣[ούτ](ων) | οὕ̣τωc ἐκ̣κειμέν(ων), ὅτι μ[(ὲν)] καὶ γ̣ί̣(ν̣ε̣τ̣α̣ι̣) δ⟦ ~~ιοτ~~ ⟧ιὰ τὰc ἀρτηρία{ω}c ἀνάδοcιc, ὑπεμνήcαμ(εν), κτλ.
>
> cela étant ainsi établi, que la distribution [i.e., de nourriture] a également lieu par l'intermédiaire des artères, nous l'avons rappelé, etc.

En xxx 14, nous lisons « ελκου̣.[c. 8]κ̣ν̣ω̣ (ou]κ̣ν̣ο̣) », tandis que l'éditrice voit « ελκοτ̣ι̣[c. 8]κ̣ν̣ο̣ (ou]κ̣ν̣ω̣) » et suggère la conjecture « ἑλκωτ̣ι̣[κὸν καὶ δά]κ̣ν̣ω̣(δεc) »/« ulcératif et mordant » (en parlant de l'urine), qui, pour le premier mot, ne s'accorde pas avec ce que l'on déchiffre sur le papyrus.[27] À la page 99, D. Manetti édite les « frustula » du cadre n° x, que H. Diels se borne à mentionner dans son édition [1893b, 76], mais elle oublie le fragment, placé à l'envers, du cadre n° xi, qui était également mentionné par Diels, et sur

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[25] Pour une construction similaire, voir iii 39–40, où c'est le terme « νόcοc »/« maladie », qui est employé, et non « νόcημα »/« condition morbide ».

[26] Cette lecture était déjà suggérée par W. H. S. Jones [1947, 30] : « In 1.10, perhaps πάθοc ».

[27] H. Diels éditait « ἑλκού̣ς̣[ηc, δῆλον] ».

lequel on ne voit guère que la trace d'une lettre sur une première ligne, deux lettres («]πω[») sur la suivante, et la trace d'une lettre sur la dernière.

On mentionnera enfin quelques coquilles dans le texte grec. L'oubli des parenthèses, pour signaler un mot abrégé, est assez fréquent ; c'est le cas, ainsi, de

μ(έν)/μ(εν)	i 5, ii 18, iii 17, ix 39, xii 5, xvii 14, xxii 45, xxvi 49, xxviii 15, xxix 7, xxx 37, xxxviii 4
τ(ῶν)/τ(ων)	vi 37, xiii 22, xvii 13 *bis*, 44, xviii 36, xxiii 13, xxv 47, xxvi 3, 23, xxvii 8, xxxi 2 *bis*, xxxv 1, xxxvi 26, xxxvii 29, xxxviii 56, xxxix 28
π(ρόϲ)/π(ροϲ)	xxii 40, xxiii 6, 7, xxv 16, xxvi 3, xxxii 49, xxxvii 36, xxxviii 27, xxxix 1
γ(άρ)	iii 31, v 37, vi 19, vii 30, viii 19, xiii 14, xiv 32, xxiv 27, 43, xxv 34, xxvi 16, xxvii 10
κ(ατά)/κ(ατα)	xxvi 48, xxxiv 52, xxxviii 40.

En xxi 32, il faut écrire « (ἐϲτι) » et non « ἐϲτι » ; en xxv 8, « γινομένηϲ » et non « γινομέν(ηϲ) ».

On trouve également plusieurs erreurs dans l'accentuation des mots, surtout en présence d'enclitiques ou de ponctuation,[28] et, parfois, l'orthographe « τῷ̣ », alors que l'*Anonyme* écrit « τῶι » [xx 24].[29] En xiii 18, il faut écrire « ἔχῃ̣ », et non « ἔχηι » ; en xxii 25, « ταῦ̣τ̣ῃ̣ » et non « ταῦ̣τ̣ηι̣ ».

Enfin, remarquons qu'en vi 32, il faut écrire « ἀναθυμ(ιαθεῖϲαι) » et non « ἀναθυμ(ιαϲθεῖϲαι) » ; entre les lignes xii 28–29, il y a une *paragraphos* non signalée par D. Manetti (de même, en xx 37–38 et en xxx 39–40) ; en xiv 22, « πυροῦ » est évidemment le génitif de « (ὁ) πυρόϲ »/« le blé », et non de « (τὸ) πῦρ »/« le feu », comme indiqué dans l'index ; en xxvi 49, il faut écrire « δ' (ἐϲτι) » et non « δέ (ἐϲτιν) » ; en xxxii 30, « ὀλιγ[ότρ]ο̣φοι » et non « ολιγ[ότρ]ο̣φοι » ; en xxxii 35, « ἐλάττω⟦ι⟧ » et non « ἐλάττω{ι} » ; en xxxii 47, « εγδη » (pour « ἐκ δή »)[30] et non « εγδε » (pour « ἐκ δέ ») ; en xxxiii 29–30, le mot

[28] Voir, notamment, iii 38–39, iv 9, vii 18, viii 15, 19, 32, xv 8–9, xvii 22, xviii 21, xxi 14–15, xxiv 45, xxvi 5 *bis*, xxviii 31, xxix 23, xxx 10, xxxii 52, xxxiv 32, xxxv 33, xxxvii 24, 28, xxxix 17

[29] C'est le cas également en xvi 23 (« ῥαιδίωϲ » et non « ῥα̣δίωϲ », mais voir la note *ad loc.*), xxvi 30, xxxvi 37 et xxxix 32.

[30] À noter que H. Diels édite « ἐκ δή ».

« θηρευ|ταί » est écrit par l'auteur « θηρο|ται », et non « θηρω|ται » ;[31] en xxxiv 17, il faut écrire « ὑφ' ἡμῶν » et non « ὑφ ἡμῶν » ; en xxxv 7, « ἀ̣ποφέρ̣[(εται)] » et non « ἀ̣ποφέρε̣[ρ(εται)] » ; en xxxv 48, « ἀποφο[ρ]ὰ̣ » et non « αποφο[ρ]ὰ̣ ».

L'ouvrage est complété par l'index des noms communs et des noms propres. On trouvera ci-dessous une liste des termes « nouveaux », c'est-à-dire absents du texte de la première édition, ensuite, une liste des termes qui ne figurent plus dans l'*Anonyme* (les mots entre « [...] » sont partiellement ou totalement restitués). Sont attestés, dans l'édition de D. Manetti, mais non dans celle de H. Diels, les noms :

> ἀγαρικόν,[32] ἄθλιπτος, [ἀλογιστία], [ἀμύητος], ἀπόμνυμι, [ἀποφαίνω], ἅπτω, ἀτάρ, ἄτοπος, [γεννάομαι], [δακνώδης],[33] δάκνω,[34] [διασκίδνημι],[35] [διασπείρω] *bis*,[36] διάστασις, διαστολή, [δισσῶς],[37] δυσκράτως *bis*,[38] [εἰλύω][39] ἐκπονέω,[40] ἑλκωτικός,[41] [ἐμφαίνω], ἐμψύχω, [ἐναλλαγή],[42] [ἐπιχειρητέος], [εὐκόλως], εὐκράτως *bis*,[43] εὐοδέω, [εὐρύνω], [θέρεια], [θνητός],[44] κακόχυμος ou κακόχυλος,[45] καταβρίθω, κατακινέω, καταρράπτω,[46] κατέχω,[47] [λεαίνω], μείγνυμι, [μειζόνως], μελετάω, [μετρίως],

[31] Voir dans l'apparat : « θηρωται P ».

[32] L'agaric est le deuxième ingrédient de la recette au verso. De son côté, H. Diels déchiffrait « τάριχος »/« salaison ».

[33] Cette restitution est douteuse.

[34] Le mot est suggéré dans l'apparat. Son déchiffrement est incertain.

[35] H. Diels restituait « [κατασκίδνημι] ».

[36] H. Diels restituait « [κερματίζω] ».

[37] Cette restitution est suggérée en note.

[38] H. Diels lisait « δυσκρότως », au lieu de « δυσκράτως ».

[39] Cette restitution est préférable à celle de H. Diels (« [εἰλυσπάομαι] »), mais peut-être faut-il conjecturer « [ἐκλύω] », selon Manetti en note.

[40] H. Diels déchiffrait « ἐκπέμπω ».

[41] Voir cependant notre remarque ci-dessus, p. 90.

[42] Cette restitution est suggérée en note. H. Diels conjecturait « [ἐναλλάττειν] ».

[43] H. Diels lisait « εὐκρότως », au lieu de « εὐκράτως ».

[44] Cette restitution est suggérée en note.

[45] Sur ce mot, voir également l'apparat de H. Diels.

[46] Ce mot n'est attesté qu'une fois [xxii 29 « κατεραμμένον », pour « κατερραμμένον »]. H. Diels rattachait cette forme participiale au verbe « καταρραίνω ».

[47] Pour D. Manetti, dans son index, le verbe « κατέχω » n'est attesté qu'une seule fois [xxix 18], sous la forme « κ̣αθήξει », qu'il faut selon nous interpréter comme la 3e pers. de l'indicatif futur du verbe « καθήκω ».

[οἴδημα], ὄρτυξ,[48] [παραλείπω], [περίcθημι], πλεύμων,[49] [πληcίον],[50] ποικιλτός, πρό, [προπάcχω], προcαναπληρόω, προcαρτάω,[51] [προcίcτημι], [προcφορά], πῦον, [cιτέω], cτάμνος, [cυγγίνομαι],[52] cυναιcθάνομαι, cυναναcτομόω, cύνειμι, τοιούτως, [τρέχω], [τρῖψιc], τυρός, [ὑπέρ], [ὑπερμέτρωc],[53] [φλεγματώδηc], φύλλον, [χύλωcιc], ψεῦδος,[54] ὡcεί.

En revanche, les termes suivants ne figurent plus dans la présente édition :

[ἀγχόνη] (« ? »),[55] [ἀδήν], [ἀερώδηc], ἄλγοc, [ἀλλοιόω] (« ? »),[56] [ἀλογία],[57] ἀναξήρανcιc, [ἀπαλλάττω], [ἀπορέω] (« ? »), ἄρχω,[58] [ἀτμίζω], βάλλω, [βίοc], [βόειοc] (« ? »), βορά, [γλῶccα], [διαγωγή], [διαδίδωμι], [διαθρυλέω], [δίαιτα], διάκριcιc (« ? »), [διαπνοέω], διπλοῦc, δυcκρότοc *bis*, [δυcτυχῶc] (« ? »), [ἐγκαθίζω] *bis*, [εἴδωλον], εἵλη (« ? »),[59] [εἰλυcπάομαι], εἶμι (« ? »), [ἐλέγχω], [ἔμφυτοc], [ἐναλλάττω], [ἔντεχνοc], [ἐπείπερ], [ἐπιβιόω], [ἐπίδοcιc], [ἐπιθυμέω], ἐρεύγω (« ? »),[60] [ἕτοιμοc], εὐκρότωc *bis*, [εὔχυμοc] *bis*, [ἕψιc],[61] [ἥδομαι], [Ἡρακλεόδωροc], [ἠρεμέω], ἦχοc, [ἰατρόc], [ἰπνόc], ἴτριον, καιάταc, κακοχυμία, κάλαμοc *bis*, καλῶc, καπνόc, καταιcθάνομαι, [κατακαίω] (« ? »), [καταπνέω], καταρραίνω, καταρρέπω, [καταcκεδάννυμι], καταχυλόω, κάτοπτρον (« ? »),[62] [κερματίζω], [κορέννυμι], [λείανcιc], [λιμόc], λιτόc, [μέλλω], μεταιονάω,[63] [μηνιαῖοc], [νόμοc], νωθήc (« ? »),[64] οἴcυποc, [ὁμοιειδήc], [οὐδέτεροc], [ὀχυρόc], παραίτιοc,[65] [παραρρέω], [παραcκευάζω] (« ? »), [περιγίνομαι], [περιλαμβάνω], [πλάτοc], [πνέω], [πονέω], προαποδείκνυμι, [προcδέχομαι], [πρόcειμι], [προcτάccω],

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48 Sur cette leçon, voir l'article de P. Kotsia-Pantele [1989] évoqué ci-dessus.

49 Voir xix 45. H. Diels édite « πνεύμων ».

50 H. Diels restituait « [<παρα>πλήcιοc] ».

51 Le mot est suggéré dans l'apparat.

52 Ce mot est suggéré dans l'apparat.

53 H. Diels restituait « [ὑπερκόρωc] ».

54 H. Diels restituait « [ψεύδω] ».

55 Cette restitution est suggérée en note.

56 Cette restitution est suggérée en note.

57 Selon H. Diels, dan l'index, il faut peut-être corriger « ἀλογία » en « ἀλογι<cτί>α ».

58 Sur ce verbe, voir la note de Diels, dans son index.

59 Le mot est suggéré en note.

60 Sur ce verbe, voir l'apparat de Diels.

61 Cette restitution est suggérée en note.

62 Le mot est suggéré en note.

63 Le verbe était déjà considéré comme un *hapax* douteux par Liddell, Scott, et Jones 1968, *s.v.* « μεταιονάω ».

64 Le mot est suggéré en note.

65 Pour D. Manetti, ce n'est pas l'adjectif « παραίτιοc », mais le substantif « παραιτία », qui est attesté dans l'*Anonyme* [xxii 34].

[ῥήγνυμι], [Cηλυμβριανός], [σκώρ], [στέγω], συλλέγω, σύμμετρος *bis*, [συναφίημι], [συναύξω], [συστομόομαι], [τείνω], [τέρψις], τήγανον (« ? »), [τοπάζω], ὕαλος (« ? »), [ὑμήν], [ὑπερκόρως], [ὑποτύπωσις] *bis*, [ὕστερον], Φασίλας,[66] φλεγμασία, [ψεύδω], [ὦσις].

Résultat de près de trois décennies de labeur, ce volume met à la disposition des chercheurs une édition entièrement revue de l'*Anonyme de Londres*, papyrus exceptionnel, notamment par les nombreuses théories nosologiques, étiologiques et physiologiques qu'il contient, avec des citations des opinions de près de vingt-cinq médecins et philosophes anciens (tels qu'Aristote, Asclépiade, Démocrite, Érasistrate, Hérophile, Hippocrate, Philolaos, ou encore Platon), dont peu sont postérieurs au IVe siècle avant notre ère, et au moins six sont totalement inconnus par ailleurs. Concrétisant le souhait exprimé par H. Diels, lorsque, à la fin de la préface de son édition [1893b, xvii], l'érudit allemand reprenait à son compte ce passage des *Questions naturelles* de Sénèque [7.25.7], « que les générations apportent, elles aussi, quelque contribution à la vérité », l'édition de D. Manetti encouragera sans aucun doute de nouvelles études sur ce témoin capital tant pour l'histoire de la médecine et de la philosophie, que pour celle de la librairie antique. On attend avec impatience l'ouvrage qu'elle promet sur le texte, la doctrine et les sources de l'*Anonyme* [xvii n35]

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[66] En réalité, sur le papyrus, il faut déchiffrer « Φασίτας », plutôt que « Φασίλας ».

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Chasing Shadows: Mathematics, Astronomy, and the Early History of Eclipse Reckoning by Clemency Montelle

Baltimore, MD: Johns Hopkins University Press, 2011. Pp. xiv + 408. ISBN 978–0–8018–9691–0. Cloth $75.00

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Eclipses are the most dramatic of regular astronomical events. Temporarily removing one of the two most prominent objects from the heavens, they must have been terrifying to primitive observers; their universal interpretation as bad omens is no surprise. For cultures that studied the sky systematically, they posed some of the fiercest challenges to their predictive abilities. To handle an eclipse well, precise understanding of the motions of both the Sun and Moon is a necessity. Getting an eclipse prediction wrong would have been as public a failure as an astronomer could imagine.

Clemency Montelle's *Chasing Shadows* is a detailed technical study of the approaches of four different pre-modern cultures—Mesopotamian, Greek, Indian, and Islamic—to capturing these elusive phenomena. Eclipses are the obvious focus of discussion but the scope is actually much broader: eclipses are a well-chosen case study to observe historical astronomical practice in general. Although the four approaches are dealt with in separate chapters, the contrasts between them and their interactions are the highlight of the book. Montelle makes several telling cross-cultural observations and comparisons, while resisting the trap of engaging in philosophical or sociological exposition beyond the evidence. Much of the book may be a stiff challenge for the casual follower of the history of science; its heart is mathematical. But for those with a technical inclination and a bit of determination, *Chasing Shadows* rewards careful attention most richly.

The book opens with an inviting and accessible general introduction to the main actors, their perspectives, and their sources. Perhaps the most useful aspect of the first chapter is Montelle's discussion of the interplay between observation and theory. With limited apparatus and problematic effects

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such as parallax, ancient astronomers' observational data were necessarily limited in their dependability. Pre-modern astronomers often relied explicitly on shockingly few observations, and this contributed to their positing relationships between theory and observation that are foreign to a modern reader. Many misunderstandings of ancient astronomy derive from a failure to appreciate this and it is a credit to Montelle that she places the issue front and center.

The second chapter introduces the reader to the mathematical basics of eclipse theory that are needed to comprehend the more challenging discussions to come. These include notions such as the celestial sphere, the configuration of lunar and solar eclipses, and the measurement of eclipse magnitudes. Two aspects of eclipse theory are particularly important. The first is the identification of several types of lunar month (especially the synodic month or period of the Moon's phases and the draconitic month or period of the Moon's crossings of the ecliptic, the solar orbit on the celestial sphere). The second is the effect of parallax. Since we observe eclipses from the Earth's surface rather than from its center, different terrestrial observers see the Moon and Sun in slightly different places in the heavens. This has no effect on lunar eclipses (the Moon passes through the Earth's shadow, so everyone sees them simultaneously) but is crucial for solar eclipses, where a displacement of a few hundred kilometers can turn a total eclipse into a non-event. The concepts are beautifully depicted by diagrams from Charles Hutton's *Mathematical and Philosophical Dictionary* of 1795, which is a bit anachronistic for this book, perhaps, but still illustrative.

We begin with Mesopotamian eclipse reckoning. As the earliest substantial astronomical culture, the Mesopotamians had no choice but to make their own observations: there were no existing data on which to rely. The first fundamental record is *Enūma Anu Enlil*, an omen compendium from the second millennium BC comprising around 70 tablets. The omens contain no clear distinction between observations and predictions or even between events that are or are not predictable. Nevertheless it cannot be overlooked; traces of its content and structure may be found in surprisingly many places as late as medieval India.

The first millennium BC saw a recognition in Babylon of deeper patterns in the recurrences of eclipses, particularly the Saros cycle (223 synodic months = 242 draconitic months). One may wonder at the apparent incongruity between identifying and exploiting patterns in eclipse records, and considering eclipses to be portents of Earthly misfortunes. But modern notions of cause and effect were not present here, other than divine action. Indeed, Montelle frequently makes the point that Mesopotamian astronomers were interested solely in predictions of the heavens and appeared to have no interest whatever in underlying causes.

Babylonian eclipse reckoning reached its highest level of sophistication with the Astronomical Cuneiform Texts (319 BC–AD 42). Modeling various celestial motions using step and zigzag functions as building blocks, the authors of astronomical cuneiform texts were able to develop complicated arithmetic models to predict eclipses. Unfortunately, they were no more accurate than previous efforts had been. It would have been helpful to see relevant excerpts of some of the tables produced by these brilliant computational scientists, but Montelle's descriptions of the intricacies of the models in astronomical cuneiform texts and what is known of their motivations are nevertheless remarkably clear.

We turn next to eclipses in Greek astronomy. Montelle stresses the Greek desire for theoretical explanation, a need to begin with physical laws that govern the heavens and shape a resulting geometry that does what the heavens do. Of course, this feels much more familiar to us and more powerful. It was, in fact, partly through geometrical models that Greek astronomers first became aware of parallax and its importance. But without a sufficiently adequate number system and arithmetical apparatus, quantitative science was difficult or impossible. Transmission from Babylon, around the time of Hipparchus of Rhodes in the second century BC, brought both the number system and the observational data to allow geometry and arithmetic to merge into a system capable of both explanation and prediction (incidentally, giving birth to trigonometry).

Our knowledge of Hipparchus and his colleagues is sadly deficient because Claudius Ptolemy's *Almagest*, written almost three centuries later, achieved such dominance that it virtually obliterated all earlier texts. The *Almagest*'s clear mathematical exposition, carefully constructed in a precise logical order on a foundation of a small set of observations, became the archetype both in Greece and medieval Islam. Montelle's outline of the *Almagest*'s eclipse theory is typical of her coverage elsewhere in the book: a careful, step-by-step account of the mathematical arguments with occasional comments on transmission of parameters (mostly from Babylon) when something reliable can be said. Montelle also spends time on Ptolemy's *Handy Tables*, a manual deriving mostly from the *Almagest* but emphasizing computation and prediction rather than theoretical explanation.

At 130 pages, the chapter on India is almost as long as the chapters covering the other three cultures combined and its contents represent *Chasing Shadows*' most valuable contribution. At first read, Indian astronomy can seem almost incomprehensible. Part of the reason for this is the genre: since much the subject needed to be memorized as part of an oral tradition, it was composed in extremely concise verse, often with all explanations and commentary excised. Without Montelle's exegesis of the verses described in her chapter, deciphering these cryptic texts would seem nearly impossible.

Even when one understands the words, the texts cannot be read as one reads European astronomy or even the *Almagest*. Several times, concepts and methods from other cultures found their way into Indian astronomy and were modified heavily to fit their new context. There was almost no effort to reconcile contradictory approaches sitting side by side in a text or even intermingled into a single procedure. There was no need; logical consistency was not valued as dearly as in Greece or medieval Islam. Rather, Indian astronomers invented intensely clever computational schemes to predict eclipses and other phenomena (more or less inventing iteration to solve the difficult equations that arose), using received knowledge as helpful aids rather than as a foundation. It would be interesting to hear more on how Montelle coordinates this with her 'prediction *versus* theory' thesis of the previous two chapters. She makes the fascinating point that Indian astronomy was inherently conservative, unwilling to alter older approaches, due partly to their belief that the texts came from 'Gods and Sages' and were therefore inspired.

Indian astronomy is grouped into five schools or *pakṣas* defined mostly by geographical region rather than by chronology. The early works, called *siddhānta*s (astronomical treatises), show signs of transmission from both Babylon and Greece (this is contested by some) but with an entirely unique approach and a number of novelties. These include a streamlined trigonometry using the sine function rather than the chord, the use of iteration to solve the difficult problem of moving from true to apparent Sun-Moon conjunction, and the division of parallax into longitudinal and latitudinal components.

The nonagesimal, the point on the ecliptic 90° from the ascendant above the horizon, is an early Indian innovation; it played an important role in eclipse calculations and parallax, and found its way to Arabic astronomy as well.

Montelle outlines with care and precision the methods of a number of Indian astronomers, spending the most time with the two works, the *Brāhmasphuṭasiddhānta* and the *Khaṇḍakhādyaka* by Brahmagupta (seventh century AD), for whom the Brāhmapakṣa school is named. (Curiously, the latter work is in the Ārdharātrikapakṣa tradition; perhaps it is easier to outshine a rival by visiting his own house.) Brahmagupta often provides two methods to compute a given quantity: firstly in the traditional manner and secondly by his own more sophisticated approach. In both of these *pakṣas*, we find traces of a second episode of transmission of mathematical methods from Greece (particularly spherical trigonometry), although there is no evidence that Ptolemy's *Almagest* ever saw the light of day in medieval India. Montelle concludes her coverage of Indian eclipses with a jump forward from the 10th to the 15th century, with four treatises devoted exclusively to eclipses by Parameśvara, an astronomer of the Mādhava school in Kerala famous for its work with infinite series. We find here sophisticated methods of calculation, of course—although the astronomical approach to eclipses is not revolutionary—but we also find a new attitude of respect for the role of observations and a willingness to admit the possibility of future improvements.

Montelle's final visit is the astronomy of medieval Islam. Here the transmission story is also interesting and complex, although the evidence is easier to find. The early Islamic astronomers of the eighth and early ninth centuries worked mostly with Indian material, exemplified by al-Khwārizmī's *Zīj al-Sindhind* (a corruption of the word 'siddhānta'). A *zīj* was a comprehensive astronomical handbook filled with tables empowering the user to compute many astronomical phenomena, including eclipses. In their absence of theoretical discussions, *zīj*es resembled Ptolemy's *Handy Tables* more than the *Almagest*, although ironically they took inspiration more often from the latter. In al-Khwārizmī's eclipse parameters and calculations, Montelle finds evidence of Indian sources, although also a trace of the *Almagest*. The ninth century saw an increased presence of the Greek style of astronomy; by the time of al-Battānī's *zīj* (AD 900), the conversion to Ptolemy's way of thinking was complete. However, many of the mathematical methods that the Islamic astronomers exploited (such as trigonometric functions and iterative methods) were borrowed from India or, in the case of the late 10th-century revolution in spherical trigonometry, constructed by Islamic mathematicians themselves.

Montelle outlines the contributions of nine Islamic eclipse calculators, although mostly not in the depth of the previous chapter. One exception is al-Khāzinī, a 12th-century Iranian whose methods rely mostly on Ptolemy but with some Indian overtones. Two figures of particular interest are worth noting. Naṣīr al-Dīn al-Ṭūsī (13th century) played a major role in a movement to overturn the *Almagest* and propose new models of planetary motion—not to achieve a better fit to observations but to fit better certain cosmological constraints such as uniform circular motion. The *Tadhkira*, his book on the subject, nevertheless reveals Ptolemy's influence in its eclipse theory, although unlike those in the *Almagest* the parameters used by al-Ṭūsī permit the possibility of annular solar eclipses. Finally, Ibn al-Shāṭir (14th-century Damascus) not only developed complete alternate planetary models but actually constructed a *zīj* with them (the *Zīj al-Jadīd).* As revolutionary as this *zīj* was, Montelle shows that its eclipse theory contains traces of Greek, Indian, and earlier Islamic influence—exemplifying her larger point that eclipse theory was a grand collaboration between astronomers and cultures, a sometimes chaotic mixture of tradition and innovation, conservatism and revolution.

And this leads us to the chief value of the book. As a topic for a case study in the role of transmission of scientific knowledge between cultures, you cannot go wrong with eclipses. Our four cultures are, to some degree, incommensurable: the Babylonians cared only for arithmetic predictions, not geometric theories; the Greeks developed logical progressions from theory toward prediction; the Indians appropriated their heritage into systems of computational genius with not much concern for theoretical contradiction; and Islamic astronomers valued the primacy of cosmological theory even more rigidly than the Greeks. In these environments, what can be transmitted and what cannot may vary dramatically, depending on the situation. Montelle carefully, and appropriately, does not attempt grand philosophical conclusions, instead laying out the evidence and making observations pertinent to the data. Much more work (beyond eclipses) would need to be examined to make a larger case for the nature of scientific transmission; there is room here for several sequels.

The only substantial failing in *Chasing Shadows* that I can find to lament is its endpoint. If 'the primary purpose of this study was to determine the ways in which knowledge about eclipses was originated, developed, preserved, and transmitted' [325], it seems a bit arbitrary to stop just before the astronomy of Copernicus, Brahe, and Kepler. A rich discussion on these themes could have followed. Of course, full treatment of European eclipse theory may have required a second volume.

Overall, the book is splendid. It is a sophisticated scholarly work with important broader theses. It is technically accurate (with only a few trivial mistakes) and yet as clear as can be. Finally, it does not try to be more than it should be. *Chasing Shadows* will be a first contact for scholars on the history of eclipse theory for many years to come.

Πτολεμαίου Πρόχειροι Κανόνες. *Les «Tables Faciles» de Ptolémée: 1a. Tables A1–A2. Introduction, édition critique* by Anne Tihon

Πτολεμαίου Πρόχειροι Κανόνες. *Ptolemy's* Handy Tables*: 1b. Tables A1–A2. Transcription and Commentary* by Raymond Mercier

Publications de l'Institut Orientaliste de Louvain, 59a–b. Louvain-La-Neuve, BE: Université Catholique de Louvain/Peeters, 2011. Pp. vi + 210, vi + 219. 978–90–429–2429–1, 978–90–429–2436–9. Cloth €100.00

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Ptolemy's *Handy Tables* contain a set of astronomical tables intended to provide numerical solutions to the astronomy-related problems known at his time (second century AD) and a text explaining their use. In fact, it is the first known set of tables for this purpose ever compiled, inaugurating a genre that was mostly followed by Arabic astronomers in the handbooks called *zij*es. Ptolemy composed the *Handy Tables* after the completion of his *Almagest*, a major mathematical text also including tables which was the culmination of the astronomy developed by his predecessors. The *Handy Tables*, which are largely based on the models and tables in the *Almagest*, can be considered an updated version of it. In general terms, the driving force in the transition from the *Almagest* to the *Handy Tables* seems to have been that of providing tools to facilitate computation by offering new tables on certain topics (e.g., the equation of time) and newly presented tables on others (e.g., right ascension), by reducing an unnecessarily high number of sexagesimal digits in other tables (e.g., mean motions), by enlarging the number of entries in still others to simplify interpolation (e.g., planetary equations), and by defining a new epoch for timekeeping. Ptolemy succeeded in making the *Handy Tables* more user-friendly than the tables in the *Almagest* and they became a model for medieval astronomers to imitate.

The *Handy Tables* circulated extensively in Antiquity and the Middle Ages, directly or through commentaries, according to the number of fragments in papyri and manuscripts preserved (more than 45). The most widely diffused commentaries were those by Theon of Alexandria (fourth century), who

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commented on the *Handy Tables* twice, producing a *Great Commentary* [see Mogenet and Tihon 1985, Tihon 1991, and Tihon 1999] and a *Little Commentary* [see Tihon 1978]. The tables themselves were first edited in 1822 by N.-B. Halma, and then transcribed and analyzed by W. D. Stahlman in 1960.

As indicated by Anne Tihon and Raymond Mercier, the critical edition of Ptolemy's *Handy Tables* and the mathematical analysis of their contents is a large, and long awaited, project for which six volumes are planned. The first volume is presented here in two parts (called volumes 1a and 1b). This is not a jointly authored work, for Tihon takes care of the philological part [vol. 1a] and Mercier is responsible for the mathematical part [vol. 1b]. As a result, we are given two lists of references, two indexes, two lists of manuscripts and papyri, and two tables of contents. Two authors and four languages, as Tihon points out in the general preface: Greek (Ptolemy's), Latin (critical apparatus), French (Tihon's text), and English (Mercier's text). But there is more to it because in volume 1b we also find here and there pieces in Assyrian script [1b.55], Ethiopic [1b.75], Hebrew [1b.77], Syriac [1b.180], and Arabic [1b.192–194]. This has certainly required a complex process of typesetting and a considerable editorial effort, resulting in a superb edition; and thus it is only regrettable that some of the mathematical data and formulae are not properly presented [see, e.g., appendix G in vol. 1b].

In addition, Paolo La Spisa is responsible for a short paleographic note on an Arabic palimpsest in annex V of vol. 1a.

The scope of Ptolemy's *Handy Tables* is defined in volume 1a. It consists of 22 tables, of which 20 are astronomical, one geographical, and one chronological. That is, only the tables explicitly given in Ptolemy's text are considered to be authentic [1a.11–12], although other tables associated with the *Handy Tables* are presented in this volume. Volumes 1a and 1b deal with two astronomical tables for right and oblique ascension for the seven climates (Meroe, Syene, Lower Egypt, Rhodes, Hellespont, Mid Pontus, and Borysthenes). They also include the edition of a table for the oblique ascension of Byzantium, not belonging to the original *Handy Tables* but usually found among them and probably compiled in the seventh century [1a.9]. The other volumes planned in this project, numbered from 2 to 6, will be devoted to the tables for the two luminaries, planetary tables, star catalogues, a translation of Ptolemy's text, and an account of the manuscript tradition, respectively.

An accurate edition of the tables for right and oblique ascensions fill in most of volume 1a and exhibits the highest level of scholarship. The principle used for the edition consists in reproducing faithfully the tables found in one carefully chosen manuscript (Florence, Biblioteca Medicea Laurenziana, MS 28/26), and presenting all variants found in other manuscripts and papyri [1a.55]. This principle definitely seems the most reasonable and respectful option for transcribing astronomical tables.

Volume 1b gives a transcription of the three tables considered here, that is, right ascension, oblique ascension for the seven climates, and oblique ascension for Byzantium. In a few cases, we are also given additional data 'whenever any entry [in the table] departs from the correct calculated value' [1b.9], where 'correct calculated value' probably refers to values recomputed with modern means. Then follows an in-depth commentary in six chapters. Most interesting is the chapter devoted to chronology, where we find among other topics a large amount of information on the eras of Nabonassar and Philip, the death of Alexander, the regnal years of Alexander the Great and his successors, and the Seleucid era. The crucial point is the fact that in the *Handy Tables* Ptolemy uses as epoch the era of Philip (noon, −323 November 12) and not the era of Nabonassar (noon, −746 February 26), as he did in the *Almagest*.

Even more remarkable is the reconstruction of the working methods used by the 'calculator of column 3', as Mercier calls the author, to compute the entries of the equation of time. This is done by means of a very detailed analysis consisting in making various assumptions for the quantities and computing methods involved, up to 180 different combinations. Mercier argues convincingly that, in order to produce the entries displayed for the equation of time (not tabulated in the *Almagest*), the value for the tropically fixed solar apogee was taken to be 66°rather than 65;30°, which is the explicit value in the *Almagest* [see, e.g., 3.7]; and that the underlying values for the right ascension were derived by ordinary linear interpolation from those already found in the *Almagest*.

The other chapters and appendices contain insightful comments on oblique ascensions, the calculation of horoscopes, the textual tradition of the *Almagest* and the *Handy Tables*, and the derivation of modern formulas in relation to the tables analyzed in this volume.

As mentioned above, volumes 1a and 1b are the first installment of the complete edition of Ptolemy's *Handy Tables*. The two volumes presented by Tihon and Mercier set a very demanding standard for the rest of the work: a most respectful principle for editing astronomical tables, an insightful method of analysis of their contents, and an outstanding level of scholarship. When finished, this long-range project is certainly going to become a major contribution to the history of ancient astronomy.

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Nostradamus, une médecine des âmes à la Renaissance by Denis Crouzet

Paris: Payot, 2011. Pp. 460. ISBN 978–22–289–0644–9. Paper €27.50

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Aucun universitaire français ne s'était jusqu'alors aventuré à consacrer un ouvrage entier aux *Prophéties* de Nostradamus. Pierre Brind'Amour était canadien et avait obtenu de substantielles subventions pour son ouvrage de 1993 [cf. Guinard 2006–2011, 125]. Denis Crouzet aura surmonté la puissance des tabous et du bouclage idéologique dans les universités françaises, et osé traiter d'un sujet jusqu'alors réservé à l'édition populaire.

Crouzet se démarque en avant-propos de toute « prospective augurale » s'énonçant « dans une surenchère à la fois hallucinée et hallucinatoire » et méconnaissant, selon lui, « l'histoire, ses méthodes et postulats herméneutiques » [10]. Dumézil en 1984, ignoré dans son texte comme en bibliographie (!), et avant lui deux-trois siècles de nostradamologie censée s'égarer « dans le fantasme d'une épistémologie de l'énigme ou du rébus » [11]. Ce gommage autorise Crouzet à démarrer son étude à partir d'un corpus herméneutique quasi-vierge, et l'on verra la valeur de son expertise concernant l'histoire, voire l'histoire du texte nostradamien et de ses sources et ce, dès la page 13 dans cet avant-propos, où il reprend une citation latine de la lettre de Nostradamus à Henry II, « Quod de futuris non est determinata omnino veritas » (« En ce qui concerne le futur, il n'est pas de vérité entièrement déterminée »), en omettant le passage qui suppose au contraire que Nostradamus estimait être en mesure d'en éclairer certains avènements :

> supputant presque autant des aventures du temps advenir, comme des eages passez, comprenant de present & de ce que par le cours du temps par toutes regions l'on cognoistra advenir tout ainsi nommeement comme il est escript. [*Lettre à Henry* 12]

Crouzet n'indique ni la source du texte nostradamien, les *Prophéties*, ni la source médiévale de son inspiration qu'il n'a pas recherchée, croyant que cette sentence latine a été forgée par Nostradamus.

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Un point positif de l'ouvrage consiste à se démarquer de la récente mode rationalisante et à faire observer que l'interprétation passéiste des quatrains s'avère totalement abusive [cf. Guinard 2006–2011, 64], que l'exercice ne consiste qu'en « surinterprétation absolument arbitraire par recherches de référents historiques » [359]. Sont convoqués ici Brind'Amour [128], Prévost 1999 et Clébert 1981, mais ignorés Schlosser 1985, Clébert 2003 ou encore Lemuselier 2003.

Affirmant sa volonté d'« enlever l'astrophile à tous ceux qui l'ont enfermé dans leurs mirages prédictifs » [354] et l'urgence de « sortir de la production massive touchant à l'interprétation des quatrains » [358], Crouzet n'en a probablement effleuré que les pages de couverture, sinon il saurait, s'il l'avait sérieusement étudiée, que cette supposée production se résume à quelques rares ouvrages et interprètes originaux, recopiés et pillés par des hordes de faiseurs.[1] En outre, ces hordes ne sont pas plus peuplées que celles traversant la production académique rattachée à la littérature de salon et d'écoles : une production massive d'exégètes de second plan dont n'émergent que quelques rares philologues originaux, et peut-être pas ceux ayant appris la gestuelle rhétorico-herméneutique sur les bancs universitaires.

Ce sortir de l'interprétation s'accompagne d'un sortir « de la question de l'authenticité des éditions » et d'un troisième sortir « des pratiques et sources astrologiques de Nostradamus » [358]. Quelle recherche reste-t-il à accomplir, une fois évacuées la lecture sémantique des quatrains, l'exercice comparatif et textologique sur le corpus, et la recherche intertextuelle sur les sources, astrologiques, historiques et littéraires ? Presque rien, si ce n'est l'opinion alimentée par l'analytique académique moderne, essentiellement française et littéraire, transfigurant les *Prophéties* en un texte pré-piétiste.

Le « travail visionnaire » de Nostradamus [70], prévoyant selon Crouzet « la défaite des Habsbourgs, la défaite du Turc et la défaite du pape » [72], et l'avènement d'un grand monarque pacificateur d'origine gauloise, n'est qu'accidentel. Car le texte nostradamien ne dit rien, et il est inutile de chercher à l'élucider : il n'y a rien à comprendre, et les propositions énigmatiques des *Prophéties* et *Pronostications*, qu'elles soient authentiques, trafiquées ou apocryphes (peu lui importe), « n'ont pas vocation à être résolues ou identifiées »

[1] Cf. un comparatif des interprètes du passé dans mon *Nostradamus ou l'Éclat des Empires* [2011].

[22]. « Le sens serait donc de dire qu'il n'y a pas de sens » [43]. « Révéler, c'est révéler qu'il n'y a pas à savoir. Prophétiser, c'est dire qu'il n'y a rien à dire » [232], etc. Ainsi se résume la très brillante thèse du sorbonnard.

Nostradamus, psychologue des âmes et adepte du non-savoir, de la *via negativa* de Denys, de la docte ignorance du Cusain, de la nescience de Cornelius Agrippa, et de « l'ineffabilité du Logos » [163], se servirait de l'énigme comme d'un outil propédeutique voire thérapeutique, comme d'un moyen d'accès aux écritures bibliques [17, 36, etc.] : « ce qui est dit n'est pas ce qui est dit » [165]. « Le Dieu de Nostradamus serait bien le Dieu d'Érasme, le Dieu de la philosophie chrétienne, le Dieu en soi, à qui un culte intérieur doit être rendu » [77]. La référence apollinienne serait un trompe-l'oeil, la représentation de Dieu en Jupiter un jeu humaniste [91]. Le ludique affecterait la sémantique comme l'énonciation et la syntaxe : Crouzet reprend à Polizzi [2001 : cf. Guinard 2006–2011, 59] l'idée d'une écriture des quatrains par collage et déformation d'éléments narratifs [26]. S'il n'y a rien à dire, le travail herméneutique se réduit alors à paraphraser le texte oraculaire dans une présentation stylée et appuyée par quelques gourous de la rhétorique littéraire, où l'aléatoire et l'inexplicable tiennent les premiers rangs.

L'exégèse est parfois réduite à un simple appareillage analogique, souvent artificiel : « le chien est emblématique de la fidélité et donc de la foi » [280] ; « le rocher est synonyme de la certitude salvifique » [296] ; la date 3797 figurant dans la préface à César (qui désignerait la fin des Temps alors qu'elle crypte l'achèvement de l'oeuvre oraculaire) signifierait la Trinité (3), jointe aux Planètes (7) et au Ciel (9), et le redoublement du 7 soulignerait « qu'un cycle de temps s'achève et qu'un autre commence » [302]. Gageons que cette explication ne satisfera pas les quelques rares numérologues éclairés, si elle peut donner le change à ceux qui ignorent le b.a.-ba. de leur discipline. L'herméneutique glisse parfois encore sur les pentes de la faute, du repentir, de la pénitence et du désarroi chrétiens, en affirmant que pour Nostradamus la vie humaine ne serait qu'un « reflet du mal qui est en lui et qu'elle n'est que mal et donc malheur » [245]. Cette « philologie de l'angoisse » [257], cette lecture panique du texte nostradamien que Crouzet reprend de son *Les guerriers de Dieu* [1990 : cf. Guinard 2006–2011, 50], cet existentialisme pré-kierkegaardien me semblent aussi anachroniques qu'incongrus. Nostradamus était un bon vivant comme Rabelais, souvent farceur et facétieux, et opérant hors ce théâtre de jérémiades et pleurnichements dans lequel Crouzet cherche à le confiner.

Ces rêveries d'un Nostradamus résolument agnostique, s'appuyant au besoin sur des textes apocryphes et des faux (des almanachs Regnault, des écrits de l'imposteur Mi. Nostradamus le Jeune, etc.), justifie dans la foulée la docte ignorance de l'université à son égard (voire son bannissement et son exclusion des études autorisées) : Quelles raisons d'étudier un auteur qui ne dit rien et se contente de s'en tenir à un « fidéisme aconfessionnel » [74] qui n'intéresse que marginalement l'histoire culturelle et ses employés qualifiés ? Crouzet dévoile enfin le pot-aux-roses à ses lecteurs académiques destinés, quitte à pardonner à leurs aînés leur cécité littéraire envers l'un des plus purs stylistes de la Renaissance.

L'historien sorbonnard, qui n'explicite strictement aucun quatrain dans le détail, ne parvient finalement à saisir le texte nostradamien qu'à travers les visières calviniennes, en parcourant les catégorisations et grilles idéologiques mises en place par les patrons francophones de la théologie protestante. « Qui est le Dieu de Nostradamus ? » finit-il par se demander, « Ne faudrait-il pas alors glisser vers une interprétation plus radicale du positionnement nostradamien ? » lit-on enfin au tiers de l'ouvrage [141]. Ce serait effectivement l'interrogation essentielle, compte tenu de l'orientation du traité. Crouzet ira-t-il jusqu'à poser l'indispensable question spinozienne, celle de l'enracinement néoplatonicien, paracelsien, voire pré-spinoziste de l'astro-philosophie du Saint-Rémois [cf. Guinard 2006–2011, 84] ? Ou pour le dire dans le jargon idéologique de Calvin : Nostradamus doit-il être rattaché à ces « Libertins qui se nomment spirituelz » ? « Je ne chercherai pas à y répondre », conclut Crouzet, « et n'irai pas plus loin dans cette perspective. » Le lecteur déçu pourra refermer l'ouvrage. Car ce courant stigmatisé par Calvin, Bèze et autres idéologues évangélistes francophones, n'a jamais été reçu, cultivé, ou mis en valeur dans l'espace culturel francophone, et il faudrait élargir le champ d'investigation outre-Rhin, où se comptent la plupart des correspondants de Nostradamus, pour comprendre la modernité du provençal, si maltraité depuis des siècles (et qui continue de l'être) par les intellectuels mis en poste et rétribués selon leur degré d'acculturation idéologique.

En 1990, Crouzet faisait de Nostradamus le héraut d'une littérature alarmiste et apocalyptique en accointance avec l'apologétique papiste d'un Artus Désiré ; vingt ans après et son passage au CURA [http://cura.free.fr/mndamus.html], il le déguise à l'inverse en un contemplatif pré-piétiste aux intentions parallèles à celles de l'engagement évangélique. Il est décidément difficile de lire Nostradamus hors les ornières des schémas et apprentissages scolaires.

La thèse de Crouzet, incompatible avec les affirmations précises des préfaces aux *Prophéties* (« par plusieurs foys j'aye predict long temps au-paravant ce que depuis est advenu », « esperant toy declarer une chascune prophetie des quatrains ici mis », etc.), se heurte encore à un obstacle d'envergure : aucun des contemporains et adversaires de Nostradamus, aucun de ses nombreux lecteurs (qui ont maintenu son oeuvre vivante et populaire alors que l'université l'a arrogamment ignoré depuis plus de quatre siècles), aucun même parmi ses critiques les plus acharnés, n'aurait accepté la vision d'un Nostradamus qui ne serait qu'un simple croyant pré-piétiste, amusant ses lecteurs par des jeux de mots sans conséquence ou par des découpages aléatoires de syntagmes choisis, les abreuvant de Rome pour soi-disant les inciter à lire la Bible. Tous savaient que le discours nostradamien est destiné, empli d'un fatum nécessitant, et qu'il vise un futur inquiétant.

Pour Nostradamus, l'humanité, qui vole inéluctablement vers le pire, est menée par des tyrans malignes, des monarques ignorants et violents, et aujourd'hui par les impériaux, marionettistes et pantins au service de la finance aveugle et quasi-inculte. La surdité des dirigeants épouse et pilote la veule imbécillité des populations, voire la lâcheté des clercs. Mais derrière ce decorum sinistre et trop évident, le texte nostradamien recèle un message de renouveau auquel l'exégèse superficielle n'a pas accès.

Addenda et corrigenda

- Crouzet n'ignore pas certaines de mes études parues au Corpus Nostradamus. Je suis cité à la page 56 [en note p. 364] pour le quatrain 1.35 relatif au décès d'Henri II, et le texte de la page 56 reprend mes extraits de l'*Almanach* pour l'an 1557 mais sans entrer dans le détail de mes analyses [cf. Guinard 2006–2011, 51] ; à la page 199 [en note p. 386] pour les *Pronostications* pour les années 1550 et 1552 [Guinard 2006–2011, 2 et 4] ;
- aux pages 369–370 [notes 4, 8 et 9] pour mon analyse de l'*Épître à César* [Guinard 2006–2011, 33], dont quelques lignes de la page 167 de ma thèse de 1993 (le sait-il ?), reprises au CURA [http://cura.free.fr/mndamus.html] en 2002 dans « Le temps des philosophes. De Pla-

ton à Nietzsche, et de Nietzsche à Platon » puis citées au Guinard 2006–2011, 33 :

> C'est l'âme qui vit le temporel. L'éternité caractérise la « substance indivisible », permanente, incorporelle ; la temporalité cyclique la « substance divisible », changeante, matérielle. Le temps est ce par quoi l'éternité se manifeste. Il est son media, une illusion de l'âme, une « image mobile de l'éternité ». Les cycles planétaires et la sphère des « fixes » servent de repérage temporel, car « le temps est né dans le ciel ». Le temps est le milieu de manifestation de l'âme, et « le ciel » la mesure de ses transformations et de ses états. Temps, âme et mouvement coexistent. Le temps est une représensentation psycho-mentale de l'inscription des cycles planétaires dans la psychè, diront les astrologues post-platoniciens.

- ou encore aux pages 129–131 et 377 pour ma présentation du manuscrit de l'Orus et, tout en mentionnant l'édition « P. Roullet » (*sic*, pour Rollet), pour ma transcription qui la rectifie, de l'épigramme « Que voulent ilz signifier par l'estoylle » [Guinard 2006–2011, 28]. Crouzet, différenciant assez mal les éléments qu'il a empruntés de ceux à partir desquels il spécule, sans toujours citer ses sources ou les citant mal à propos et avec des décalages, en arrive à de curieuses inconséquences comme pour la date du manuscrit de l'Orus : « vers 1545–1547 » [129] et « dès 1541 » [132 et 140], une date hypothétique que j'ai avancée contre l'opinion commune en février 2005, et que Crouzet reprend à son compte sans se soucier de la contradiction. Comment peut-il répéter que Nostradamus a élaboré son manuscrit dès 1541 (contre l'opinion de Rollet, Aulotte, Brunon, Benazra, Brind'Amour, etc.) si ce n'est au moins d'après le titre de mon article, mais sans jamais entrer ni dans le détail ni même au coeur de mes propos ?
- L'historien reprend mon idée, toujours sans indication de source, que Scaliger « serait visé » dans les *Présages* pour 1557 : « l'un que je congnois ne parlera jamais, je suis desplaisant de l'inconvenient qui luy adviendra avant le bout de l'année » [387 : cf. Guinard 2006–2011, 76], et quelques termes de ma traduction de la lettre de Nostradamus à Claude de Savoie [405 : cf. Guinard 2006–2011, 17].

Crouzet n'aura pas trouvé dans son entourage universitaire de correcteur susceptible de lui indiquer les erreurs figurant dans son texte, dont certaines, de vingt ans, sont reprises du chapitre 2 de son *Les guerriers de Dieu* [1990].[2] Signalons :

- la mention de « Chevillard » (boucher ?) pour Chevignard [147, 150, 363, 379, 388, etc.], voire de « Bertrand Chevillard » pour Bernard Chevignard [362 et 437].
- l'édition Pierre Roux des *Prophéties*, supposée de 1555 [21 : cf. Guinard 2006–2011, 25].
- la connaissance, pas même rudimentaire, de l'astrologie et de son histoire (non enseignées à la Sorbonne) : « dans l'Aquarius », « au Poisson », etc. Crouzet suit aveuglément en ce domaine Brind'Amour, même quand ce dernier s'égare, comme en VIII–91 [30].
- l'authenticité supposée de l'épître à Jean de Vauzelles [53].
- la confusion concernant l'expression « grand de Bloys » attestée dans toutes les éditions anciennes, et non « grain de bloys », conduisant Crouzet à des remarques déplacées : à éviter de « se laisser prendre au piège des mots » [56], il reste la dupe d'une fausse historicité du texte nostradamien, en prenant le texte authentique pour le faux et inversement.
- la distinction entre Archidamus et Crespin qui désignent le même personnage [62].
- la croyance que Dupèbe a retrouvé en 1983 les lettres de la correspondance de Nostradamus [68], partiellement traduites par Lhez dès 1961 !
- la mention fautive du titre de *La Grand pronostication nouvelle pour l'An Mil cinq cens soixante* [366] sans aucune indication de l'origine du texte que Crouzet a connu soit par le fac-similé de Mario Gregorio, soit plus probablement au Guinard 2006–2011, 95,[3] qu'il commente en ces termes : « attaque directe…contre celui qui, à Genève, a médit

[2] Cf. Guinard 2006–2011, 59 : « Misère de la recherche académique et universitaire sur Nostradamus ».

[3]

> plusieurs de ma profession qui ne remplissent leurs papiers que de mesdire contre Nostradamus & de je ne scay quoy de resveries, comme celuy qui a esté faict a Geneve, qui ne parle que de malediction & sans y avoir inseré ne cathalogue de sainctz.

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de lui et n'a pas même inséré une liste des saints dans son almanach » [69].

- l'arrangement du texte de la préface à César à son goût, amalgamant l'astrologie à la magie réprouvée par Nostradamus [104].
- la supposition qu'une première édition du *Traité des Fardements et des Confitures* daterait de « 1554 » [146].
- la version caduque du quatrain pour l'an 1555 [185 : cf. Guinard 2006–2011, 15].
- la mention de textes fantaisistes comme la *Prognostication* ou *Revolution* pour 1565 et la *Prophetie* merveilleuse jusques en l'an 1568, qui sont des écrits de l'imposteur Mi. Nostradamus le jeune. « Une certaine évolution dans l'imaginaire de l'astrologue s'y fait sentir » [251] : et pour cause !
- l'ignorance d'Hutten au quatrain 1.84 supposé avoir été influencé par les Actes des Apôtres [261 : cf. Guinard 2006–2011, 47].
- la référence au texte des *Prophéties* dans l'édition « Pierre Rigaud, 1566 » [366, 446] : une édition avignonnaise apocryphe du début du XVIIIe siècle.
- la conversion supposée de Vidon Gassonet « vers 1454–1455 » [418] : en réalité avant 1430.

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1001 Inventions: The Enduring Legacy of Muslim Civilization edited by Salim T. S. al-Hassani

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1001 Inventions: The Enduring Legacy of Muslim Civilization is the companion book to the exhibition '1001 Inventions: Discover the Golden Age of Muslim Civilization' that ran from 3 August 2012 to 3 February 2013 under the auspices of the National Geographic Society, Washington DC, in cooperation with the Foundation for Science, Technology and Civilisation (FTSC), Manchester. The difference in title between exhibition and book is significant and substantial. The exhibition claimed to focus on the long gone past. The book insists explicitly on the relevance of this past to today, in particular, to today's sciences and technologies.

This third edition of *1001 Inventions* has undergone a visible effort of moderation, alteration, and adaptation. I cannot judge, of course, whether this is also the case for the spirit of the exhibition, which its director, Prof. em. Salim al-Hassani, claimed is identical with the previous ones. The changes in the book concern aspects of political correctness, religious expressions, and various errors, great and small, of content and language. 'Mankind', for instance, has been changed to 'humankind'. The constant repetition of 'pubh' ('peace be upon him') has been almost completely abolished. Claims such as 'the universal nature of Islam' were deleted. al-Kindi no longer appears as the head of a team appointed by Caliph al-Maʾmun for translating Aristotle's works. The often faulty English of the second edition has been improved somewhat, though there still remain phrases and sentence that are unidiomatic in English. The Arabic has not been not corrected and continues to suffer from numerous mistakes and different styles of transliteration, some of them apparently of South Asian origin, others derived from academic usage. So many gross errors have slipped through the nets of the two editorial teams of the National Geographic Society and the FTSC that I wonder

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whether this was due to careless browsing, lack of historical knowledge, or incompetence in mathematics, the sciences, or the arts. Several central messages of the earlier companion books have been omitted, as I will outline below. In their place, new messages of *cross-cultural respect* and the search for 'the cultural roots of science' have been introduced. al-Hassani, the Chief Editor of the book, now delivers speeches laden with these two catchphrases, the historiographical meaning of one of which, namely, 'cultural roots of science', he does not understand at all as the book leaves no doubt.

All in all, the fundamental distortion of history embedded in the thesis that today's sciences and technologies depend on inventions and discoveries made by medieval Muslim scholars, characteristic of the second edition, continues to be the guideline of the third edition too. This misguided presentation of the many impressive achievements of scholars from past Islamicate societies precipitates many exaggerations according to which Muslims laid the foundations for almost every science of today, invented almost every important technological device or gadget in use today, or revolutionized everything they learned from writings of scholars who lived before the seventh century or outside the realm of what is dubbed in the book 'the Muslim world'. In short, the companion book with its texts, images, and blurbs suffers from a severe case of 'Muslim precursoritis', to pick up an ironic term used by Abdelhamid Sabra many years ago in his much appreciated criticism of our own research practices as historians of science in Islamicate societies. As the academic field itself, regrettably, the book suffers in addition from Phil-Arabism, to the detriment of other peoples and communities that contributed importantly to the sciences and technologies of past Islamicate societies. Again, as the academic field itself, regrettably, the book exhibits widespread disinterest in the precise historical contexts of the various scholarly activities and their results, and sadly reflects the shortcomings of academic research on past sciences and technologies in Islamicate societies. The serious errors by the book's compilers and editors are, however, not caused by any of my colleagues, despite Salim al-Hassani's repeated protestations. In contrast, it is both annoying and sad that *1001 Inventions* misses the chance to popularize the many profoundly new dicoveries by historians of science concerning the ideas and practices of scholars in medieval societies of Europe, Asia, and Africa; and that instead it misrepresents the past, deriving false pride and pleasure, rather than learning and teaching how to respect, appreciate, and admire past scholars in their own contexts.

Since *1001 Inventions* contains on almost every page substantial errors, misrepresentations, or, sadly, sheer inventions of its own, I have compiled a list of some of its major falsehoods, myths, and delusions without trying to achieve completeness and append it at the end of my discussion of the book's messages, rhetoric, imagery, and other features. It is most regrettable that the National Geographic Society lent its authority to promoting this kind of ideological misrepresentation of an important part of the history of science. No amount of political correctness and nicety about cultural cohesion and mutual understanding can counterbalance the ideologically based and amateurishly executed falsifications of history and science that dominate this book from page 1 to page 351.

The Politics of *1001 Inventions* and its Changing Messages

The exhibition '1001 Inventions' began modestly in London (Croyden), and has since then, in a larger and more ambitious format, toured several countries and cities (London, Science Museum; Istanbul, Sultan Ahmet Square; New York, New York Hall of Science; Los Angeles, California Science Center; Abu Dhabi, Abu Dhabi Science Festival; Saudi Arabia, Dhahran, Saudi Aramco Cultural Program). It was endorsed or inaugurated by such high ranking politicians as the Turkish Prime Minister and the US Secretary of State. Prince Charles, the Queen, the British Parliament, and Middle Eastern royals have hosted and supported the exhibition and its organizers' messages in oral as well as written form. Most recently, it was opened 30 August 2013 at Karlstad by Åsa Hallén, Director of Värmlands Museum, Lena Adelsohn Lijleroth, Sweden's Minister of Culture, and HRH Prince Carl Philip of Sweden, the Duke of Värmland. Visitor numbers in London, Istanbul, New York, and Los Angeles ranged between 400,000 and over 2 million. This is big money, to say the least.

The third edition of the companion book shows a major improvement in design, imagery, and quality of paper. The second edition had too much text and image per page and worked with too many newly made drawings and paintings in Orientalist style with false cultural elements. The third edition continues to work with fictitious pictures called artist's impressions. But they are less numerous. Those newly introduced are less glaringly Orientalist or Multiculturalist. Those retained from the second edition continue to represent their themes in a style heavily reminiscent of the Orientalist painters of the 19th century. The majority of the images are reproductions from manuscripts, early modern printed books, photographs of today's objects, and diagrams. While most of the false cultural visual elements were taken out, a few remain: for instance, on page 89, quills are portrayed as writing utensils instead of the reeds used in Islamicate societies and women are shown as scholars working side by side with men in an observatory.

Two major problems continue to be inscribed in the visual presentation of the companion book. These two problems are found in the texts too. One consists in the modernization of medieval ideas, methods, instruments, work places, and practices. A glaring example is the diagram of the blood circulation [167]: it suggests that Ibn al-Nafis described the small blood circulation through the heart and the lungs. This is, however, false as Emilie Savage-Smith and other historians of medicine have argued. The text, moreover, places Ibn al-Nafis' work in a tradition of anatomical research, while his speculative ideas evolved in a religious context of reflections on the soul, as Nahyan Fancy has explained in his doctoral thesis [2013].

The second problem with images and texts consists in their suggesting false claims. An apparently Oriental miniature [277] shows three men dressed in Muslim garb holding big telescopes before their eyes to observe the sky. The caption seems to clarify that the miniature is fictitious by describing it as 'Ottoman-style'. But since the otherwise usual label that the image had been drawn by an artist is missing, the uninformed reader could easily conclude that the image is a genuine product of some long dead Muslim miniature painter. Worse, s/he might be tempted to believe that Muslim men indeed studied the sky with telescopes in the 17th century. Such a claim is not backed by evidence in Arabic, Persian, or Ottoman sources, although telescopes were sold by merchants and carried along by travelers from various European cities in the Ottoman Empire as well as the Indian Subcontinent. We only know for certain that telescopes were used for observations of the sky by Muslim and Hindu scholars and princes in the early 18th century.

Modernization, false claims, the omission of almost all context, the neglect of chronology, and the treatment of the many different Islamicate societies as if they had been one single whole unchanged over time except for the gloriously continuous scientific and technological progress are the major setbacks of the companion book. They are not merely accidents of sloppy work or expressions of a serious lack of competence in the topics presented in the book. They are that too. But first and foremost, they are results of the overall message that the originators of the exhibition and the companion book wish to disseminate. It is summarized on the book cover:

> IMAGINE IT IS THE SEVENTH CENTURY. As most of Europe continues its descent into a long period of intellectual dormancy, a quiet yet powerful academic revolution is erupting in another corner of the world. Over the next centuries, the geniuses of Muslim society will thrust the boundaries of knowledge forward to such a degree that their innovations still shape civilizations to this day. The staggering achievements of these men and women influenced the development of modern mathematics, science, engineering, and medicine. *1001 Inventions: The Enduring Legacy of Muslim Civilization* sheds new light on this golden era that was once lost to so many, and celebrates the heritage we all share.

I have often been asked by friends and strangers alike whether it is true that Muslim scholars contributed significant achievements to various intellectual domains, particularly, in mathematics and the sciences. When I confirm that this was indeed the case, the follow-up question often is: 'What has remained from these contributions in our current scientific and mathematical practices and theories?' Here, however, I have felt compelled to answer 'Very little'. Almost everything that pre-modern Muslim and non-Muslim scholars in Islamicate societies had studied, reflected upon, and written down or transformed into instruments, maps, or globes belongs to achievements in human intellectual development that have been replaced, invalidated, or made irrelevant to today's concerns. I am hard pressed to find a single item, be it theoretical or practical, in today's mathematics and sciences that could rightfully be claimed as a pre-modern contribution from an Islamicate society. I am of course referring here to those parts taught at universities and discussed by researchers. My answer would be slightly richer were I to restrict the question to matters taught at primary and secondary school levels. Although here, too, nothing is identical with ideas, methods, theories, or modes of notation as found in Arabic, Persian, or Ottoman-Turkish manuscripts, on instruments or in maps, the parentage of those older layers of knowing and doing can still be recognized if we carefully investigate those matters.

Such a careful investigation and appropriate appreciation of the intellectual concerns of mostly men and very rarely women in pre-modern Islamicate societies was not, however, the goal of the presenters of '1001 Inventions'. Its story is not one of manifold efforts, failures, obsolete theories, creative appropriations, new insights, original results, and nasty conflicts in very different societies, communities, territories, and periods. It is a single, unified tale of geniuses, as the back cover of the companion book states. It is a story of heroism and glory. The result of this aggrandizement permeates all levels of the exhibition and its companion book. It is exemplified by gross simplifications, almost complete decontextualization, staggering exaggerations, and bizarre fictions. As a colleague said to me when talking about the second edition: '*1001 Inventions* is the modern form of *1001 Nights*.' It is a fairy tale about the glamorous, all-knowing, perpetually revolutionizing orient. It is Orientalism pure, not by Orientalists but by Muslim amateurs, by believers in a lost splendor of what they consider their past. It is a Muslim-centric, anachronistic tale about a past invented for improving the reputation of the religious creeds that they adhere to and the cultures that they grew up in or are connected with through many family ties and other links. It is a call for overcoming a widespread feeling of inferiority by stressing an imaginary superiority of a long gone past that the narrators claim continues to live in today's sciences and technologies.

This web of ideological commitments and goals was explicitly described in the second edition [al-Hassani 2007]. In the third edition, produced for and with the National Geographic Society, most of these messages are no longer explicitly made. They were replaced in Roland Jackson's foreword by a new central message and in al-Hassani's introduction by modified versions of previous statements and newly formulated goals. al-Hassani now writes, for instance:

> A number of colleagues well established in the subject, began a lecturing campaign in Britain, Europe, and abroad. A large number of people from all walks of life derived pleasure and inspiration from this knowledge. Presentations to the younger generation, especially the ones I gave to the Youth NGOs at the European Parliament in Brussels, sparked enormous interest in science and technology, and especially in the lives of Muslim pioneers in chemistry, physics, medicine, biology, algebra, engineering, architecture, art, agriculture, and its numerous manufacturing industries who have impacted so positively on our modern civilization. It was clear this under-appreciated subject was finally coming of age....The first two editions of the *1001 Inventions* books sold more than 100,000 copies. However, this was just the start of what would be a much greater flowering of international interest in our work, alongside increased dialogue about the cultural roots of science and new opportunities to promote social cohesion and intercultural respect and appreciation....The book identifies in an

> enjoyable, easy-to-read format aspects of our modern lives that are linked with inventions from Muslim civilization. It is our hope that through those pages we can enhance intercultural respect while at the same time inspire young people from both Muslim and non-Muslim backgrounds to find career role models in science, technology, engineering, and mathematics. [13]

In the second edition he had written:

> A number of colleagues well established in the subject, began a lecturing campaign in Britain, Europe and abroad. A large number of people from all walks of life derived pleasure and inspiration from this knowledge. Presentations to the younger generation, especially the ones I gave to the Youth NGOs at the European Parliament in Brussels, sparked enormous interest in science and technology, and especially in the lives of Muslim pioneers in chemistry, physics, medicine, biology, algebra, engineering, architecture, art, agriculture, and its numerous manufacturing industries who have impacted so positively on our modern civilization. Young Muslims, however, find in such knowledge a new identity, allowing them to be European whilst at the same time Muslims. They find exciting role models, male and female, for innovation and invention, and begin to recognize that these pioneers, unlike many today, had expressed their religious commitment and faith through deeds useful to society, be it Muslim or non-Muslim, and that ineptness, looking inwards and reliance on governments was not their tradition.…The book identifies, in an enjoyable, easy-to-read format, aspects of our modern lives that are linked with inventions by Muslims or were inspired by Islam.…Amongst the main objectives we hope to fulfill are:…Inspire young people from both Muslim and non-Muslim backgrounds to find career role models in science and engineering. [7]

Jackson, the Chief Executive of the British Science Association, also emphasized two of the new points found in al-Hassani's rewritten introduction. He praised *1001 Inventions* as

> a tribute to the efforts of the Foundation for Science, Technology and Civilisation in promoting the cultural roots of science as a means of encouraging intercultural respect and appreciation and in helping us understand the past to build a better future together. [6]

As I have stated already, these modified messages do not permeate the body of the exhibition and its companion book. The concepts of cultural roots and intercultural respect and appreciation function merely as rhetorical devices of a special kind of political language. They can be appreciated as a rejection of the abusive political language against Muslims in much of Europe, the Americas, Australia, and parts of Asia. But here they did not guide the presentation and, hence, the understanding of past intellectual and technical activities in Islamicate societies and their possible connections with our lives around the globe today. They seem instead to be an ill-conceived attempt to understand the cultural roots of science that is hobbled by limiting the project's content to 'Muslim inventions'.

As the texts of exhibition and companion book amply demonstrate, the editors of *1001 Inventions* believe in a cumulative, universal science and technology that they claim was directed for 1,000 years by Muslim scholars, men and women. They believe that the existence of one piece of knowledge or one technical device at two different places and two different moments of time proves the two are connected and that one is the direct heir of the other. They do not try to trace the cultural conditions and forms of such knowledge or artisanal production in specific past contexts in societies with either Muslim minorities or majorities. Nor do they undertake any serious effort to demonstrate the historical connections between such forms of past knowledge and objects with those of today. The editors' essentialist rhetoric of one single 'Muslim civilization' and 'Europe' demonstrates a failure to understand the particularities of any of the societies within the two big cultural blocks that they posit so uncritically as well as their lack of any awareness that these concepts belong for many academic historians nowadays to a phase of conceptualizing the 'Old World' and its various parts that is long past and best forgotten. The result of their old-fashioned and outdated approach to history is a use (or abuse) of the past in proposing that it shaped a general, unified, high-tech present and for demanding tribute to 'Muslim geniuses' as ground-breaking creators of our own times.

Thus, the project's ideological orientation has four main outcomes, none of which is commendable:

(1) the omission of all conflicts and rejections of scientific, philosophical, and medical doctrines and practices that were part and parcel of the intellectual struggles in past Islamicate societies;
(2) a silence about the many 1000s, if not 100,000s of elementary texts that are contained in numerous libraries across the globe and speak of the often very limited mathematical and astronomical knowledge taught in many cities and towns of Islamicate societies;
(3) the suppression of all intellectual fields that are no longer considered sciences; and

(4) an unwillingness to engage with hotly debated historiographical problems, thus oversimplifying past and present grossly.

Any history that starts with philosophy, medicine, mathematics, or technology in ancient Greece and continues with the Renaissance is rightly called Eurocentric and would be rejected by many of my colleagues. Likewise, a narrative that admits pre-Islamic antecedents of philosophy, medicine, and the mathematical sciences in Islamicate societies as well as non-Muslim contributors to the intellectual life in those societies but develops its main narrative thread by effectively denying these antecedents and largely ignoring those contributors is equally unacceptable. Many Islamicate societies, however, had no notable groups of scholars or craftsmen who could have made it into the pages of *1001 Inventions*. Others had notable scholars but in fields which do not interest the editors of *1001 Inventions* at all—the religious sciences, history, rhetoric, philosophy, philology, astrology, alchemy, letter magic, and related areas. Terms like 'chemistry', 'physics', 'biology', 'engineering', 'manufacturing industries', 'robot' describe historical states reached first in some non-Islamicate societies in Europe between the late 18th and 20th centuries. *1001 Inventions*' historiographical one-sightedness also extends to almost all neighbors and contemporaries of Islamicate societies with one exception: 'Europe'. Its editors have little to no interest in processes of exchange between different societies in Asia or Africa.

The ideological goal of the exhibition also finds its expression in the themes chosen for representation. Except for the newly added 'Map of Major Contributions in Muslim Civilization' and 'Chapter One: The Story Begins', companion book and exhibition cover seven domains: Home, School, Market, Hospital, Town, World, Universe. Disregarding the slightly unfortunate choice of 'World' and 'Universe' for the last two domains, this structuring of the material is appealing in its simplicity and apparent clarity. The subsections, however, indicate the problems with this structure. To include, for instance, 'the agricultural revolution', farming manuals, water management, dams, or windmills under the header of 'Market' comes as a surprise and cannot be defended on grounds of content or historical conditions, to say nothing here of the deeper issue of calling agricultural changes and innovations in the medieval period a 'revolution'. More than one subsection has next to nothing in common with either 'science' or 'technology', whether understood as medieval or current phenomena, since they do not discuss issues like nutrition, weaving or spinning technologies, procedures of dying, color and ink production, or methods for differentiating between true gemstones and fakes. Without aiming for completeness, I mention the following examples:

- chapter 2 (Home): 'The Coffee Trail', 'Fine Dining', 'The Three-Course Meal', 'Carpets', or 'Fashion and Style';
- chapter 3 (School): 'Chess', 'Art and Arabesque', 'The Scribe', or 'Word Power';
- chapter 4 (Market): 'Jewels'and 'Currency'; and
- chapter 5 (Town): 'Public Baths' or 'From Kiosk to Conservatory'.

Hence, contrary to the opening claim of chapter 1.1 'The Golden Age'—'This volume looks at the scientific legacy of Muslim civilization...' [18]—*1001 Inventions* does not focus on scientific discoveries and technological inventions alone but presents many cultural items produced in other contexts in various Islamicate societies and appropriated in non-Islamicate societies in Europe through trade, war, conquest, diplomacy, travel, and transfer of ideas, to use major terms of the companion book. This imbalance between title, messages, and content is only one of the many signs of questionable work by editors whose efforts are otherwise undeniable.

Verification, Witnessing, and Rhetorical Devices

1001 Inventions and its parent organization, the FSTC, claim time and again to rely exclusively on the best scholarship available. The list of errors bespeaks the deep-seated problems that the makers of the companion book and exhibition have with serious scholarship. Another indicator of the enormous distance between the tales of *1001 Inventions* and academic scholarship is the primary reliance on journalists, TV series, and educators; moreover, on those rare occasions when they do turn to a historian, it is to historians of science in the 19th and early 20th centuries. The lack of any precise referencing and the substitution of more cautious statements from academic sources to the effect that someone may perhaps have done or written something with statements of bald fact exacerbate this sad situation. Except for one historian of engineering, the late Donald Hill, not a single one of my numerous colleagues who have changed our knowledge of the scholarly works undertaken in classical and some post-classical Islamicate societies has been given voice. The subsection on mathematics of chapter

3 (School) is recognizably based on the work of the Egyptian historian of mathematics Roshdi Rashed but without crediting him [84–86].

I have repeatedly tried to verify the companion book's most dubious factual claims but was often unable to find out whence the authors of the texts appropriated them or to find any source coming close to what was claimed. The quotations in the blurbs on the book's cover as well as in its main body are often, but fortunately not always, even 'more false' than *1001 Inventions* itself or simply serve the purpose of supporting its various exaggerations and errors.

Three sets of examples have to suffice to illustrate this feature of the blurbs:

(1) Brian Whitaker, a journalist of the *Guardian*, is quoted for his account of the House of Wisdom:

> The House of Wisdom was an unrivaled centre for the study of humanities and for sciences, including mathematics, astronomy, medicine, chemistry, zoology and geography...Drawing on Persian, Indian and Greek texts—Aristotle, Plato, Hippocrates, Euclid, Pythagoras and others—the scholars accumulated the greatest collection of knowledge in the world, and built on it through their own discoveries. [73]

As I will elaborate below, this institution was a library, not a research institution. The few translations explicitly linked to it were made primarily from Middle Persian texts, mostly on astrology; there were no such things as humanities, chemistry, or Indian texts as a parallel to Persian and Greek texts, but Sanskrit texts. The names given are actually authors, not texts, and all of them are Greek. There was no genuine text by Pythagoras known in Antiquity and, hence, none was translated into Arabic. It is highly doubtful that the material, mostly letters and documents, stored in the caliphal library can be called 'the greatest collection of knowledge in the world'. But even if we take this superlative to concern the texts composed by Christian, Jewish, Zoroastrian, Sabian, or Muslim scholars during the eighth and ninth centuries, it still remains questionable whether the knowledge taught in Indian or Chinese cities of the period can be measured sensibly and, hence, described as less than the one praised in the quotation.

(2) Rageh Omaar, then a BBC journalist, today with al-Jazira English, and *1001 Inventions*' most favored witness, has pronounced numerous absurdities in his TV documentary 'An Islamic History of Europe'

for BBC Four (August 2005), which are repeated in the book, among them the following three:

(a)

> Teams of Muslims, Jews, and Christians translated texts into Arabic, then into Castellan Spanish and Latin. It required close cooperation and religious tolerance. The Andalusian word for this is 'convicencia' and means 'living together.' [82]

I am not aware that a single text was translated in Toledo into Arabic by a team of Muslims, Jews, and Christians. In the ninth and tenth centuries, some Latin texts were translated by Christians or newly converted local Muslims into Arabic but it is not clear that this happened in Toledo and it was not a part of the events of the 12th and 13th centuries to which the quotation refers. 'Castellan' designates the administrative head, so to speak, of a castle not Spanish, while Castilian is the language meant here: if 'castellan' is meant as the Castilian word for the language it should be 'castellano/a' depending on the genus of the noun with which it is linked. In any case, Castilian Spanish is an improper doubling of two names for the same thing when looked at from outside Spain. 'Convivencia' is not an Andalusian but a Castilian word and it was introduced into the historical debate in the first half of the 20th century. We know of only a few cases of cooperation in translating Arabic texts into Romance and then Latin between Jews and Christians in Toledo, even less of the cooperation between a foreign and a local Christian, and, as far as I am aware, nothing of any cooperation between a Muslim and a foreign Christian. We know of other cases of such cooperation outside of Toledo, for instance, in Barcelona or in the Ebro valley; but this kind of reliance by foreign as well as local Christians on the skill and knowledge of Jewish scholars and Muslim speakers of Arabic is not called 'convivencia', a term used for the relationship between members of the three religions under Muslim rule. Even if the quotation is meant to refer to the translations made at the court of Alfonso X in the 13th century, no Muslim participated in them and the number of translated texts, while greater than in many other cases, remained nonetheless fairly limited.

(b)

> He [Averroes] would launch Paris as the intellectual capital of Europe...Averroes was trying to defuse a conflict between science and

> religion because the truth revealed by science was often at odds with the truth of divine revelation. This attempt had the opposite effect when his ideas came to the attention of the Christian church. They immediately banned Averroes [*sic*] and Aristotle's works. The Paris intellectuals fought back and a debate raged for years. [83]

Ibn Rushd never came to Paris nor could he launch the city as the intellectual capital of Europe, which in itself is a gross exaggeration. The description of Ibn Rushd's discussion of the relationship between law and philosophy is old-fashioned and contradicts what the philosopher wrote in his work *Kitab fasl al-maqal*.[1] The Christian church is not 'they' and it did not ban Averroes' and Aristotle's works immediately. The first time that works by Aristotle were condemned was in 1210: the condemnation was pronounced by the Synod of Sens and referred exclusively to the Faculty of Arts at the Sorbonne. Philosophical positions maintained by Ibn Rushd (doctrine of the soul, monopsychism) and Aristotle (God as Unmoved Mover) were banned 60 years later by the bishop of Paris Étienne Tempier, who headed a group of theologians appointed by him for this purpose.

(c)

> The staggering array of geometric patterns shows the way the Muslim craftsmen explored the concept of infinity through mathematical repetition. [101]

Muslim craftsmen in Fez, for instance, have wooden models which they simply copy, one after the other; no exploration of anything is involved. Moreover, the journalist has no grasp of infinity, since it cannot be explored by anyone through repetition but by abstract thought alone. Amani Zain, presenter in 2005 of the BBC Documentary 'What the Ancients Did for Us', claimed: 'Arabs invented the technique that makes these clay pots into art' [142]. The subject of this false and at the same time ridiculous claim is luster glaze, the first appearance of which seems to have been on glass, not pottery. There are different theories of who invented the technique—Romans, Copts long before the Arabic conquest of Byzantine Egypt, or ethnically unspecified craftsmen,

[1] There is an English translation at http://www.fordham.edu/halsall/source/1190averroes.asp.

maybe Copts, maybe, but less likely, Arabs, or even alchemists in early Islamic Fustat (today part of Cairo) as claimed in the very same BBC documentary. Pottery, however, was produced as an art form millennia before the advent of Islam.

(3) (a) Ruth S. Mackensen, an early 20th-century historian, writes:

> Books were presented and many a scholar bequeathed his library to the mosque of his city to ensure its preservation and to render the books accessible to the learned who frequented it. And so grew up the great universities of Córdoba and Toledo to which flocked Christians as well as Moslems from all over the world. [70]

General statements like the first sentence of this quotation are difficult to prove or disprove: there are many mosques today with none or very small holdings of books compared to a few famous ones with collections of 100s and in some cases 1000s of manuscripts. When the habit of donating a private collection of manuscripts to a mosque or a *madrasa* emerged is not clear, but it differed in all likelihood from region to region. My vague impression is that it became more customary with the foundation of *madrasas*, i.e., *circa* the 12th and 13th centuries, but I may be wrong. The second sentence of the quotation, however, is utter nonsense: there were not only no universities in Cordoba and Toledo under Muslim rule; there was also no flocking of people, whether Muslim or Christian, from all over the world either under Muslim or under Christian rule. This is simply a careless exaggeration of what is known about Muslims from different regions (al-Andalus, the Maghrib, Egypt, Iraq, and occasionally even Iran) who visited scholars in cities of different Islamicate societies in order to study with them a set of specific texts.

(b) S. P. Scott, a 19th-century historian, writes:

> The Spanish Muslims' agricultural system was the most complex, the most scientific, the most perfect, ever devised by the ingenuity of man. [113]

Even for the later 19th century, this statement is wrong–too much praise and too little rational analysis. Today it is an even less appropriate evaluation in its timelessness, lack of context, and lack of reliable comparison with other forms of agriculture.

One of the most tiresome features of the companion book is the constant use of superlatives coupled with the emptiness in content of many of the sentences. The formulas used continuously serve to suggest academic reliability and familiarity with the latest research results. But their references remain either unspecific or point to manuscripts with vague dates, no authors, no titles, and no locations of preservation. No reader could ever check such claims nor learn more about the sources used. The words and phrases most loved by the Chief Editor al-Hassani and his team are: 'huge', 'vast', 'amazing', 'ground-breaking', 'buzzing', 'bursting', 'powerhouse', 'greatest', 'largest', 'oldest', 'richest', 'revolutionize', 'revolutionary', 'the first', 'the founder', 'laying the fundaments', 'incredible', 'massive', 'hundreds', 'thousands', 'golden age', 'rational thought', 'experimentation', 'direct observation', 'breathtaking', 'brilliant', 'gifted', 'public', 'global', 'when Muslims are concerned', and flocking to some place in the 'Muslim world' when 'Europeans' were the subject at hand.

Consider, for example, the following three statements:

(1) The ethos of learning was a culture where inquiring minds searched for truth based on scientific rigor and experimentation, where opinion and speculation were cast out as unworthy pupils. This system of learning embodied by medieval Islam formed the foundation from which came exceptional inventions and discoveries. [63]

Whoever came up with this description has never read a biography of a medieval scholar that describes the standard learning methods such as memorizing a chapter or, in the more demanding circumstances, an entire treatise, listening to a teacher reading a text aloud and commenting on it word by word so that the students could write it down carefully for later remembrance, and studying one and the same elementary text on arithmetic, algebra, astronomy, or medicine more than once with different teachers. Neither did the writer ever bother to read any of these elementary school texts that fill manuscript libraries across the globe in great number, outweighing by far anything that might count as exceptional.

(2) Muslim learning hit [!] medieval Europe in the 12th century. A massive [!] translation exercise [!] began of Arabic works from the previous 500 [!] years into medieval Latin, making available the rational ideas from experiments [!] to a new audience. The availability of well-referenced material kick-started [!] European tertiary education [!] and questioned the idea that there had to be conflict between religion and

> science [!]. At Chartres [*sic*] cathedral school in the 1140s, Thierry of Chartres taught that the scientific approach [!] was compatible with the story of creation in the Bible, paving the way for the Renaissance [!]. The first university in western Europe was at Salerno in Italy, which burst [!] into life in the late eleventh century after the arrival of Constantine the African. The French city of Montpellier was an offshoot of Salerno and a major center for the study of Muslim medicine and astronomy. It was close to Muslim Spain, with its large population of Muslims and Jews. By the beginning of the twelfth century, the intellectual powerhouse [!] of the Western world had shifted to Paris, 'a city of teachers', as the knowledge of Arabic works continued to spread with traveling scholars. Indeed, many historians today say that the blueprints [!] of the earliest English universities, like Oxford, came with these traveling, open-minded scholars and returning Crusaders who, as well as visiting Muslim universities in places like Córdoba, brought back translated books based on rational thought rather than confined to religious thought. [71]

The exclamation marks are meant to highlight some of the aforementioned vocabulary of exaggeration, modernization, and utterly improper representation of medieval times. Muslim learning did not hit Europe whatever the date, neither did it arrive in Europe only in the 12th century: the first Arabic texts on the ancients sciences arrived on the Iberian peninsula in the ninth century and on Sicily at the very latest a century later. The translations were not an exercise but a serious undertaking of many individuals, most of whom we do not know by name. The texts translated into Latin covered primarily astrology, magic, and divination. Treatises on what we consider today as scientific were translated on a much more modest scale. But even those that we acknowledge today as scientific did not make available rational ideas from experiments so much as rational ideas based on axiomatic systems like Euclidean geometry or on astronomical observations, cosmological theories, mathematical models like those in Ptolemy's *Almagest*, as well as on philosophical or medical theories found in works by Aristotle, al-Farabi, Ibn Sina, Ibn Rushd, al-Ghazali, Zakariyaᵓ al-Razi, ᶜAbbas al-Majusi, and others. Mediaeval Arabic or Persian scholarly texts, whatever their field of knowledge, are often not well-referenced: many of their borrowings remained anonymous. Salerno's university was founded in

1968. Constantine the African was perhaps a Christian but we do not really know much about his life in North Africa and how he came to arrive at Salerno around 1070. The many tales about him that can be accessed on the Internet are unreliable: he produced his translations of Arabic translations of Greek medical texts as well as newly composed Arabic medical texts based on Greek theories most likely at the Benedictine monastery of Monte Cassino. The city of Montpellier was not an offshoot of Salerno: it was mentioned already in the late 10th century. Since Montpellier's medical school was only founded in 1220, it is equally impossible that it was an offshoot of Constantine the African's translations at Salerno; indeed, their professors did make use of Latin translations of Arabic texts in their teaching of medicine but their teaching of astronomy was a kind of preparation for studying astrology that was needed for predictions and diagnosis. Thierry of Chartres did not write about the relationship between the scientific approach and the *Bible* in his *Heptateuchon*, in which he included texts translated from Arabic (translated from Greek or newly composed) on the Iberian peninsula; he rather wrote on the relationship between the *trivium* and the *quadrivium* and the use of arguments from the quadrivial disciplines (number theory, geometry, astronomy, music) for proving claims about God. Neither he nor his works paved the way to the Renaissance. The claim that Paris had become the 'intellectual powerhouse of the Western world' at the beginning of the 12th century contradicts the previously quoted claim by Ragheb Omaar that Ibn Rushd had 'launched' the city as such in the 13th century. Moreover, the Western world did not yet exist as a cultural, economic, or political concept. I could not find any contemporary historian who actually believes that 'open-minded scholars and returning Crusaders' brought a 'blueprint' for Oxford's university from the Muslim world in the 12th century. There are historians like Charles Burnett who have argued for the possibility that copies of Arabo-Latin translations came to England *via* Paris and Mont St Michel but this is a different kind of claim and evidence. The early history of Oxford University is not well documented: there is reason to believe that it grew out of older monasterial schools in the region.[2]

(3) There are quite a few mathematical ideas that were previously be thought to have been brilliant conceptions of 16th-, 17th- and 18th-century Europeans. From the studying and unearthing we now know that Muslim mathematicians, about four hundred years earlier, were calculating with great intensity. Many of these mathematicians came from the Iraq/Iran region around 800 CE, when the House of Wisdom was the leading intellectual academy in Baghdad. [65]

This passage is utterly confused in dates, concepts, and geographies. In the calendar that I use, the 16th century came 700 years after 800; hence, the intensely calculating mathematicians of the 12th century cannot have worked at the House of Wisdom. Moreover, it remains unclear who these calculating hotheads were; I at least cannot offer even a single name as a candidate for the honor, at least not for the ninth or 10th centuries. The 13th or 14th centuries would be another matter: here I could point to at least two *muwaqqits* (scholars who had special expertise in the astronomical, geometrical, and arithmetical methods as well as in the instruments used for determining prayer times, the prayer directions towards Mecca, and the beginning of a month), namely, Shams al-Din al-Khalili and Najm al-Din al-Misri, about whom David King and François Charette have written important works apparently unknown to the author of the subsection on mathematics. The Iraq/Iran region is rather large and certainly much, much larger than Baghdad—maybe the author simply wished to indicate that numerous scholars in eighth- and ninth-century Baghdad had come from what we call today Iran and Iraq but botched the sentence. Finally, whether some Muslim mathematicians calculated whatever it was with great intensity—does this mean that they calculated very fast or very much or with great passion?—has nothing to do with mathematical ideas or brilliant conceptions. These are rather two different conceptual levels: on the level of English, unearthing has to come before studying; on the level of material objects, very few of such manuscripts have in fact been unearthed or excavated, since they are preserved in libraries and are to be found in catalogues or on shelves, if possible.

2 http://www.newadvent.org/cathen/11365c.htm.

Editorial issues

The editorial efforts to produce a better version of the companion book are clearly visible and are substantial, in particular in regard to English. Nonetheless, the editors did not manage to purge the 351 pages from every misuse of the English language or all the misunderstandings of mathematical and scientific topics or technical works. Some examples are:

(1) English:
- 'For the last eight centuries, chess has gone from strength to strength, producing a few funny stories along the way,…' [47];
- 'These medieval brains met every day for translating, reading, writing, and discourse' [72];
- '…al-Kindi, who commissioned the translation of Aristotle, and Hunayn ibn Ishaq, who translated Hippocrates' [74];
- 'Ibn al-Haytham did his experiments in complete darkness,…' [80];
- 'As well as Michael Scott and Daniel of Morley, the city of Toledo was buzzing with contemporary translation scholars' [83].

(2) Arabic:
- 'were known as the Banu Musa brothers' [52–53];[3]
- 'halaqa' [70] for 'halqa';
- 'Algebr wal Muqabala' [23] should be transliterated as 'al-Jabr wa'l- muqabala'; the Arabic article 'al' is not spelled with capital 'A' in transliteration;
- 'al-Muʿtadid' not 'al-Muʾtadhid';
- 'Harun al-Rashid', not 'al-Rashid' [72].

(3) Geography/history:
- 'From Andalusia, the game spread among Christian Spaniards and the Mozarabs…' [47]: the Mozarabs were Christians who lived across the Iberian peninsula and who differed from other Christians by having adopted Arabic and major elements of the culture of their Muslim neighbors;
- 'Carpets were first made before Islam by the Bedouin tribes of Arabia, Persia and Anatolia. They used carpets as tents, sheltering them from sandstorms…' [60]: there are no sandstorms

[3] 'Banu' means 'brothers'.

in Anatolia and pre-Islamic tribes in Anatolia and Iran are not labeled Bedouin;

- ‘The critique and commentary on Aristotle by Ibn Rushd,...was the real start of Europe’s classical revival, and this 200 years before the start of the European Renaissance’ [82]. Ibn Rushd’s commentaries were translated in the early 13th century into Latin as well as Hebrew. Regrettably, the role of such Hebrew translations of Arabic texts is not a part of the book’s tale; indeed, in this tale, neither Jews nor Muslims were a part of European culture/s. Further, before the translation of Ibn Rushd’s works, during the 12th century, many other Arabic texts had been translated into Latin. Hence, the claim of a ‘real start of Europe’s classical revival’ with Ibn Rushd is false and misleading in more than one respect.

(4) Sciences, mathematics, technologies:

- al-Khwarazmi’s algebra

> was a revolutionary move away from the Greek concept of mathematics, which was essentially based on geometry. Algebra was a unifying theory that allowed rational numbers, irrational numbers and geometrical magnitudes to all be treated as ‘algebraic objects’. It gave mathematics a whole new dimension and a development path, much broader in concept than before. It also enabled future development. Another important aspect of the introduction of algebraic ideas was that it allowed mathematics to be applied to itself in a way that was not possible earlier. The torch of algebra was taken up by the successor of al-Khwarizmi, a man called al-Karaji, born in 953 CE. He is seen by many as the first person to completely free algebra from geometrical operations and to replace them with the arithmetical type of operations which are at the core of algebra today. He was first to define the monomials x, x^2, x^3,...and $1/x, 1/x^2, 1/x^3$,...and to give rules for products of any two of them. He started a school of algebra which flourished for several hundred of years. [64]

Scholars of Islamicate societies had no concept of rational and irrational numbers. For them, algebra was a branch of arithmetic, not mathematics as a whole, which either dealt with equations (in most extant texts with linear and quadratic equations) or which focused on operations with exponents of integers or fractions with a numerator = 1, their sums, products, and quotients. Algebra remained a much less well grounded part of mathematics than geometry for scholars who wrote in Arabic, Persian, or Ottoman Turkish. Even authors like al-Karaji, who was *not* the direct successor of al-Khwarizmi, appreciated geometry more highly than algebra because of the former's solid axiomatic and deductive foundation. R. Rashed's belief that he freed algebra from geometry was and is not shared by many historians of mathematics in Islamicate societies: the 'arithmetical type of operations' that al-Karaji used in his algebraic treatise is not that of modern algebra, since modern algebra has a profoundly different character than that of any medieval Muslim writer on the topic. Further, the powers of integers and fractions were not introduced first by al-Karaji but can be found already in Diophantus' *Arithmetica*. This also applies to their products. Again, al-Karaji did not found a school of algebra and al-Samawʾal in the 12th century was not 'a member of al-Karaji's school'. The nonsensical claims about the developmental path, the much broader concept of mathematics, and the opening up of the future that set algebra apart and above geometry do not deserve any serious comment.

◦ The first Muslim, and perhaps person, to make a real attempt to construct a flying machine and fly was Cordoban ᶜAbbas ibn Firnas in the ninth century. He was the usual polymath of the time, becoming a renowned poet, astrologer, musician, astronomer, and engineer. But his greatest fame was for constructing a flying machine, the first of its kind capable of carrying a human into the air. He flew successfully a number of times over desert regions, improving his designs before attempting his two famous flights in Córdoba in Spain. The first flight took place in 852, when he wrapped himself in a loose cloak stiffened with wooden struts and jumped from the minaret of the Great Mosque of Córdoba. The attempt was unsuccessful, but his fall was slowed enough that he got off with only minor injuries, making it at least one of the earliest examples of parachute jump....Ibn Firnas was one to learn from experience, and he worked hard to improve his next design. Accounts from various eyewitnesses and medieval manuscripts described it as a machine consisting of large wings. So about 1,200 years ago, the nearly 70-year-old ᶜAbbas ibn Firnas

> made a flight machine from silk and eagle feathers. In the Rusafa area on the outskirts of Córdoba, Ibn Firnas mounted a hill and appeared before the crowd in his bird costume, made from silk covered with eagle feather, which he tightened with fine strips of silk. Ibn Firnas explained with a piece of paper how he planned to fly using the wings fitted on his arms: 'Presently, I shall take leave of you. By guiding these wings up and down, I should ascend like the birds. If all goes well, after soaring for a time I should be able to return safely to your side.' He flew to a significant height and hung in the air for more than ten minutes before plummeting to the ground, breaking the wings and one of his vertebrae. After the event, Ibn Firnas understood the role played by the tail, telling his close friends that when birds land, they normally land on the root of the tail, which did not happen for him because he did not have one. All modern airplanes land on their rear wheels first, which makes Ibn Firnas's ahead of its [*sic*] time. [296–297]

There are a number of absurdities in this text: who ever saw a bird land on its tail? The picture on page 297 shows a swan landing where it should land: on its feet. Ibn Firnas (d. 887) could at best be said to have been ahead of his time if airplanes landed on their back. This, however, would be akin to a crash-landing: the rear wheels are the feet of the airplane, not its tail. Then, there is the issue of the contraption designed by Ibn Firnas for his imitation of birds. It certainly contained no mechanical parts and thus was not a machine. Moreover, there seem to be only two Arabic sources that record bits and pieces of the event, none of them describes it as a machine: the 10th-century historical chronicle *al-Muqtabis* by Ibn Hayyan (987/8–1076), and the 17th-century chronicle by the Maghribi scholar Ahmad Muhammed al-Maqqari (d. 1632). The latter, who was probably the direct or indirect basis for *1001 Inventions*, quotes a line in a verse of a colleague of Ibn Firnas from the ninth century and presents his view on how Ibn Firnas had not flown but glided in the air:

> Among other very curious experiments which he made, one is his trying to fly. He covered himself with feathers for the purpose, attached a couple of wings to his body, and, getting on an eminence, flung himself down into the air, when according to the testimony of several trustworthy writers who witnessed the performance, he flew a considerable distance, as if he had been a bird, but,

> in alighting again on the place whence he had started, his back was very much hurt, for not knowing that birds when they alight come down upon their tails, he forgot to provide himself with one. [White 1961, 101]

E. Lévy-Provençal summarizes the information from Ibn Hayyan as follows:

> He (Ibn Firnas) was even a distant precursor of aviation, thinking out a sheath furnished with feathers and mobile wings; had the courage to put it on, to jump from the top of a precipice and to hover in the air for a few seconds before falling—escaping death by a miracle.[4]

Both sources hence agree that Ibn Firnas fabricated some kind of 'bird's costume', as *1001 Inventions* states at one instance, too. With such a contraption he could neither have flown nor glided: the picture of a stable construction of a glider on page 298 is thus misleading.

Errors, Exaggerations, Inventions

The main editorial shortcoming is the continued presence of numerous old errors and the introduction of new ones, all indicating a low level of familiarity with history on the part of both editorial teams (the FTSC and the National Geographic Society). The Map of Major Contributions in Muslim Civilization [14–15] claims, for instance, that the mosques of Cordoba and Toledo were built using 'gothic ribs' which 'inspired European architects and their patrons to use them in the Romanesque and Gothic movements'; that Ibn Khaldun's work *al-Muqaddima* with its ideas about how societies evolve, change, and disappear 'forms the basis of sociology and economic theory'; or that al-Idrisi produced 'an atlas with 70 maps called the "Book of Roger", showing that the Earth was round, which was a common notion held by Muslim scholars'.

The last claim is illustrated by a circular world map that was no part of the 70 rectangular maps to which the quotation refers. It was found several years ago in an earlier geographical work, the *Book of Curiosities*, by an

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4 http://0-referenceworks.brillonline.com.fama.us.es/entries/encyclopaedia-of-islam-2/abbas-b-firnas-SIM_0021?s.num=0&s.f.s2_parent=s.f.book.encyclopaedia-of-islam-2&s.q=%27abbas+ibn+firnas.

anonymous Egyptian author and, thus, in all likelihood, is not al-Idrisi's creation. Since Emilie Savage-Smith, one of the scholars who edited, partially translated, and commented on the *Book of Curiosities*, was an academic advisor of FTSC and *1001 Inventions* until 2007, this incorrect ascription of the circular world map to al-Idrisi must be from an older article appropriated from FTSC's website http://www.MuslimHeritage.com. Furthermore, it indicates that the repeated claim by the editors of *1001 Inventions* that they rely exclusively on the best scholarly works and would publish nothing that is doubtful or not approved by leading scholars of the various historical fields cannot be trusted.

The preceding three little examples show, furthermore, that the writers of these particular snippets as well as those of many other texts are lacking in even elementary historical, scientific and philological skills and understanding. Gothic ribs were, of course, not part of Romanesque architecture. The roundness of the Earth was already believed in by scribes in ancient Mesopotamia. The issue at stake was the planet's sphericity in which many, but by no means all, Muslim scholars believed, as al-Ghazali's (d. 1111) scorn, heaped on those who did not, illustrates. Ibn Khaldun's (d. 1406) cyclical theory of society's development, which owes much to Aristotelian and other ancient Greek theories, is not the basis of today's sociological or economic theories and had—as far as I know—no impact to speak of during the 18th and 19th centuries, when predecessors of today's theories were created.

The following is a list of selected errors (big and small), mostly present in the previous edition of the companion book and now carried over in the third:

(1) There was no 'golden age of discovery' that 'flourished from the seventh century until the sixteenth century' [17]. The standard beginning of the unfortunate historiographical metaphor of a 'Golden Age' is the early ninth century, when the bulk of translations from Greek into Syriac or Arabic and from Syriac into Arabic was produced, while its end has been determined by different historians differently (we find a trace of these different opinions on page 80, where this end is given as the 13th century). The subsequent centuries were often summarily labeled decline, a concept that has rightfully attracted much critical attention during the last decade or two when it was shown that advanced scholarly debates, in particular, on planetary theory and 'philosophical theology' (to be brief), also took place in the

16th or 17th century. The substantive contribution of this period was not so much one of discovery but one of appropriation, adaptation, amalgamation, modification, and innovation.

(2) The labeling of the period between 450 and 1492 as 'the Dark Ages' was originally limited to historians of Great Britain; other communities of historians applied the term only to the early Middle Ages. Since almost half a century, at the very least, the concept of a 'Dark Age' representing medieval intellectual history in Europe, whatever the cultural context, has been abandoned in talk of the High and Late Middle Ages. Recently, the label has also been challenged successfully by British, German, and other historians in its application to the early Middle Ages. It remains widespread, however, among amateurs and, apparently, such would-be-historians as the now retired professor of engineering and Chief Editor of *1001 Inventions* Salim al-Hassani.

(3) 'The House of Wisdom' did not bring 'men and women together from far and wide, from all backgrounds and faiths, to work side by side to study and better understand our world' [6]; nor was it either 'a prestigious academy and library,...founded a thousand years ago' where 'Muslim, Christian, and Jewish scholars cooperated in translating knowledge, fueling scientific debate and discovery' [18] or 'a major center of research, thought, and debate in Muslim civilization—the intellectual powerhouse of its day' [74]. Despite the very limited information that is provided in medieval Arabic sources about this institution, it is certain that it was founded before 833 and that it was not an academy, as it is briefly described in the first quotation from chapter 1 and the second quote from chapter 2 where it is described verbosely [72–75]. Rarely is any line on these four pages correct. Many are pure inventions. Others are shameless exaggerations. Whoever composed them must have read a novel about Baghdad's intellectual life in the early Abbasid period. How else would s/he have come to imagine that

> Caliph Harun al-Rashid,...built the scientific collection and Academy of Science. Caliph al-Maʾmun...extended the House of Wisdom and designated a section or wing for each branch of science, so the place was full to bursting with scholars or ʾ*ulama* [*sic*], art scholars, famous translators, authors, men of letters, poets, and professionals in the vari-

> ous arts and crafts. These medieval brains met every day for translation, reading, writing, and discourse. The place was a cosmopolitan melting pot, and the languages that were spoken and written included Arabic (the *lingua franca*), Farsi, Hebrew, Syriac, Aramaic, Greek, Latin, and Sanskrit, which was used to translate the ancient Indian mathematics manuscripts [*sic*]. [72]

No mathematical text is known to have been translated in the eighth or ninth centuries from Sanskrit into Arabic. Three or four such texts were translated from Sanskrit into Persian in the 17th and 18th centuries on the Indian Subcontinent but this is a different story. 'ᶜUlama' was not yet a term for scholars who worked primarily on philosophy, medicine, or the mathematical sciences. Arabic was not a *lingua franca*, albeit it became the main language of philosophy, medicine, and the sciences as a result of the many translations and the fact that Arabic was the primary spoken, religious, and administrative language of the Abbasid dynasty, although not yet of the society which they ruled. Syriac is an Aramaic dialect. Latin was definitely not spoken in Baghdad except perhaps by the occasional ambassador or merchant. Farsi is a recent silly replacement for the perfectly fine English word for this language, namely, Persian. But first and foremost, the House of Wisdom was not an academy nor a place of research. As Dimitri Gutas and Marie-Thérèse Balty-Guesdon have shown after a meticulous analysis of the extant, very limited testimonies, the House of Wisdom was primarily a library, with very few people directly connected to it either as charges of a director like the three brothers Musa (Muhammad, Ahmad and al-Hasan) after the death of their father or as directors. The few translations undertaken in this institutional context concerned mostly translations of Middle Persian (Pahlavi) astrological texts into Arabic [see, e.g., Gutas 1998, 53–60].

(4) The remarks in chapter 1 on the universities and subsection 2 (Universities) in chapter 3 are based on the misguided assumption that the teaching institutions for higher level education that emerged over time in various Islamicate societies were the basis for the universities that were founded between the 11th and 13th centuries in Bologna, Paris, Oxford, Cambridge, Salamanca, Montpellier and other cities. Even if one followed George Makdisi and assumed an influence of *madrasa* teaching forms and methods upon some or all

of those universities, the differences in legal status, setup, structure and organization are considerable, too great to consider both types of institutions as principally the same. Universities were not founded in any Islamicate society before the second half of the 19th century. Most of them were set up only in the 20th century, often in declared contrast to the old types of teaching institutions and their methods. Old mosques and their *madrasas* received this status also only in the 20th century, often against heavy resistance of their scholars, as was the case of the al-Azhar.

Particular mistaken claims are that 'all over the Muslim world, advanced subjects were taught in mosques, schools, hospitals, observatories, and the homes of scholars' [68], that the *ijaza* is a certificate equivalent to a final degree like a master or a diploma [69],[5] that there was something equivalent to a PhD called 'Risaleh' [*sic*][6] given after 10 years at the university of Sankore in Timbuktu, where 25,000 students studied not merely law and other religious fields, some elementary mathematics and astronomy, as well as some logic, physics, chemistry, surgery, art, linguistics, but also learned a vocation in trade, farming, fishing, shoemaking, tailoring and navigation [69]. Similarly unguarded and unjustified claims can be found on the Internet.[7]

The author of this part of *1001 Inventions* has, however, gone far beyond the modernizing aggrandizement of the level and scope of learning and scientific themes taught from the 12th to the 16th centuries at Timbuktu and elsewhere in West Africa. Not only did s/he extend the period generously to the 10th century, ignoring that Timbuktu was only founded a century later, but s/he also apparently never checked the location of Timbuktu on a map. Why someone far away from the ocean should learn anything about navigation remains a puzzle. Not being an expert on West Africa, I do not wish to express too strong a rejection of the various highly suspicious statements regarding the *madrasas* of Timbuktu. But I find it very difficult to believe that art, physics, chemistry, linguistics, and other modern

5 Compare the descriptions of the various kinds of teaching certificates in Islamicate societies by Adam Gacek [Gacek 2009, 51–59].

6 This is a Pakistani or Persian transliteration, not an Arabic one.

7 http://www.timbuktufoundation.org/university.html.

disciplines where studied there or that they offered in adjunct shops vocational training of the kinds mentioned. Neither do I give any credit to the claim that a work similar to a PhD was part of their teaching. Other claims like the one [70] that 'baccalaureus' derives from the Arabic expression 'bi-ḥaqq al-riwāya' ('on the authority of an oral transmission')[8] are most likely wrong because they are based on articles that neither investigated other linguistic backgrounds of 'baccalaureus' nor considered the much earlier appearance of clearly related terms like 'baccalaria' and their possible philological backgrounds.[9] Since about the ninth century, the owner of a *baccalaria*, a piece of land leased from a big landowner for rent, was called *baccalarius*. The same term was applied to male or female adolescents, squires as well as noblemen, who could not afford their own banner and to low-ranking members of other organizations like guilds or the Church.[10]

The main methodological shortcomings of such speculations consist, however, in the absence of any study of possible socio-cultural contexts of the transmission of concepts—like the Arabic one proposed as the origin of 'baccalaureus'—and analysis of the fundamental cultural differences between the two terms. 'Bi-ḥaqq al-riwāya' was a formula within a certificate of audition expressing that the piece of text which was certified had been transmitted by someone (transmitter or author) by oral instruction [see Gacek 2009, 53]. 'Baccalaureus' was, in contrast, the title for a young man who had passed his exams at the lowest faculty of the university as well as the *disputatio* and was now permitted to lecture there.[11] Again, 'minbar' began its life as one of several words for a seat for a ruler or for a judge. Only in the middle of the eighth century does the word seem to have taken on the more limited meaning of a stair leading to a seat for delivering the Friday prayer, the *khutba*. This means it became something that in

[8] This statement signified that the certified text had been read aloud in the presence of a teacher who was linked to the text's author in an unbroken chain of transmitters or was the author himself.

[9] For an example of such a one-sided discussion of the possible origin of 'baccalaureus', see Ebied 2003.

[10] http://peter-hug.ch/lexikon/Baccalaureus.

[11] http://peter-hug.ch/lexikon/Baccalaureus.

Christian tradition would be called the pulpit. While teachers used to choose an elevated place to sit on, the sources that I am familiar with do not talk all that much about it and do not call it by a specific name. Hence, I conclude the idea that the professorial chair as a cultural concept was derived from the *minbar*, as *1001 Inventions* claims [70], is not backed by the evidence in hand.[12]

(5) Caliph al-Maʾmun cannot have written to the king of Sicily nor could this king have sent copies of his manuscripts, since there was no king of Sicily in the third decade of the ninth century [73]. Sicily was part of the Byzantine Empire as of 535 and was ruled by a Byzantine governor. From 827 to 902, the North African Aghlabids held southern Sicily through a governor. Now, the Aghlabids may have sent Byzantine manuscripts to al-Maʾmun, although I do not know of any evidence for this. But their governor certainly was not the king of Sicily; it was only in 902 that the Aghlabids gained almost complete control of the island.

(6) Concerning al-Maʾmun, the first claim below is an exaggeration and contested in research, while the second is simply wrong:

(i) al-Maʾmun did not merely steer the House of Wisdom but built an astronomical center in Baghdad [73]. There is no proof in the sources that he directed the House of Wisdom; nor is there any reference in the early sources to anything like an astronomical center. Scholars have taken different positions on whether there was a special building reserved for the purpose of repeated observations.[13]

(ii) al-Maʾmun established

> many higher institutes, observatories and textile factories. It is said that the number of higher institutes during his reign reached 332. They were packed with students pursuing various subjects in the arts and sciences. [73]

Though I know nothing about textile shops in Baghdad (which is not famed for its production but its import of textiles), I am

[12] http://0-referenceworks.brillonline.com.fama.us.es/entries/encyclopaedia-of-islam-2/minbar-COM_0744?s.num=0&s.f.s2_parent=s.f.book.encyclopaedia-of-islam-2&s.q=minba.

[13] See, for instance, http://0-referenceworks.brillonline.com.fama.us.es/entries/encyclopaedia-of-islam-2/marsad-SIM_4972?s.num=1&s.f.s2_parent=s.f.book.encyclopaedia-of-islam-2&s.q=observatory.

certain that, according to the available sources, higher institutes of learning did not exist in this period and that the arts and sciences were not studied formally in (classes) packed with students. It is already difficult to find information about anything like a class on a scientific subject beyond the statements about al-Kindi's or Thabit b. Qurra's teaching the one or the other son of their patrons (Caliph al-Muᶜtamid in al-Kindi's case, Muhammad b. Musa in the case of Thabit b. Qurra).

(7) The library of the Umayyad ruler al-Hakam II (reg. 961–976) in Cordoba was not destroyed by crusading invaders but by al-Mansur, the chamberlain of al-Hakam's son Hisham. al-Mansur is said to have dispersed and destroyed in particular the scientific books. It is highly unlikely that the library contained '600,000 Islamic books' whatever the meaning of 'Islamic' here may be [22].[14]

(8) The following is a totally absurd and unfounded claim about the origin of the Indo-Arabic signs for the numbers 1...9:

> They (these signs) are believed to have been based on the number of angles each character carries, but the number 7 carries a challenge, as the medial horizontal line crossing the vertical leg is a recent 19th-century development. [86]

The fourth little diagram on page 87 shows 'the numbers 1 to 9 we use today based on the use of angles'. First, we do no write these numbers in the angular fashion shown; second, the diagram above this angular nonsense shows three specimens of written forms of the signs from Arabic manuscripts that plainly contradict the angular hypothesis. The lack of critical discernment of what can or cannot be a reasonable hypothesis is characteristic of many writers for *1001 Inventions* and could not be illustrated more clearly. This third diagram is headed by the claim that 'the Muslims devised modern numerals' which is partially correct and partially an exaggeration. Only very few scribes, as compared to the entire population, participated in producing variant after variant of the nine numerical signs plus the sign for zero. Thus, the definite article is inappropriate in the header.

[14] For a discussion of al-Hakam's library and argument that the number of items expresses awe and is not the result of the librarian's counting of each and every manuscript, see Wasserstein 1990–1991.

As far as the manuscript evidence shows, there is no straightforward, clear progress from one form to other; it took centuries to finalize the forms that we use today in scripts based on the Latin alphabet and those used in scripts based on the Arabic alphabet. These final forms in the first case emerged partly in Arabic and partly in Latin as well as in vernacular texts in North Africa and parts of Europe (mainly the Iberian peninsula, France, England, Italy, Germany).[15]

(9) Fatima al-Fihri [*sic*][16] did not found a university but a mosque in Qayrawan [26, 69: cf.8]. Robert Grosseteste did not study in Cordoba [29]. Neither Ibn al-Haytham nor Ibn Khaldun were alumni of the al-Azhar *madrasa* [68]. Ibn Khaldun taught at al-Azhar [68], but Maliki law; he also taught a divinatory art (geomantics). St. Jerome did not write a *History of Ancient Nations* nor was such a work translated into Arabic [70]—the author of this section has copied this from Sibai [1987, 53], who mistakes St. Jerome for Orosius and the latter's *Historiae adversus paganos libri septem* (fifth century AD) for the *History*. Yuhanna b. al-Bitriq did not translate a Latin translation of Aristotle's *Historia animalium* (not 'Book of Animals') into Arabic but a Greek version [72–73]. al-Jahiz did not live in the eighth but in the ninth century [77]. Gerbert of Aurillac did not study at Cordoba and then return to Rome [86–87]. The Ottomans did not 'develop' Kufic style nor Naskhi calligraphy [102]: both came into being many centuries before, as correctly stated a page earlier [101].

(10) The armies of Aragonese, Castilian, and other local Christian rulers from the north of the Iberian Peninsula, which often also included Muslim forces due to various alliances between rulers of different faiths, are not called 'crusaders' [22]. This term is usually reserved for Catholic invaders in Egypt, Syria, Palestine, and Byzantium. Sicily was lost to Muslim rulers centuries before the Iberian peninsula came fully under Catholic rule and Timur conquered Iran and parts of Central Asia. 'The Muslim world' did not 'suffer the onslaught of Timur' as a 'foreign' invader like the crusaders and the Mongols, as implied in *1001 Inventions*' effort to explain what its editors term 'the coming to an end of such an enlightened era' [22]. Timur was

[15] For the sake of brevity I use modern geographical labels.

[16] This has to be 'al-Fihriyya'.

raised as a Muslim; he was a tribal upstart who sought to carve out a territory to rule and plunder like many other Muslims who conquered various parts and pieces of Asia, Africa, or Europe [22].

(11) Robert Boyle, John Wallis, or Johannes Hevelius did not translate Arabic manuscripts [23]. Their interests in mathematical, astronomical, and other manuscripts in Arabic or Persian were often limited to very special issues like the parallel postulate or observational data. They were neither the first nor the only scholars during the 16th and 17th centuries who asked their colleagues for help regarding such texts, who corresponded with the Royal Society about potential projects of translation, or who wrote lists of questions about nature, commerce, culture, and so on, for merchants visiting foreign lands, in particular China, India, the so-called Spice Islands, or the Ottoman Empire. The editors of *1001 Inventions* present these bits of historical knowledge as if they were the first to discover them, not acknowledging anywhere the academics who researched and wrote about them long before al-Hassani and his collaborators started dabbling in history [23]. It is not true that the Latin translations from Arabic to Latin made in the 12th and 13th centuries 'fed the scientific and philosophical revolution of the 1600s and kept the flame of knowledge alive' [23], although it is true, in my and some of my colleagues' understanding of the intellectual activities in Catholic and Protestant countries, that several of those translations were among the resources that scholars drew upon during those two centuries, some defending their academic traditions and others taking them apart [see, e.g., Russell 1994].

(12) The 'Muslim world' did not 'stretch for more than a 1000 years from the seventh century onward from southern Spain as far as China' [24]: the last Islamicate society on the Iberian peninsula was conquered by Catholic troops in 1492.

(13) Caliph ᶜUmar (reg. 634–644) did not govern with ministers, let alone with a female health and safety minister [23]; Jabir b. Hayyan is not considered the 'father of chemistry' except by amateurs [23]; al-Razi was not the 'father of clinical and experimental medicine' [24]; Lubna (not Labna) was not a mathematician and scientist but is said to have known some arithmetic, probably as part of her training as scribe or secretary of the Umayyad Caliph al-Hakam (not Hakim).

(14) The image of a windmill [23] does not show a medieval specimen; the painting of Cordoba does not portray the city in its Islamic period, but in the 18th or early 19th century [23].

(15) The first reference to chess in a Latin source is not by Countess Ermessind of Barcelona in 1058 but by Count Ermengaud I of Urgel (today Spain) in 1008. The diagram in a copy of *Muntakhab*[17] *Kitab al-shatranj* is not a miniature; and though the text itself is ascribed to al-Suli, it is more likely the work of a later person since the title of the book, which al-Suli apparently wrote together with a physician from Sarakhs, is simply *Kitab al-shatranj*. This work is described as a manual either on problems and openings or on openings alone; hence, it seems to be unlikely that it is a collection of chess games that were played by correspondence, which according to the caption is considered by scholars a possibility. Since this general, anonymous reference to scholars is missing in the second edition, I do not trust it here: it would have been more convincing if some specific evidence had been added that chess was indeed played at long distance. The text on chess is imprecise in its formulations suggesting for instance that 'Arabs' brought chess from the 'Persian court', meaning the Sasanian dynasty, to 'medieval Spain' without indicating the many centuries separating them—the reader is invited to believe that the 'Persians' and the 'Arabs' in this presentation were contemporaries. The text is confused when naming important chess players at the Abbasid court in Baghdad, ignoring one of the most famous of them, namely, al-ᶜAdli, and giving a name that I could not find in Ibn al-Nadim's list, i.e.. al-Aadani [*sic*]—which certainly is misspelled—and mistaking Ibn al-Nadim, the author of the list, for a leading chess player. The sequence of the two first names, i.e., al-Suli and al-Razi, reverses the order of their lifetime, with Suli having been born in about 845 when Razi was already at the height of his success. Both players (or their ancestors) came from Iran; thus, the chance that they were among the 'Arabs' who brought chess from the 'Persian' court is slim. The name of the Russian grandmaster Averbakh is misspelled as 'Averbak'. That he appropriated an opening from al-Suli without saying so, as suggested by the text, is probably another

[17] This is misspelled as 'Muntahab' on page 47.

overstatement; at least, the biographical notes about Averbakh on the Internet mention his study of and his high appreciation for al-Suli's descriptions of chess games. The title of the book produced in 1283 for Alfonso X is 'Libros de ajedrez, dados y tablas' ('Books of Chess, Dices, and Boards').

(16) Many of the early buildings have not survived the vicissitudes of time and, thus, we cannot say as *1001 Inventions* does that '(l)ike many Muslim buildings, schools were constructed with no expense spared, and beauty was an important consideration' [65–66]. There were certainly many beautiful, spacious *madrasas* built by wealthy rulers, their wives, daughters, relatives, and officials which we still can admire today in Cairo, Damascus, Sivas, Erzerum, Istanbul, Isfahan, or Samarqand, to give only a few well known examples. But many *madrasas*, not to speak of the *kuttab*, were houses donated as a *waqf* by their previous owners and, depending on the individual wealth, taste, and status of these donors, could have all sorts of sizes, sumptuousness, and decorations. Many small *qubbas* with one or two tiny rooms can be seen when traveling through North Africa, for instance. Except for some of the dynasties, among them the Almohads and the Ottomans, there was no official supervision of teaching and certainly no state office responsible for such an educational policy: individual sultans, shahs, and governors interfered repeatedly in the appointing of teachers for *madrasas* but so did the *madrasa* teachers themselves who manipulated members of the military aristocracy to snatch away a chair that they coveted and whose incumbent they objected to. Things never were or are that easy, straightforward, orderly, and glorious as *1001 Inventions* portrays.

There are many more errors, exaggerations, simplifications, and inventions to be found in the third edition of *1001 Inventions*, more than I care to report. I am certain that those I have listed and their variety of types will make clear that *1001 Inventions* is unreliable, disastrous, and, as I know from my experiences in class rooms, dangerous. Even academics have fallen for this glamorous, superficial, heavily distorting fabrication of a further variant of Muslim-centric history of science. Some of them have actually contributed actively to its tales.

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The Teachings of Syrianus on Plato's Timaeus *and* Parmenides by Sarah Klitenic Wear

Studies in Platonism, Neoplatonism, and the Platonic Tradition 10. Leiden/ Boston: Brill, 2011. Pp. xiv + 353. ISBN 978–90–04–19290–4. Cloth €108.00, $153.00

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Syrianus succeeded Plutarch of Athens as head of Plato's Academy and held that position for only a brief period from AD 432–437. He was a philosopher in the Iamblichean tradition in Neoplatonism and lectured on the works in the Iamblichean canon. Syrianus' views are known to us through surviving commentaries on four books of Aristotle's *Metaphysics* (B, Γ, M, and N) as well as through the references to him by his most famous pupil, Proclus (410–485). We also possess notes on Syrianus' lectures on Plato's *Phaedrus* taken by another of his students, Hermeias.

Syrianus' commentaries on the *Metaphysics* have now been translated into English by John Dillon and Dominic O'Meara [2006 and 2008]. In 1997, Hildegund Bernard provided a German translation of the work by Hermeias. In this careful and thorough book, Sarah Klitenic Wear seeks to complete the work that remains by extracting from Proclus' extensive commentaries on Plato's *Timaeus* and *Parmenides testimonia* for Syrianus' views on these dialogues. I should disclose that I was a reader for this manuscript when it was submitted. I thought then that it would be a valuable addition to scholarship on late antique Platonism and I think now that the final version justifies that initial assessment. (If anyone, apart from me, would like to congratulate me for my good judgement in these matters, I am of course only too happy to accept!)

It has long been known that Proclus' philosophical writings are heavily indebted to those of his teacher. The question of how original Proclus was and to what extent he systematized and recorded the largely oral teachings of Syrianus is probably one that cannot be definitively answered. In selecting pas-

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sages in Proclus as those that give us fragments—or, perhaps more strictly, *testimonia*—for Syrianus, Klitenic Wear has been cautious. She confines herself to points in the commentaries where Proclus specifically mentions Syrianus or uses familiar phrases to refer to him, such as 'our teacher' or 'our father'.

Even with this very conservative methodology, Klitenic Wear is able to assemble 25 fragments from Proclus' *Timaeus* commentary that she regards as solid evidence of Syrianus' views on that dialogue. It is certainly possible that Syrianus' presence in the work is far more pervasive than this. Proclus' biographer Marinus described the composition of this work as taking place while Proclus was studying with Syrianus:

> Working day and night with tireless discipline and care, and writing down what was said in a comprehensive yet discriminating manner, Proclus made such progress in a short time that, when he was still in his twenty-eighth year, he wrote a great many treatises, which were elegant and teeming with knowledge, especially the one on the *Timaeus*. [Marinus, *Vit. Proc.* §13, trans. Edwards 2000, 76]

Even so, Klitenic Wear's conservative strategy is the only methodologically sound option available. While we might suspect that Proclus and Syrianus speak with one voice in many more places, the only principled method for selecting *testimonia* is the one that Klitenic Wear adopts. With respect to Syrianus' views on the other key dialogue for the Neoplatonists—the *Parmenides*—Klitenic Wear locates 10 fragments from Proclus' commentary and adds another five from Damascius' work on this dialogue. Thus, this collection provides us with 40 fragments in total.

For each fragment, Klitenic Wear provides Greek text (or Latin for that portion of Proclus' *In Parm.* where we possess only Moerbeke's version), with facing English translation. This is followed by an extensive discussion of each fragment. With respect to Proclus' *In Parm.*, she uses Steel's new Oxford Classical Text edition [2007–2009]. For the *Commentary on Plato's* Timaeus, she uses Diehl 1903–1906 of course.

On the matter of translation, Klitenic Wear largely follows the translation of Morrow and Dillon [1987] for Proclus' *In Parm.* The translation of the passages from Proclus' *In Plat. Tim.* was completed by Klitenic Wear prior to the publication by Cambridge University Press of the multi-volume translation of that work by Tarrant [2007], Runia and Share [2008], and Baltzly [2007 and 2009]. However, she took account of it when completing the revisions of her book.

The fragments are preceded by a 29-page introduction in which Klitenic Wear succinctly summarizes the results of her findings. She takes care to show the points at which Syrianus disagrees with, or further refines, the views of Iamblichus. She also sets out and justifies her methodology for fragment selection. There is nothing especially new in her methodology—and, of course, that is no bad thing! Klitenic Wear adopts Anne Sheppard's four-stage model for the composition of Proclus' commentaries—a process that involved collaboration with Syrianus. In this model, a lecture by Syrianus was followed by discussion with Proclus, which in turn led to a lecture by Proclus. This was then followed by the transmission of the lecture in the written form that we find in the *In Plat. rem pub.* Following Dillon's work [1973] on Iamblichus' fragments in Proclus, Klitenic Wear adopts the hypothesis that, when Proclus reports Syrianus' view with «γάρ» and direct speech, he agrees wholeheartedly; and that when he introduces a slight correction, he often uses uses «γάρ» with accusative and infinitive. Klitenic Wear does offer a suggestion of her own in addition to these methodological observations. She hypothesizes that Proclus' use of the imperfect «ἔλεγεν» to report Syrianus' views may indicate that the teaching was delivered orally. The introduction concludes with a two-page round-up of recent scholarship on Syrianus.

The translation and commentary on the fragments takes up the next 300-odd pages. The volume concludes with a list of frequently cited abbreviations, a bibliography, an index of philosophical terms and names, as well as an index of passages from ancient authors. The book is nicely presented, as we have come to expect with Brill.

Specialists in the area of Neoplatonic studies will find many interesting observations on Syrianus' fragments in Klitenic Wear's commentary. Anyone working in this area will want to have this book on a shelf nearby. But if we move above the specific issues that absorb scholars of late antique Platonism, what does Syrianus have to teach us as students of ancient philosophy and science more generally? The answer to this question—as have I just posed it—is 'Not a great deal'. Far from being a negative judgement on this book or on Syrianus, however, I think that this verdict in fact tells us something about the kinds of questions that we ought to pose about Neoplatonic authors.

Since this is a review for *Aestimatio*—a journal that provides reviews in the history of *science*—let us concentrate on Syrianus' commentary on the *Timaeus*. This, after all, was thought by the Neoplatonists to be work on nature [Proclus, *In Plat. Tim.* 1. 1.5: cf. *Anonymous Prolegomena* 22.21, ff]. Granted, it is supposed to be a distinctively Platonic kind of physics and that makes it more theological than Aristotle's *Physics*, but it is closer to natural philosophy than the purely theological *Parmenides*. What does Syrianus tell us about this dialogue?

Syrianus begins right at the beginning, entering into the dispute about the very first line of the dialogue [*Tim.* 17a]. The fourth participant from the previous day's discussion (depicted in the *Republic*) is missing because it is fitting that these higher mysteries should have a smaller audience. Natural philosophy is a more elevated subject matter than politics. So too the Pythagoreans distinguished those who were able to grasp profound matters from those who heard more superficial teachings. This is consonant with the Iamblichean order of the Platonic curriculum in which the *Republic* and the *Gorgias* pertain to the cultivation of a lower order of virtues—the political virtues.

Syrianus' second fragment comments on *Tim.* 18d–e and reveals an interest in puzzles about human reproduction. Given the sharp distinction between soul and body in Platonism, why does breeding the best male and female guardians produce the best offspring? Does this implicitly commit Plato to the view that the soul enters with the seed, as Longinus thought [Patillon and Brisson 2001, fr. 27]? Syrianus' solution is to give the physical factors in reproduction responsibility for the body alone. But good bodies attract good souls, just as well-made theurgic statues afford an opportunity for the divine to dwell therein.

Fragment 10 discusses Plato's denial of any kind of Democritean plurality of worlds at *Tim.* 31a. There seems to have been an objection to Plato's argument that the unique paradigm implies a unique visible imitation: the cosmos. After all, the Form of Rabbit permits many instantiations. Why should not the same be true of the Form of Living Being Itself on which this cosmos is modeled? Why not many κοςμοί? Syrianus—like Porphyry and Iamblichus before him—sought a principled reason why some forms (e.g., the intelligible Sun) have a unique instantiation, while others may be multiply instantiated.

Fragment 11 also pertains to physics, at least in the Platonic sense. It deals with an objection raised by the third-century Platonist Democritus about Plato's claim at *Tim.* 32a–b that one number is sufficient to establish geometric proportion between two plane numbers. This is one of the few points where I think that Klitenic Wear's commentary is not quite sufficient to the task of placing Syrianus' words in their proper context.

The circular motion of the universe at *Tim.* 34a provides Proclus with an opportunity to invoke Syrianus' views on the *Statesman*'s myth of cosmic reversal [*Pol.* 269a, ff]. The Platonist Severus took this notion literally. Syrianus denied this reading of the *Statesman*. It seems likely that Proclus identified the cycle of Zeus and the cycle of Kronos [*Pol.* 272b] with distinct but simultaneous Demiurgic activities [cf. I*n Plat. Tim.* 3.309.20; *Plat. Theol.* 5.6, 25.3]. Thus, Syrianus and Proclus both rejected anything like a Stoic ἀποκατάστασις in which the existing world order was destroyed or reversed.

Fragment 19 is, in a sense, an exercise in the classification of living things—a kind of Neoplatonic biology. *Tim.* 39e–40a gives us a four-fold division of living beings based on their residence (or on the organs appropriate for beings with that address):

(1) the celestial gods,
(2) the winged kind,
(3) the aquatic kind, and
(4) the kind with feet.

How does this classification intersect with the classification in terms of gods, angels, daemons, heroes, and so on, that is also part of the Neoplatonic tradition? Klitenic Wear takes Syrianus' position to be that there are gods, angels and daemons in the celestial realm; with further gods, daemons and birds in the aerial realm; but with 'spirits proper to water and fish' in the water; and only mortal creatures on land. I find that this is one of the few points where I disagree. This reading seems to me to be inconsistent with the following fragment 20, where Syrianus says that the daemonic kind predominates down here (in the terrestrial realm) but that the divine kind is found here as well. In fact, Proclus' insistence that in populating the cosmos with the four kinds of living being the Demiurge bestows 'wholeness in the parts' [cf. *In Plat. Tim.* 3.97.24–98.6] *requires* that all things should be in all places but in each according to its nature. Hence, I think that we must have gods in water and in the terrestrial realm as well. The Earth itself is an example of the latter, since it is the first and most senior of the encosmic gods [*Tim.* 40c].

Tim. 41c–d provided an occasion for Neoplatonists to address the question of the relation between the mortal and immortal parts of the soul, or, more generally, the relation between soul and body. The astral body provided an intermediary through which these distinct existences were alleged to be connected. But even here there was disagreement. Porphyry had claimed that the psychic vehicle was itself mortal, while Iamblichus championed its indestructibility. In fragment 23, Syrianus characteristically combines both positions to distinguish a higher, indestructible psychic ὄχημα (vehicle) and a lower, destructible one. Needless to say, even vaguely sympathetic modern readers are unlikely to find such a strategy satisfactory. Surely, the imposition of yet more halfway houses of various degrees of materiality does not adequately resolve the problem of how an impassive and unextended soul relates to an extended body.

Many of Syrianus' fragments from his commentary on the *Timaeus* take up theological questions. These include:

- the classes of gods that correspond to the classes which Solon's Egyptian informant discusses at 24a [fr. 3],
- the position of the Demiurge among the assorted intelligible and intellectual triads [fr. 6],
- the correlation of Plato's Demiurge with the Orphic cosmology [fr. 7] and the relation of the Demiurge to the Paradigm [fr. 8],
- the nature of evil [fr. 9],
- correlations between features of the World Soul and the various divine orders [fr. 16],
- the position of Eternity in relation to other intelligibles [fr. 17],
- the identity of Gaia and Ouranos at *Tim.* 40e [fr. 21],
- the nature of encosmic as opposed to hypercosmic gods [fr. 22], and
- the relation of the Mixing Bowl or Κρατήρ discussed at *Tim.* 41d to various other divine names [fr. 24].

Other fragments address specific points of interpretation in the tradition of commentary on the *Timaeus*. Among these are:

- the senses of the word «λόγος» [fr. 4],

- the correct understanding of the contrast between Being and Becoming [fr. 5],
- the question of whether Plato alludes to a hypercosmic soul [fr. 13],
- the manner in which the psychic 'divisions' into portions and the harmonies between them are to be understood consistently with the unity of the World Soul [fr. 14],
- how the World Soul is capable of the opposite motions of the circles of the Same and the Different [fr. 15],
- the sense in which day and night are said to be 'parts' of time at *Tim.* 37d–e [fr. 18], and
- the necessity for each human soul to descend into Becoming at least once in every cosmic cycle [fr. 25].

Historians of science or philosophers whose primary concern is the understanding of Plato's *Timaeus* may regard Syrianus' contributions as small beer. In some sense, this would be just. By comparison, Proclus seems much more engaged with natural philosophy in general and astronomy in particular. Or—if it is reckless to say that much—it is at least true that Proclus does not mention his teacher in those places where he discusses topics such as the precession of the equinoxes or Aristotle's arguments for the fifth element. Proclus may have come down on the wrong side of some of these questions (after all, he simply denied the phenomenon of precession) but at least the questions are substantive. But, on the other hand, Proclus also fills page after page with discussions of topics that seem to us every bit as trivial as the relation of Plato's Mixing Bowl to other divinities in the system. So while his 'substance to trivia' ratio may be a bit better than Syrianus, there is an awful lot of what now appears to us to be trivia.

This may, however, be indicative of a failing on our part, not on the part of the Neoplatonists. We do not presently have a framework for thinking about the writings of the late antique Platonists that allows us to see how they could have regarded some of these disputes as important. In my view at least, a necessary first step is to recognize that these writings have their origins in the instructional setting of the schools at Athens or Alexandria. Contextualizing them first requires philosophers and historians of science to investigate more thoroughly the content and significance of late antique παιδεία in general. Then, we must consider the ethical goal of the Neoplatonic curriculum—becoming like god—and form hypotheses about how discussion of the apparently trivial features of Plato's dialogues could have been thought to facilitate this goal. This may require subjecting these texts to a kind of analysis that is as much rhetorical as it is philosophical. Arguably the great glory of the broadly analytic style in the history of philosophy is the deployment of distinctively philosophical imagination in the rational reconstruction of arguments. But we may need new imaginative resources to reconstruct not merely the arguments that might plausibly stand behind the conclusions but the very *point* of presenting arguments on these topics. Some steps in this general direction are presently being taken but more work is needed before late antique Platonism can claim a place in the standard philosophical canon alongside Hellenistic philosophy. In my view, the nature of that work will be very different from that which propelled Stoic and Epicurean philosophy into the canon in the period 1970–2000.

Professor Klitenic Wear's book does not undertake any such task but it is the sort of fundamental research that must precede that bigger interpretive project. As such, it is an extremely valuable contribution to our understanding of late antique Platonism. I simply wish to signal to those readers who wonder what philosophical payoff justifies this effort on the part of Klitenic Wear and others like her that at least some of us who undertake these labors feel the force of the problem too. We are working on it.

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Aristotelianism in the First Century BCE: Xenarchus of Seleucia by Andrea Falcon

Cambridge/New York: Cambridge University Press, 2012. Pp. xi + 228. ISBN 978–0–521–87650–6. Cloth $95.00

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Andrea Falcon's splendid new work on one of Aristotle's ancient Greek followers, Xenarchus, offers an elegant example of the potential for the commentary format to address larger questions than the ideas of a single author. Falcon not only offers text, translation, commentary, and interpretative essays on his particular subject but also considers much broader questions of orthodoxy, school practice, philosophy in the first century BC, and the formation of the commentary tradition in the post-Hellenistic period. No scholar studying this period, or interested in the Aristotelian school, would want to miss this.

The exploration into the work of Xenarchus of Seleucia—one of the few figures whose work survives from the Aristotelian revival of the first century BC—is nicely framed by a quote from Pierre Duhem, contrasting the reception of Aristotle's ideas by medieval philosophers with the 'rebellious' attitude of his immediate successors. Although most of what we know about Xenarchus' views concerns his criticism of Aristotle's doctrine of the fifth substance—the idea that the heavens are composed of a distinct material and form a realm separate from the sublunary spheres—Falcon nonetheless argues that Xenarchus should be regarded as a Peripatetic philosopher. His main reasons are that Xenarchus' notorious criticisms are reached from close study of Aristotle's text and are thus congruent with school practice, and that, on other issues where we know about his ideas, they are less critical.

By focusing attention on what exactly constitutes 'orthodoxy', Falcon challenges a common response to those philosophers who are identified with a school and yet seem not to accept all opinions of its founder. As Falcon notes, the doctrine of the fifth substance was widely critiqued by Aristotelians as

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well as other schools. Falcon's other point is that 'orthodoxy' is scarcely possible at a time when there was no sense of Aristotelianism as a system [21]. He suggests that Aristotelian philosophers of this period felt a need to consolidate their founder's work as systematic in order to compete with Stoicism.

In addressing Xenarchus' most famous departure from Aristotelian views, Falcon stresses that Xenarchus was not drawing on Hellenistic theories of motion or on Stoicizing influences [39–42] but was in fact engaged in a close reading of Aristotle's own text and pointing to inconsistencies [17, 177, 202]. He positions Xenarchus as part of a 'return to Aristotle and Plato' of the late first century BC [17] and draws out the evidence for the beginnings of the commentary tradition in this period, noting that no particular literary form was standard during this period [25].

Falcon contrasts the basis of Xenarchus' divergence with that of Strato of Lampsacus, an earlier scholarch from the third century [21ff]. The latter is indeed an important foil, although I suspect that in contrasting the two, Falcon obscures an important commonality, which is the extent to which both philosophers depart from Aristotle's views on the basis of new discoveries of the Hellenistic period. The mathematical analysis of the cylindrical helix is crucial to Xenarchus' critique of the arguments for the fifth substance, a point that is somewhat underplayed in Falcon's focus on internal tensions. And in stressing the role of textual exegesis in the first century revival, Falcon—who doubts the story that Aristotle's school lacked copies of his books [169]—might have noted that Strato's work shows sufficiently detailed responses for us to doubt this (and also offers an early example of collections of ἀπορίαι).

Falcon seems right to note that the reports of Xenarchus likely place undue emphasis on a particular controversial issue. Falcon brings out the intrinsic interest of Xenarchus' most famous contribution to ethics, which is the attempt to find Aristotelian antecedents for the Stoic notion of the πρῶτον οικεῖον in Aristotle's account of love [42ff]. This influential Stoic doctrine points to a baby animal's innate impulse towards that which is beneficial to it. The attempt of other schools to read this concept back into the work of their school founder illustrates an attitude that is nicely analyzed: Falcon makes a good case that this was not seen as anachronism but as an attempt to read Aristotle correctly. Translating this technical terminology is notoriously difficult and Falcon does it well, although I did not find 'first appropriate thing' [42] or 'prerational desire for ourselves' [156] especially felicitous at conveying the sense to readers unfamiliar with the Greek originals.

These are minor quibbles, however. This book provides much more than a traditional edition and commentary, even while it does the primary task very well. Falcon modifies the traditional format where it does not fit the particular case, such as with distinction between direct quotations and *testimonia*; he supplies the text of Aristotle's work for the reader's convenience where appropriate and divides long reports into manageable chunks with accessible commentary. Some really excellent, quick introductions to difficult topics include the historical notes on Xenarchus' biography [11–12] and discussion of the religious attitude of later commentators [96] or the possible ambiguity in Aristotle's use of «ἐπιπολάζειν» [112]. Some discussions might even have been expanded, such as the controversy over a change of language to rephrase a modal claim as a claim about dispositional properties [118], the controversy over the criteria of simplicity used in classifying the cylindrical helix among mathematical lines [68–70], or the significance of 'assent' in Hellenistic philosophy [151].

The short but excellent essays accompanying the material on Xenarchus provide larger historical background. A succinct introduction to the role of Alexander of Aphrodisias in the transmission plays generous tribute to the fine work of R. W. Sharples, to whom the book is dedicated. Falcon's impressive scholarship shows especially in tracing the reception of Xenarchus' work through the Arabic scholarly tradition and into the Middle Ages and Renaissance. This is an excellent volume by a thoughtful and careful scholar sensitive to philosophical as well as historical issues: it sets a high standard for an accessible yet significant volume on one of the more obscure philosophers of late antiquity. It does its job too well to imagine that it will be superseded in the foreseeable future.

Forgotten Stars: Rediscovering Manilius' Astronomica edited by Steven J. Green and Katharina Volk

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Despite his participation in the Golden Age of Latin literature under Augustus, and despite many passages of surpassing artistry, Manilius and his *Astronomica*, a 4200-line poem describing the heavens and the astrological methods of forecasting, have until recently received little attention in the English-speaking world. Modern scholarly work has generally appeared in German, French, or Italian, and has been crowned by an excellent two-volume Italian edition with translation and extensive commentary on the literary and astrological matters relevant to the text [Feraboli, Flores, and Scarcia 1996–2001]. Unfortunately this edition is somewhat difficult of access in the United States. The Anglosphere's access to Manilius is through G. P. Goold's indispensable Loeb edition [1977], which provides a thoroughly edited text, a fine translation, and a 120-page introduction with a detailed explanation of Manilius' astrology. Now Katharina Volk, the author of two previous books on Manilius, and Steven Green, who has written on Manilius' contemporary Ovid, have edited an outstanding collection of essays on the *Astronomica*, presenting in English the results of recent German (Hübner, Heilen), French (Abry), and Italian (Flores) work, along with essays by English and American scholars. The editors hope—reasonably—that this collection will restore Manilius to his proper place in scholarship, if not in popular appeal.

The book is edited with an eye to the reader's convenience: Manilius' Latin text is quoted extensively but always with an English translation, usually Goold's; footnotes are at the bottom of the page; background information and important comments are in the text where they belong, not relegated to the footnotes. The book's binding and printing are first rate; any typographical errors escaped my notice.

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Katharina Volk's 'Introduction: A Century of Manilian Scholarship' introduces the poet and his poem, focusing on the textual and manuscript history, including the several Renaissance editions and commentaries that appeared after the poem's discovery in 1417. The definitive modern edition, she notes, appeared in A. E. Housman's five volumes [1903–1930]. It should be added that Housman's openly expressed contempt for his author (as well as for all previous students of Manilius) probably did much to discourage English scholarship on Manilius. Volk also discusses the poem's date, political context, and intellectual background, particularly its Stoicism. She raises the interesting question concerning Manilius' decision to write an astrological poem in a political atmosphere hostile to the art, especially after Augustus' decree of AD 11 against astrologers. Volk likewise discusses the poem's didactic genre and its poetics—her book of 2002 is on this topic—and finally the reception of the poem in the Renaissance. Each of the topics mentioned by Volk is also addressed in the other essays collected here.

The essays are divided into five categories arising from the nature of the contributions, not from any requirement derived from Manilius' work itself. This review will describe each essay under its category.

1. Intellectual and Scientific Backdrop

The essays in this section describe Manilius' philosophical background, with particular emphasis on his Stoicism. Citing passages from Cicero and Seneca, Elaine Fantham's 'More Sentiment Than Science' outlines the conventional Roman attitudes to the stars and celestial phenomena: non-scientific, sceptical about the stars' predictive value, and suspicious of astrology in general. These attitudes derive from upper-class scepticism about scientific topics and from political caution, especially after Augustus' decree of AD 11 against astrologers.

Thomas Habinek's 'Manilius' Conflicted Stoicism' discusses the contradictions in the poem's philosophy. Stoic physics emphasized the corporeality of everything; it rejected Platonic 'ideas', Aristotle's contrast of matter and form, and any type of non-physical manifestation. The universe is a single body with rational causation. The four elements interact with each other through the *pneuma*, or life-breath; this *pneuma* explains how celestial bodies influence things on Earth. Manilius' difficulty lies in the traditional astrological doctrine that stars interact through the geometrical figures of trine, square, opposition, and conjunction, all being incorporeal geometric figures, not bodies. Habinek also devotes several paragraphs to a criticism of Goold's translation for being too metaphorical. For example, 'quibus aspirantibus' [5.142] is translated by Goold as 'beneath their influence'. Habinek suggests that this should be 'when they, i.e., stars, breath [on them, i.e., those born when the stars are visible]', treating 'aspirare' as 'to transmit the *pneuma*'. In short, Goold has watered down Manilius' Stoic physics.

Daryn Lehoux's 'Myth and Explanation in Manilius' begins with the conventional contrast of myth *versus* science; the history of science is the history of not-myth. So what is myth? Lehoux uses Manilius as a test case. For Manilius, myth can be a series of poetic tropes, traditional in epic. These are not to be taken seriously because 'Earth creates the cosmos from which it hangs' [2.38]: the mythical figures have no independent existence but are simply representations of the qualities of early creatures. Myth can also be allegory which captures some truth about the universe, for example, the interrelationships among the nature of earthly creatures (bears), its celestial representation (Ursa Major), and those born under the influence of these celestial bodies. Myth enables us to express the meaningful arrangement of this rational universe.

2. Integrity and Consistency

The central essay in this section is Katharina Volk's 'Manilian Self-Contradiction.' Volk cites the following example of a contradiction: in book 3, Manilius describes two ways of calculating the rising times of each zodiacal sign. The first is a fairly sophisticated stepwise method of deriving the rising times for each sign as the day-length changes at the different latitudes from the equator to the North Pole [3.247–482]. So far so good. But immediately thereafter, Manilius presents another totally inaccurate method: assign two hours rising time for each sign throughout the year at every latitude [3.483–509]. Manilius gives both methods equal validity. How does the reader deal with such contradictions? Previous explanations have included ignorance on the part of Manilius, hasty composition, or verses interpolated by a later, incompetent writer. Volk suggests that Manilius' presentation of traditional topics in traditional language gives rise to many of these contradictions. In addition, Manilius desires to orchestrate an effect in each chapter rather than to create a coherent whole and, hence, is less worried about contradictions.

The other essays in this section respond to Volk. John Henderson's 'Watch This Space (Getting Around 1.215–46)', a densely argued postmodernist *tour de force*, argues that in fact many so-called contradictions are no such thing. Earlier commentators had accused Manilius of:

(1) confusing the northern and southern hemispheres of the globe with the eastern and western;
(2) thinking that the Moon is eclipsed differently in different parts of the Earth; and
(3) believing that stars visible in the southern hemisphere (like Canopus) cannot be seen in the northern hemisphere.

By explicating lines 215–246 in detail, Henderson explains away the contradictions. He also describes Manilius' vocabulary relating to space and time, and he shows how Manilius plays with and off the knowledge, language, and poetic devices of astronomical epic. Occasionally Manilius' metaphorical language violates logic because he, like Icarus, reads the universe from above, with a vantage point beyond the terrestrial.

Wolfgang-Rainer Mann's 'On Two Stoic "Paradoxes" in Manilius' discusses two contradictions (paradoxes) that derive from Manilius' Stoicism. The first is 'Every human being has the capacity to understand because of inborn reason' *versus* 'Only an elite can grasp the real nature of the universe through astrology.' The second is that 'The universe is reasonable and wants to be understood' *versus* 'The universe is hidden and needs Manilius to reveal the truth.' Mann shows how these paradoxes are solved in other Stoic authors: reason is indeed an intrinsic potential capability in any rational being but is in fact a hard-won achievement.

In '*Arduum ad astra*: The Poetics and Politics of Horoscopic Failure in Manilius' *Astronomica*', Steven Green, one of the editors of this collection, approaches Manilius as if he, Green, were a student of limited knowledge attempting to learn astrology from the author. He fails but then explains why. Manilius has intentionally written a defective or incomplete account of the science to avoid political problems. Astrology was not in good odor under Augustus, the dedicatee of the *Astronomica*. Hence, Manilius praises the science to the emperor as science but he does not actually present the material necessary to allow a civilian to learn the science. A corollary to this incompleteness is that the contradictions may well be intentional, a method to mislead the unwary. This essay is well argued but the hypothesis seems to this reviewer to be unlikely.

In fact, the four essays in this section, 'Integrity and Consistency', ignore the circumstances under which a literary work (I intentionally avoid the term 'book') was published in antiquity. Frequent references in ancient literature show that authors read sections of their works to patrons,[1] to peers,[2] or to occasionally unwilling guests.[3] Manilius doubtless read free-standing sections of his poem to his audiences over a period of several years. Under such circumstances, perfect consistency between one section and another is not to be expected [see Markus 2000].

3. Metaphors

These essays dissect Manilius' verbal artistry. In 'Tropes and Figures: Manilian Style as a Reflection of Astrological Tradition', Wolfgang Hübner, the contemporary dean of Manilian studies with publications going back to 1975, points out that poetry and astrology are both metaphorical. In astrology, the figures in the heavens are modeled on figures and events in human life and, in return, human life is considered to be governed by these heavenly figures.[4] Leo the celestial lion is derived from the Earthly lion and in turn makes those born under him lion-like in character, resulting in a two-way metaphor. Hübner reviews Manilius' use of figures and tropes—word order, comparisons, verbal antithesis, metonymy, and metaphor in general—and shows how this verbal artistry enriches Manilius' astrological doctrines.

Duncan Kennedy, in perhaps the most original contribution to this collection, 'Sums in Verse or a Mathematical Aesthetic?' addresses Housman's famous comment on Manilius, that he had an 'eminent aptitude for doing sums in verse which is the brightest facet of his genius' [Housman 1903–1930, 2.xiii; this was not a compliment]. After several pages of introductory matter on Housman, Manilius *versus* Lucretius, and the status of astrologers under the empire, Kennedy arrives at his main point: in Manilius 'ratio', usually

1 As Vergil read parts of the *Aeneid* to Augustus and Octavia [Donatus, *Vita Verg.* 32].

2 The popularity at court of Lucan's poetry made Nero jealous [Tacitus, *Ann.* 15.49].

3 In a few invitations, Martial promises not to recite [*Ep.* 5.78.25, 11.52.16], thus humorously encouraging attendance; elsewhere he mentions the recitation of an entire book of epigrams [1.118].

4 Compare line 2.38, p. 168 above.

translated 'reason', more often means 'calculation.' According to Manilius, it is through calculation that human beings can penetrate and understand the heavens. The numbers so slightingly mentioned by Housman are in fact not just sums but an essential way of knowledge which has been hitherto inaccessible and unknown in poetry. Manilius' chief boast is that he is the first to reveal the methods of calculation in verse.

In his 'Census and *commercium*: Two Economic Metaphors', Patrick Glauthier discusses two words from the business world, 'census' ('a census, wealth, resources') and 'commercium' ('commerce, interchange, trade'). The two spheres, the terrestrial globe and the celestial sphere, have *commercium* with each other in two ways: they carry on an exchange through their influences on each other, and the celestial sphere makes its wealth of knowledge available to the terrestrial astrologer. Glauthier analyses other economic words which extend the metaphor: 'censor', 'scrutor' ('investigator'), the verb 'potior' ('to possess'), 'fines' ('territory'), 'pretium' ('value, price'). Astrological knowledge is a valuable commodity and the astrologer plays an important role in the cosmic economic system.

4. Didactic Digressions

Three contributions make up the fourth section. In 'Digressions, Intertextuality, and Ideology in Didactic Poetry: The Case of Manilius', Monica Gale demonstrates that Manilius engages with previous literature in three important digressions: his history of civilization (beginning of book 1), his discussion of comets (end of book 1), and his vignettes of the four seasons (end of book 3). For Manilius, human civilization has progressed under the guidance of a benevolent Stoic divinity. This view contrasts with that of Manilius' chief rival Lucretius (first century BC), for whom this progress is a result of impersonal and mechanical atomic motion, uncontrolled by any external force. It also contrasts with the 'Golden Age' hypothesis of Hesiod and Vergil, according to whom humanity has degenerated from gold to silver to iron or worse. Concerning comets, Manilius believes that they presage disasters which God in pity has sent men as forewarnings. In epic verse, Manilius cites the comets which presage the famous plague at Athens, Varus' defeat in Germany, and the various battles of the Roman civil wars. Again, his doctrine contradicts that of Lucretius, for whom comets simply occur and have no meaning; it is only fear of them that exacerbates previously existing difficulties. Both poets write set pieces on the plague at Athens to illustrate their points [Lucretius, *De rer. nat.* 6.1138–1286; Manilius, *Ast.* 1.884–895]. Finally, Manilius responds to both Lucretius and Hesiod in his description of the four seasons. Rather than focus, like the earlier writers, on agricultural work through the year, Manilius' interests center on Rome's imperial ambitions; he describes the seasons for military campaigns and makes parallels between the divine and the imperial order.

Josèphe-Henriette Abry unhappily died before this book reached print. Her contribution, 'Cosmos and Imperium: Politicized Digressions in Manilius' *Astronomica*', was revised by the editor Steven Green. Abry considers the relationship between three digressions in Manilius and three monuments of Augustan Rome. In his digression on the Milky Way [1.761–804], Manilius' list of brave souls who inhabit this region may reflect the statues erected in the Forum Augustum, which opened shortly before 2 BC and for which there is some archeological and literary evidence. Less certain is the relevance of Manilius' digression on day-length [3.443–482] to the Horologium Augusti, which was perhaps a giant sundial but more likely a solar meridian built to track the changes in day- length and concomitantly the progress of the Sun through the zodiac [see Heslin 2007]. Abry suggests that both the monument and Manilius' digression may reflect Roman intellectual curiosity. Finally, Manilius gives the first complete description in Latin of the οἰκουμένη, the inhabited world [4.585–743]. Abry suggests that this literary description reflects the map commissioned by Agrippa and erected in the Campus Agrippae by Augustus. No fragments of the map survive, only a description by the Elder Pliny, but the left-to-right orientation of Manilius' description of the world, some expressions ('Sardinia looks like a footprint'), and the strange emphasis on wind directions and small islands indicate that Manilius is describing some sort of visible map. Manilius' digressions on these monuments again make parallels between the celestial and political orders, between cosmos and imperium.

In 'A Song from the Universal Chorus: The Perseus and Andromeda Epyllion', James Uden analyses Manilius' version of this story, so popular in ancient drama, with those found in plays by Euripides, Sophocles, Ennius, and Accius, not to mention a treatment by Manilius' contemporary Ovid [*Meta.* 4.663–5.249]. Uden shows how Manilius' small epic [5.538–630] minimizes the amatory, personal elements (grief of parents, Perseus in love, Perseus' conflicts with other suitors), which were the essence of the story for everyone else. Instead, Manilius emphasizes the natural world: the sea is enraged, the birds grieve, the breeze soothes Andromeda in her grief. For Manilius, the characters of the myth are not only people but also constellations; and he emphasizes the Stoic concept of συμπάθεια, the interconnections between the natural world and humanity.

5. Reception

The three essays in this section treat Manilius' later influence. In a brief essay 'Augustus, Manilius, and Claudian', Enrico Flores, one of the editors of Manilius' Italian edition, addresses the date of the *Astronomica*. For centuries the *communis opinio* was that Manilius wrote during the last years of Augustus. Nineteenth-century scholars opted for a date under Tiberius. Housman and Goold were persuaded that the poet began writing under Augustus and finished under Tiberius. By comparing passages from the late Roman poet Claudian's *In Rufinum* (written *ca.* AD 395) and from book 4 of the *Astronomica*, Flores shows that Claudian believed that Manilius wrote under Augustus. We are left to decide for ourselves how decisive Claudian's opinion should be.

The final two essays address Manilius' influences on the writings of Giovanni Pontano and (at greater length) Lorenzo Bonincontri. In her 'Renaissance Receptions of Manilius' Anthropology', Caroline Stark outlines Pontano's *Urania* (1480), an astrological poem in five books modeled on the *Astronomica*. Pontano describes the non-deterministic celestial forces acting on mankind, concluding that all good is from God, all evil is man's doing. Astrology simply supplies valuable knowledge and informs man's choices; it does not replace free will. Stark also introduces us to Bonincontri (*fl.* 1475), who was the first to lecture and write a commentary on the *Astronomica*. He also wrote *De rebus naturalibus et divinis* (1475). In this work Bonincontri's version of the Endymion myth serves to show that astrological knowledge coupled with right choices leads to divine inspiration and enables mankind to ascend to heaven.

Stephan Heilen's 'Lorenzo Bonincontri's Reception of Manilius' Chapter on Comets (Astr. 1.809–926)' is a significant contribution to scholarship in itself, editing some 120 lines on comets from Bonincontri's *De rebus naturalibus et divinis* with detailed commentary. Unfortunately, he does not include an English translation. Heilen begins with a brief account of Bonincontri's life and works, certainly the most accessible summary in English. He then contrasts Manilius' view of comets—their physical nature, their shape, their negative significance [1.809–926]—with Bonincontri's hypothesis in *De rebus* that individual planets create comets, which again always forecast disaster. Bonincontri describes at length the comet of 1456 (a real one; most comets in ancient literature are fictional) and its associations with dreadful events in Naples: the earthquake of 1456, the plague, the death of King Alfonso in 1458, wars, and (more personally) the deaths of Bonincontri's wife and children. Heilen suggests that this traumatic series of events convinced Bonincontri to study the *Astronomica*. In the appendix to his essay, Heilen edits with commentary this section on comets from *De rebus* [1.474–591]. In the body of his essay, Heilen also describes Bonincontri's philological-historical commentary of 1484 on the *Astronomica,* in which Bonincontri made conjectures on Manilius' difficult text, cited literary parallels, and occasionally showed Manilius' relevance for his (Bonincontri's) own time, as in the passage on comets. Bonincontri focuses on the comet of 44 BC, which appeared at the funeral games of Julius Caesar, and the disastrous comet of 1456. Bonincontri considers comets to be warnings sent by God.

The bibliography included in *Forgotten Stars* is the most extensive on Manilius available anywhere, far larger than that included in Feraboli, Flores, and Scarcia 1996–2001. The book concludes with an *index locorum* (ancient authors only; Pontano and Bonincontri might have been included), and a highly selective and inadequate one-page general index.

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Pseudo-Democrito. Scritti alchemici con il commentario di Sinesio. Edizione critica del testo greco, traduzione e commento by Matteo Martelli with a preface by Tiziano Dorandi

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Ce volume contient la réédition et une nouvelle traduction, accompagnées d'une ample introduction et d'un commentaire continu, des extraits alchimiques attribués à Démocrite ainsi que d'un commentaire de ces extraits, attribué à Synésius et adressé à un certain Dioscore, prêtre du temple de Sérapis d'Alexandrie. Jusqu'à présent, ces textes étaient disponibles uniquement dans l'ancienne collection pionnière de Berthelot et Ruelle, publiée à Paris entre 1887 et 1888 [= CAAG]. Cette collection, qui a le grand mérite d'exister et d'avoir rendus disponibles les textes des alchimistes grecs, demande d'être mise à jour. Or, l'ouvrage de Martelli se situe justement dans le mouvement de reprise de l'édition et traduction complètes des alchimistes grecs, inauguré à Paris en 1981, par la Collection des Alchimistes Grecs des Belles Lettres.[1]

Les textes alchimiques du pseudo-Démocrite constituent l'une des couches les plus anciennes de l'alchimie gréco-alexandrine (I^er^–III^e^ s. apr. J.-C.), celles des recettes, qui portent essentiellement sur l'imitation de l'or, de l'argent, des pierres précieuses et de la pourpre. Il s'agit de textes encore très techniques mais on y trouve un effort systématique et déjà l'idée d'une unité fondamentale de la matière et celle des rapports de sympathie entre les substances,

[1] Les volumes parus jusqu'à présent sont :

1. *Papyrus de Leyde, Papyrus de Stockholm, Fragments de recettes*, par R. Halleux (1981)
4. *Zosime de Panopolis, Mémoires authentiques*, par M. Mertens (1995)
10. *Anonyme de Zuretti*, par A. Colinet (2000)
11. *Recettes alchimiques (Par. Gr. 2419 ; Holkhamicus 109) Cosmas le Hiéromoine—Chrysopée*, par A. Colinet (2010).

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notions qui seront à la base de la notion, plus tardive, de transmutation. Synésius (IVe s.) appartient à l'époque des commentateurs. Il commente les textes du ps.-Démocrite en fournissant des témoignages précieux surtout sur l'évolution des techniques et des instruments.

Le volume se divise en trois parties. La première, consacrée à la tradition manuscrite byzantine et syriaque, à l'identification de prétendus auteurs et aux contenus, constitue une ample introduction aux traités réédités. La deuxième partie contient la nouvelle édition et la traduction italienne des textes suivants :

- la version abrégée des quatre livres pseudo-démocritéens sur les teintures dont les manuscrits conservent deux extraits, intitulés respectivement :
  - « Φυσικὰ καὶ μυστικά » (« Questions naturelles et secrètes ») [Berthelot et Ruelle 1887–1888 2. 41–49] et
  - « Περὶ ἀσήμου ποιήσεως » (« Sur la fabrication de l'argent ») [2. 49–53] ;
- certaines sections des *Katalogoi* pseudo–démocritéens contenus dans une collection de recettes, éditée par Berthelot-Ruelle sous le nom de « Chimie de Moïse » [2.306–307] ;
- le commentaire de Synésius de l'œuvre alchimique pseudo-démocritéenne, intitulé « Du philosophe Synésius à Dioscore, annotations sur le livre de Démocrite » [2.56–59] ainsi qu'
- un extrait anonyme, intitulé « Sur le blanchissement » [2.211].

Enfin, la troisième partie est constituée par un long commentaire, sous forme de notes, consacré à l'explication des choix textuels et à une interprétation technique très approfondie des recettes et des procédés.

Le volume est complété par une bibliographie exhaustive et par quatre index : des substances, des auteurs anciens et modernes, des passages cités et des illustrations.

L' édition de Martelli se distingue de l'ancienne édition Berthelot-Ruelle par l'introduction de trois éléments nouveaux dans les apparats critiques. Tout d'abord, la reproduction des signes alchimiques, employés pour remplacer les noms des substances, tels qu'il apparaissent dans les manuscrits ; ensuite, la mention des variantes syriaques, souvent suivies de leur traduction latine ; enfin, l'importance accordée à la tradition indirecte dans les apparats qui accompagnent la traduction italienne. Ces trois éléments enrichissent considérablement l'horizon de cette discipline et fournissent les instruments pour une véritable approche scientifique de ces textes. En effet, cet ouvrage, offrant à son lecteur une remarquable quantité d'informations sur les phases les plus antiques de l'alchimie gréco-égyptienne, constitue une contribution importante pour la compréhension de la constitution de l'alchimie comme discipline autonome et dotée de ses propres règles.

À ce propos, je voudrais souligner trois aspects qui émergent de ce travail et qui me semblent particulièrement intéressants pour la caractérisation du savoir alchimique. Le premier consiste dans la « fluidité » de la tradition manuscrite. Comme l'a bien souligné Tiziano Dorandi dans l'introduction, la tradition des textes alchimiques grecs est « fluide », à savoir, toujours ouverte à additions, retouches, précisions, réécritures et mises à jours. En effet, comme d'autres textes scientifiques pratiques, ces écrits étaient considérés comme des textes d'usage, comme des instruments à adapter aux découvertes les plus récentes et aux expériences menées par leurs auteurs. Par ailleurs, Martelli avance l'hypothèse de l'existence de différentes anthologies de textes alchimiques qui circulaient déjà à l'époque byzantine et qui constitueraient les sources des manuscrits principaux et la raison de leur nature composite ainsi que des différences de présentation et d'élaboration du même matériel. Or, cette situation demande une révision et une adaptation des critères habituels de la philologie car on a à faire avec une littérature *sui generis* dont les contenus évoluent dans le temps. En effet il ne s'agit pas de reconstituer un texte unitaire dans sa cohérence originelle à travers la transmission manuscrite, comme cela pourrait se passer pour un traité d'Aristote ou un dialogue de Platon, mais de comprendre les raisons des choix, des présentations et des taxinomies adoptées dans les différents témoins, ce qui témoigne justement de la constitution en devenir du savoir alchimique. C'est pourquoi, le choix de Martelli de fournir un apparat critique « large », qui se fonde sur les manuscrits principaux, la tradition indirecte des *testimonia* et sur des passages parallèles dans le corpus alchimique, ainsi que sur les versions syriaques, est fondamental. Or, ces deux caractéristiques de la tradition manuscrite des textes alchimiques, la « fluidité » et le caractère anthologique, dans un certain sens, semblent, paradoxalement, réduire l'importance de la question des rapports et de la dépendance réciproque des manuscrits car chaque témoin a sa propre valeur scientifique et son histoire de même que chaque traité ou groupe de traités.

Le deuxième aspect concerne la pseudépigraphie alchimique et ses raisons. Martelli démolit aussi bien l'identification précédemment soutenue, entre pseudo-Démocrite et l'Égyptien Bolos de Mendès qu'entre Synésius et son homonyme, le philosophe néoplatonicien et évêque de Cyrène. Or, la question des « pseudos » dans les textes alchimiques est fondamentale : le fait de comprendre les raisons de l'attribution d'un traité à un auteur connu est le premier pas à faire. Or, en ce qui concerne Démocrite, la critique s'accorde sur le fait qu'il s'agit d'un Pseudo. Mais, si ce n'est pas Bolos, pourquoi cette identification à Démocrite d'Abdère ? Martelli rapporte cela à la production de textes pseudépigraphiques liés à la renommée du philosophe Démocrite à l'âge hellénistique comme τεχνίτης, expert en différentes techniques comme la coloration des métaux, qui aurait eu une approche méthodologique et systématique de sciences de la nature. Cette explication laisse pourtant ouverte la question de savoir pourquoi cette tradition ne fait aucune mention de l'atomisme, qui est la caractéristique principale de la philosophie de Démocrite le φυσικός. S'agit-il d'une autre voie doxographique, une doxographie « technique » qui ne tient pas compte des composantes ultimes de la matière ? L'historien de la pensée ne pourra s'empêcher de se poser ces questions.

Le troisième aspect concerne les taxinomies de Démocrite que l'on peut considérer comme des ébauches de théorisation. Martelli souligne justement que dans l'histoire de la définition et de la théorisation de l'art alchimique, Démocrite représente la première expression d'une alchimie « mûre », à savoir une alchimie qui accompagnait les procédés décrits d'une doctrine. En effet, après les textes purement techniques des papyrus de Leyde et de Stockholm, on peut dire que le pseudo-Démocrite constitue le degré zéro de la théorisation de la τέχνη alchimique. Ce qui expliquerait pourquoi les auteurs successifs le considèrent comme l'un des fondateurs de ce savoir.

Or, l'attitude systématique du pseudo-Démocrite se manifeste surtout dans un effort taxinomique visant à classer les ingrédients et les opérations selon des principes généraux et surtout dans la réduction à un petit nombre de principes. En effet, il oppose souvent la pluralité de la matière confuse à une nature capable d'obtenir le même résultat que plusieurs espèces. Le principe théorique fondamental est exprimé par la « petite » formule révélée :

> La nature se réjouit (τέρπεται) de la nature, la nature vainc (νικᾷ) la nature, la nature domine (κρατεῖ) la nature.

Cette formule, malgré son allure initiatique et métaphorique, contient, à l'exception peut être de « τέρπεται » (qui est le contraire de « λύπειν ») des termes fréquents dans la physique ancienne. Les termes de combat « νικᾶν » et « κρατεῖν » sont utilisés souvent par Platon et Aristote pour décrire les transformations et les actions réciproques des corps naturels. Dans le *Timée* de Platon, les transformations réciproques des éléments feu, air et eau par agrégation et séparation des triangles de base, sont décrites en termes de combats, défaites, victoires, conquêtes, et donc par des verbes comme « μάχομαι », « νικᾶν », « κρατεῖν » [cf. 56d-e, par ex.]. Chez Aristote, on trouve souvent « κρατεῖν » pour exprimer l'action d'une qualité qui domine, comme la chaleur. Par exemple, en *Meteor*. 3.3.358a12, Aristote dit que quand la chaleur ne domine pas, dans les organismes se produit du résidu alors que dans les corps qui brûlent, des cendres. Quant aux terme « φύσις », accompagné d'un adjectif, on le trouve souvent chez Aristote, dans le *De generatione et corruptione*, pour déterminer la qualité d'un corps ou d'un élément.

Dans le pseudo-Démocrite, les φύσεις semblent indiquer à la fois les ingrédients et leurs propriétés tinctoriales. La formule semble présenter les trois possibles actions réciproques entre les propriétés des corps. Mais de quelles actions s'agit-il ? Du moment que chaque recette se termine avec une seule de trois actions de la formule, est-il possible de comprendre sa fonction à la lumière de la recette ? En d'autres termes : peut-on établir un lien intelligible entre la recette et la formule qui lui est associée ? Encore une fois, ce sont des questions que le lecteur se posera en lisant ces textes.

On reste impressionné aussi bien par la précision chirurgicale de cette édition que par l' extrême prudence de Martelli à l'égard des problèmes les plus difficiles de ces textes, à savoir les rapports entre les manuscrits et l'identification historique de leur auteurs. Ces problèmes, tout en étant développés en profondeur, restent ouverts. De fait, le but de ce travail n'est pas de trancher les questions mais celui, plus généreux, de préparer le terrain à des recherches ultérieures et, peut-être, ouvrir la voie vers des solutions, en offrant au public savant un grand nombre de données triées, élaborées, éditées et traduites, bref un matériel bien établi sur lequel réfléchir. C'est pourquoi cet ouvrage représentera dorénavant un instrument de travail précieux et incontournable non seulement pour les études sur pseudo-Démocrite et Synésius, mais aussi sur toute l'alchimie grecque dans son ensemble.

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Jonathan Barnes et al., *Eleatica 2008. Zenone e l'infinito* edited by Livio Rossetti and Massimo Pulpito

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The book arises from the Eleatica lectures of 2008 delivered in Italian by Professor Barnes at the Fondazione Alario per Elea-Velia in Ascea (province of Salerno) on the topic 'Zeno and Infinity'. It includes the Italian text of the lectures (the English original was translated by M. Pulpito), together with the responses submitted by eight scholars, the rejoinder (in English) to each respondent by Barnes, and an introduction by the editors. The introduction is preceded by a shorter (anonymous) overview in English, which in part overlaps the introduction. The latter includes a short survey of modern and contemporary studies on Zeno (starting with Renouvier in about 1860), which would be more useful if less selective (for instance, the contributions by Gregory Vlastos are ignored) and if, in mentioning controversies, gave more information as to what the controversies were about. At the end of the introduction we are told who is Jonathan Barnes, including the information that (after the lectures) he obtained the eagerly desired (*ambita*) distinction of being elected an honorary citizen of Elea-Velia, thus becoming a fellow-citizen of the Zeno who was the object of his lessons. This is apparently taken very seriously, for Livio Rossetti also mentions in his curriculum the same distinction as one of the most important facts. Evidently, there is the conviction at work that the committee (presumably of citizens of Ascea, a little town close to the ruins) which elects these honorary citizens are worthy direct descendants of the citizens of ancient Elea. Concerning the ancient town, Barnes himself remarks that such a little town gave a greater contribution to philosophy than the big metropolis of Rome.

In his lectures, Barnes concentrates on Zeno's fragment B1 [Diels and Kranz 1951, ch. 29] of which I reproduce the translation given in the overview:

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> But if they [things generally] exist, it is necessary that each has a certain size and thickness, and that the one bit of it is distant from the other. And the same remark goes for the projecting bit, for it too will have a size, and a bit of it will project. Now it is all the same to say that once and to say it forever; for no bit of it is last in such a way that there will not be one bit in front of another. Thus if several things exist, it is necessary that they be both small and large—so small as to have no size, so large as to be infinite.

In fact, Barnes declaredly concentrates not on the whole apparently contradictory conclusion but on its second part, that involving infinite greatness. (This explains the title of the lectures, which would not be equally applicable if also the first part were considered.) As he tells us, assuming Proclus' testimony that Zeno produced 40 arguments, he is concerned with one 80th of the philosopher's production. Thus, though he occasionally makes reference to some other Zenonian arguments, he is not concerned with offering 'a full and rounded account of Zeno' [186]. Further, he declares that he

> decided that the lectures would tackle some philosophical questions and not touch—save incidentally—on the philological and the historical [185],

without intending to suggest by this that these other two sorts of question are not important. What he maintains is that, at least in the case of Zeno, a philosophical analysis can be conducted without having to presuppose that his paradoxes 'can only be grasped after some historico-philological effort' [186].

In his exposition, in the first chapter (entitled 'Zenone paradossologo'), Barnes does say something about Zeno in general. He dismisses as mere fiction not only the story of Zeno's visit to Athens together with Parmenides but also the presentation of his position in the first part of Plato's *Parmenides*, where it is suggested that he elaborated his arguments against 'the many things' with the intent of defending Parmenides' monism. Against this the suggestion is given that Zeno was only interested in elaborating paradoxes in the modern sense of the word, i.e., arguments that are seemingly flawless in logic and yet reach absurd conclusions. Thus, Barnes is induced to define the Eleatic thinker as 'a philosopher without philosophy' inasmuch as the conclusions reached by means of paradoxes give rise to no point of doctrine.

In chapter 2, Barnes proceeds (I rely on the overview) to point out that the notion of infinity is neither difficult nor technical, and certainly not incoherent or contradictory; and, therefore, that it is not in infinity *per se* that we should expect to locate the primary source of paradoxality. Not difficult, because the fact that it is hard to picture an infinite magnitude does not mean that the concept itself is hard to grasp. Not technical, because, although there are some technical notions in mathematics involving the infinite, these technicalities do not apply to the ordinary concept of the infinite, which is the only one that is at play in the paradoxes of Zeno (and which alone is germane to their solution). Not intrinsically paradoxical, because the undeniable existence of the paradoxes of infinity does not mean that to think of infinity necessarily involves us in contradictions. In the main, to use that concept is just to recognize that we have to do with sequences (e.g., the sequence of cardinal numbers) that can be prolonged without ever having to stop before a limit.

In chapter 3, Barnes examines the Zenonian argument quoted above, which, in the form it has come down to us, clearly leaves out some steps. He affirms that he agrees with the classical reconstruction of the argument, according to which every body, inasmuch as it has a certain size, is potentially divisible into an infinite number of bits, each of these bits in turn having a certain size. As the initial size of the body at issue is the sum of the sizes of its bits, and these bits are infinite in number, then the whole body will have infinite size. Barnes calls this argument the Dichotomy, since it is assumed that the bits in question are a sequence of halves starting with the first half into which the original magnitude is divided.

In the sequel Barnes points out that Zeno's argument requires some additional premises, concerning first of all whether the bits of a magnitude are such as to be both exhaustive and exclusive. Leaving out some details, however interesting, attention should be given to the crucial assumption made by Zeno, namely, that the sum of an infinite number of quantities is infinite. This assumption appeared plausible to various Greek philosophers after Zeno (as an example Barnes quotes Epicurus, *Epistula ad Herodotum* §57). However, this appearance of plausibility is not sufficient. What emerges in this analysis is that the Dichotomy is exposed to a well-known mathematical objection. In the case of the so-called 'convergent series', i.e., series whose elements converge to a finite number, it is not true that the sum of an infinite number of magnitudes is equal to an infinite magnitude. This is precisely the case of the sequence that is involved in Zeno's argument: the successive addition of the elements of the sequence does yield a convergent series.

Barnes concedes that the mathematics with which Zeno is likely to have been acquainted was not so advanced. He also points out that there is another way of understanding the Dichotomy paradox. (This interpretation was advanced by W. E. Abraham [1972], though this is not pointed out either by Barnes or by Rossetti and Pulpito in their introduction. Notice that the text of fr. 1 does not specify the way in which the partition is made.) This is suggested by the version of the paradox which is given by Porphyry: what is contemplated is not a succession of halves of ever decreasing magnitudes but rather that *all* halves are divided into their sub-halves, creating a top-down hierarchy of increasingly dense partitions. In this case, at all levels the series is divergent, not convergent. And yet, Barnes argues, these partitions will continue to produce a number of bits, whose sum is equal to a finite number, for they will always be identical to the size of the original magnitude, whatever the number of bits for any one partition may be.

With the exclusion of this alternative, we come back to the objection to the validity of the argument from a mathematical point of view. It is remarked that the argument is not properly refuted by adding the elements of a convergent series, for this operation cannot be completed: we only have an approximation to a finite number. But this consideration does not, of course, show that Zeno is right. Any reply, including his, that is given to the question 'What is the sum of a series such as $1/2 + 1/4 + 1/8 + 1/16$... equal to?' cannot be correct. If we change the example and ask what the sum is of an infinite sequence of units, it is the same as asking what the sum is of many units; and this makes as little sense as asking how long is a piece of rope. If an arithmetician does give a reply to the first question, it is because he makes certain stipulations which are convenient for doing mathematics.

In what follows, I will not make any mention of the responses, with a partial exception for two of them, for (as it often happens in these meetings) they are more statements of their authors' interpretation of Zeno's arguments than an attempt to come to grips with Barnes' suggestions. On the latter, I make the following comments. The first is that admitting that Zeno was elaborating paradoxes does not oblige us to regard Plato's testimony as wholly ungrounded. Zeno's arguments are paradoxical (as Barnes seems to concede) also in the sense that they go against common assumptions about the existence of a plurality of bodily entities and their movement and this can be seen as a way of supporting Eleatic monism.[1] From Plato's testimony itself one gets the impression that Zeno's intentions in his writing were not explicit. Thus, one can only speculate about them and it is quite possible that he had more than one intention, for it is rare that a thinker be wholly single-minded. Zeno's taste for paradoxes certainly led him beyond what would have been needed for a wholly serious defense of an Eleatic point of view. One cannot suppose that he was really convinced, for instance, that Achilles would never overcome the tortoise. He belongs to a period (as rightly stressed by Rossetti in his comment) in which a number of thinkers showed themselves more interested in making intellectual experiments than in putting forward views to be accepted as true.[2] One can detect some affinities with Gorgias, who, in addition to claiming paradoxically that Helen was innocent because she could not resist the seduction exercised on her by Paris (by means of enticing words and so on), offered a demonstration that nothing exists which, in his case, is a reversal of Eleaticism rather than its defense but still has a relation to it.

The second comment is that Barnes' suggestion that what is at issue for Zeno is an ordinary concept of the infinite that presents no difficulties seems to make it too a-problematic. This can appear to be so because attention is given in an exclusive way to the process, exemplified by the intellectual division of continuous magnitudes (and by the opposite operation of their summation), of traversing a sequence of magnitudes which has no end.[3] This case was paradigmatic for Aristotle in propounding his conception of the infinite as only potential. But Aristotle himself, when dealing with time (which for him is eternal, as the world is eternal), had to admit, rather paradoxically, that events which recur forever, like a day, are both potential and actual [cf. *Phys.* 3.6.206b12 ff.]. A further and proper paradox is stated in Kant's *Critique of Pure Reason*, in the thesis of the first antinomy of pure reason: the world cannot be eternal (as conceived, e.g., by Aristotle) since,

1 There is the complication that Barnes is the author of a paper in which he maintains that monism is an invention of Melissus, not of Parmenides. This would be too long to discuss: I can only say that I am not convinced. Further, in his *The Presocratic Philosophers* [1982], he questions the prevailing view that Zeno's arguments are *reductiones ad absurdum*; but this not unimportant point is left out in the present lectures.

2 For this idea and for a survey, see Solmsen 1975.

3 Cf. 49: 'Qualcosa è infinito se non ha limiti, confini, frontiere; se continua sempre, non si ferma mai, non giunge ad una sosta.'

to reach the present, there should have passed a series of moments which cannot be completed. The same point however is not so paradoxical when applied to space, for Lucretius could claim that the extension of the universe is such that a thunderbolt could not only cross it, even if its motion were everlasting, but not even make smaller the extension of space that there is to traverse [cf. *De rer. nat.* 1.1002 ff.]. The example serves to show that the space of the universe is not just immense: it is truly and positively infinite. Concerning time, he argued (at the end of book 3) that, since the condition of death to which we are destined is everlasting, it does not matter how long we live, for not one bit can be subtracted from that everlastingness. Clearly all these considerations concern an infinite which is regarded as actual and not as merely potential (thus implicitly rejecting Aristotle's approach). It is possible that this intuition, at least in its application to space, goes back to the first atomists, who were more or less contemporary of Zeno.

Does all this make a difference to our understanding of Zeno's argument? It does, for it was remarked by some scholars [see, e.g., Vlastos 1967, 372] that Zeno appears to be assuming that the division is completed and, thus, that an (actually) infinite number of bits is obtained. It was also remarked that Zeno, by making this assumption, is not consistent, because he clearly assumes in some of his arguments (like Achilles and the tortoise) that the series cannot be traversed because there always remains some extension (before Achilles), and because in the very argument under discussion he states:

> Now it is all the same to say that once and to say it forever; for no bit of it is last in such a way that there will not be one bit in front of another.

It can be added that the first half of another Zenonian argument against plurality goes as follows:

> if there are many, it is necessary that they be as many as they are, neither more nor fewer. But if they are as many as they are, they must be finite[ly many].

Here it is manifestly assumed that any number that is given to existing things is a finite one. However, these inconsistencies tend to show, in my view, that Zeno was ready to use any means he had at hand to reach his paradoxical conclusions, confirming that he was not a serious thinker like Parmenides (who presumed to be inspired by a goddess). He was not quite a philosopher either, since he does not show that he reflected on the concept of infinity of which he made use.

Barnes in his discussion tacitly excludes this interpretation of the argument but not on grounds of consistency. The reason for his approach becomes evident in his reply to the observations by Pulpito. This scholar remarked that Zeno could defend his position at least in the Porphyrian version of the argument by admitting that the infinitieth partition can be reached, which, he also remarks, is what the Eleatic seems to be assuming. In a note, he raises the question: 'Is there such a thing as the infinitieth partition?' [cf. 167–168 and n13]. Barnes in his reply draws attention to this note and makes the following comment:

> The phrase 'the infinitieth partition' has no sense.... The sequence of partitions is infinite: each element in the sequence has a succeeding element, and each element is of course the nth element in the sequence (for some natural number n). The expression 'the infinitieth element' is nonsense. The sequence of natural numbers is infinite: each number has a successor, and every number is the nth number (for some finite natural number n). The expression 'the infinitieth number' is nonsense. The adjective 'infinitieth' is nonsense. [204]

Repetition does not yield persuasion. Barnes is committed to an Aristotelian view of the infinite and is confident that a sequence of numbers and of other quantifiables cannot ever be completed. I do not think that in matters of infinity one can be so confident of this (or of the opposite). One need not assume (and Zeno does not appear to have assumed in this case) that the sequence of divisions be intellectually traversed step by step, instead of imagining that it is completed, thus obtaining an (actually) infinite number of parts or bits from the given magnitude. Further, Barnes manifestly has in mind some definition of natural number according to which each number has a successor. But he himself remarks, in another connexion, that the results we obtain depend on the conventions we adopt. So why not modify that definition in the sense that each number, except the infinitieth one, has a successor?

Even if Barnes were right in thinking that from the point of view of modern mathematics and logic the infinitieth number is nonsense, he seems to concede that some ancient thinkers, not influenced by Aristotle, thought otherwise. In fact, it can be remarked that Plato treated as nonsense the proposition that the worlds are infinitely many [*Tim.* 55c–d] but that the atomists who put it forward clearly did not think they were talking nonsense.

This leads to the question of what the task of the scholar should be. Barnes insists that he is interested in the philosophical dimension of Zeno's paradoxes.

Now, it is, of course, quite possible to discuss the paradoxes even without knowing who Zeno was. (Barnes himself is persuaded that Zeno was a *paradoxologus* but Barnes could have added that it does not matter whether the Eleatic really was one or not, since he is considering his arguments in any case as paradoxes.) Bertrand Russell (in his *Principles of Mathematics*) and other modern thinkers have tended to discuss the paradoxes in this way. But a historian of philosophy cannot do the same. Before the misgivings of some of his interlocutors, of which he shows awareness, Barnes does not say in a definite manner whether he wants to proceed as Bertrand Russell or as a historian of philosophy [186]. I would not say (as he apparently wants to) that a historian of philosophy does not confront philosophical questions. The contributions that history and philology have to make are instrumental to the aim of understanding the thought of the ancient philosopher and are not all that distinguishes the historian of philosophy and the pure philosopher. Indeed, if one admits—as Barnes has admitted elsewhere [see 2000, 2007]—that the historian of philosophy aims at that understanding, it must be a philosophical understanding. Moreover, the question of truth or whether the thinker examined is right or wrong, while it especially concerns the philosopher, must indeed not be wholly ignored by the historian but kept in suspension. The difference between the two emerges with sufficient clarity just in the case of the notion of the infinite. If one says that certain assertions about the infinite are nonsense, one is proceeding as Bertrand Russell does, claiming that so and so is true (independently of the question whether Russell would have agreed with Barnes). If one does not start from a preconceived view on the matter and admits that certain assertions about the infinite did make sense to certain ancient thinkers and tries to understand how and why, one is proceeding as a historian of philosophy. As this review shows, I consider myself a historian of philosophy. However Barnes considers himself, it remains that his treatment of Zeno is very instructive, if perhaps a little too longwinded: bringing to light all the tacit assumptions in Zeno's arguments is important but it goes too far when the obvious is labored.

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Salamanca y la medida del tiempo by Ana María Carabias Torres

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Among the numerous names associated with the construction of our present civil calendar, the most noteworthy is perhaps that of Luigi Lilio (or Giglio, 1510–1574/76), a native of Cirò in Calabria. Earning his living as a lecturer in medicine at the University of Perugia, Lilio spent many years working out a plan of how to bring the date of Easter back in line with the astronomical phenomena (vernal equinox and Full Moon) from which it had strayed due to the defects of the ecclesiastical calendar. Aided by the successful lobbying of his brother Antonio, his ideas were posthumously adopted by an expert commission which had been set up by Pope Gregory XIII (1572–1585) for the purpose of reforming this calendar.[1] During the preparation of this reform, one of the commission's members, the theologian and mathematician Pedro Chacón, summarized Lilio's proposal in a *Compendium novae rationis restituendi kalendarium* (1577), which was subsequently sent to rulers and universities throughout Europe in hope of approval and further suggestions. In Chacón's native Spain, such requests reached the universities in Salamanca and Alcalá de Henares as well as the private address of the famed clockmaker Juanelo Turriano.[2] The response from Salamanca, dated to 1578, is still preserved in three manuscript copies. As one would expect, one of these ended up in the Vatican Library (lat. 7049), while another one is kept at the University of Salamanca's Biblioteca General Histórica (ms. 97). Besides expressing agreement with the Lilian proposal, the report in these two manuscripts also attaches an earlier and much longer text, otherwise lost, which the University of Salamanca drew up in 1515. In a situation analogous

[1] See most recently Steinmetz 2011.

[2] On the latter, see Fernández Collado 1989 and García-Diego and Gonzáles Aboin 1990.

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to the report of 1578, the text of 1515 was written in response to Pope Leo X and Ferdinand II, the king of Spain, who had solicited an expert assessment on a reform proposal that had been produced in the context of the Fifth Lateran Council (1512–1517). No definitive result was achieved at the time but texts such as the Salmantinian report of 1515 still testify to the amount of scientific and literary activity that was sparked by the papal request. The best known case of an astronomer being inspired by the Council's 'call for papers' is doubtlessly Copernicus, who mentions the reform effort in the preface to his *De revolutionibus orbium coelestium* [1543, fol. 4v].

While the immediate steps that lead to the Gregorian reform of 1582, which adopted Lilio's plan with minor modifications, are relatively well known, the wider history (and pre-history) of this reform remains a gigantic map with many blank spaces, which also cover most of the Iberian peninsula. The aforementioned reports from Salamanca are a case in point. Ferdinand Kaltenbrunner, whose studies on the history of calendar reform (published 1876–1880) remain foundational, already knew of the Vatican manuscript but was not allowed to see it due to its badly damaged binding [1880, 34]. His contemporary Joseph Schmid [1882, 394–396] had more luck and was able to summarize the contents of the report of 1578. By contrast, Demetrio Marzi [1896] passed over both documents in silence in his still-indispensable account of the reform proposals made in the wake of the Fifth Lateran Council. Back in Spain, the mathematician and science educator Acisclo Fernández Vallín [1893, 220–222] felt induced to include a transcription of the report of 1578 in his *Cultura científica de España en el siglo XVI* but with unsatisfactory results.

In a new monograph, *Salamanca y la medida del tiempo*, Ana María Carabias Torres, professor of modern history at the University of Salamanca, sets out to rectify this situation of relative neglect by offering a proper edition of the two reports (based on the Vatican and Salamanca manuscripts), augmented by a comprehensive study of their history, authorship, and institutional contexts, as well as an assessment of their role in the development of the Gregorian calendar. Her edition [260–318], which also includes a Castilian translation, is supplemented by a facsimile reproduction of the aforementioned ms. 97 from Salamanca. Readers interested in the original Latin will greatly appreciate this addition, for although Carabias Torres is quite outspoken about the obvious flaws of Fernández Vallín's previous edition [38], faulty transcriptions and other typographical errors abound in her own rendering of the text.

In contextualizing the document, the author decided to cast her net widely. Her generous introduction ('El problema del tiempo en la historia y en la historiografía') reviews past work on the history of calendar reform and various related subjects, whilst lamenting that historians of Iberian science have failed to appreciate the importance of time-reckoning for their field [21–60]. Another 70 pages [60–133] are spent on the historical circumstances of the reports of 1515 and 1578, with a particular focus on the study of astronomy at the University of Salamanca and its elevated status during the decades before and after 1500 as illustrated by the towering figure of Abraham Zacuto. Fascinating as this material may be, it is not always clear how the many names, books, and ideas that Carabias Torres mentions relate to the reform of the calendar, which was an undertaking focussed on a relatively narrow sector of astronomy concerned with no more than the length of the solar year and the calculation of the lunar phases.

That said, both these and other sections are worth reading for their copious references to literature relevant to the history of science on the Iberian peninsula, some of it rarely accessed by scholars outside the Hispanosphere. For the history of calendar reform, Carabias Torres manages to cite a number of little-known early modern books by Spanish authors such as Pedro Ciruelo [157]. She also discusses new manuscript material, including a report on the calendar by the University of Alcalá [221] and two explications of the Gregorian reform [230–234] written respectively by the Toledan archbishop García de Loaysa y Girón (1534–1599) and the Salmantinian music theorist Francisco Salinas (1513–1590). Among the sources that she has missed is a *Disputatio de anno in quo possimus dicere dominum fuisse passum et de quibusdam erratis in kalendario*, composed in 1468 by Pedro Martínez de Osma, professor of theology at Salamanca, whose ideas might have shed additional light on the calendrical texts produced at the same university during the 16th century.[3] One case in point is Martínez de Osma's interest in the Jewish calendar, also evident in the report of 1515, to which was appended a set of explanatory canons. These outlined the principles of Jewish lunisolar

[3] See now Nothaft 2013 and the edition by Labajos Alonso [2010, 354–383], with a Castilian translation by Pablo García Castillo.

reckoning and showed how to convert the resulting dates into the Christian calendar, based on a set of tables that has not been preserved [309–315].

Unfortunately, this exotic annex is only one of several salient aspects that receive little attention in Carabias Torres' analysis of the assessments of 1515 and 1578. Most of her coverage of these texts [133–237] is indeed not so much concerned with their technical content as with their historical and institutional background as well as with the biographies of the scholars involved. In the case of the report of 1578, Carabias Torres argues that its astronomical and mathematical substance was mostly contributed by Miguel Francés, an Arts master originally from Zaragoza whose other collaborators included the famous poet and theologian Luis de León [194–217, 253–254]. The identity of the members of the commission set up in 1515 is much more difficult to establish and must remain a matter of speculation, although two professors of natural philosophy, Juan de Oria and Juan de Ortega (different from the mathematician of the same name), are among the more likely candidates [158–169]. In outlining their arguments, Carabias Torres' main concern is to show that the Salmantinian experts produced an absolutely exceptional document in the history of attempts to reform the calendar. As she proudly writes [235], her university

> *inventó en 1515 un procedimiento matemático que permitía enlazar en un cómputo convergente el distinto ritmo del Sol y de la Luna; y...lo hizo de forma tan exitosa como para haber sido este procedimiento el que finalmente ratificaron los expertos vaticanos y el propio pontífice como base de la reforma gregoriana del calendario.*
>
> invented in 1515 a mathematical procedure that permitted to fit together the distinct rhythm of the Sun and the Moon in a convergent calculation; and...it did this in a successful enough manner for this procedure to become the one that the Vatican experts and the pope himself ultimately approved as the basis for the Gregorian reform of the calendar.

thus demonstrating

> *la excepcionalidad de los conocimientos matemáticos y astronómicos existentes en el seno de la Universidad de Salamanca en torno al año 1515.* [236]
>
> the exceptionality of the mathematical and astronomical knowledge that existed within the University of Salamanca around the year 1515.

In fact, she sees so many similarities between the proposal of 1515 and the final version of the Gregorian reform as to necessitate a re-evaluation of Luigi Lilio's historical role. Far from being the 'father' of the present calendar, Carabias Torres claims that the man from Calabria copied most of his ideas from his Salmantinian predecessors. Aside from the perceived affinities between both reform proposals, the evidence that she adduces for this claim is flimsy at best. Luigi Lilio's brother Antonio, she points out, was a member of Pope Gregory's reform commission and would thus have had access to the report of 1515 in the papal archives. If the original manuscript is no longer extant in the Vatican Library, this may be explicable by Lilio's use of said report, which he may have kept among his records at the time of his death, leading to its displacement [218, 236].

I shall leave the problematic chronology of this hypothesis—it is quite likely that Luigi Lilio's reform plan predates the institution of the papal calendar commission or Antonio's admittance to the same[4]—on one side and instead focus on the question whether the suggestions that were sent by the University of Salamanca to Rome in 1515 really prefigure the later calendar reform in a way that would justify the author's revisionist account. A summary of these suggestions is slightly impeded by the fact that the Salmantinian experts discuss a number of parallel scenarios in a somewhat non-committal and disorganized manner, trusting that the report's addressees would be able to pick out the ideas they liked best. One major problem to be faced was the receding vernal equinox, which, due to the over-estimation of the length of the solar year in the Julian calendar, had moved away from its traditional seat on 21 March and was presently found on 10/11 March. Here, the two basic options were:

(1) to leave the date of the equinox as it is and simply make adjustments to prevent its further drift towards the beginning of the year or
(2) to restore the equinox to a particular date, preferably 21 March, to where it had been assigned by the late antique founders of the Christian Easter *computus*, by dropping a certain number of days from the calendar year either *en bloc* or in installments.

[4] For the pertinent details, see now Mezzi and Vizza 2010, who also argue that Luigi had already died in 1574.

On balance, the report of 1515 recommends the omission of 11 days from the year 1519 to get the equinox back from 10 to 21 March [295–296]. Here there is indeed a parallel to the Gregorian reform, where 10 (rather than 11) days were dropped from October 1582. Such radical excisions, however, were by no means a new idea in 1515, as can be seen from a reform decree drafted at the Council of Basel (1437), which, had it taken effect, would have ordered the omission of a whole week from the calendar [Kaltenbrunner 1876, 412–414].

As is well known, the Gregorian reform commission sought to prevent a further drift of the vernal equinox by modifying the leap-year rule of the Julian calendar: every leap-year evenly divisible by 100, but not by 4 (e.g., 1700, 1800, 1900, but not 2000) loses its bissextile day. By thus omitting three days over the course of 400 years, the Gregorian calendar effectively subtracts 0.0075d from the Julian year (365.25d), leading to an average length of 365.2425d. As Noel Swerdlow [1974] pointed out many years ago, this is the length of the solar year found both in the Alfonsine tables, Copernicus' *De revolutionibus*, and the Prutenic tables, if stated in sexagesimal notation and rounded down to 365;14,33d. The reform was, hence, in harmony with the best science of the day. Ignoring for the moment that a lot of the material relevant to the history of calendar reform remains unstudied or has yet to be discovered, we find that the Alfonsine length of the tropical year (365d 5h 49m 16s = 365.242546...d), with its implied error rate of 1d in 134y, was already relied upon by John of Murs and Firmin of Beauval in their *Epistola super reformatione antiqui kalendarii* (1345) and an attached treatise addressed to Pope Clement VI. That the equinoxes recede at this rate and that the defect can be cured by dropping a day every 134 years was subsequently argued by cardinal Pierre d'Ailly, whose *Exhortatio* of 1411 became an often-cited 'classic' in the literature on calendar reform.[5]

From this it should be clear that the only innovation the University of Salamanca could possibly lay claim to would have to concern the intercalation scheme of the Julian calendar, modified so as to reflect the more accurate year-length. Far from staying close to the Gregorian $^{3d}/_{400y}$ or the Alfonsine $^{1d}/_{134y}$, however, the report of 1515 suggests an omission of a bissextile day in every 152nd year [300]. The decision to add 18 years to previous proposals was evidently motivated by the parallel necessity of reforming the ecclesias-

[5] Both these facts are duly mentioned in Carabias Torres' own account [147–148].

tical lunar calendar [see below], whose error had since medieval times been gauged at ≈ $^{1d}/_{304y}$, i.e., exactly twice the aforementioned number of years. The length of the solar year implied by this correction is 365.243421...d. In order to re-align the calendar with the Alfonsine value in the long run, the Salmantinian doctors signal that further leap-days could be dropped after 1,212 and 15,804 years [304–305]. Since Carabias Torres does not address this part of the proposal, I shall briefly spell out the implied value:

$$365.25 - \frac{1}{152} - \frac{1}{1212} - \frac{1}{15804} = 365.242532...d$$

—not identical with the Alfonsine year length, but close enough. In any case, it should be clear that there are considerable differences between the Gregorian solution and the one implied in the Salmantinian document. It is, therefore, not reasonable to point to the latter as the specific template for Lilio's proposal. Neither is it particularly precise to write that the Gregorian omission of three bissextile days in 400 years was

> *equivalente a la anulación extraordinaria de la intercalación bisextil cada 1000 años propuesta por los salmantinos.* [192]

> equivalent to the extraordinary cancellation of the bissextile intercalation every 1,000 years, proposed by the Salmantinians.

Having dealt with the solar year, we can now turn to the calendrical tracking of the lunar phases for the purposes of reckoning Easter, which today remains a lesser-known aspect of the Gregorian reform, although it was deemed just as important at the time and turned out to be technically more demanding. As a matter of fact, Luigi Lilio's greatest contribution to this reform is his invention of a scheme of 'epacts', which made it possible to retain a cyclical lunar calendar without losing track of the observable New and Full Moons. Carabias Torres claims that such a 'tabla de epactas' was

> *ya propuesta por la Universidad de Salamanca; tabla que no había presentado Salamanca a León X en su informe de 1515 porque, según expresaron,* bastaría el trabajo de 15 días de un mediano calculador astrológico *para elaborarlas. Lilio fue, pues, ese* mediano calculador astrológico *del que hablaron los salmantinos.* [192]

> already proposed by the University of Salamanca; a table which Salamanca did not present to Leo X in its report of 1515, because, as they expressed it, *15 days of work from an average astrological calculator would suffice* to draw them up.

>]Lilio was, then, that *average astrological calculator* whom the Salmantinians spoke of.

Was Lilio just an 'average calculator' who carried out an idea first formulated by the University of Salamanca in 1515? A look at the contents of the Salmantinian report does not bear this out in the slightest. In the passage that Carabias Torres refers to, the anonymous authors discuss the possibility of abandoning the old 19-year lunisolar cycle used by the Church in favour of a calculation based on astronomical tables. This way, the date of the Paschal Full Moon (on which the date of Easter depends) could remain unaffected by the Julian calendar and its modified leap-year rhythm. Solutions of this kind are also found in other reform treatises submitted in the wake of the Fifth Lateran Council, such as the one written by Andreas Stiborius and Georg Tannstetter (who appears as 'Stannstefter' in the present book [152, 176]) on behalf of the University of Vienna. As Carabias Torres correctly notes [152, 236], Stiborius and Tannstetter wanted the Church to base the calculation of Easter on the true positions of Sun and Moon, whereas the Salmantinian doctors remained content with mean values. Without going into great specifics, they envisioned an advance tabulation of the date of the Paschal Full Moon for several millennia, to be inserted into the breviaries so that parish priests could simply look up the date of Easter on a year-to-year basis. To construct such a list, they confidently write, would take even an

> average astrological calculator just half a month's worth of work.
>
> *Quibus tabellis constituendis mediocris etiam astrologici supputatoris semestris industria sufficeret.* [292]

Carabias Torres' claim that this suggestion anticipated the Gregorian reform might be an idea worth discussing, provided that Lilio had actually drawn up a list of the times of Full Moon of the kind mentioned in the report. In reality, his 'epact' system is a calendrical not an astronomical device, an artful modification of the traditional 19-year lunar cycle which keeps the New and Full Moons in line with the phenomena whilst responding adequately to the changed leap-year rule of the solar calendar.

It should be stressed that nothing resembling the 'Lilian epact', where every day of the year can become the seat of the New Moon according to a complicated predetermined sequence (a cycle that effectively lasts 300,000 years!), appears in the document of 1515. What we do find, in addition to the aforementioned 'astronomical solution', are various suggestions of how to reset the traditional 19-year cycle and make it useable again by bringing the 'Golden Number' back in line with the actual day of conjunction. The downside of such a solution was that a number of additional adjustments became necessary, not only to prevent the New Moons from receding farther and farther (at the aforementioned rate of $^{1d}/_{304y}$) but also to account for subsequent corrections of the solar year. According to the Salmantinian scheme already mentioned, there would have been a suppression of a bissextile day in every 152nd year, which meant that twice as many days would have been dropped than was adequate for the lunar cycle. In order to counteract this over-compensation, the 'Golden Number' had to be reset by one day after every 304 years. Further adjustment would have become necessary in case the additional omission of leap-days after 1,212 and 15,804 years had been implemented.

Ironically, this is much closer to the spirit of Lilio's solution than the astronomical tables referred to by Carabias Torres. In stark contrast to the Italian scholar, however, whose 'epact' system makes precise provisions to balance out the solar and lunar corrections, the Salmantinian doctors only vaguely hint at the required steps, leaving it to the papal commission to work out the details. Moreover, Lilio's principle of increasing the lunar epacts by eight one-unit steps over 2500 years in order to keep the calendar aligned with the lunar phases implied an error rate of $^{1d}/_{312.5y}$, which was a significant departure from the traditional $^{1d}/_{304y}$ used in the report of 1515. Since the basic ideas discussed here—modifying the 19-year cycle *versus* a purely astronomical approach—can be found in numerous other reform treatises submitted in the wake of the Fifth Lateran Council as well as in earlier proposals,[6] there is little merit in the suggestion that the Gregorian reform has specifically Salmantinian roots. In light of the rather half-hearted and meandering way in which both reform solutions are offered in the document of 1515, it is in fact startling to read statements like the following:

> *El razonamiento es impecable y su propuesta coincide con la opción ratificada finalmente por Gregorio XIII bastantes años después, en la que sólo se añadió un algoritmo corrector en la celebración del año bisiesto.* [182]
>
> The argument is faultless and their proposal coincides with the option ultimately approved by Gregory XIII a number of years later, in that he only added a corrective algorithm to the celebration of the bissextile year.

[6] For details, see Kaltenbrunner 1876 and Marzi 1896.

Why, in spite of all the differences just mentioned, does Carabias Torres show herself so impressed with the Salmantinian report and its supposed proximity to the Gregorian reform? The answer to this question, it must unfortunately be said, marks out the present book as a cautionary tale for any historian who intends to study the history of a scientific subject without commanding the necessary technical background. In the case of the history of calendar reform, this background is in fact relatively modest. All it takes is a basic grasp of concepts such as the tropical year and the mean synodic month, a reasonable overview of the history of Christian Easter computations, and some awareness of the astronomical parameters that were known and used at a particular point in time (as well as, perhaps, the ability to wield an electronic calculator). Unfortunately, Prof. Carabias Torres' arguments give the impression that she did not immerse herself in the subject to an appropriate degree. This impression is reinforced by the disconcerting number of errors—some small, some hair-raising—that appear in her book whenever technical issues are addressed. One of the most startling examples is found on page 144, where we read:

> *Los propios padres de Nicea tenían que conocer de antemano que el contenido de sus decretos sobre la Pascua eran necesariamente una solución temporal debido a la presunta precesión de los equinoccios...este movimiento lento del eje de rotación de la Tierra, que se creía la causa de las diferentes longitudes de los años sideral y trópico.*
>
> The fathers of Nicaea themselves had to know beforehand that the content of their decree on Easter was necessarily a temporary solution, owing to the presumed precession of the equinoxes...this slow movement of the rotational axis of the Earth, which was believed to be the cause of the differing lengths of the sidereal and tropical years.

Not only is there no source attesting to any awareness of precession among the Nicaean bishops but the phenomenon described is utterly unrelated to the calendrical issues that were discussed in 325 and 1582. While one might conceivably talk about a 'precession' of the equinoxes in the Julian calendar, this precession would be no more than a simple change of the respective dates owing to the fact that a Julian year of 365.25d is roughly 11 minutes longer than a mean tropical year of 365.2422d. As a result of this discrepancy, the equinox will fall one day earlier every 128 years. Yet Carabias Torres seems to think somehow that the cause for this calendrical shift is the astronomical precession discovered by Hipparchus, which, as she goes on to mention, was conceptualized by medieval cosmologists as a movement of the eighth sphere (where the fixed stars reside) relative to the equinoctial points. Indeed, she explicitly writes that

> *debido a esta circunstancia, en el siglo XVI el equinoccio no tenía lugar el día 21 de marzo, como se había fijado en Nicea, sino varios días antes.* [144]
>
> owing to this circumstance, the equinox in the sixteenth century did not take place on the 21st day of March, as it had been fixed in Nicaea, but several days before.

Several further examples for this kind of mishandling of basic astronomy could be adduced. To mention but a few: at the beginning of the report of 1515, the Salmantinian experts correctly cite the Ptolemaic estimate of the tropical solar year as $365.25 - {}^{1}\!/_{300}$ d (= 365.2466...d). Carabias Torres misunderstands this to mean

> *que los comisionados opinan que Ptolomeo se equivocó en 4,8 minutos, o 288 segundos, en esta estimación, cuando hoy sabemos que su error era de 11 minutos y 12 segundos.* [170]
>
> that the committee members thought that Ptolemy was wrong by 4.8 minutes, or 288 seconds, with this estimate, whereas we know today that his error was 11 minutes and 12 seconds.

Clearly, the error under discussion is that of Julius Caesar and his advisor Sosigenes, not Ptolemy. On page 182, she makes another elementary mistake when claiming that the commission suggested a removal of 11 days from the calendar,

> *como el año solar sobrepasaba un poco más de 10 minutos y 4 segundos al año eclesiástico.*
>
> since the solar year surpassed the ecclesiastical year by a little more than 10 minutes and 4 seconds.

This might have been correct the other way around. On page 139, she claims that there is a difference of one year between 'el calendario juliano proléptico' and 'el calendario gregoriano proléptico' as far as the beginning of the Jewish calendar is concerned (3760 *versus* 3761 BC), which is pure fiction.

Yet other blunders are historical: on page 24, she confuses the mythical age of Romulus with the heyday of the Roman republic when she states that the Roman year before the introduction of the Julian calendar consisted of only 10 months or 304d. On page 142,

> *un computista romano desconocido llamado el Pseudo-Cipriano*
>
> an unknown Roman computist called pseudo-Cyprian

is said to have introduced in the third century a new 84-year cycle called the *Laterculus*. In reality, scholars locate pseudo-Cyprian in North Africa, while the *Laterculus* is everywhere attributed to his compatriot Augustalis. Historians of astronomy will be surprised when reading on page 35 that Kepler's planetary laws were first published in the *Mysterium cosmographicum*. What is disconcerting about such slips is that the handbooks and studies which Carabias Torres duly cites at many places in her work would have contained all the information necessary to forestall them. Her sloppy reading of the relevant literature is particularly conspicuous on pages 25 and 204, where she twice attributes a quote to Juanelo Turriano that in the referenced source is clearly marked as coming from a metrical epitaph on Turriano by Pierleone Casella. Elsewhere, Joseph Scaliger's famous Julian period is described as comprising 7,980:

> *años sidéreos...prescindiendo del año bisiesto y de los cálculos medios, que habían sido usados en las tablas alfonsíes.* [31]
>
> sidereal years...ignoring the bissextile years and the mean calculations that had been used in the Alfonsine Tables.

As the very name should alert us, the Julian period was plainly and simply based on the Julian year.

In mentioning these mistakes, it is not my intention to depreciate Carabias Torres or her merits as an early modernist. Clearly, there is no shame in being out of one's depth in a technical field such as astronomy or chronology. What is problematic, however, is that she did not try harder to make up for her lack of competence in this area, e.g., by having her text proofread by an experienced historian of astronomy.[7] This would have been a prerequisite for accurately assessing the contents and 'originality' of the Salmantinian reform proposals and, hence, for determining their role in the history of time-reckoning. In the present form, her case for Salamanca as the birth place of the Gregorian calendar is fundamentally flawed, both technically and historically. On the technical side, there are serious differences between the suggestions made in the assessment of 1515 and the rules that govern the Gregorian

[7] On page 258, the author states that she approached a professor of theoretical physics, who seemed initially willing but eventually withdrew from the project.

calendar, which should have been properly acknowledged in the present book but are instead glossed over. Historically, one can find such a wide range of other possible 'precedents' for the Lilian reform plan that Carabias Torres' claims of Salmantinian anteriority turn out to be weakly motivated. One suspects that her tendency to treat the report of 1515 as an exceptional document is in part owed to an insufficient acquaintance with the literature on calendar reform produced during the 12th to 16th centuries, where similar ideas crop up time and again. As a result, Carabias Torres shows herself greatly impressed by the fact that the Salmantinian experts would mention al-Battānī as an authority on the length of the solar year. In her view, this demonstrates the astronomical expertise of the members of this commission, who were able to read and understand such complicated and technical material [181]. Yet surely, the cited tidbit of information, according to which al-Battānī's year-length implied an error of $^{1\mathrm{d}}/_{106\mathrm{y}}$ [306], cannot be enough to prove first-hand acquaintance with his works, given that it also appears in other medieval and early modern texts on astronomy and calendar reform.[8]

With all points taken into consideration, it becomes clear that Carabias Torres has no ground to stand on when she avers that Luigi Lilio was dependent specifically on the report from Salamanca as the model for his reform plan. In postulating such a connection, she evidently relied on a claim found in the letter addressed to Pope Gregory XIII that accompanied the report of 1578. Speaking about Lilio's proposal, which their university had been asked to assess, the *salmantinos* state that it corresponded 'marvelously' (*mirifice*) with the assessment drawn up by their predecessors in 1515 [218, 236, 318]. Needless to say, this is an exaggeration on the part of the authors, who sought to highlight the contribution of their university to the reform's advancement. Carabias Torres' willingness to jump uncritically on a statement of this sort seems to betray similar motivations. It should be mentioned that *Salamanca y la medida del tiempo* opens with no less than three laudatory prefaces written by dignitaries of the Salmantinian academy: Manuel Carlos Palomeque López, Cirilo Flórez Miguel, and Ramon Aznar i García. All of them have rather nice things to say about the author and the importance of the volume, which, they insinuate, sheds new light on the exceptionally

[8] To cite just two examples, one early, the other contemporary to the report: *Compotus Constabularii* (1175), ed. Moreton 1999, 81; Giovanni Maria Tolosani, *De correctione calendarii* (1515), ed. Marzi 1896, 252.

advanced state of Salamanca's astronomical school in the 16th century. This unusual degree of attention has to do with the fact that the present volume belongs to a book series specially created for the buildup to the eighth centenary of the University of Salamanca, which will be officially commemorated in 2018. In line with this prestigious setting, the book was ceremoniously launched in November 2012 and received a good deal of coverage in the local news media. It is anyone's guess whether this kind of fanfare would have been reserved for a work that presented the same kind of dry and technical material but without the implied paean to Castilian glory. Viewed from this angle, her book may be even counted as a success, provided it was the author's objective to simply produce a crowd-pleaser adequate for the local patriotism of her environment. In this case, she will surely forgive a foreigner for being a little less sanguine about the result.

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Galen's treatise *On Problematical Movements* (*De motibus dubiis*)—its critical edition by Vivian Nutton is under review here—has not been very well known in modern times, although in the Middle Ages it was widely circulated, mainly *via* a Latin translation from the Arabic. While in 1968 Margaret May even said that *De mot. dub.* had totally disappeared, the last 20 years have seen it slowly come back to light, mostly thanks to Carlos Larrain, who meritoriously published and commented on it [1994, 1996] using every piece of evidence that he could discover. Despite his efforts, the result was still unsatisfactory due to a number of difficulties inherent in the intricate tradition of this text. There are a very few fragments remaining from the lost Greek original,[1] which was still read in late Byzantine times, a Latin version translated from the Greek by Niccolò da Reggio, an Arabic version (apparently unknown to Larrain) compiled by the well known Syriac translator Ḥunayn ibn Isḥāq, and the aforementioned Latin version of Ḥunayn's text by Mark of Toledo. It is indisputably to Nutton's credit that he calls all these witnesses of the lost Galen to the stand and provides a critical text for each of them—the Arabic version is edited here for the first time by Gerrit Bos. This is the only way to ascertain the genuine argumentative path of this medical treatise.

De mot. dub. is, as a matter of fact, a thoughtful look into a very uncertain field of research at a time when the physiology was not well understood

[1] These fragments were recognized by Larrain [1993] and are printed by Nutton in the first apparatus.

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because of a lack of anatomical knowledge. Herein lies the starting point of Galen's inquiry:

- What distinguishes voluntary and involuntary movements?
- Is the cause of movement always the same?
- Studying the human will means studying the soul but what about those organs whose action does not usually require the intervention of will, although it can occasionally be altered by will itself?

In the beginning, Galen accepts and professes the paradigmatic (Alexandrian or, rather, Erasistratean) distinction between voluntary and involuntary motion: voluntary motion is produced by muscles and nerves, the impulse coming from the brain (which is the site of the ἡγεμονικόν, the ruling part of the soul) or at least from the spinal cord, while involuntary motion does not involve the brain. What, at first, is declared to be merely a problem of definition is quickly reframed by Galen as an anatomical issue. Dissection or vivisection might guide the practitioner to recognizing the source of each movement: for instance, the action of speaking or making sounds is to be imputed to the recurrent laryngeal nerves (those that 'run alongside the carotid arteries') because, if they are cut, the animal remains voiceless.

But still, there are some movements which are difficult to classify in such a rigid manner. For example, Galen asks, 'Are the protrusion of the tongue outside the mouth or the erection of the penis voluntary movements or not?' and explains that both are caused by the *pneuma* that springs from the arteries and inflates the pipe-like structures in those organs. Thus, we discover a voluntary movement which does not involve nerves or muscles. The same may be said for vomiting: it is allegedly caused by the action of the exterior tunic of the esophagus *and* by 'the expulsive power in the stomach'. While for most people vomiting is a natural function which occurs when necessary with no interference of the will, some people have trained themselves to control it. Breathing is one of the most complex movements in this regard: that it is a natural, i.e., involuntary, motion is self-evident because we breathe even when we are sleeping; but we can stop breathing for a while and, moreover, breathing is managed by nerves because, if they are cut, the animal stops breathing. The same is to be said about coughing or sneezing. But Galen could find no explanation for the fact that tickling under the armpits provokes laughing. Such a strange admission of defeat led Joubert [1579] to consider this treatise unworthy of Galen.

Galen meditated on such problematic movements as a philosopher. He was aware that evacuation is a double kind of motion: what is excessive or inappropriately ordered in the bladder or the intestine has to be expelled involuntarily; yet the will may make the process easier by relaxing the muscles that control the organs involved or may prevent it for a limited period of time. Whether relaxing the muscles may be properly called an action is a topic unworthy of consideration according to Galen, whereas refusing to act is a deliberation of the will and is, thus, an action (unlike what the Stoics say about deserters). There is also a sympathetic method of transmission of motion, just as of disease: watching someone who suffers from ophthalmia fills the observer's eyes with moisture; watching someone urinating or yawning causes the impulse to urinate or yawn. Not only the will but also states of mind (fear, anxiety, anger, and so on) can have physical effects on the body (Hippocrates had already said this in his *De humoribus*, quoted by Galen) and, moreover, imagination has a similar power: when a man thinks of his lover, his penis has an erection even if he does not want it to. Nutton claims that the basis of such an assumption is the Platonic tripartite soul, though Galen also inherited the understanding of the process of decision-making from Aristotelian philosophers.

What is striking in *De mot. dub.*—but which is also a feature that can be recognized in many Galenic works—is the flexibility of Galen's argument. He begins with the aforementioned paradigmatic distinction between voluntary and involuntary motion but soon discards it and moves on to a very pragmatic analysis of problems. His doubts about the origin of laughing, his coming back to previously addressed issues, his claiming the importance of anatomy as a way to test any hypothesis, are the methodological pattern of a practitioner who asked questions that nobody had asked before and turned his gaze onto unexplored fields of medical science without refraining from admitting his occasional failure to answer them. As Armelle Debru wrote, '*aporia*, in this case, has a heuristic value. This is *positive* anatomy' [2002, 81]. This compound way of looking at the human body, 'as a coherent organism to be investigated anatomically, physiologically, and philosophically' [18], is the most convincing proof of Galen's authorship.

As Nutton rightly points out, the textual arrangement of *De mot. dub.* is quite disappointing since 'it reads like a relatively impromptu exposition, just as if it was being dictated to a copyist' [10]: there is no harmony between the parts and each problem seems to spring from the previous one. For instance, in 8.14 and 8.28, Galen apparently contradicts himself in explaining why one catches ophthalmia after looking at someone who suffers from that illness (i.e., because of the weak nature of the eye or because of thinking about it). But such hesitation is likely to be the effect of the genesis of the treatise from dictation or from some didactic activity.

Indeed, doubts about the authorship date back to the very Medieval tradition: the Bolognese erudite who annotated the unique manuscript of Niccolò da Reggio's version suspected that it contained at least some interpolations. Most recently, similar doubts have been raised by Armelle Debru, even though, in her opinion, the impression of a 'cento, produced in the Christian centuries', is counterbalanced by the perception of its 'almost entirely Galenic content' [2002, 85].[2]

It is never easy to judge a text which is not preserved in its original form and has to be evaluated on the basis of secondary evidence. Nutton firmly attributes the work to Galen after having closely scrutinized Niccolò da Reggio's version,[3] which is assumed to be the most faithful to the original Greek text: as a result, Nutton has been able to discern many distinguishing Galenic expressions. One of the most successful features of this edition is its in-depth inquiry into the vocabulary of the Latin translators (although without providing a glossary of Latin words) and their 'styles of translation'—the 'almost pedantic accuracy of Niccolò da Reggio' and his attempt to imitate as faithfully as possible the original through his Latin, Ḥunayn's concern with meaning rather than wording, and Mark of Toledo's desire to be clear even when it meant concealing his (sometimes) poor understanding of the Arabic version by recasting it. The difference is very evident if we compare their attitudes toward difficult terms: Niccolò has a tendency to transliterate them,[4] while Mark mixes paraphrase and definition.[5] This clearly explains the suc-

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[2] She adds: 'in spite of its considerable obscurities and its strange structure'. Such oddities can now be better understood thanks to Nutton's scientific and much more reliable edition.

[3] This was also the opinion of Garofalo [2004, 553].

[4] 8.20: *syrangosa substantia* from «cηραγγώδηc»/«cυραγγώδηc» ('porous'), where Mark translates 'porositates modice'.

[5] 6.1: *id quod ymaginamur extimando*, where Niccolò has 'fantasiis que secundum cogitationes'.

cess of Mark's version in the universities, as well as the poor circulation of Niccolò's translation [cf. Garofalo 2004, 554].

The wide distribution of manuscripts containing Mark of Toledo's version made it possible for Nutton to conduct an in-depth and inestimable inquiry into the role and significance of Galenic science for medieval medicine. The combination of two translation movements, one stemming from Arabic Spain and the other from Constantinople and Southern Italy, created the so-called 'New Galen', a new, richer collection of Galenic writings to be used in the universities in the 13th and 14th centuries. The manuscript evidence suggests that Paris (or perhaps Chartres) seems to have played an important role in the making of such a corpus. The presence of *De mot. dub.* in university teaching is certain because of the fact that most of its manuscripts have a typical university layout (large format, double columns, broad margins, annotations by more than one owner). However, what is remarkable is that the New Galen was never utilized as a means to circulate medical knowledge but continued to be read only by very expert practitioners or teachers. This is clear in the case of *De mot. dub.*: the marginal notes often deal with 'the interaction of the body and the will, the proof that each part of the body has both an attractive and an excretive faculty, and disorders such as ophthalmia and satyriasis', i.e., with topics designed to be handled in university classes. If occasionally in these notes there are improvements on philosophical issues, there is never any evaluation of Galen's statements regarding anatomy on the basis of the annotator's own experience. In only one documented case can it be said that *De mot. dub.* had a substantial influence on the method of anatomical research: Mondino de' Liuzzi (*ca.* 1270–1326), a Bolognese professor who actually practised dissection, is caught by Nutton summarizing Galen and, moreover, concentrating his interests on those parts of the human body (primarily, the throat and the thorax) that had already been investigated in *De mot. dub.*

As is known, in antiquity the Empiricists were opposed to dissection, considering it useless since the very act of dissection was thought to alter the body on which it was practised [Rocca 2008, 246]. Galen was aware of these criticisms and was very careful in *De mot. dub.* to point out to the reader the proper way in which to perform dissection (or even vivisection). For instance, in 11.17, Galen, who wanted to explore the process of swallowing, suggested starving the animal and depriving it of drink for long enough before vivisecting it to be able to watch the animal trying to eat and drink before bleeding to death; and he makes it as clear as possible that *human* dissection would be best (as his Alexandrian predecessors had been able to do but he was not)[6] and, accordingly, censured his opponents by saying that they had been misled by the anatomy of monkeys. We should not forget that dissection for the purposes of investigation soon disappeared after Galen's time and, thus, that Galen's anatomical texts represented an odd challenge for Medieval scholars: this explains why the real impact of this and other[7] Galenic texts on learned physicians was still quite limited.

I had previously assumed that Niccolò's version was the most faithful to the original Greek text in light of its literal phrasing.[8] Actually, the usefulness of Ḥunayn's and Mark's versions cannot be discounted, in part because Niccolò was frequently working with a defective Greek manuscript. Thus, in my opinion, Nutton abstains from publishing a back-translation to Greek with good reason: he aims instead to provide a reliable edition of the Latin and Arabic versions of the text as Niccolò, Mark, and Ḥunayn wrote them. However, the English translation tries to take a step forward: it is based on Niccolò's text but often looks at those of Ḥunayn and Mark, especially to fill the gaps in Niccolò's version, and in this way tries to reconstruct Galen's thought. For instance, in 5.10–11, when Galen explains the anatomy of the muscles that move the tongue, Niccolò's text suffers from a major loss which makes it impossible to understand Galen's description properly. Unfortunately, this section of Ḥunayn's text also appears to be marred by significant mistakes, which would require a lot of care (and anatomical knowledge) to correct: e.g., he makes Galen say that the tongue is moved downwards by muscles inserted from above ('descendit per lacertum qui continuatur ei desuper' in Mark's rendering), which is the opposite of both reality and what we know Galen stated elsewhere. As we cannot ascertain

6 Nutton [2004, 231] remarks how difficult performing human dissection was even for Galen, as many of his anatomical accounts are true for animals but not for human beings.

7 The last six books of *Anatomical Procedures*, which are almost useless to people who do not dissect, have no Greek tradition. Cf. Nutton 2008, 357.

8 With well-supported arguments, Garofalo [2004, 558–559] praises Mark's version as more skillful: he stresses Niccolò's unfamiliarity with Latin technical vocabulary and asserts that Mark's version is much more understandable, especially regarding anatomical terms.

whether the mistake had already occurred in the Greek tradition (and surely it already had occurred in the Arabic), Nutton is right to print Mark's text in the anatomically wrong (but philologically correct) form.

I wonder whether the obscurities in the treatise can always be traced back to its genesis as a dictation: many of them could be due to some kind of abridgment made during the Greek Medieval tradition. This might explain why, in *Anatomical Procedures* 4.3, Galen explains that 'more is said' about the nerves and muscles of the lips in *De mot. dub.*, which is not true, at least according to the text that we can read now. However, the question is quite complicated and Nutton may be right when he says that this could be a reference to a planned work, not to one that was already written, and that the work was then perhaps not actually written in the way it was planned.

Nutton's edition is followed by a very useful commentary in which there are not only philological discussions that lead to a better understanding of the meanings of the text but also information about Galen's methodological patterns and their historical significance—information which is very helpful to people less familiar with the history of ancient medicine. Among many examples, of particular interest are the notes on page 345 where Nutton explores a quite vague hint ('cuidam phylosopho absque servitute et invidia phylosophiam exercenti') by briefly reconstructing Galen's acquaintance with the philosophical *milieu* in Rome.

On strictly philological grounds, one of the most important advances that this edition offers is the in-depth inquiry into the Latin manuscript tradition of Mark of Toledo: 31 manuscripts are described and classified into two families, α and β. Nutton verifies that no manuscript is parent to any other and draws [65] a detailed *stemma codicum* (with some contamination that affects two manuscripts and the corrector of a third one). He ascribes family α to Bologna and remarks that it sticks more literally to the Arabic model; family β is more extensive both geographically and chronologically and provides a much more elegant and refined text. Nutton hypothesizes that at least some of the variants between the two families originate from the author and thus chooses to follow β in the constitution of the text, assuming that β reflects an attempt at stylistic refinement made by Mark of Toledo himself.

These assessments are of great importance, not only to those who intend to study *De mot. dub.* but also to the future editors of other Galenic translations (the New Galen) which are preserved in those manuscripts. This is a fact. A bit oddly (but not overly so), Nutton draws a further conclusion:

> This division simplifies and improves the task of selecting codices for collation when the choice is apparently vast.... Rather than relying on what is closest to hand and thereby risking a great deal of time collating many mss of the same family and missing or underestimating important mss of the other, editors of Galen need select only one or two mss from each family in order to gain a reasonable impression of the accuracy of a medieval Latin translation. [50]

The specific purpose of such a statement is perfectly understandable and acceptable, since Nutton is thinking of editors of Greek texts who need to establish the reliability of Latin versions *before* constituting the original text. But, generally speaking, the idea of selecting, somehow randomly, one or two witnesses from each family, instead of analyzing the stemmatic relationships between *all* the manuscripts of each family (as Nutton actually did), sounds unmethodical[9] and may become an unfortunate outcome of the fact that such studies are very long and need consistent funding which is, nowadays, more and more difficult to find. Our hope is that Nutton's very precious work may be continued by him and/or by other scholars, and assumed as a 'template' (as he defines it) onto which the manuscript tradition of other Medieval Latin translations of Galen may be studied and systematized.

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[9] Nutton himself says that 'the quality of individual mss varies within each group' (more remarks about that in Nutton 2007); moreover, nobody should exclude *a priori*, i.e., without closely evaluating variants, that, in the same manuscript, the text of *De mot. dub.* might belong to one family and the text of a different Galenic work to the other one (or even to neither). Such a possibility may not be likely but, nevertheless, should not be ruled out.

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Teleology, First Principles, and Scientific Method in Aristotle's Biology by Allan Gotthelf

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Students of Aristotle may take it nearly for granted today that a rigorous study of his biological writings is worthwhile not just because of the intrinsic merits of those works but also because they can help us to acquire a deeper and more nuanced understanding of other parts and aspects of Aristotle's *oeuvre*—especially his metaphysics and natural philosophy, and his philosophy of science. This view, however, was considerably less popular just a few decades ago. David Balme, and subsequently Allan Gotthelf, James Lennox, and other influential scholars, contributed significantly to this paradigm shift in the field of Aristotelian studies. Besides his work devoted to Ayn Rand's philosophy, Gotthelf is well known as the author of many seminal articles and chapters on ancient philosophy, and as the editor or co-editor of several important books on Aristotle's biological treatises and their philosophical implications. His prominent place in the recent history of Aristotelian scholarship has been acknowledged, among other things, by the publication of *Being, Nature, and Life in Aristotle: Essays in Honor of Allan Gotthelf* [2010].

Teleology, First Principles, and Scientific Method in Aristotle's Biology gathers 16 articles (some based on conference presentations), of which four—chapters 4, 12, 13, and the concluding chapter—were not published previously. Chapter 3 has been significantly expanded. The other 11 have largely been republished here with only relatively minor updates and other modifications, including the cross-references which further accentuate the overall unity of this collection. There is no introductory chapter but the preface is a useful guide to the structure of the book and to the author's intellectual background: for example, readers will find—to their surprise, perhaps, if they are not already acquainted with Gotthelf's work—that his

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exposure to Ayn Rand's thinking was instrumental in his own interpretation of Aristotle's philosophy and science.

Gotthelf's analyses are always remarkably clear and are also often infused by a sort of dramatic or quasi-dialogical element. What I mean is that, while other such studies are replete with somewhat impersonal references to earlier scholarship, this book manages to convert many references into a dialogue *sui generis* as the author would seem to engage other scholars before our eyes, as it were; the mention of this or that article or lecture is often accompanied by a vivid narrative excursus—for instance, his impressions from a symposium where a speaker's reaction to questions from the audience was tellingly foreshadowing a controversial book later published by that speaker.

The 15 plus one chapters (the last one is named a 'coda' rather than being numbered) are grouped into five parts. The first four are largely correlated with Aristotle's *Generation of Animals*, *Parts of Animals*, both these works, and his *History of Animals*, respectively.

The first part is concerned primarily with teleology. In 'Aristotle's Conception of Final Causality', which is accompanied by a 'Postscript 1986', Gotthelf aims to clarify a crucial point that Aristotle himself never quite fully elucidated (despite a few theoretical discussions, including *Phys*. 2.8). The question at the heart of this chapter is:

> ...what, precisely, does Aristotle mean when he asserts that the coming to be (or any stage in the coming to be) of a living organism is for the sake of the mature, functioning organism which results? [6]

This question is the preamble to an investigation into the nature of teleological explanations. The author's analysis turns on the notion of biological irreducibility and implicitly on Aristotle's main explanatory concepts in such contexts—nature (φύϲιϲ) and potential (δύναμιϲ). The potential for the end of the development of an organism is, Gotthelf believes, not reducible to what he calls element-potentials (although, as he is careful to point out, the actualization of that potential does involve the actualization of various element-potentials). The claim, then, that earlier stages in the development of an organism are for the sake of the mature organism cannot be grounded on a mechanistic approach to nature or on a set of what we would call today physical and chemical laws. It is also important, we are told, to be mindful of the empirical character of Aristotle's firm reliance on teleology, as reflected by a number of passages from *De gen. an.* and elsewhere (a point that will be re-emphasized in other chapters). The 'Postscript 1986' responds to interpretations offered by Sorabji and others, and reaffirms the view defended by Gotthelf 10 years earlier about the irreducibility of the potential for form and its philosophical significance.

The second chapter, 'The Place of the Good in Aristotle's Natural Teleology' addresses an aspect that was not examined in the previous chapter (and is central to several scholars' accounts of final causation), namely, the link between the corresponding notions of goal and good. Gotthelf's main point here is that Aristotle does not take a reference to the goodness of an end to be a necessary ingredient of an account of what it is to be a goal or an end. The notion of end does not hinge in biological contexts on a prior concept of good. Instead, goodness itself is to be understood by first grasping the functions of concepts such as actuality and end. The structure of this argument is quite complex and involves several segments devoted to the good, the better, the well, the honorable/valuable, and the divine. Its overall purpose is to argue for a non-normative analysis of ends which appeals systematically to potential or capacity (e.g., potential for development to maturity and for continued life) as the main explanatory tool in Aristotle's science of life. The appendix to this chapter is a response to Scaltsas' suggestion that 'teleological relations cannot be analyzed in terms of the potential-actual relation' [63].

The third chapter, 'Understanding Aristotle's Teleology', while continuing the line of thought deployed in the first two and in other chapters, is meant, among other things, to provide a more comprehensive context for an inquiry into the nature of Aristotelian final causation. Gotthelf outlines some guiding questions under the three headings:

(1) Analysis:
  - How do natural teleology and final causation in the domain of human action compare with each other?
  - And how are 'being for the sake of' (as a part is for the sake of the whole organism) and 'becoming for the sake of' (pertaining to the development of an organism) related to one another?;

(2) Basis:
  - Is there an ontological basis for Aristotle's handling of natural teleology, as the author believes is the case, or is it merely an indication of how we understand the workings of nature?;

(3) Extent:
- What is the scope of Aristotle's natural teleology?
- Is it all-encompassing *sub specie primi moventis*?
- Can we also talk of an anthropocentric teleology?

Gotthelf chooses to focus here on questions under the second heading and much of this chapter is a summary and assessment of several approaches to it. Towards the end, he turns his attention to the relevance of Aristotle's notions of irreducible potential and final causation to modern conceptions of directiveness in biology.

In 'Teleology and Embryogenesis in Aristotle's *Generation of Animals* II.6', in addition to studying the theory presented in *De gen. an.* 2.6, Gotthelf explores the larger significance of the scientific and philosophical enterprise in that treatise. His focus, though, is on clarifying the relation between efficient causation and final causation there, and this clarification is achieved partly by taking into account Aristotle's emphasis on the order in which the parts of an animal come to be. The position defended through a careful analysis of the text is that *De gen. an.* 2.6 does not provide a 'bottom-up' account of the early stages in the development of an organism and that it actually conveys

> a single, unified account of the entire embryogenesis—an account in which the embryo's formal nature (its potential for form) is playing the central efficient-causal role by 'making use of' material-efficient agents.... [100]

'"What's Teleology Got to Do with It"? A Reinterpretation of Aristotle's *Generation of Animals* V' (co-authored with Mariska Leunissen) places special emphasis on *De gen. an.* 5.1 and 8, but the conclusions reached by the authors go far beyond the confines of those two sections. The goal of this chapter is to offer an interpretation which corrects a string of misunderstandings in other studies concerned to various extents with *De gen. an.* 5. According to Gotthelf and Leunissen, this fifth book comes naturally after the investigation carried out in *De gen. an.* 1-4. Besides, it does not deal exclusively with accidental features which are materially necessitated. The analysis of ch. 1 is intended to weaken a possible reading in which Aristotle's distinction between explanations based on material and efficient factors and explanations centered on final causation renders the topics discussed in *De gen. an.* 5 irrelevant to his teleological outlook. In their examination of *De gen. an.* 5.8, the authors distinguish what they call a secondary form of teleology. Even if certain processes, such as the differentiation of teeth, are explained in material terms (in this case, as being due to what happens with the residual stuff generated originally for the growth of the bones), they are put to work by the nature of an organism in a way that serves that organism. We should, therefore, make a distinction between vital and essential parts which are 'wholly due to form' and subsidiary parts whose causation 'begins from material by-products of the former process' and 'are formed at a later stage and then act according to their own natures' [131].

The last and shortest chapter in this first part is entitled 'Teleology and Spontaneous Generation in Aristotle—A Discussion' and is largely a critique of James Lennox's interpretation of *De gen. an.* 3.11. As Gotthelf readily admits, the theory of spontaneous generation is conceivably problematic for his understanding of Aristotelian teleology: if spontaneous generation is due entirely to element-potentials, then why would the potential for form in non-spontaneous generation be irreducible? And, if the amount of natural heat involved in spontaneous generation is species-specific, then why would teleological explanations apply only to sexual reproduction? On his interpretation, however, the 'pneumatic heat' is not species-specific in spontaneous generation and this sort of generation is likely to involve 'a non-species-specific irreducible potentiality' [145]. Finally, we are cautioned [149–150] not to rely on 'actual' instances of spontaneous generation in Aristotle's works in order to make claims about teleological accounts, since Aristotle contrasts teleology with cases that are imaginary or otherwise distinct from the sort of 'actual' spontaneous generation that he occasionally writes about.

The second part of this book is devoted to 'First Principles and Explanatory Structure in Parts of Animals'. Chapter 7 ('First Principles in Aristotle's Parts of Animals'), is an (avowedly incomplete) answer to the question whether Aristotle's theory of science, as set forth mainly in the *Posterior Analytics*, is compatible with the types of definitions and explanations used in *Parts of Animals* (and possibly elsewhere in Aristotle's biological corpus). Can we find demonstrations 'in a fairly strong sense' [174] in *De part. an.* 2–4? Is there an axiomatic structure in the explanatory apparatus of *De part. an.*? Gotthelf argues that we can detect an implicit axiomatic structure there and pays special attention to the first principles (i.e., facts 'which are not themselves explained by reference to more basic facts' [155]) incorporated in those structures. First principles that underlie the explanations offered in *De part. an.* 2–4 involve both defining aspects of the material nature of animals or parts of animals and formal (and final) aspects. Partial definitions are also afforded 'at all levels of generality' [178] in *De part. an.*

Throughout this chapter and indeed throughout this book, as I mentioned at the outset, one is left with the impression that Gotthelf has been engaged in a very lively dialogue indeed, and that voices other than his are also distinctly audible in this interpretative drama. This chapter starts with a critique of Barnes' observations about the discrepancy between the *Posterior Analytics* and Aristotle's biology, and concludes with sympathetic reflections on Kosman's view that the *Posterior Analytics*

> should be understood as offering a formal description of proper science, not a requirement that proper science itself be formal [181]

and with a rebuttal of Lloyd's distinction between lumpers and splitters (the debate, more appropriately put, we are told, is between integrators and fragmenters).

The topic of chapter 7 is scrutinized in the next one too ('The Elephant's Nose: Further Reflections on the Axiomatic Structure of Biological Explanation in Aristotle'), where the emphasis is on the use of premises in *De part. an.* 2.16. The complexity of the explanation at hand is demonstrated in part by two diagrams. The first one indicates a generic 'linear, branching structure'; the second one is meant to show that in practice this structure is more complicated, as the number of final features is bound to be much greater than that of primitive features. We are also reminded that the overall structure of the explanation is in fact far more intricate even than the second diagram if we combine the explanations for all the distinctive features of the elephant, not just for its trunk, let alone if we also care to expand this explanatory model by taking into consideration the features of other species discussed in *De part. an.* as well. The list of premises used in this particular explanation include: material principles which echo works dealing with the so-called elements and with uniform stuffs; 'the postulation of the existence of an elephant kind' [190]; and two types of teleological principles (formal natures are ends; formal natures operate for the best).

In 'Division and Explanation in Aristotle's *Parts of Animals*' [ch. 9], Gotthelf draws our attention to a paradox: Aristotle seemed keen on reforming Plato's method of division—he does away with sheer dichotomy and uses, for instance, multiple *differentiae* simultaneously—and elaborated on this topic at some length in the *Posterior Analytics* [e.g., 2.13] as well as in chapters 2 and 3 of *De part. an.* 1. What then, wonders the author, is the role played by the method of division in the biological works proper, that is, in *De part. an.* 2–4 and elsewhere? And, if that method is used to any significant degree, what might be the connection between its application in biology and the more theoretical passages in *De part. an.* 1 and the *Analytics*? With a modesty and a frankness that are admirably displayed on several occasions in this book, Gotthelf acknowledges that he can only try to shed more light on this issue and cannot assume in this relatively short chapter the task of answering this twofold question exhaustively and definitively. After surveying several major contributions to the study of the method of division (notably by James Lennox), Gotthelf notes the importance of division at a pre-explanatory stage in Aristotelian science. The question remains, however, whether division is involved in 'unqualified demonstration'. On his reading, *De part. an.* 1.5 yields the following requirement: one is to

> explain the differences in some generic attribute (e.g., feathers) across sub-kinds of a large kind (e.g., Bird) by reference to the differences, across these sub-kinds, in the features which explain the presence of that generic attribute in the large kind. [204]

This requirement is taken to hold of much of *De part. an.* 2–4. In the conclusion to this chapter, Gotthelf demonstrates that the differentiations of the features that are supposed to be explained and of those that are mentioned in order to explain the former amount to genuine divisions. Those divisions mirror Aristotle's more theoretical discussions about διαίρεϲιϲ quite faithfully with respect to his 'reformed' method and to the functions it is supposed to fulfill. The answer to the question 'Are divisions involved in demonstration?' is, thus, a firm 'Yes'.

The title of part 3, 'Metaphysical Themes in *De part. an.* and *GA*', announces not a new topic in this book (virtually all the chapters gathered in this collection deal to some extent with the metaphysical implications of Aristotle's biology), but a more direct approach to those themes and a more sustained effort to explain them. In 'Notes towards a Study of Substance and Essence in Aristotle's *Parts of Animals* II-IV', Gotthelf reminds us about the intrinsic importance of exploring the metaphysical content of biological works like *De part. an.*, and about the possibility that such studies could put us in a better position to explain a number of complicated issues emerging, e.g., from *Metaphysics* Z. The task at hand is limited to answering a question about Aristotle's biology, especially his *De part. an.* 2–4: 'With what conceptions of substance and essence, if any, does Aristotle operate in the biology?' [217]. The bulk of this chapter is a succinct but illuminating commentary on 10 passages—nine from *De part. an.* (mainly from book 4) and one from *Progression of Animals*—containing partial definitions. These are the passages where Aristotle either contends that a particular feature belongs to the οὐϲία or λόγοϲ τῆϲ οὐϲίαϲ, and so forth, of an organism of a certain kind or makes more general methodological statements involving references to substance or essence. The 'interim conclusions' focus on the wide range of formulations used to express the definitional relation between some feature and the οὐϲία of a certain type of organism, as well as on the content of those partial definitions (covering soul-functions, uniform and non-uniform parts, and the 'chemical' composition of a certain kind of organism) and on the different levels of generality at which the definition is situated, no level (e.g., the *infima species*) being granted a privileged status.

The second panel in this diptych is a chapter on 'Biological Provenance. Reflections on Montgomery Furth's *Substance, Form and Psyche: An Aristotelian Metaphysics*'. Here we are invited to consider the plausibility of Furth's strong claim that the theory of material substance as articulated in the central books of the *Metaphysics* has its ultimate source in (and was initially meant as a 'deep theoretical foundation for') his biological theories. Following a historical survey intended to underline the radical, nay 'reactionary', nature of Furth's position, Gotthelf evaluates its accomplishments as well as its somewhat surprising shortcomings. Furth resorts to a sort of intuitive argument that the overall landscape of Aristotle's biological corpus points to the origin of some of Aristotle's presumably later, more 'metaphysically' formulated concerns with material substance. Gotthelf is sympathetic to the general direction of this argument but justifiably deplores the absence of a clearer, more concrete, and deliberate examination of textual evidence for that strong claim. His own contribution to this discussion is probably clearest on pages 250–251, where he notes that 'the very same irreducibility that underwrites Aristotle's natural teleology underwrites his theory of substance' (cf. the first part of this collection). In other words, a formal nature or an animal's 'mode of action as a whole' cannot be fully explained in terms of the δυνάμειϲ of an organism's material constituents. This chapter ends with a qualified encomium: perhaps Furth did not sufficiently bolster his strong thesis but he did mount a vigorous and effective defense of the weaker claim, namely, that the study of the biological corpus can help us to understand better some of the dominant and sometimes intractable aspects of Aristotle's metaphysics, as treated, e.g., in the central books of the *Metaphysics*.

The protagonist in part 4 is *Historia animalium*. Chapter 12 ('Data-Organization, Classification, and Kinds: The Place of the *History of Animals* in Aristotle's Biological Enterprise') deals with a set of baffling questions pertaining to this massive treatise:

- What are its goals?
- Is it (mainly) concerned with a classification of animals?
- Are the kinds invoked in *Hist. an.* systematically embedded in such taxonomy?
- Does *Hist. an.* reflect precepts central to Aristotle's theory of science?

The long debate surrounding the functions of *Hist. an.* within Aristotle's biological corpus was fueled in part by the perplexing organization (or apparent lack thereof) of the vast number of observations and correlations that one can find there. A succinct history of attempts to determine the goals of *Hist. an.* culminates with a series of extensive comments on David Balme's approach.

Balme rejected the notion that *Hist. an.* displays a systematic classification of animals. The 'very large kinds' (μέγιϲτα γένη)—fishes, birds, and so on—do not constitute an exhaustive list and there are no obvious intermediate kinds forming genuine natural kinds. The core of this chapter is an outline of Balme's positive view of the goals of *Hist. an.*, a view that has been confirmed and substantially enriched more recently by Gotthelf, Lennox, and others. Aristotle's detailed account of διαφοραί makes it possible for Aristotle to delineate 'significant groupings of *differentiae*', which in turn are a necessary condition for implicit or explicit causal explanatory accounts. This connection echoes tenets of Aristotle's theory of science and marks

> a two-stage progression...that first reaches a knowledge that (ὅτι) an attribute is possessed by a subject kind and then moves from there to knowing why (διότι) it does. [278]

Gotthelf continues by elaborating on suggestions by Charles and Lennox that, although *Hist. an.* is not an attempt to build a proper classification of animals, it may still provide evidence for how the μέγιστα γένη could be established); and on their epistemological implications.

This chapter, the text of a two-part seminar, is accompanied by a short appendix, an excerpt from a paper written with Pieter Beullens ('A Case for the Ordering of the Books of *Hist. an.* 7–9 and a Question about the Biological Study of Man that Arises Therefrom') which argues that the ordering offered by the manuscripts is preferable to Theodore Gaza's reordering.

Chapter 13 ('*History of Animals* I.6 490b7-491a6: Aristotle's *megista genē*') reads like a natural continuation of chapter 12 and, in some way, it is an expanded version of a qualification made there: although establishing animal kinds is not the principal aim of *Hist. an.*, it was nonetheless on Aristotle's mind when he wrote what we refer to today as *Hist. an.* 1.6. The passage appears to be an important methodological statement but its elliptic style and the potentially confusing transitions between its three main sections make the interpretation all the more arduous. Part of what is going on there has to do, Gotthelf suggests, with Aristotle's addition of two groups which did not have any names consecrated by tradition (live-bearing four-footed animals and egg-laying four-footed animals) to a list of seven μέγιστα γένη 'already accepted from common language' [299]: birds, fishes, cetacean, hard-shelled, insects, and so on [see 490b7–14]. The last sections of this chapter focus (in the context of the division of four-footed animals into two very large kinds, live-bearing and egg-laying) on the nature of sub-kinds or intermediate kinds.

The third chapter in part 4 ('*Historiae I: Plantarum et Animalium*') is a comparative study which contends inspiredly that Theophrastus' *Historia plantarum* is basically modeled after Aristotle's *Hist. an.* and that Theophrastus was mindful of the goals of *Hist. an.* when working on his own treatise. The analysis is confined to *Hist. plant.* 1 and *Hist. an.* 1–4. In the section about *Hist. an.*, Gotthelf sums up Balme's (re)interpretation and Lennox's 'amendment' (accepted by Gotthelf), which essentially connect the organization of data in *Hist. an.* and (implicitly) its discussion about animal differences with Aristotle's causal explanations of the natures of animals (a project actually carried out in other biological works) as well as with his theory of demonstration as outlined in the *Posterior Analytics*. In the section on Theophrastus' *Hist. plant.*, Gotthelf presents and evaluates the striking or subtle similarities between the structure of (portions of) *Hist. plant.* and of *Hist. an.*, and also between the criteria used for the division of *differentiae* in the two *Historiae*. Theophrastus' treatment of μέγιστα γένη like trees and herbaceous plants may mirror the way in which very large kinds are circumscribed in *Hist. an.* but seems 'less sure', possibly because it betrays his inability 'fully to master the material, shaping it to the intended structure, or an inability fully to master the theoretical structure itself' [329]. To return to the goals of *Hist. plant.*: as Gotthelf argues convincingly, it is not a mere natural history where the material is arranged according to types of plants; rather, it is 'a collection, an analysis, by *differentiae*' [333], much like *Hist. an.* However, there may be more to *Hist. plant.* than one might be tempted to acknowledge after a cursory reading of its nine books. Based on his close analysis of passages from *Hist. plant.* as well as from Theophrastus' *Metaphysics*, Gotthelf suggests that the ultimate purpose of *Hist. plant.* is very likely the discovery of causes—and this is reminiscent of the ultimate goal of *Hist. an.* Both treatises set forth the *differentiae* that mark out kinds of animals or plants, while also aiming to provide the foundation for a causal explanation of the natures of living beings. If *Hist. plant.* was indeed modeled after *Hist. an.*, and if our author's interpretation is correct, this further supports the approach to *Hist. an.* defended by Balme, Gotthelf, and Lennox.

The fifth part ('Aristotle as Theoretical Biologist') could very well serve as a double introduction to the whole book, albeit it is placed at the end. Readers who are familiar with Darwin and want to learn more about earlier episodes (and their reception) in the history of the life sciences, may want to read chapter 15 first. Alternatively, readers who want to get first a bird's-eye view of some of Gotthelf's most influential views pertaining to Aristotle's biology, will find the last chapter (the coda) to be a particularly clear and helpful guide.

The chapter entitled 'Darwin on Aristotle' was motivated, as the author confesses in the preface, 'by a strong streak of hero-worship' [ix]. On 22 February 1882, Darwin wrote a letter to William Ogle, who had recently translated *Parts of Animals* and had sent him a copy of the book. Gotthelf's polemic is directed at those who attempt to demonstrate that Darwin's overt enthusiasm at realizing Aristotle's contribution to the birth of biology is but a polite expression of gratitude for Ogle's generous gesture. As Gotthelf points out, though, this is a second and unsolicited letter to Ogle, and so is likely to give the measure of Darwin's genuine admiration for an important section of Aristotle's biological corpus, admiration that—our author speculates plausibly—may have been based to some extent on his partial grasp of Aristotle's treatment of teleology.

The coda to this book, 'Aristotle as Scientist: A Proper Verdict (with Emphasis on His Biological Works)', is based on a paper that Gotthelf presented on various occasions between 1987 and 2001. It is chiefly an *apologia* meant to allow a wide audience to become properly acquainted with Aristotle's biological works and with their philosophical significance. This chapter effectively dismantles traditional misunderstandings such as that Aristotle is an armchair theorist, indeed a non-scientist, that he qualifies as a pre-scientist, and that he is a scientist who distinguished himself as a careful observer of nature. This third claim is not fundamentally wrong but it is clearly insufficient. The accuracy of Aristotle's observations is revealed here by a brief discussion of several memorable examples (including the catfish and the octopus). While those observations and insights—some of which were confirmed only rather recently—are themselves impressive, the larger scientific and philosophical project underlying the main biological treatises (as well as *Progression of Animals*, *Motion of Animals*, *On the Soul*, and *Parva naturalia*) was 'to understand what animals are (i.e., what features they have), and why they are as they are' [379], and to organize a vast amount of data in a way that will make such understanding and explanation possible. The remaining segments of this chapter are succinct overviews of cardinal aspects of Aristotle's biology, such as the empirical approach that permeates his treatment of final causation and the implicit axiomatic structure of his scientific explanations. Thus, the coda gives new prominence to several major topics explored throughout the book and strengthens the symphonic unity of this comprehensive investigation. It is also a convenient reminder that these are Gotthelf's crucial contributions to this field; indeed, the author himself notes in the preface that

> ...I view my interpretation of Aristotle's natural teleology, and my account of the broadly axiomatic structure of biological explanations, as my most important work. [ix]

Gotthelf's readers—whether they are just embarking on the study of Aristotle's biological theories or whether they are already accomplished scholars in this field or are more generally interested in ancient philosophy—will find in this collection an eminently reliable and illuminating study of one of the most fascinating episodes in the history of science and philosophy.

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This volume collects a number of essays on Seneca and the natural sciences, many of which were first presented at a seminar held at Ravenna in 2008. All were later revised and further developed for publication, and some new ones have been added. The book presents a special interest in that Seneca's scientific writing is dealt with from different standpoints and with different approaches. This entails the collaboration of different branches of learning, which include not merely the traditional literary and philosophic approaches but also perspectives belonging in fields that can be associated in various ways with the history of science. The university of Bologna in general, and the editors of this volume in particular, have proved well-deserving of our praise in promoting this fruitful approach to ancient scientific texts. This volume marks a new milestone along a path that has already arrived at some important results, and will hopefully be followed by further contributions in the study of the scientific texts of antiquity.

In the first essay, 'Il concetto di legge naturale in Lucrezio e Seneca' [1–17], Marco Beretta analyzes the concept of natural law, not merely in Lucretius and Seneca but from Democritus down, on the basis of the use of juridical terms («νόμος», 'lex', 'foedus', 'ius') variously applied to natural processes, and its influence on the idea of 'natural law' in modern science. The main focus is the Stoic conception of 'natural law' as it is related to and, to a certain extent, identified with the divine reason ruling the cosmos—an idea later accepted by the Christians, who added the notion of 'miracle' as God's exceptional intervention temporarily changing or suspending the natural laws that he has established.

The opposite view is represented by the Epicureans and is illustrated in conjunction with Lucretius' use of such expressions as 'foedera naturai'

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[1.586], 'fati foedera' [2.254], 'aevi...leges' [5.58], and the like. Any theological or metaphysical connection is, of course, excluded: all natural phenomena are caused by the atoms moving in the vacuum and are regulated by rules inherent in the natural processes themselves (*foedera*) rather than by laws dictated by an external power; yet all occur inescapably within the frame of temporal succession (*aevi leges*). Contingency and necessity somehow coexist in Lucretius' universe. Beretta's main contribution here lies in calling attention to the slight changes taking place over time in such phenomena as human evolution [book 5], which he connects with the Epicurean doctrine of the atomic swerve (*clinamen*). According to him, then, Lucretius does not uphold the fixity and immutability of natural processes.

Only one page and a half are devoted to Seneca. After an initial remark about the 'Lucretian' spirit of the *Naturales quaestiones*, aiming to deliver men from superstition (for which a reference to Italo Lana's still fundamental *Lucio Anneo Seneca* [1955] might have been in order), Beretta analyzes some instances of Seneca's use of 'lex' and 'ius' in connection with nature. His conclusion is that, though Seneca accepted the Stoic idea of natural law as stemming from a divine and providential mind, he sometimes associates it with the simple task of explaining some regular occurrences in natural phenomena. It can be said that, to a certain extent, he concurs in this with Lucretius and that the two contributed to the elaboration of the concept of natural law in modern science in which all metaphysical references have disappeared and natural law is conceived of as the common principle underlying a definite set of phenomena.

This essay offers a fascinating panorama of the evolution of a basic scientific concept, though rather cursorily for reasons of space and approach (the study of the actual appearance of juridical terms in relation to nature). The treatment of Seneca in particular would have benefited from a wider outlook. Also, some statements ought to be qualified. For example, though Cicero did, as remarked by Beretta [5-6], uphold the rationality of the cosmos, he was far from accepting all the consequences drawn by the Stoics from the alleged interconnection of all its components, as is clearly demonstrated by his attitude to divination [cf., e.g., Setaioli 2005, 241–263].

Piergiorgio Parroni, in his paper 'Il linguaggio «drammatico» di Seneca scienziato' [19–29], offers an inquiry into Seneca's use of poetic quotations and allusions as a tool to win the reader's emotional involvement in the investigation of nature—or rather, a sample of an inquiry still largely to be carried out. He first illustrates a case [*Nat. quaest.* 7.10.1] in which the quotation of Ovid, *Met.* 2.71 is accompanied by several hints at the same Ovidian passage in the immediate context. He then goes on to point out what he terms 'hidden quotations' in Seneca's text. He only dwells on a few passages, which he analyzes with unquestionable flair for poetic traces. For example, at *Nat. quaest.* 1.3–4, he convincingly points out the influence of a famous Ovidian passage, *Met.* 4.121–124 (Pyramus and Thisbe), and the full import of the subsequent textual quotations from *Met.* 6.65–67 (the multicolored fabric woven by Arachne) to describe the rainbow. So, he contends, when Seneca in the immediate context appeals to *pictura* in relation with the rainbow ('in picturae modum'), this must refer to the colors and figures in a tapestry rather than to a painting. I should like to point out that Parroni's interpretation is supported by a passage in Aristotle's *Meteorologica* 3.4.375a23 ff. Not only does Aristotle refer to tapestries and embroidery, he does so in order to illustrate the difficulty of clearly perceiving the fading of one shade into the other in the rainbow—the same effect that Seneca, who certainly knew Aristotle's work, chooses to illustrate through Ovid's words: *ut ait poeta*.

At times, when an expression is used in its proper meaning by Seneca, it may be unsafe to take it as a 'hidden quotation' of a poetic passage in which it is used metaphorically,[1] as maintained by Parroni; but the assumption is supported by Seneca's frequent use of *Met.* 15 and is well illustrated by him. The influence of Lucretius' section on earthquakes [*De rer. nat.* 6.567: *tantam terrarum...molem*] appears probable in itself, since it refers to the same phenomenon. But Parroni points out a whole web of reminiscences of this Lucretian passage in Seneca's work.

Sometimes, however, one may proceed too far along this way. According to Parroni, 'portenta vincimus' [*Nat. quaest.* 1 *praef.* 5] is reminiscent of 'portenta perempta / si non victa forent' [Lucretius, *De rer. nat.* 5.37–38]. He is certainly right when he contends that Seneca's text needs no correction and should be understood in the sense that conquering the passions (the monstrous *portenta*) is not enough to attain virtue; but, since in Lucretius the *portenta* are the real monsters conquered by Hercules, one must suppose a shift in meaning effected by Seneca. This is by no means impossible. But it is surely difficult to accept Parroni's suggestion that Seneca read 'vincta'

[1] *Nat. quaest.* 6.4.1 *tanti molem ponderis* ≈ Ovid, *Met.* 15.1 *tantae pondera molis*.

(‘chained’) in Lucretius—the reading given by the two main manuscripts, the Oblongus and the Quadratus—rather than ‘victa’ (‘conquered’) found in the Itali, which, as Parroni himself concedes, is undoubtedly the correct reading in Lucretius. He proposes to take Seneca’s ‘vincimus’ as the first person plural of ‘vincio’ (‘vincīmus’: ‘we chain, restrain the passions’) rather than of ‘vinco’ (‘vincĭmus’: ‘we conquer the passions’). In his edition too [2002, 11], Parroni translates ‘incateniamo dei mostri’. However, what comes immediately before in Seneca, ‘superiores sumus’, surely anticipates ‘vincĭmus’.[2] In the very passage that Parroni adduces to support his interpretation, ‘quo maior nulla victoria est, vitia domuisse’ [*Nat. quaest.* 3 praef. 10], taming the vices is presented as a victory—more: the greatest possible victory. In my opinion, then, Seneca is speaking of conquering the passions, not of chaining or constraining them.

In the next essay, ‘Originality and Independence in Seneca’s *Naturales quaestiones* Book 2’ [31–47], Harry M. Hine analyzes some of Seneca’s ways of bestowing a Roman stamp on his philosophy of nature on the basis of the second book of the *Naturales quaestiones*.

He starts by pointing out that Seneca has inserted a lengthy discussion of the Etruscan divination from lightning—i.e., a native Italian subject matter—in a book on the physics of this phenomenon as previously investigated by the Greeks. This is of course true but it is not entirely correct to state, as Hine does [34], that in his treatment Seneca makes no room for the philosophical problems posed by divination that one would expect to find discussed in a Greek treatise. Quite the opposite: at the very beginning of this section [2.32 ff.], we find a philosophical discussion culminating [2.38] in the opposition between predestination and free will—one of the thorniest problems of Stoic philosophy—which Seneca tries to solve by resorting to Chrysippus’ refutation of the Idle Argument (ἀργὸς λόγος): to the objection that, if a sick man is fated to recover, he will whether he takes the trouble to call for a doctor or not, Chrysippus—and Seneca—reply that the sick man is fated to call for a doctor as well [see Setaioli 2014a].

Hine then emphasizes the fact that Seneca carries out his discussion on the basis of an original Latin terminology rather than a rendering of Greek terms. Hine does recognize that Seneca’s distinction between ‘fulmen’ and ‘fulgur’

[2] Cf. *Nat. quaest.* 4b.13.1, where ‘superior futura est’ anticipates ‘vincat’.

is his way of rendering the one existing in Greek between two words coming from different stems, «ἀστραπή» and «κεραυνός»; but the deeply Stoic (and Greek) ideas underlying Seneca's discussion of the archaic form 'fulgĕre' and the more recent 'fulgēre' [2.56.2] seem to escape Hine. Like the Greek Stoics, Seneca stresses the gradual loss of the close correspondence between language and reality in the passage from ancient forms to more recent speech: the archaic 'fulgĕre', with its short vowel, mirrored the swiftness of lightning much more closely than 'fulgēre' [cf. Setaioli 1988, 39 and n123].

Hine then tries to explain why, if the third book was originally the first of the *Naturales quaestiones*, the second (which by this reckoning would be the last one) opens with the fundamental distinction of heavenly, meteorological, and earthly phenomena that one might expect at the beginning of the work, followed by a treatment of *spiritus*, which may have been in place in all books treating atmospheric phenomena. He suggests that Seneca, as in the *Epistulae morales*, addresses his teaching to beginners and starts, therefore, with visible phenomena, reserving the theoretical principles to the end. Hine has undoubtedly grasped one of Seneca's most conspicuous traits: he is a teacher addressing pupils who need to be instructed. It is rather difficult, however, to equate a physical treatise (however unsystematic) with the philosophical and ethical project traced in the letters, which will necessarily start with admonition and only after the pupil's 'conversion' permits appeal to his reason and imparting Stoic philosophy's theoretical foundations [cf. Setaioli 2014b].

Hine then goes on to the most important point of his essay. In connection with 2.21.1, where Seneca states that from this point on he will dismiss his teachers and start moving on his own, he remarks that the philosopher is not claiming complete originality but rather independent thinking, implying critical appropriation of the wealth of earlier thought, which is also the necessary condition for any real progress of science. This is absolutely true. Seneca's statement at 2.21.1 only marks the return to his Stoic source influenced by Aristotle after the previous doxographic insertion [cf. Setaioli 1988, 395–396]. But I cannot but completely agree with Hine's view concerning Seneca's idea of independent thought, which exactly corresponds to my contention in a paper first published in *Aufstieg und Niedergang der römischen Welt* [Setaioli 1985, 849–856] and then collected and updated in a book on Seneca [Setaioli 2000, 111–217, 397–408, esp. 206–215]. In this paper, I pointed out in much greater detail how Seneca developed this idea from the rhetorical theory of imitation. Regrettably, it seems to be unknown to Hine but the concurrence of the conclusions does amount to support of the correctness of our results.

Finally, Hine discusses a scientific statement by Seneca which may indeed be his own since it is presented by him as his own conclusion: the reason why wine congealed by lightning is poisonous after it is melted down again [2.53.1–2: cf. 2.31.1]. As no other ancient writer known to us alleges that wine can be congealed by lightning, Hine suggests that Seneca may have confused two different phenomena described as noteworthy by ancient authors: the evaporation of wine caused by lightning and its freezing in cold weather. Be that as it may, Seneca is merely offering a plausible (*veri simile*) explanation of the phenomenon: the presence in lightning of a *vis pestifera*, allegedly recognizable in other phenomena too, which is left behind in the congealed wine. In other words, lightning poisons wine because...it is poisonous! A rather obvious truism.

Seneca's real contribution, clearly, is not in his originality in the modern sense but in his nonetheless modern idea of how reading and culture contribute to the molding of an independent mind which will be able to proceed farther on the path traced by the great men of previous ages.

Francesca Romana Berno, in a paper entitled 'Non solo acqua. Elementi per un diluvio universale nel terzo libro delle *Naturales quaestiones*' [49–68], examines the description of the cosmic flood closing the third book of the *Naturales quaestiones* in the light of the doctrine of the transformation of the four elements into one another that was previously presented in the same book [ch. 10]. The flood is brought about, mainly, by earth's transmutation into water, which disrupts the cosmic balance, thus causing the return to primeval chaos and the end of the world as we know it. The widespread doctrine of the transformation of the elements, Berno points out, had also been put forward by a poet not too far in time from Seneca, whom the philosopher knew very well: Ovid, in Pythagoras' long speech in the 15th book of the *Metamorphoses*. She then proceeds to illustrate Ovid's presence in the third book of the *Naturales quaestiones* by discussing the numerous quotations from, as well as allusions to, the *Metamorphoses*. In particular, before the description of the flood, Seneca [3.20.3–6, 3.26.4] resorts to quotations from Pythagoras' speech in Ovid's 15th book in order to exemplify—and support with the poet's authority—some unusual phenomena connected with water. Here, then, Ovid is used as a reliable scientific source. Things change in Seneca's description of the flood [3.27.19–28.2], where he quotes Ovid's treatment of the same subject in the first book of the *Metamorphoses*. Here the emphasis is on the literary aspect: though Seneca calls Ovid *poetarum ingeniosissimus*, he criticizes him for marring an otherwise powerful description with petty and irrelevant details. One might perhaps have stressed the fact that this criticism may have been prompted, besides by personal emulation in the treatment of a similar subject, by the different levels (mythological and scientific-philosophical respectively) of Ovid's and Seneca's descriptions. One might have equally remarked that the latter's criticism [2.27.14 *lascivire*] of Ovid is in keeping with his own father's [Seneca, *Rhet. contr*. 2.2.12] and Quintilian's [*Inst*. 10.1.88 *lascivus*, 10.1.98], and that it is based on the principle of what is fitting (τὸ πρέπον) [*Nat. quaest*. 2.27.15 *quid deceat*].

Berno concludes that in the description of the flood Seneca may have intended to go beyond Ovid's treatment of the subject in the first book of the *Metamorphoses* by resorting to the 'scientific' picture sketched by Ovid himself in his 15th book concerning the transformation of the elements into one another and, particularly, of earth into water. In other words, he may have wished to correct Ovid the mythological poet through Ovid the 'physicist', to put it in terms that we have suggested before.

It should be added that Berno conducts her argument with extreme clarity and an admirable command of all the relevant literature.

In 'Le piene del Nilo nelle *Naturales quaestiones* di Seneca' [69–80], Pasquale Rossi offers a mere report of the doxography concerning the floods of the Nile contained in the surviving part of book 4a of the *Naturales quaestiones* and in the summary of the lost part given by Ioannes Lydus in his *De mensibus*. Rossi's paper may be supplemented with the next one by Daniele Pellacani, 'Le piene del Nilo. Nota bibliografica' [81–92], which offers a survey of the bibliography on the main aspects of book 4a. Both papers are useful as collections of material but contain no original contributions.

Arturo De Vivo's 'Seneca e i terremoti (*Questioni naturali*, libro VI)' [93–106] investigates the structure of the sixth book of the *Naturales quaestiones*. After observing that Seneca here presents the study of nature as a return to earlier interests [6.4.2] rather than as a breach with his past pursuits, as he has done in a previous book [3 *praef.* 1–2], De Vivo stresses the symmetrical arrangement of the various parts of the book. The proemial section and the epilogue, roughly equivalent in extent, serve as a frame enclosing a doxography on earthquakes and their causes. They both refer to a contemporary event—the recent earthquake in Campania—and both are ethical and admonitory in character. Together they make up well over a third of the whole book and take up a space that is well over the half of the physical section proper, which they encompass. The quotations from Virgil are also evenly distributed: the proem and the epilogue have one each, while there are six in the doxography in between. Two historical excursus are inserted in the doxography—also symmetrically, that is, roughly at the same distance, respectively, from the beginning and the end of this section. One deals with Nero's expedition to the source of the Nile, flatteringly presented as prompted by the emperor's love for truth [6.8.3–5]; the other is introduced in reference to a quotation from Callisthenes and bitterly condemns his murder at the hands of Alexander the Great [6.23.2–3].

De Vivo contends that the two excursus interact with each other and that the reference to Nero should be read in the light of the negative judgment passed on Alexander. He refers to a passage in the 10th book of Lucan's *Pharsalia* in which Caesar expresses in words[3] undoubtedly reminiscent of Seneca's praise of Nero in the first excursus[4] his strong desire to learn about the source of the Nile. As De Vivo points out, Lucan [10.272] also pairs Caesar with Alexander, whom he condemns as strongly as Seneca. This contention is the main point made by De Vivo's paper. Though it must necessarily remain a hypothesis, it is undoubtedly well argued and plausible.

Francesco Citti, in 'L'opzione della scienza. A proposito di Seneca, *De otio* 4,2' [107–117], after an exhaustive survey of the interpretations of the expression 'mari ac terris inserta'[5] at *De otio* 4.2, accepts Dionigi's explanation: lands contained in the sea (such as islands) and waters contained in the land (such as rivers and lakes). By referring to a wealth of texts attributable to, or influenced by, Posidonius, he then suggests that Seneca meant to prompt

[3] Lucan, *Phar.* 10.188–189 *cum tanta meo vivat sub pectore virtus, / tantus amor veri.*

[4] *Nat. quaest.* 6.8.3 *ut aliarum virtutum ita veritatis in primis amantissimus.*

[5] This is Erasmus' text. Manuscript A has *maria a terris*, while R and V have 'emaria terris'.

the would-be investigator of nature to reflect on the interweaving of land and water which creates the different continents.

The last three essays deal with the reception of the *Naturales quaestiones*.

Hiro Hirai, in 'Seneca's *Naturales quaestiones* in Justus Lipsius' *Physiologia Stoicorum*: The World-Soul, Providence and Eschatology' [119–142], offers an interesting survey of the way in which Justus Lipsius uses Seneca's work in his *Physiologia Stoicorum*. Hirai systematically analyzes all of Lipsius' 53 quotations of the *Naturales quaestiones*. He points out that Lipsius uses Seneca in his effort to harmonize Stoicism and Christianity, not rarely forcing the Roman philosopher's positions. As Hirai clearly shows, the parts of Seneca's work that Lipsius regards as most significant, and repeatedly quotes are: the preface to the first book, chapter 45 of the second book, and the description of the deluge (and universal conflagration) at the end of the third book [3.27–30]. He uses the first of these texts to stress the ethical ends of the investigation of nature, to confirm the existence of providence, and to identify God with the World-Mind, that is, with an incorporeal creator—a conception at odds with authentic Stoicism. Lipsius draws the same conclusions from 2.45, which lists the names which can be applied to God in accordance with Stoic pantheism. Finally, he uses the third Senecan text in order to illustrate his own conception of the end of the world. A useful appendix lists the titles of all chapters of the three books of the *Physiologia Stoicorum* and all the passages from the *Naturales quaestiones* quoted by Lipsius.

Bardo Maria Gauly's 'Aliquid veritati et posteri conferant: Seneca und die Kometentheorie der frühen Neuzeit' [143–159] offers an extremely interesting survey of the reception of book 7 of Seneca's *Naturales quaestiones*, containing his theory on comets, by such astronomers and scientists of the early modern age as Tycho Brahe, Galileo, Kepler, and Libert Froidmont (Libertus Fromondus). Though Tycho Brahe had proved that comets must be placed beyond the Moon and revolve around the Sun (and cannot, therefore, be regarded as atmospheric phenomena, as maintained by Aristotle and denied by Seneca), he still considers them to be accidental and temporary. Galileo is even less innovative in that he admits regarding comets as phenomena either in the atmosphere or in outer space as equally defensible. Even Kepler takes comets to be temporary and, like Tycho Brahe and Galileo, does not accept Seneca's idea that they move along established, though yet unknown, orbits. Froidmont, though he develops his theory of comets in close connection with Seneca's, is closer to Aristotle; while he admits that certain comets move in the space beyond the Moon, he maintains that others do form in the atmosphere.

In the seventh book, Seneca admits in passing [7.2.3] that the heliocentric system of Aristarchus of Samos might be accepted—he is probably refering to the revolution rather than the rotation of the Earth—and Froidmont connects this passage with the Copernican system, though he was closer to Tycho Brahe's model of the cosmos.

Though Seneca rejected the vulgar connection of comets with contingent situations and political upheavals, he nevertheless, like a good Stoic, considered them to be part of the universal system of signs produced by cosmic 'sympathy' [7.28.2]. But for his repeated mention of the comet of AD 60 as portending happiness under Nero's reign, he was scathingly criticized by Kepler.

The most frequently quoted part of the book is the prophecy that what is still unknown (like the comets' orbits) will be discovered in the future [7.25.4–7: cf. 7.30.5–6]. Froidmont distances himself from Seneca, doubting that his prophecy will ever come true; but Kepler—though still rejecting the idea that comets follow a regular orbit—presents himself as one of the scientists who are contributing to fulfill Seneca's prediction and places a quotation from *Nat. quaest.* 7.25.7 on the title page of his *De cometis libelli tres* (1619). Seneca's prophecy was indeed regarded as the link between his theories and the discoveries of the early modern age.

In the last and longest essay, 'Per una rassegna sulla fortuna delle *Naturales quaestiones*' [161–252], Fabio Nanni and Daniele Pellacani offer a systematic survey of the reception of the *Naturales quaestiones* from antiquity to the 20th century. The first three sections ('L'antichità', 'Il Medioevo', 'Il Quattrocento') are by Nanni; the rest ('Il Cinquecento', 'Il Seicento', 'l Settecento', 'L'Ottocento', 'Il Novecento') is due to Pellacani. The authors have scrutinized an imposing amount of bibliography—the list takes up no less than 17 pages at the end of the essay. The survey must perforce be rather cursory in view of the nearly two millennia that it covers but it will certainly prove invaluable to anyone planning an in-depth research of the reception of Seneca's work in any given period of time.

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A Response to Nothaft on Carabias Torres, *Salamanca y la medida del tiempo*[⋆]

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As Philipp Nothaft points out in his review, the history and prehistory of the Gregorian calendar reform remains 'a gigantic map with many blank spaces, which also cover most of the Iberian Peninsula'; and indeed, prior to the publication of my book *Salamanca y la medida del tiempo*, the Iberian Peninsula was mostly covered by such blank spaces. He also credits me with the following contributions [191–193]:

(1) identifying the authors of the 1515 and 1578 Salmantinian reports on calendar reform;
(2) determining the institutional context of these reports (as well as the broader, overarching context), which is difficult to pin down for the year 1515 due to the dearth of documented sources;
(3) the establishment, transcription, and translation of scientific reports from Salamanca in answer to requests from Popes Leo X and Gregory XIII, and from the Spanish kings Fernando el Católico and Felipe II, as well as the reconstruction of texts in which 16th-century scribes or, in some cases, 19th-century researchers made mistakes when transcribing the original documents;
(4) the facsimile reproduction of ms. 97 from the Biblioteca General Histórica of the University of Salamanca, in so far as 'readers interested in the original Latin [would] greatly appreciate this edition';
(5) the analysis of new manuscript material, such as the report from the University of Alcalá and two explanations of the reform written by the Archbishop of Toledo, García de Loaysa y Girón, and Francisco Salinas, respectively—I was indeed unaware of the *Disputatio*

[⋆] See http://www.ircps.org/aestimatio/10/190-204.

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de anno... (1468) by Martínez de Osma, which professor Nothaft analyses. This new study reveals that Osma made no worthwhile contributions to solving the problem—and

(6) the use of relevant studies for the history of science in the Iberian Peninsula, some of it rarely accessed by scholars outside the Hispanosphere.

On the other hand, Nothaft objects to the weak arguments that suggest that Lilio could have drawn on the 1515 Salmantinian report. However, as I pointed out [Carabias Torres 2012, 236]:

> *Es verdad que, analizando la tradición computista desde la Edad Media, cabe pensar que Lilio pudo no haber copiado específicamente el informe salmantino de 1515 -aunque a mí me parece seguro que lo conoció y que en parte copió-, sino que quizá recogió y expuso el contenido y las proposiciones de esta tradición que arranca de Roger Bacon y pasa por Pierre d'Ailly, John de Murs, Fermín de Belleval, Paul von Middelburg, la Universidad de Salamanca, Pedro Ciruelo y seguramente otros.*

> The fact remains that an analysis of the computist tradition from the Middle Ages onwards suggests that Lilio may not have specifically copied the 1515 Salmantinian report (even though I feel certain that he was aware of its existence and that he copied it in part), and that he might have collected and outlined the contents and propositions of this tradition, which originated with Roger Bacon, followed by Pierre d'Ailly, John of Murs, Firmin of Beauval, Paul of Middelburg, the University of Salamanca, Pedro Ciruelo, and surely others as well.

Why do I feel certain that Lilio knew it and used it? Because only the Salmantinian report of 1515 is missing (or is not where it belongs) in the Vatican archives. Luigi Lilio did not belong to the Vatican commission for calendar reform; and it is, therefore, reasonable to assume that his brother Antonio, who did belong to the commission, faced with the Pope's insistence on implementing the reform as soon as possible, might have lent this document (among others) to his brother, as it was not easy to take a report out of the Vatican archives.

My initial hypothesis that Salamanca contributed decisively to the calendar reform is supported by Nothaft himself when he states that [195]:

> from this it should be clear that the only innovation the University of Salamanca could possibly lay claim to would have to concern the intercalation scheme of the Julian calendar, modified so as to reflect the more accurate year-length.

Specifically, the 1515 Salmantinian report proposed the omission of a bissextile day every 152nd year [Carabias Torres 2012, 300], showing that its authors had performed some uncommonly accurate astronomical calculations. Thus, as I stated, Salamanca provided an important contribution to the Gregorian calendar reform.

The best way of making headway in knowledge is to work first and then to correct and improve the work that has been done. Nothaft has provided us with invaluable data that give nuance to the astronomical question but there is little, I fear, that he could have added to the above-mentioned historical issues 1, 2, and 3.

Thus, not only do I forgive the youthful vehemence of a foreigner for being a little less sanguine about the result, I also thank him for his meticulous analysis in which he delves further into the participation of the University of Salamanca in the Gregorian calendar reform, a subject that has long been neglected by Spanish and foreign researchers alike. The success of my study is already tangible, as Nothaft has already quoted it twice and has used its contents once again in his latest publication [see 2013, 522, 543, 550]. On the other hand, prior to reading my study, this widely acknowledged expert in Christian chronology was only aware of the chronological studies by two Salmantinians, Alonso de Madrigal and Martínez de Osma [see 2011, 203 ff.], and did not remember the existence of such important manuscripts on the subject as the 'Tabulae ad meridianum Salmantinum', a key work for understanding the level of astrological teachings in Salamanca at the time of Martínez de Osma [Oxford, Bodleian Library, ms. Can. Misc. 27],[1] or the 'Responsum Academiae Salmanticensis SS.D.N. Gregorio XIII de compendio quoddam et reformatione Kalendarii consulente' [Fuente and Urbino 1855, 20].[2]

[1] Cited in *Illuminated manuscripts in the Bodleian Library, Oxford* [Pacht and Alexander 1966, 1.69] and commented on in studies by Chabás, García Avilés, and Chaparro.

[2] Nothaft was unaware of this despite the fact that there was already reference to it in Gallardo 1863, 1.1087; Kaltenbrunner 1881, 34; the *Inventarium codicum latinorum Bibliothecae Vaticanae* [Anonymous 1882, Nº 7049]; Schmid 1882, 395; Picatoste 1891, 67; Fernández Vallín 1893, 220–224; Alonso Getino 1907, 301; Marcos Rodríguez 1931, 299; Muñoz Delgado 1979, 135; Flórez Miguel 1999, 88; and the recent *Catálogo de manuscritos de la Biblioteca Universitaria de Salamancaz* [Lilao Franca and Castrillo González 1997–2002, 1.96].

I am also glad to have contributed to the expansion of his field of research, as his work of 2011 *Dating the Passion: The Life of Jesus and the Emergence of Scientific Chronology (200–1600)* omits Spanish scientific works on chronology in the 15th and 16th centuries altogether. He failed to mention Abraham Zacut and Jerónimo Muñoz in chapter 7, 'Time for Controversy: Catholic Chronologers and the Date of the Passion in the Fifteenth and Sixteenth Centuries' [2011, 203 ff.]. The *Almanach perpetuum* (Salamanca, 1469) by Zacut is an invaluable source for his research; the *Libro del nueuo cometa y del lugar donde se haze[n] y como se vera por las Parallaxes quan lexos están de tierra y del prognostico deste*... by Muñoz (Valencia, 1573) discusses the supernova of 1572, which was also observed by Tycho Brahe.[3]

Fortunately, after reading my book, Nothaft [2013, 531] has included information on Abraham Zacut and the 'Tabulae...' by the Salmantinian Professor of Astrology Nicolás Polonio in his latest work, though he still seems to be unaware of the existence of important manuscripts that are relevant to his studies, such as 'Comento o exposición a las crónicas o tiempos de Eusebio' (five vols.) by Tostado in semi-cursive Gothic script with marginal annotations [Biblioteca General Histórica. Salamanca. Mss. 2485–2489].[4]

It is not always easy for researchers, despite their best efforts, to be aware of all sources; fortunately, however, we can all contribute in order to increase our common knowledge and I am deeply thankful to Professor Nothaft for the instruction that he has provided us in his review.

3 Nothaft 2011 seems unacquainted with the works of Juan de Salaya (translator of Zacuto), Diego de Torres [1485, 1487, *ca.* 1520], Rodrigo Basurto [1494, *ca.* 1497], Antonio de Nebrija [*ca.* 1516], Pedro Margalho [1520], Sancho de Salaya [1536], Juanelo Turriano [1990], Pedro Sánchez Ciruelo [*ca.* 1515], Hernando de Aguilera [1554], Juan de Arfe y Villafañe [1854], Juan Pérez de Moya [1573], Diego de Zúñiga [1584], the *Tractatus Segurae Mathematicae Professoris in universitate complutensi de nova restituendi kalendarium tempore Gregorii XIII* [Anonymous 1578], for example. There is more information in Mordechai and Navarro 2006 and Navarro Brotóns 2004.

4 These manuscripts are also cited in Marcos Rodríguez 1957; Keightley 1977; Fernández-Ordóñez 2006 and 2009; Hernández González 1998; Parrilla 2002, 158; the *Catálogo de manuscritos de la Biblioteca Universitaria de Salamanca* [Lilao Franca and Castrillo González 1997–2002, 2.847], for example.

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Measuring the New World: Enlightenment Science and South America by Neil Safier

Chicago/London: University of Chicago Press, 2012. Pp. xviii + 387. ISBN 978–0–226–73362–3. Cloth $27.50, £18.00

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The cover illustration to this book shows a savant in European clothes (including cocked hat) writing while seated at a table that is precariously balanced on a narrow raft poled along by nearly naked Indians, and riding scarcely more than a few inches above the Amazon river as it rages through a narrow gorge. The savant looks down onto the page in front of him rather than around to the surrounding world of teeming nature. He and the Indians make no contact with each other, even though his life depends on their skills.

The savant is Charles-Marie de La Condamine, member of the Paris Academy of Sciences, mathematician, explorer, and surveyor; and the jacket picture contains all the major themes of this book. The context is the eight-year French expedition to Peru starting in 1735, of which La Condamine was not the leader but made himself its best-known member by his writings. For years debate had raged over the shape of the Earth. English Newtonians believed (correctly) that the Earth was an oblate spheroid; and French Cartesians, that it was a prolate, rather like an upright egg. The expedition, timed to coincide with a similar one to Lapland in 1736–1737, was an attempt to settle the question by measuring the precise length of a degree of latitude at the equator. At the time that Safier wrote this book, there was no modern history of the La Condamine expedition in English. Safier concentrates on La Condamine's writings and on the textual history of the journey, which went far to form European impressions of Amazonia, rather than on the history of the measurements performed by the expedition as a whole.

Yet this book is a highly important contribution to the expanding field of Iberian science. It fits well with the emphasis in current historiography on how knowledge in areas outside Europe was created in ways that were

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contingent on social situations and forces, and manipulated in ways that were often unavowable because they were dependent on the unacknowledged support and information of Creoles and Indians. Safier's account of the way in which the map of Quito was made fits well here. Knowledge, in this historiography, is made on frontiers such as Amazonia rather than in the European centers of Enlightenment with their institutional 'centers of calculation'—to use Bruno Latour's famous phrase.

Safier's book about La Condamine's books and papers uses insights from the current history of the book and asks how these literary artifacts, including maps, were created and circulated, and how by these means La Condamine displaced prior cartographers and travelers, even while indulging in fantasies of El Dorado and tribes of Amazons. La Condamine's negative portrayal, for example, of Amazonian indigenous people as lazy and weak withstood the protests and criticism of Creole writers from the Spanish and Portuguese colonies. It was La Condamine's account which passed into Buffon's *Histoire naturelle* and Diderot's *Encyclopedie*, the two great compilations of the age.

But what was this knowledge created on the periphery? Safier's own account shows that La Condamine was able to produce a picture of Amazonia and of his own intrepid passage through it which was remarkably resistant not only to the reality of the region but also to the local knowledge which surrounded him. The knowledge that was made in Amazonia and accepted as truth in the Parisian 'center of calculation' showed that one strategically placed European traveler could by a judicious mixture of plagiarism, neglect, and silent incorporation skew the picture of an entire region.

A Response to Peter Barker and Matjaž Vesel, 'Goddu's Copernicus'*

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The review of my study of Copernicus with extensive and careful summary by Peter Barker and Matjaž Vesel is generally constructive and edifying. By relying in part on their summary, I am optimistic that some of their questions can be answered and their criticisms met satisfactorily. Naturally, there are still other matters about which we will disagree but even in those cases we can hope for greater clarity. I am grateful to the Editor for this opportunity.

The Big Questions

I begin with the 'big questions that go unanswered' [319] just before their critical evaluation since these questions frame the criticisms that follow.

A. 'Where and why did Copernicus begin his research into heliocentrism?'

This question anticipates the outcome. The outcome followed from Copernicus' critique and rejection of geocentrism. The questions that led him to that critique had arisen already in Cracow (1491–1495); further reading and analysis brought him, probably by the end of 1509 at the latest, to the conclusion that geocentrism could not resolve the main problem that he thought astronomy should be able to resolve. I will return to this 'conclusion' later. My study [2010] addressed these issues on pages 225–229, 243–261, 285–291, 326–332, and 358–360. That my answer was not clear, however, is evident from the reviewers' question and doubts. I will summarize the argument below in the course of discussing alternative answers.

* See http://ircps.org/aestimatio/9/304-336.

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B. 'Why are there so many similarities between his work and the work of Islamic astronomers?'

The question presupposes the answer. As far as we know, Copernicus learned first from Regiomontanus' *Epitome* that Arabic intermediaries had made important observations different from Ptolemy's and had expanded the observational record in ways that necessitated changes in some of Ptolemy's sidereal, solar, and lunar models. Copernicus recognized problems with Ptolemy's lunar model and the equant while in Italy, although his teachers in Cracow had also concluded that the equant was entirely fictional. Copernicus' own observations and reading of the *Epitome* probably reinforced his doubts; but it is possible that he encountered Persian/Arabic critiques and perhaps even saw models that suggested alternative ways of solving these problems. Again, I will elaborate below. My study [2010] addresses this question on pages 154–156, 261–272, and 476–486; but the reviewers' criticisms will allow me to quote my own disclaimer and to emphasize my agreement with the claim that the Maragha hypothesis provides the most complete version of the models that Copernicus could have adopted.

C. 'Did he really select a methodology that would itself have been predictably unpersuasive to contemporaries?'

Copernicus knew that he was contradicting common sense. Without an observation to correct the perception of celestial motion and of a static Earth, and without a fully developed physical theory to support an alternative account of such perceived motion, he tried to raise questions, provoke doubts, and open minds. He had no control over openness and receptivity but he could construct an attractive alternative and he seems to have been optimistic that the vision sustaining his argument would eventually prevail. But, in the short term, he had every reason to be pessimistic. His delay and hesitation are well known and it seems that Rheticus' enthusiasm, the support of Tiedemann Giese, and the publication of the *Narratio prima* were critical in convincing Copernicus to complete and publish his work.

D. 'Although Goddu presents Plato as a key source of Copernicus' dialectical method, why should Copernicus be seen as working within an Aristotelian tradition and addressing Aristotelians, rather than working within a nascent Platonic tradition and addressing Platonists?'

Here the reviewers seem to have forgotten their own summary [308] momentarily:

> For Copernicus, the Aristotelian tradition is a long way from Aristotle [Goddu 2010, 93]. According to Goddu, we should not expect to find any defining content, commentary tradition, or school at Cracow. [95]

Some of the reviewers' generalizations here require qualification. Copernicus' relation to the Aristotelian tradition, rather than being defined by a tradition of commentary and received doctrine, was rather a relation

> to his teachers and of his teachers to the 'schools' and texts on which they drew for their interpretations of natural philosophy. [Goddu 2010, 95]

Their perception [308] that I portrayed the Cracow *milieu* as 'generally hostile to Plato' is not quite accurate. I cautioned readers about how 'scholastic philosophers modified Aristotelian doctrine under the influence of Platonically inspired arguments or questions' [135]. There is need for additional clarification here.

First, in his critique, Copernicus addressed geocentrists, whatever their doctrinal allegiances. Second, the Aristotelian tradition to which he reacted and responded was an Aristotelianism often reconciled with Platonic, Middle Platonic, and Neoplatonic critiques. The dichotomy between Aristotle and Plato is anachronistic. Copernicus' own rhetorical strategy in *De rev.* 1 guided my reconstruction. In chapters 4–7, he addresses all geocentrists. But chapters 8 and 9 are directed specifically at Aristotelians by using Aristotelian categories (such as circular *versus* rectilinear, natural *versus* violent) that Copernicus had re-conceptualized. True, Copernicus makes the terrestrial celestial but he was cautious about making the celestial terrestrial.

In short, he did address Platonists but the conceptual categories were Aristotelian; yet, these were already modified by other sources on whom Copernicus relied, including Cicero, Plutarch, Pseudo-Plutarch, and Pliny, among several others. Platonism, Middle Platonism, and Neoplatonism influenced medieval scholastic Aristotelianism deeply, as my study documents [2010, 89–136]. Johannes Philoponus and Proclus, in particular, influenced medieval Aristotelianism and there is strong evidence that Copernicus relied on Philoponus (anonymously) for his doctrine of natural elemental motion [see, e.g., Knox 2005; Goddu 2010, 396, 490–491]. Given the reviewers' strong realism concerning spheres and orbs, they should have qualified their recommendation of the study by Anna de Pace [2009]. De Pace offers some potentially significant suggestions about Copernicus' sources but it comes at the cost of an exaggerated Platonic and thoroughly anti-Aristotelian reading of Copernicus [see Omodeo 2011, Goddu 2011, De Pace and Goddu 2012]. I agree, however, that we need reliable studies of Renaissance Platonism and Neopythagoreanism and their relation to Copernicus. I made some tentative suggestions along these lines in 2010, 317–320 and I will return to them below.

The Critical Evaluation

I take up and re-order the reviewers' criticisms in the following sections as follows:

(a) overreaction to my doubts about Persian/Arabic predecessors,
(b) affirmation of total spheres but silence about partial orbs,
(c) John of Glogovia and Albert of Brudzewo,
(d) Capellan and Tychonic alternatives and the origin of Copernicus' heliocentrism, and
(e) concluding reflections about texts, sources, and speculation.

A. Overreaction

Readers who know only the review of my study may be surprised to read the following sentences from my 'Excursus on Transmission' on pages 476–486 regarding the hypothesis about Islamic predecessors:

> All of that said, scholars convinced of the hypothesis should continue to search for the intermediary link. It may yet turn out to be correct, and the fact remains that the Maragha hypothesis still provides the most complete version of the models that Copernicus could have adapted both in the *Commentariolus* and *De revolutionibus*. I would welcome the discovery for it would finally put all of the speculation, including mine, to rest. [2010, 485]

Yet, in an article cited in his and Vesel's review, Barker says:

> Goddu has now followed Rosińska in emphasizing Brudzewo as a possible origin for the Tusi device in Copernicus, in part of a general attack on the plausibility of Islamic sources for Copernicus' work. [Barker 2013, 137]

In the footnote, he refers to the pages of my study and the 'Excursus'.

Did the reviewers overlook the disclaimer quoted above? Did they judge it disingenuous? Although it was not my intention to be misleading, perhaps I did not make my points clearly enough. If so, then I add the emphasis that should clarify my meaning: I too regard the hypothesis of an Islamic source as providing the most complete and concrete reconstruction. The 'attack' (not my word) was against dogmatism. My pleas were for a more concerted search for the route of transmission and for keeping an open mind while considering other options. The neglect of my disclaimer and of some other details was not intentional or motivated by hostility, I believe, though this overreaction tends to confirm some of the concerns voiced in my excursus.

I regret these comments because the reviewers have done me the generous consideration of having summarized my study so extensively and carefully. Naturally, in such a long book, it is inevitable that they mischaracterize some of my views and beliefs. I did talk about the alleged source as possibly a 'ghost' but even I consider that possibility as unlikely. Perhaps more effort has been made to find the route of transmission than has appeared in print so far. What we need is communication about such efforts and a complete report of failures as well as of promising paths to pursue. Why has so little been reported about the provenance of MS Vat. Gr. 211? What inventories, catalogues, and collections have been examined thus far and what remains to be examined? In 1973, Swerdlow [426] asked similar questions and he seemed confident that something would eventually turn up.

In the same spirit of discussion, there are additional remaining questions about the sources that have been proposed and their similarity with Copernicus' models. I address a few of them again below.

The reviewers' criticism [327–329] of Mario di Bono's claims [1995] about the figure in the*Tadhkira* I will leave for Di Bono to answer and note only the reviewers' assumption that, whatever source Copernicus saw, he would have recognized both versions of the Tusi couple. Without knowing what Copernicus saw, read, or heard, how do we know his interpretation without assuming what we need to prove? This seems to me to be the kernel of Di Bono's analysis. Copernicus applied and elaborated the devices in ways that no other predecessor did.

I agree that there are important similarities between the models and the lettering. I also note, however, some questions. The lettering in Copernicus' autograph is similar but the figure that he drew there is less similar than the one from the 1543 edition of his work [Goddu 2010, 268 Figure 4; Barker and Vesel, 328 Figure 2]. Copernicus drew the figure in the autograph [fol. 75r] on Paper C, which he may have exhausted as early as 1525 [see Zathey 1972, 2–7; Birkenmajer 1900b, 368] but probably no later than 1528 [see Swerdlow and Neugebauer 1984, 1.88]. He or Rheticus drew the figure that appears in the edition possibly no earlier than 1542. Why is there a difference between the figure in the autograph and the one in the edition of 1543? Why was it changed and what was the source for the second one? It would appear that Copernicus may have relied on more than one source and that the second figure may have been derived second-hand from another source.

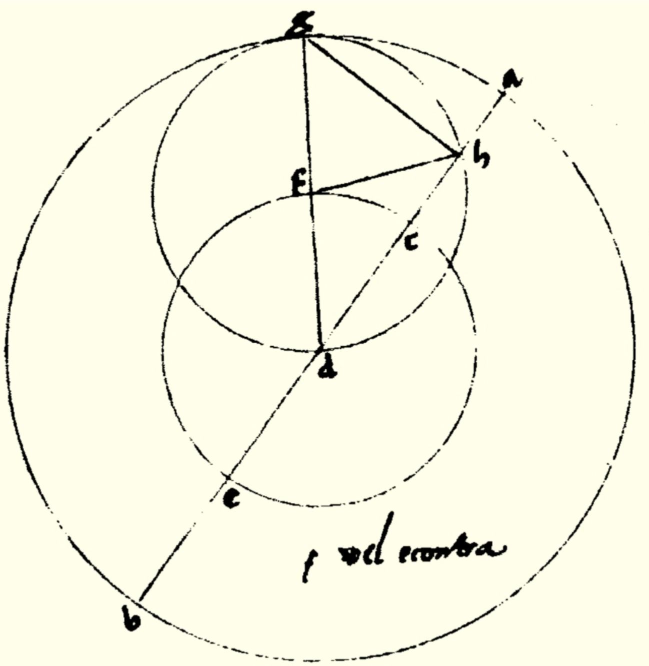

Figure from Copernicus' autograph, fol. 75r

The reviewers argue for Brudzewo's influence on Copernicus. Why, then, do they ignore Brudzewo's version of the reciprocation device that Birkenmajer, the editor of the text [1900a, 120], connected with Copernicus' so-called 'libration mechanism'? I will return to this question in the section on John of Glogovia and Albert of Brudzewo.

The reviewers rightly cite articles on Moses Galeano by Langermann [2007] and Morrison [2011] that propose another avenue of research. The Hebrew manuscript reporting the system of Ibn-al-Shatir, cited by Langermann, is from 1539 but the text itself was composed around 1500 when Galeano visited Venice; and Langermann suggests personal contact through Hieronymus Soncino's network as possible. Together with Morrison's analysis of an astronomical text by Galeano, this gives reason to search for other avenues of transmission in northern Italy connected with the circle of Regiomontanus. Aside from the additional evidence that Arabic texts or ideas in some form were in Italy, scholars may find more concrete evidence by examining early inventories, catalogues, and, best of all, by actually visiting archives and libraries and examining the contents of codices, which often contain surprises. We must also consider networks of communication, for which Domenico Maria Novara may have been helpful and which Copernicus may have utilized while in Bologna or Padua. In short, we still need more information about sources or, at least, more concrete evidence of their existence, provenance, and Copernicus' access to them. The recent efforts provide grounds for optimism. My excursus was, contrary to scepticism, a plea for more research and a plea to keep an open mind by considering alternatives.

I sympathize with the frustration expressed by Ragep [2007] over the delay in accepting the role of medieval Islamic astronomy in the European Renaissance of astronomy. But here it is important, perhaps, to recall how long it has taken for the story to develop. Some information about the Maragha achievements was known already in the 19th century. It took nearly another 100 years until the 1950s for more details to emerge and even within the last decade there have been more discoveries that hold out promise for a resolution. It was not my intention to denigrate any such hypothesis because it is not European. I am not satisfied that enough has been done to confirm the route of transmission and, however aggravating that may be to proponents of a non-European origin of Copernicus' models, I think that we should keep an open mind and consider a variety of options and multiple sources.

B. Affirmation of total spheres but silence about partial orbs

The reviewers' reconstruction [321–327] of my argument is flawed. Edward Rosen's earlier polemic was directed mostly at materially solid spheres, not spheres altogether, and it was motivated by Copernicus' silence about the penetration of spheres. Perhaps because some of his later comments are buried in footnotes, it seems that many have overlooked them. In any case, Rosen qualified his earlier assertions about the reality of spheres and orbs. In his revised translation of the *Commentariolus* [1985], he cited his earlier version and in a very long footnote [1939, 122–126n326] emphasized the problems with materially solid spheres; Copernicus' reliance on circles (*circuli*) not *sphaerae*; the misrepresentation of his [Rosen's] views about spheres as fictions with no physical existence; and the uncertainty that Copernicus found in his sources, especially Georgio Valla [1501], about eccentric and epicycle orbs. Even in his earlier version, Rosen acknowledged:

> From Copernicus' language it sometimes appears that he regarded the planet as attached to a three-dimensional sphere; but more often a two-dimensional great circle of the sphere was the geometrical figure to which he affixed the planet. [1939, 11–12]

Notice that even the second clause implies the existence of the total sphere. Likewise, in translating *De rev.* 1.4.15–16,

> Several motions are discerned herein, because a simple heavenly body cannot be moved by a single sphere non-uniformly.

Rosen commented: 'This pronouncement makes unmistakably clear Copernicus' adherence to a form of the traditional doctrine of the spheres' [1978, 348]. Rosen later again commented:

> The spheres intended by Copernicus were invisible carriers of the visible planets...in the time of Copernicus (and long before him) a visible planet was thought to be attached to an invisible sphere (*orbis*) that transported the visible planet. These invisible spheres performed the revolutions mentioned by Copernicus in the title of his *Revolutions*. [1984, 62]

It is clear, then, that Rosen did not deny spheres altogether but concluded that Copernicus' spheres were invisible and immaterial. Rosen did not elaborate further but he was evidently referring to the real total sphere in which other orbs and circles are contained. Copernicus' predecessors and contemporaries disputed the reality of the partial orbs. Copernicus himself neither affirmed nor denied the existence of partial orbs. Rosen rejected the material solidity of spheres on the ground that the descriptions would attribute terrestrial qualities to celestial entities. Later Copernicans or interpreters of Copernicus have adopted a variety of views about the celestial spheres. Some attribute solidity to them but others suggest that they are fluid or air-like and penetrable in principle.

In his article on Brudzewo's *Commentariolum*, which is mentioned in the review, Barker [2013] cites the work of Michel-Pierre Lerner [2008]; but because some of Lerner's distinctions have also been overlooked, I cite the most relevant passages here. After defining 'planetary sphere', Lerner says:

> Defined in these terms, the total spheres with their contents appear to have been considered real by a large number of astronomers, including Copernicus and Tycho Brahe (at least up to 1573). [2008, 2:3–4]

While Lerner implies the reality of the partial orbs here as well, he immediately sounds a cautionary note and adds that even those, like Albert of Brudzewo, who denied the reality of the partial orbs, affirmed the reality of the total sphere. In his first brief chapter on Copernicus [2008, 1:131–138], Lerner grants to Swerdlow the plausibility of his reconstruction of Copernicus' view and path to heliocentrism but points out several unresolved problems. For example, the Earth would have to be attached to the sphere; but air, a rare and fluid body by definition, occupies the higher region of Earth, so how could it be attached or fixed to a solid body? In traditional celestial theory, the natures of the spheres and of the celestial bodies are substantially homogeneous with the orbs moved by Intelligences. All such questions and theories do, of course, belong in the realm of natural philosophy. The point is that Copernicus did not answer questions about the nature of the spheres. He evidently made a conscious decision to leave such questions aside. Lerner returns to the problem of Copernicus' reticence and the contradictory and irreconcilable interpretations of Copernicus' spheres among 16th-century and recent interpreters [2008, 2:67–73], and adopts a different strategy in asking 'What, in the face of Copernicus' silence, is logically consistent with his new cosmological configuration?' The reviewers refer selectively to Magini, about whom more below, and Mästlin but ignore Mästlin's doubts about the nature of the orbs and neglect altogether Rothmann's unequivocal denial of the solidity of the orbs.

The reviewers acknowledge Copernicus' 'inability to choose between mathematically equivalent models' and concede that 'there was no obvious way of choosing between' alternative orb-models. They also acknowledge the incompatibility of Peurbach's reframing of the Ptolemaic system and Copernicus' dimensions. In Peurbach's version of the system,

> [E]ach set of partial orbs formed the total orb for a single planet. The total orb for one planet fitted perfectly inside the total orb for the next planet out, with the fixed stars forming a boundary to the whole system. [326–327]

But they overlook the first problem. In both Capellan and Tychonic arrangements, the orbs for Mercury and Venus are enclosed in the Sun's orb. In addition to the gaps between the orbs and the enormous gap between Saturn and the fixed stars, Copernicus' system requires abandoning the principle that each distinct motion requires a separate orb and supposing, in the case of the Moon and Earth, that the two share a total orb. In other words, for Copernicus, the Earth's total sphere carries the Moon's orbs with it around the Sun.

Lerner also points to several consequences of Copernicus' cosmological vision that are clearly inconsistent with the traditional theory of spheres and orbs. I take Lerner's point to be that Copernicus set in motion a process that contributed to questions about spheres and orbs, and I concluded [2010, 370–380, 384–386] that his retention of the total spheres to which planets are attached provides the most conspicuous evidence of his adherence to the Aristotelian tradition while otherwise remaining silent about the nature and movers of the spheres.

The reviewers criticized my claim that Sacrobosco's *Sphere* is 'of almost no practical use' and ask 'no use to whom? Practical for what?' [321]. The reviewers answer their own objection: 'It does not really teach astronomical calculations'. In other words, it is of no practical use to astronomers for calculating past or future positions. As for the *Theoricae novae* and their criticism of my claim, 'The traditional accounts of orbs never make it clear how the orbs are consistent with the mathematical models' [2010, 378], the reviewers' own illustration from Magini [322, Figure 1] highlights the problem.

This figure is clearly not constructed to scale and requires the interpretation of circles that are not spheres. Those problems to the contrary notwithstanding, I was mistaken in claiming that the mathematical models entail the penetration of orbs. A combination of orbs with the mathematical models is possible without penetration. Magini's 'Scheme of orbs, and of the centers of the sphere of the Moon' illustrates such a possibility. The assumption that Magini's scheme is compatible with the Copernican theory, however, involves a sleight of hand.

To reconcile Magini's scheme with Copernicus' lunar model [*De rev.* 4.3], we have to assume that the entire orb centered on epicycle orb E rotates counterclockwise on circle D. Epicycle orb F, however, rotates clockwise. Now, in Copernicus' scheme the epicyclet (the upper epicycle) rotates clockwise because the center of the epicyclet is on the circumference of the epicycle whose radius vector rotates clockwise. In other words, the epicyclet rotates clockwise because the radius of the lower epicycle is rotating clockwise as the deferent radius rotates counterclockwise.

So, what in Magini's scheme causes epicycle orb F to rotate clockwise around epicycle orb E? Suppose that the circumference of epicycle orb E rotates clockwise. How, if at all, can it cause epicycle orb F to rotate unless they are connected somehow? Here is the sleight of hand to which I referred earlier. Copernicus' epicyclet radius rotates counterclockwise, entailing that the radius of Magini's epicycle orb F carrying the Moon is also rotating in the same direction contrary to the clockwise rotation of its center.

There cannot be a real physical connection between the geometry and the orbs, so what causes the orbs to move in the way that they do? A mathematical description provides an account, not an explanation. An explanation requires some real connection between the orbs or some altogether extraneous explanation. Geocentrists could appeal to celestial Intelligences or angels as the movers. To what can a heliocentrist appeal? The supposed compatibility between Magini's scheme and Copernicus' mathematical models is illusory and, accordingly, the assumption of real partial orbs is unwarranted.

There are, to be sure, similar problems with Copernicus' assumption of total spheres moving the visible bodies; but we have noted his explicit testimony about their existence and that he was silent about the partial orbs. He was content to describe the models and the circles needed to calculate mean longitude, the angle of anomaly (epicycle), and the additional angle of anomaly (epicyclet), from which the astronomer can calculate the Moon's true longitude and account for the appearance of the Moon at quadrature better than Ptolemy could.

Magini's orb version raises precisely the sorts of problems that Copernicus could have seen in Giorgio Valla's translation of Proclus' *Hypotyposis* [1501, XVIII, fol. sig. gg7]. Proclus objected that the astronomers make the eccentric and epicycle circles as well as their spheres move independently and that even the circles do not move like one another but in opposite directions.

Copernicus says that rotation is natural to spheres as following their form. But why do some orbs rotate clockwise and others counterclockwise? Must we also conclude that Copernicus retained Intelligences as the movers of the spheres and orbs or perhaps that he believed that the cosmos possessed a world-soul that directs the motions of the heavenly bodies? Where does speculation end?

Of course, Copernicus says nothing about any of this except to adopt the total spheres as the movers of the planets. The entire discussion hinges on Copernicus' view of partial orbs about which he said nothing. As for the claimed connection between partial orbs and Swerdlow's reconstruction of Copernicus' path to heliocentrism, except for the question of impenetrability, there is none. I acknowledge Copernicus' adoption of the total sphere and grant that this alone suffices for Swerdlow's reconstruction. I will return to such speculation, however, subsequently.

The reviewers' argument rests in part on the assumption that Copernicus would not have been exceptional in his view of partial orbs [325–327]. Why not? What could be more exceptional than his rejection of geocentrism and adoption of Earth's motions?

Exceptionalism on some questions is characteristic of Copernicus and is neither surprising nor striking. His silence here seems consistent with his silence on other matters about which there was controversy. These were the sorts of problems that Copernicus left his followers and successors to resolve. The reviewers select only evidence of followers who adopted partial impenetrable orbs and proposed alternative theories about the fluid or air-like qualities of spheres that could penetrate, and ignore those followers who either rejected partial impenetrable orbs or were also silent about them. Why give preference to those who adopted impenetrable partial orbs over those who did not?

This is not to say that Copernicus rejected the real existence of partial orbs—he does not say that either. Here are the most general alternatives:

(1) He adopted partial orbs as real without explaining their nature or the causes of their motions. This would be consistent with his adoption of real total spheres.

(2) He rejected the reality of partial orbs because exceptions to the principles were required to accommodate a body moving around

another body that was itself moving and resulted in gaps contrary to the traditional theory. This would be consistent with his adoption of real total spheres as necessary and sufficient to account for the circular motions of bodies attached to or fixed in them.

(3) He said nothing about the reality of partial orbs precisely because there were disagreements about their reality and their relation to the geometrical models. Again, this would be consistent with Copernicus' assumption and belief that he needed real total spheres to account for the regular, circular motions of bodies attached to or fixed in them.

The first two are possible but it is the third that fits best with what Copernicus has written.

C. John of Glogovia and Albert of Brudzewo

Barker [2013] makes some questionable assumptions about John of Glogovia, Albert of Brudzewo, and the curriculum at Cracow.

Scholastic commentators distinguished between introductory summaries representing what the commentators believed the author to have meant and more advanced commentaries. The introductory summaries *might* represent the commentator's own views but we need to compare them with their more advanced treatises to confirm such an interpretation. John of Glogovia's more advanced interpretations of some questions on logical issues, for example, contradict his own commentaries on introductory texts [see Goddu 1995, 152–163]. Glogovia's advanced questions on natural philosophy often present a variety of solutions to problems, leaving readers to sort out the issues for themselves. We can usually determine Glogovia's genuine opinion but doing so requires painstaking comparison of a variety of sources. The editor of Glogovia's *Questions on the Physics*, Marian Zwiercan [1973, 98–108], concluded that Glogovia's philosophical works 'contain ideas drawn from Averroes and Averroists'. This does not mean that Glogovia was an Averroist but it does caution us to be careful about issuing definitive declarations about his doctrines. Glogovia relied on a large number of authors, among them Albert the Great, Thomas Aquinas, John Versoris, and the 13th-century Latin Averroist John of Jandun. In fact, Glogovia relied heavily on John of Jandun for his commentary on Aristotle's *De anima* [see Kuksewicz 1962]. Glogovia did lecture on Gerard of Cremona's *Theorica planetarum* but Polish experts cite only a fragment of his comments in manuscript, now lost [see Seńko 1964, 36; Birkenmajer 1900b, xxv]. In his treatise on the *Sphere*, it appears that Glogovia did accept the reality of partial orbs [see Barker 2013, 127–130] but we do not possess a more advanced text by him on the subject. In his *Quaestiones de motu*, however, he 'pondered' the Averroist rejection of epicycles and eccentrics without stating a conclusion [see Zwiercan 1973, 107–108; Markowski 1975c, 110]. Finally, a point to which I will return below, it was not only Averroists who expressed doubts about the reality of eccentrics and epicycles.

We know very little about Glogovia's relationship with Albert of Brudzewo. Glogovia taught Brudzewo [see Zwiercan 1973, 108]. They were both associated with the same student hostel. Historians of the University of Cracow and of medieval Polish philosophy have portrayed John as a typical author of the scholastic commentary tradition. According to some sources, he was a critic of such modernizing humanists as Conrad Celtes [see Morawski 1900, 2:155–158], although some contemporaneous humanists praised him [see Zwiercan 1963, 452]. Brudzewo taught Celtes and even referred to him in one letter as 'son' [see Morawski 1900, 2:177]. Whether Glogovia's criticism of Celtes, however, put any strain on his relationship with Brudzewo is unknown. Brudzewo has been linked with a humanist circle in Cracow, the *Sodalitas Litteraria Vistulana*, which supported both Renaissance Humanism and Neoplatonic philosophy. Brudzewo's adoption of Peurbach's *Theoricae* suggests that he belonged, unlike Glogovia, to the humanist circle of astronomers. The point is that Glogovia's and Brudzewo's views may have been compatible on some issues and not on others.

Indeed, aside from more advanced treatises sometimes contradicting the same author's introductory comments, Barker's assumption [2013, 129, 135] that university masters would not have disagreed with one another in introductory undergraduate courses overlooks the dialectical and disputatious nature of medieval pedagogy. Students expected their teachers to disagree and criticize one another as part of the dialectical nature of the enterprise. In general, there is a better fit between the approach adopted by Brudzewo and his predecessors in the astronomical school at Cracow than with Glogovia [see Rosińska 1973a, 1973b; Markowski 1975a, 1975b; Dobrzycki 1975].

The reviewers [320] express surprise, indeed, they even call it 'bizarre', that I would doubt Copernicus' direct knowledge of Albert of Brudzewo's *Commen-*

tariolum, as if this were not a question of fact. But we have to distinguish between Brudzewo's lectures, the manuscript copies of his book, and the published version. I agree that Copernicus must have heard lectures on the *Commentariolum* and we may presume that he took notes. But to answer questions about the manuscript copies requires our knowing more about the number of manuscripts and how they were distributed. The important point is that Copernicus knew its content. The echoes alleged by Ludwik Birkenmajer [1924, 83–98] between the *Commentariolum* and Copernicus' *De rev.* are faint and unpersuasive. The *Commentariolum* is an exceedingly rare book. There is no evidence that Copernicus owned a copy or of its having been in a Varmian library in the 16th century [see Hipler 1874], meaning that after 1502 it would have been very difficult for Copernicus to consult it. These are questions of provenance. Direct textual evidence is not necessary for acquaintance and influence unless we are looking for proof of a unique source for a specific fact (such as Birkenmajer's 'echoes') or assertion in Copernicus' texts.

Barker's account [2013, 130–139] of Brudzewo's *Commentariolum* requires four emendations concerning:

(1) Brudzewo's definition of 'sphere' or 'orb',
(2) Barker's description of the marginal annotations regarding the lunar model,
(3) his neglect of Brudzewo's description of a reciprocation or libration mechanism, and finally, the most serious,
(4) Brudzewo's comments about the solar orbs and his reflections on the reality of eccentric and epicycle orbs.

(1) Under the third way of understanding 'orb', namely, as the orb concentric to the Earth or the aggregate of all orbs necessary and sufficient to save the motions of a planet in longitude and latitude, Barker [2013, 130] omits Brudzewo's comment that the third, that is, the aggregate both with respect to its convex and concave surfaces, is the sense appropriate here, which I take to mean the 'principal subject' of the treatise. In other words, 'sphere' or 'orb' refers primarily to the total or complete sphere.

(2) Birkenmajer included some marginal annotations from manuscript copies of Brudzewo's text. In his edition of the lunar model, Birkenmajer cited figures from two manuscript versions, L and C, and the edition of 1495 E [see 1900b, 68–69]. John of Crobya copied the part of manuscript

L containing a figure with two concentric orbs and a marginal annotation in 1488 [Birkenmajer 1900b, xlvi–xlix]. Manuscript C is dated to 1493 and contains a figure perhaps copied from a lecture of Albert of Pniewy [see Birkenmajer 1900b, xlix–li]. It is possible that Copernicus attended Albert of Pniewy's lectures. The edition of 1495 also includes the figure with two concentric orbs. In combining these sources, Birkenmajer believed them to be representative of the comments made by lecturers and he emphasizes the point that they reflect the views of Brudzewo himself. In other words, these are early witnesses to Brudzewo's own interpretation and annotations. As Barker [2013, 138] explains, the motions described refer to πρόcνευcιc, what Toomer [1998, 226–227] translates as the 'direction in which the epicycle points' or what Pedersen [1974, 192] calls 'inclination'. The figure from manuscript L depicts the Moon in an epicyclic orb inside a second epicyclic orb. The outer epicycle accounts for the change of direction and the additional figures depict the Moon in all of its phases. Barker objects to Birkenmajer's claim that the motion is related to the explanation of the spots on the Moon. In fact, however, that is exactly what the annotation in manuscript L, as quoted by Barker, says.

The issue, then, is not whether Brudzewo and his students understood the relation between πρόcνευcιc, lunar phases, and the spots on the Moon correctly but rather that they believed that there was such a connection. What could be the source for their interpretation and, above all, the suggestion that a double-epicycle model could account for the phenomenon?

The likely source, as proposed by Rosińska [1974], is Sandivogius of Czechel. Indeed, the description provided by Sandivogius as quoted by Rosińska [1974, 241–242nn11–13.] is almost identical to the marginal annotation in manuscript L of Brudzewo's commentary. While it is true that Rosińska questioned the Islamic route of transmission, she also acknowledged the greater similarity between the role of the models in Islamic sources and Copernicus than between Sandivogius and Copernicus. In fairness to Rosińska, her doubts about Sandivogius' originality [1974, 243] should also be emphasized.

There is, however, a misunderstanding here. It is clear from the figures described that the solutions are represented as epicyclic orbs, as is appropriate for the *Theoricae*. It is also clear that the orbs are depicted concentrically. The differences from the Copernican model are clear. I did use the word 'device' [2010, 156–157] and so contributed to the misunderstanding; yet my claim is not, and never was, that Copernicus derived his version directly from the Cracow models but rather that the idea of a double epicycle may have impressed him. He could perhaps have developed his model independently but a Cracow source does not exclude the Islamic route. That is to say, the Cracow models may explain his receptivity to other solutions.

(3) Brudzewo's description of a reciprocation mechanism requires some comment. Without a great deal of explanation, as if it were well known, Brudzewo [Birkenmajer 1900b, 120] describes the composition of a rectilinear motion by means of several circular motions. Birkenmajer does not indicate any marginal figure but it would be prudent to consult the manuscripts. In any case, Birkenmajer does not hesitate to compare it with the so-called motion of libration in *De rev.* 3.4. Birkenmajer added:

> We do not know whether it was his own or another's creation, but even more striking is the ingenious method of Brudzewo for the kinematic elaboration of a rectilinear motion from several circular motions. [1924, 95]

In fact, I did not exclude the possibility that 15th-century Latin authors may have relied on a description of Maragha planetary theory [2010, 478]. My complaint here is not about the questions that I posed and the alternative sketch that I provided but that I am accused of an absolute rejection of the Islamic route.

(4) Finally, a similar selectivity characterizes Barker's account [2013, 132] of the solar orbs. Brudzewo [Birkenmajer 1900b, 19] asserts that the ancients understood the Sun to move not on a circle but in an orb, which is a solid and spherical body. Over the next few pages, Brudzewo continues his recitation of the contents of the *Theoricae* with comments about the relation between geometrical models and spherical orbs, mentioning objections, some deriving from Averroes about the penetration of spheres or the introduction of a void [Birkenmajer 1900b, 25]. The resolution of these difficulties, he says [1900b, 25–26], was achieved by dividing each total concentric sphere into partial orbs to account for the observations and the diverse motions of the planets. Barker [2013, 134–135] claims that Brudzewo pits the philosophers against the astronomers here. Yet it is also astronomers who divided the total orb into partial orbs, the purpose of which was to account for the observed positions and motions. Immediately after that explanation comes the comment that nearly everyone had previously interpreted as representing Brudzewo's genuine view [1900b, 26–27]. In Rosen's translation:

> No mortal man knows whether these eccentrics really exist in the spheres of the planets, unless we admit (as some people claim) that the eccentrics, like the epicycles, are made manifest by the revelation of spirits. If we reject this claim, then the eccentrics are devised solely by the imagination of the astronomer. [1939, 123]

Brudzewo follows with a quotation from Richard of Wallingford, who denies the reality of eccentrics and epicycles as fictions, products of the mathematical imagination. Indeed, Richard, as quoted by Albert, says that no one trained in this discipline could truly believe that eccentrics and epicycles exist as imagined. According to Barker, Brudzewo was quoting the views of those he rejected. Brudzewo completes his comments, omitted by Barker, however, with the following assertion:

> So says the author. We should therefore be content with these means, for through them we achieve a perfect science of the moving stars.

Is this the voice of someone who rejected the agnostic judgment expressed in the paragraph cited above? Barker [2013, 135] suggests somewhat fancifully that the reference to 'revelations of spirits' refers to a 'spirit located in the celestial regions'. It is likelier that Brudzewo was questioning the certainty expressed by realists. How do they know whether eccentrics and epicycles really exist?

Barker has quite rightly pointed out problems with my interpretation of Brudzewo's text. There are passages where Brudzewo asserts the real existence of partial orbs; but it was a standard technique in a commentary to cite the views of the author, Peurbach in this case, especially in the comments following *lemmata* or quotations of the first words of a section or paragraph. Some previous interpreters have concluded that Brudzewo was in fact a fictionalist about eccentrics and epicycles. Even I used that word [2010, 148, 158] and stated elsewhere [376] that he rejected the reality of epicycle spheres. But in my brief description of his text [2010, 164], I described him more cautiously as 'agnostic' about the existence of eccentrics. Since then, I have become more convinced that the correct word to describe Brudzewo's view is 'agnostic'. In other words, Brudzewo did not affirm their real existence but he did not categorically deny their existence either.

Such cautious reading prompts me to add that we should reconsider how the motions of the planets can be described as real. Clearly, the paths or orbits are not real entities distinguishable from the moving planets. On the other hand, the planets do not move just anywhere. What a partial orb really does describe is the area in which the visible body moves. In responding to Averroist objections, Brudzewo affirmed the necessity of the mathematical models. But their necessity does not entail the real physical existence of partial orbs.

In my view, the main flaw in Barker's analysis [2013] is the lumping together of opponents of eccentric and epicycle orbs as all Averroists. Barker ignores ancient and medieval opponents who did not follow Averroes but who raised doubts about the reality of eccentrics and epicycles. Proclus, Richard of Wallingford, John Buridan, and Henry of Hesse were at best agnostic about the existence of eccentrics and epicycles. The assumption that only Averroists adopted such a view is mistaken. Non-Averroist followers of Aristotle also raised objections based on Aristotle's conception of spherical motion around bodies. In the section quoted [Birkenmajer 1900b, 122–123], Brudzewo refers to those who support partial orbs against the Averroists and then adds the comment about their existence. He volunteers the objection, suggesting that he had doubts, but which he expresses cautiously and without commitment.

Finally, as confirmation of that reading, I quote Birkenmajer's own judgment after noting the similarity in Brudzewo's and Copernicus' scepticism about the equant:

> The same skepticism in Brudzewo and Copernicus with respect to the real existence of other circles was, however, rather moderate. We can see this in parallel passages here and there, which also testifies to the similarity in the thinking of both scholars. [1924, 91–92]

Birkenmajer follows that comment with quotations from both Brudzewo and Copernicus on eccentrics and epicycles. He calls their view of other circles 'skepticism' but moderate in comparison with their scepticism about the equant.

We may object that editors are not infallible interpreters of the text that they edit. But, if Birkenmajer had reason to believe that Brudzewo was expressing a view that he rejected, then surely Birkenmajer would have indicated a problem. What Birkenmajer does express is the contrast between the definite rejection of the equant as fictitious and Brudzewo's more cautious comments about eccentrics and epicycles. This is why, in my view, we are justified in concluding that Brudzewo was agnostic about their existence.

Such agnosticism may have influenced Copernicus, who also knew of Bessarion's doubts about the reality of mathematical models in astronomy [Goddu 2010, 223–224].

As for my neglect of contextual issues, I claim that the reviewers have not adequately reported evidence that contradicts their interpretation. They have neglected alternative interpretations of spheres and orbs and, with respect to the literature on *De sphera* and *Theorica planetarum*, they have neglected the survey by Christe McMenomy [1984], which is the most thorough study of the different ways in which these texts were interpreted and of the trends that emerged in the 15th and 16th centuries.

D. Capellan and Tychonic alternatives and the origin of Copernicus' heliocentrism

In reference to the explication of Swerdlow's hypothesis [1973] and the issue of orbs and spheres, my reasoning, as it stands, was incomplete and incorrect. I contributed to confusion here by not distinguishing clearly between mathematical models and orb or sphere models. The issue of the reality of partial orbs, however, is irrelevant. The real total sphere is sufficient to create the sort of problem proposed by Swerdlow. If, in either the Capellan or the Tychonic arrangement, the solar orb includes the epicycle orbs of Mercury and Venus, as in Theon of Smyrna's interpretation [Dreyer 1906, 127], then the physical principles for their motions differ from those for the superior planets each of which has its own set of partial orbs inside its own proper total sphere, which is, therefore, different from the total sphere carrying the Sun, Mercury, and Venus.

Already in the *Commentariolus*, after pointing out that there is no one center of all the celestial orbs or spheres [Dobrzycki 2007a, 10 *prima petitio*], Copernicus committed himself to the idea that all the spheres encircle the Sun [*tertia petitio*]. Notice the problem that arises for all orb arrangements as soon as we begin to consider the circular motion of one body around another. In the *Theoricae*, this problem does not arise because all bodies move on epicycles around empty geometrical points. Each body can be treated separately with its own set of partial orbs. In the Ptolemaic system, the celestial spheres are nested and contiguous. Because they have bodies orbiting other bodies, the Capellan and Copernican systems introduce complications with the nesting and contiguity of the total spheres, while the Tychonic system eliminates the celestial spheres altogether. We must revise that feature of the *Theoricae*, however, if some bodies move around another body that is itself moving. This is the problem that arises in the Capellan arrangement, the Tychonic arrangement, and with Copernicus' Earth in motion. Now, suddenly, we must suppose that the Sun does not have a proper total sphere but rather shares it with Mercury and Venus. It is possible that reasoning of this kind influenced Copernicus in the following way. Because he realized that Mars at opposition is closer to Earth than the Sun is, he could not, according to Rheticus [Hugonnard-Roche and Verdet 1982, 55], include the total sphere of Mars inside the sphere of the Sun, which, in turn, necessitated placing Earth's total sphere between the spheres of Venus and Mars and arranged around the Sun.

As we all know, Copernicus says nothing about this problem. As Swerdlow [1973, 478] acknowledged, the elimination of a Tychonic alternative is pure speculation. To my knowledge, Schiaparelli was the first to speculate about a Tychonic intermediary to explain Aristarchus' heliocentric hypothesis [Dreyer 1906, 143–148]. There is no mention of a Tychonic alternative by the ancients but Dreyer adds:

> [W]e can only conclude, that it was never proposed as a way of 'saving the phenomena', though Aristarchus may have first been led to it, and then immediately afterwards may have been struck by the still greater simplicity and beauty of the heliocentric system, which alone he therefore considered worth proposing publicly. [1906, 147]

Dreyer [1906, 364] also suggests that Copernicus may have considered the Tychonic alternative; but if so, 'he did not rest content with it, but proceeded at once to its logical sequel, the heliocentric system'.

Further, we know that Copernicus rejected the Capellan arrangement silently. On the assumption that Copernicus adopted Theon of Smyrna's description, the Capellan arrangement did not involve any penetration of spheres. So, why did he reject it? One of his criticisms of Ptolemy was that his system was like a mosaic, the result of which is the depiction of a monster. The Capellan arrangement is less of a mosaic but, because it proposes two centers and two principles of arrangement, it may have appeared to Copernicus to be like a two-headed monster. Copernicus rejected it because he had already come to the conclusion that the universe should have one center and one principle of arrangement of spheres around it. Dreyer does not clarify what he means by 'greater simplicity' and 'logical sequel' but I suggest that the contrast between the first and third *petitiones* contains the answer. If Copernicus ever considered or anticipated a Tychonic arrangement, he would have rejected it for the same reason.

The supposed unambiguous reference in the 'Letter Against Werner' [Dobrzycki 2007b, 32; Clutton-Brock 2005, 211] to the existence of eccentrics and epicycles uses the word 'circles' (*circuli*).

> What might have been is an abstraction
> Remaining a perpetual possibility
> Only in a world of speculation. [Eliot 1943, 3]

All of this speculation, though not logically impossible, is superfluous. Copernicus does not answer our questions as explicitly as we would like but he does express his reasons sufficiently enough for us to reconstruct his path to the heliocentric theory on the basis of his own words. He enumerates the problems with geocentrism in both the *Commentariolus* and *De revolutionibus*. He adds three further explicit comments in the *De revolutionibus* that are not in the *Commentariolus*, although they are not inconsistent with anything in the *Commentariolus* and, I claim, are already implicit in his early remarks.

The *Commentariolus* and *De revolutionibus* agree on a number of problems with geocentrism. Geocentrists disagree about how to preserve the uniform motions of the celestial spheres and to account for the planets' apparent motions. Homocentric hypotheses cannot account for the observed motions without eccentrics and/or epicycles. Ptolemy and his followers, however, could not preserve uniform motion by means of eccentrics and epicycles without adding equant circles that violate the planets' uniform motions around the center of the deferent sphere or the centers of their epicycles. These difficulties led him, Copernicus says, to search for a more reasonable arrangement of circles (*modum circulorum*) that would preserve uniform motion and account for the observations.

The preface of the *De revolutionibus* adds several other criticisms of which three are most notable:

(1) geocentrists could not derive the principal consideration, the structure of the universe and the harmonious arrangement of its parts, resulting in a mosaic that resembles a monster;

(2) there is a flaw in their method; and
(3) Copernicus' frustration that the motions of the world machine, created for our sake by the best and most systematic Artisan, were not understood with greater certainty.

The disagreement over structure relates principally to lack of consensus among geocentrists about the ordering of Mercury and Venus [*De rev.* 1.10] and to Copernicus' distance-period principle [Goldstein 2002, 220–222], already implicit in the *Uppsala Notebook* and *Commentariolus* [Goddu 2006, 39–46]. In other words, nothing in geocentrism compelled a choice among the alternatives.

The failure to reach agreement on the planetary order was the result of a flaw in their method. Copernicus admits obscurity here, saying that it will become clear in the proper place. Where else does he discuss these issues other than in chapters 4–10 of *De rev.* 1, which he summarized very briefly in the *Commentariolus* [Dobrzycki 2007a, 11.19–21]? In fact, he presents the solution in the *Commentariolus* along with the observation that the arguments in support of Earth's immobility rest on appearances.

What, then, was the flaw? Following Aristotle, geocentric astronomers had inferred the structure of the whole from the observation of one part. We see heavy bodies fall in straight lines towards the center of Earth. The motion of the whole must be the same as the motion of the part; therefore, Earth, if it moved, could move only in a straight line toward the center. We see celestial bodies, however, move in circles; hence, they do not possess the same tendency as heavy bodies. From those premises, geocentrists concluded that Earth must be immovable at the center of the universe. The flaw is the logical fallacy of arguing from part to whole, the result of which is that geocentrists cannot agree on a unique principle for ordering the spheres. That result frustrated Copernicus. Had the most perfect Artisan constructed the universe arbitrarily, not in the best way possible and with no principle of arrangement as suited to a harmonious structure, so unknowable by us? Impossible, thought Copernicus. As he considered alternatives, he realized or hypothesized that by having all planetary spheres arranged around a stationary Sun, a unique principle for their harmonious arrangement emerged. In other words, we must begin with the whole organized in a harmonious fashion to infer the ordering of the parts and come, then, to a decision about the places and motions of the parts [Goddu 2009].

I return, as promised, to Copernicus' reliance on the Platonic tradition. Where did Copernicus get the idea that the universe must be organized harmoniously according to a unique mathematical principle? These are ideas that we associate with Platonism, Neoplatonism, Neopythagoreanism, and even with scholastic or Christian Neoplatonism. The authors and predecessors that he cites provide us some clues. We need to consider a variety of possibilities here, including his reading of Cardinal Bessarion's *In calumniatorem Platonis* [1503] and of Ficino's translation of, and commentary on, Plato's dialogues [1484], his references to authors who are associated with Pythagorean views about harmony and mathematics, and, lest we forget, Ptolemy himself. After all, Copernicus suggests that he adopted Ptolemy's program and his promise of progress, and eventually concluded that because of the flaw in their method summarized above, Ptolemaic astronomers had failed to achieve the hoped for results.

E. Concluding reflections on the summary of my study, textual evidence, and speculation guided by contextual considerations.

If I have expressed frustration with the oversight concerning the principal results of my study [2010, 285–291, 358–360, 384–386, 425–427], I have to conclude that, because I buried them in such a long book and did not summarize them clearly enough at the end, the reviewers overlooked my claims. It is my good fortune that they have given me the opportunity to summarize what I thought I had made clear.

The reviewers were right to complain about lapses of clarity, reluctance to speculate without textual foundations, and the incompleteness of some arguments. In my defense, I refer above to the passages that address the first, remind readers here of my speculative reconstruction of Copernicus' education at Cracow [2010, 5–167], and call upon my references to supporting evidence in my other publications. The questions and criticisms that they raise have allowed me to respond directly to problems that I perhaps should have anticipated but which I thought I had addressed. In retrospect, it is clear that I did not do so adequately.

My chief complaint about the speculation concerning partial orbs is that it adds nothing to our understanding of why or how Copernicus proposed a heliocentric system. If Swerdlow is right, it suffices to affirm the existence of the total spheres and their impenetrability, regardless of whether partial orbs exist or not. In my view, Swerdlow's speculation, unsupported by any text in Copernicus, is superfluous. We can reconstruct his path to heliocentrism by relying on his words, his criticisms of geocentrism, and his arguments in support of Earth's motions.

To focus this conclusion and my response to the reviewers' main criticism, I must ask the reader to decide whether we should base a reconstruction of Copernicus' achievement on what he wrote or on what we suppose he may have thought about matters on which he chose to remain silent. It seems to me that we should resort to the second only when we have exhausted his words and a close study of the sources that we know he used. We are not finished studying his sources. For example, no one to my knowledge has examined carefully Copernicus' copy of Bessarion, *In calumniatorem Platonis* [1503]. My brief summary of that text [2010, 220–225] had as its primary goal to confirm that Bessarion's defense of Plato did not entail a rejection of Aristotle. I did, however, suggest even then that Bessarion influenced Copernicus to be cautious about realist interpretations of astronomical models. As models for this sort of reading of Copernicus' books and his annotations, I commend the studies of Dilwyn Knox [2005, 2012], who has taken the considerable trouble to identify carefully Copernicus' doctrines and then to search the sources which we know he used for the best fit.

When we do resort to contextual considerations, we should be comprehensive in laying out the alternatives, and, if we choose one over another, in explaining why we have eliminated the others. Even in cases where we may disagree with the reconstruction, the consolation will be in knowing that the evidence has been presented fully. In the end, I do not think that we can eliminate subjectivity from the conclusions that we prefer. Although I criticize the reviewers for their selectivity, their criticisms are clearly motivated by what they perceive to be a far too narrow reliance on texts. I have explained my shortcomings here as due to the conviction that Copernicus made the conscious decision not to express himself on some topics that were controversial or unsettled among the experts. We can sometimes reconstruct his genuine view from other assertions that he makes but, in some crucial cases, we must consider the possibility that he did not know the answer and adopted the sort of agnosticism that I claim he learned from Albert of Brudzewo (or one of his students) and perhaps Bessarion.

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Scienza antica in età moderna. Teoria e immagini edited by Vanna Maraglino

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Introduction

This book is the first of a new series, Biblioteca della tradizione classica, and contains 17 papers, most of them written in Italian and otherwise in German. The book is divided into four sections dedicated to the following topics:

(1) the military art,
(2) geography,
(3) medicine, and
(4) the natural sciences.

The explicit aim of the book is to investigate the role of images in the processes of the transmission of ancient science during the early modern period. The book represents the proceedings of a conference held in 2011, 'La tradizione della scienza antica nell'età moderna attraverso l'immagine'.

The subjects addressed by the sections of the book are all disciplines and activities which, in Aristotelian terms, doubtlessly belong to what has been defined as τέχνη. Discussing the role of images in the history of the reception of such traditions of knowledge thus means discussing the role of images as mediators. Not only do images mediate between the content of a work and the general cultural environment in which the work originated, they also, in the subjects touched upon in the book, mediate between practical and theoretical knowledge. From a diachronic perspective, moreover, images transmit, integrate, and, finally, transform scientific knowledge over time; and they do this continuously from one epoch to another. Such a history of the reception of ancient science during the early modern period with its focus on the role of images, therefore, concerns the history of the processes by which knowledge is transformed. In short, the act of mediation is finally to be defined as an act of transformation.

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The same concept of image, in this context, requires a more elaborate taxonomy. Images have entered scientific practice in history as figurative illustrations, diagrams, technical drawings, and as a sort of gloss to the text, either singly or in sequences. All these types of images have specific functions, specific origins, and specific consequences in the processes by which knowledge is transformed and their history.

During the early modern period, images in science developed into an autonomous language. As Paolo Galluzzi has stated [2002], this process started during the 15th century. What pushed the process of development and diffusion of pictorial language in science during the Renaissance so effectively was the need to explore and clarify the technical knowledge or the practical knowledge transmitted from antiquity. Leonardo played a key role in this context. He redefined the function of drawings, especially concerning machines. Not only was he a keen observer of reality, he was also able to 'transfigure reality through graphic registrations' [Galluzzi 2002, 53]. Leonardo moved toward an anatomical approach while examining (and not only reproducing) machines by means of drawings. He isolated the mechanical components and introduced the use of perspective in technical drawings as well as the use of *chiaroscuro*. To put it in Galluzzi's words,

> Leonardo liberated drawing from the restrictions that bound it tightly to the figurative arts, transforming it into a powerful tool of investigation and demonstration. [2002, 63]

The emergence of modern technical drawing found its roots in Leonardo's works and developed further until it became an essential element of the scientific enterprise. As the historian Wolfgang Lefèvre has shown, for instance, the emergence of highly professionalized technical drawing during the early modern period originated in and, at the same time, contributed to the emergence of new forms of the division of labor which mirrored the practical organization of the working procedure together with new hierarchies defined in terms of responsibility concerning the final products. Technical drawing was also connected to new paths and methods of propagation of knowledge, including new didactic institutions such as the Accademia del disegno, founded in 1563. At this point in time, a new type of image was created, one which Lefèvre calls the 'learned image' [2004, 71].

During the process described by Lefèvre, the new figure of the engineer-scientist emerged [see Valleriani 2010, 2013]. Engineer-scientists increasingly combined practical knowledge with theoretical elements. As engineers, they conceived and designed new artifacts by working on and thinking with images. Through the development of a new scientific language, the engineer-scientists also established an autonomous pictorial language [see Engel, Queisner, and Tullio 2012].

Scientific images inform about the social context of scientific practice and also convey meanings that are not explained in texts, thus superseding texts in their epistemic and didactic functions. Images in science integrate different domains of knowledge and transform texts over epochs in continuously different new scientific and cultural frameworks, as Joyce van Leeuwen was able to show in reference to the Pseudo-Aristotelian *Mechanical Problems* [2012].

The use of images in science goes back to antiquity. Famous is the case of Archimedes who introduced the practice of representing physical bodies by means of abstract diagrams. Construction lines in the diagrams helped to achieve the possibility of applying quantifications, thus mediating between reality and mathematics. The 'rational artists' of the early modern period are linked to the entire tradition going back to antiquity [see Roche 1993].

As mentioned, the 17 papers are organized in four sections: 'Military Art', 'Geography', 'Medicine', and 'Natural Sciences'. Each section contains only the papers: there is no introductory text to help the reader approach the papers or even to offer a short overview of the subject matter.

1. The military art

This section includes four papers of very heterogenous character and style. Only two of the four papers, the second and fourth, deal directly with the role of images in the framework of the process by which ancient knowledge was received. With the exception of the fourth paper, moreover, the works presented here do not pay any attention to the real practice followed in the context of the art of war contemporary to the historical sources—whether ancient or early modern—that are taken into account. The resulting approach is, therefore, genuinely philological.

The first paper is by Corrado Petrocelli, 'Racconti di guerra. Figure della narrazione delle «Storie» di Tucidide' [5–33]. He discusses the emergence of literary genres in antiquity related to events of war and thus also examines war manuals as historical reports and reconstructions of wars. In particular, he shows the fundamental role played by Thucydides' *History of the Peloponnesian War* in shaping such genres. The paper refers to the narrative figures created by Thucydides and, therefore, touches on the main subject of the book only at a metaphorical level.

The second paper, Immacolata Eramo's 'Disegni di guerra. La tradizione dei diagrammi tattici greci nell'«Arte della guerra» di Niccolò Machiavelli' [35–62], is the result of an in-depth philological study of the images used by Niccolò Machiavelli in his *Arte della guerra*. In particular, the author shows the strong influence of tracts on the military strategy of Vegetius, *Epitoma rei militaris* and, especially, of Aelianus Tacticus, *On Tactical Arrays of the Greeks* on Machiavelli's work.

The third, Klaus Fabian's 'Des Hopliten Schutz und Trutz oder Philologie auf Lanzenspitzen' [63–138], is a very long essay that brings an imbalance to the whole section. This paper of some 75 pages in length shows little internal structure or subdivision of the argument. Furthermore, the goal of the argument is never expressed with the clarity that such a lengthy text definitely requires. In a style marked by strong polemical verve, it seems that the author intends to hurl abuse at the image of ancient cultures and society as created by modern and contemporary cinematography.

The fourth and last paper, Gastone Breccia's 'La geometria di Marte. Polibio e Cesare nelle incisioni di Andrea Palladio: il volto rinascimentale della battaglia' [139–156], shows the results of analyses undertaken in reference to the images published by Andrea Palladio in his *Commentari* of Julius Caesar (1574) and the images produced for Palladio's planned edition of *The Histories* of Polybius, which he never published because of his death in 1580. The author clearly shows the originality of Palladio's work, pointing out that his images have a strong explanatory character that is achieved by means of a scenic geometrization. Contextualized in the process of the geometrization of the art of war during the early modern period, the paper is able to demonstrate effectively the fundamental role of images in the process of professionalization which the art of war underwent in this period.

2. Geography

This section contains four outstanding papers which build a complete narrative. Because of the relation between geography and cartography, it might appear obvious nowadays to assume that geography is one of the best examples or one of the most appropriate disciplines to investigate the role of images in the history of the reception of ancient knowledge. All four papers reveal precisely why this assumption is not obvious. Firstly, all four of them are able to show that, after the ancient age of mathematical cartography had come to an end, the medieval traditions followed completely different paths so that the history of geography does not show linear developments. Secondly, all papers also reveal that alternative ways of conceiving the work of geographers were given in antiquity and that such ways were not always provided in a close relation to cartography. This implies that the re-emergence of mathematical geography and cartography during the early modern period requires peculiar investigations concerning the reception of specific works from antiquity. This narrative and also a historiographic overview of the discipline of 'history of geography' are both offered in this section.

The first paper, Francesco Pontera's 'Geografia antica nella cartografia medievale: l'Asia in un codice di San Gerolamo' [159–179], analyzes the influence of Greek mathematical and empirical cartography as well as the influence of the Roman cartography of a practical character on the cartographic tradition of the Middle Ages. The merit of this study is to throw light on the broader cultural process that led to the separation of proper geographic knowledge from its graphical representation, as is typically found in medieval cartography. In particular, the author is able to show that such separation was due to the intrinsic limits of Greek mathematical cartography—for instance, the scant number of astronomical data available to 'fill' the mathematical grid conceived by Ptolemy—and due to a tendency in that direction which emerged as early as late antiquity and which can be recognized in works such as Pliny the Elder's *Naturalis historia* and Pomponius Mela's *De situ orbis libri III*.

The second paper, Nicola Biffi's '«È simile a ...». L'uso delle immagini nella «Geografia» di Strabone' [181–214], is an interesting reading of Strabo's *Geography* which aims to identify the heuristics used to shape the territory figuratively. As the author convincingly argues in the conclusion, Strabo's *Geography* might represent a peculiar genre in the context of the discipline which however did not transform into a fortunate tradition of knowledge. The peculiarity of such a genre consists in the fact that Strabo's *Geography* is not provided with any maps or charts, though the intention of the work is to furnish a universal description of the known territories. As an alternative to a geography accompanied by cartography, Strabo implements the idea of associating the territories described with either regular, geometric figures or with images of well-known objects from everyday life; for instance, he associates the shape of the coast starting from the Caspian Sea and the blade of a butcher's knife. This erudite paper not only offers a rare but also very relevant overview of such an important yet nevertheless often neglected work on antiquity, it also gives the opportunity to reflect critically upon the much discussed subject of the existence or non-existence of maps from antiquity from a new perspective. In particular, the use of geometric or known images certainly was an efficient method to systematize knowledge taxonomically and, at the same time, to make sure that such knowledge could be spread easily over cultures and handed down over time.

The third paper, Vladimiro Valerio's 'La «Geografia» di Tolomeo e la nascita della moderna rappresentazione dello spazio' [215–232] focuses on Ptolemy's *Geography*, a work that re-emerged in Florence at the end of the 14th century and featured 27 maps, whose ancient provenance was not doubted at the time. This work exerted enormous influence during the era of humanism because Ptolemy's *Geography* was the first example of metric geography after the tradition of medieval geography and cartography, which, in its three forms (*mappae mundi*, Portolans, and maps of geographically limited territories, mostly for military purposes) did not show any metrical character. The paper describes in detail the process that led to the development of metric geography during the early modern period as intimately connected to the activities of astronomers. It is convincingly argued that this was a process similar to the one that occurred during antiquity and which led, in the interval from Hipparchus to Ptolemy, from the work of mapping the stars to that of mapping the Earth.

On the basis of the analysis of such specific sources as Johan Stabius' *Planisphere* of 1515, the author is, moreover, able to show that the emergence and establishment of metric geography during the early modern period can only be historically explained by means of the simultaneous development of the technique of perspective—this is a natural development implicitly suggested by Ptolemy's third kind of projection in the seventh book of his *Geography*. The author concludes the argument by pointing to the fact that the emergence of the new early modern cartography is based not only on the revitalization of ancient geography but also on the technique of perspective that was being developed, eventually also under the influence of Ptolemy's works. Particularly interesting is the author's point regarding the long-lasting debate on whether ancient culture knew the technique of perspective: although it is true that the reconstruction of Ptolemy's techniques (which he described carefully) does not lead to the linear perspective as based on projective geometry, it is nevertheless also true that its technique allows the creation of a stable two-way relation between the plane image and the space that it represents, while the internal metric relations also correspond to those of the real space that is represented. The paper concludes by briefly touching upon the role that optics played in connecting figurative arts and geography during the early modern period, as was suggested by the Ptolemy as well.

The last paper, Claudio Schiano's 'La forma del mondo secondo gli antichi: un esercizio iconografico nel XVII e XIX secolo' [233–265], is an interesting excursus on the history of the history of geography. The author finds the origins of this discipline in the 15th century as a consequence of the travels and explorations that led to a continuously changing and expanding of the rediscovered ancient geography and cartography. The argument extends to the 19th century. It focuses on the historical interpretations furnished for the works of Strabo, Ptolemy, Pomponius Mela, and Eratosthenes, where the last became an object of study for the first time during the 18th century. The historians of geography taken into consideration are, among others, Cellarius and Pascal-François-Joseph Grossellin. Notably, the author shows how history of geography emerged and was established on the basis of the attention given to cartography, as if the attempts to reconstruct the maps of ancient works (even when those maps did not exist before, as in the case of Strabo's work) was the method for historically investigating ancient geographic knowledge. Unfortunately, the integrative potential of the last paper in reference to the entire section is not exploited sufficiently.

3. Medicine

Of the five papers that are presented in this section, only the last two deal with the main topic of the book.

The first paper by Domenico Ribatti, 'Simmetria e asimmetria del corpo umano' [269–276], is a very short text consisting of a series of statements aiming to clarify the concept of symmetry and how it transformed from antiquity—proportion and harmony being observable in nature—into the modern definition related to the abstract idea of invariance in the context of a group of transformations, which emerged for the first time in the 19th century in crystallography.

The second and third papers both investigate the figure of the physician in antiquity, though not the related images. In Olimpia Imperio,'s 'Immagini del medico nella tradizione comica antica e moderna' [277–292], the aim is to discuss the social role of physicians in classic societies. The study is accomplished by investigating and analyzing works in the ancient tradition of comedy. Emphasis is put on the fact that physicians were the target of polemic as they were considered to be part of the sophistic school, in the negative meaning of the term. The paper concludes with an outlook on modern comedy up to the works of Molière.

In Luigi Piacente's 'Medici, libri e biblioteche nella Roma capitale' [293–310], the aim is likewise to define the figure of the physician in antiquity, especially during the Roman Imperial Era. By starting with an *exposé* of the history of Galen's famous library, the argument focuses on the organization of libraries in imperial Rome and furnishes a key argument to understanding what might have been the physicians' role in the libraries, a curious presence nevertheless demonstrated by historical sources. The author concludes that the continuous presence of physicians in the libraries was most probably due to the practice of copying, which implied the preparation of relevant quantities of ink. The in-depth analysis shows that cases of poisoning through metallic oxides certainly occurred frequently enough to justify such organization of the library personnel.

Concerning the role of images, the fourth and fifth papers of the section respectively deal with the early modern editions of Galen's work and with a medical text from late antiquity.

After a short overview concerning the origins of the use of images in medical texts during the early modern period and, thus, the works of Andreas Vesalius and Bartolomeo Eustachius, Stefania Fortuna's 'Le illustrazioni dei testi medici: le edizioni latine di Galeno del XVI–XVII sec.' [311–338] focuses on the images provided with the Latin editions of Galen's work from the 15th to the 17th centuries. Twenty-five editions of the complete works of Galen were published in Latin between 1490 and 1625. The first to be provided with images appeared in 1545. On the basis of a philological study, the author executes a detailed review of the editions' images, taking into consideration how they are related to each other and also to the authors of the images themselves. In particular, the paper shows the enormous success of Giovanni Bernardo Feliciano's images, which were used in 11 entire editions of Galen's work between 1550 and 1679.

The last paper of the section, Raffaele Passarella's 'Aspetti di medicina ginecologica nel tardoantico: Muscione e il parto' [339–356] deals with the *Gynaecia* of Muscione, an author of the sixth century AD, and notably with the images depicting birth and the eventual difficulties that could arise during birth. According to the author, the images accompanying the text, which was supposedly well known during the Middle Ages and the early modern period, had a didactic purpose. In this way, the author is able to explain both the taxonomic character of the images and their almost diagrammatic simplicity that was maintained over the centuries.

4. The natural sciences

The section on natural sciences appears to be the most heterogeneous. Of the four papers, only the last, Franco Minonzio's 'Diffrazioni pliniane prima di Belon (1553): descrizione e classificazione di pesci in Paolo Giovio, Francesco Massari e Simone Porzio' [401–442], deals directly with images according to the general scope of the work. The paper indeed focuses on the relation between images and texts in the early modern editions of Pliny's *Natural History* and with particular reference to chapter nine which concerns aquatic animals. One of the papers, Pierfrancesco Dellino's 'Immaginare la scienza'[289–400], is surprising in that it does not seem to be the result of historical research. Although this comparatively short paper begins with the analysis of Pliny the Younger's letters concerning the eruption of the Vesuvius in the year AD 79, it is in fact more a manifesto in favor of a historical approach in the actual practice of science.

The remaining two papers both focus on textual sources, one on a poem written in the first century AD and the other on a work on geology from the 18th century. The latter, Lucio Cristante's 'Acque, fuochi, pietre, fossili tra letteratura antica e geologia' [359–374] investigates the possible relations between a modern scientific discipline, for instance geology, and ancient science. It analyzes mainly the work of Anton Lazzaro Moro, one of the founders of the discipline in the 18th century. The author shows that, in its origins, geology made profound use of ancient sources from the field of literature and also from the field of science. The former paper, Stefania

Santella,'s 'L'«Aetna (App. Verg.)»: scienza ed etica' [375–388], analyzes a short anonymous didactic poem from the first century AD 'Aetna' whose content refers to what is today known as vulcanology. The text is fundamentally influenced by Lucretius' work and, according to the author of the present paper, conveys all the aspects of scientific knowledge concerning ancient vulcanology. At the same time, the poem also represents a moral call for scientific investigations and therefore unifies scientific practice and ethics.

Conclusion

In spite of the somewhat narrow focus suggested by the book's title, not all of the papers investigate the role of images in the transmission history of ancient scientific works during the early modern period. Clearly, some of them explore histories of transmission between epochs, which do not always belong to the ancient or the early modern epochs. This certainly is a positive aspect, as reception history as a historiographical category can unquestionably be understood on the basis of multiple relations between epochs. More problematic, however, is the idea that images play a significant role only in the context of transmission, as the editor seems to indicate in the introduction to this work. As mentioned, and as many of the papers also demonstrate, the role of images is much too profound and relevant to be reduced to a simple transmission of knowledge from one epoch to the other. The section on geography and some of the papers of the other sections clearly show the integrative and productive role of images in their construction and in their use for scientific practice in any epoch and in reference to the connections between epochs. Although some of the papers are of outstanding quality, like for instance those dedicated to geography, it can only be concluded that the editor has missed the chance to deepen the fundamental historiographical question concerning the role of images in the history of scientific practice.

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Domninus of Larissa, Encheiridion *and Spurious Works. Introduction, Critical Text, English Translation, and Commentary* by Peter Riedlberger

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Domninus of Larissa, a contemporary of Proclus, the great head of the Platonist school of Athens in Late Antiquity (412–485), has enjoyed a somewhat intriguing, if marginal, existence in the history of philosophy and of mathematics as a fellow-student of Proclus—their teacher was Syrianus, Proclus' predecessor as head of the school—whose Platonism would be 'purged' in a book by Proclus, and as the author of a short manual, the *Encheiridion*, where, according to the great 19th-century historian of science, Paul Tannery, we find a reaction to the arithmetic of Nicomachus of Gerasa and a return to the rigor of Euclid. Tannery's view has since been repeated and indeed embroidered on, reaching almost dramatic dimensions in the only book devoted in particular to Domninus [see Romano 2000].[1] However, the present monograph by Peter Riedlberger shows how ill-founded the received view is by means of a comprehensive presentation and detailed analysis of the evidence concerning Domninus. Riedlberger provides us here, I believe, with the most thorough and reliable examination of the subject as a whole.

In the introductory part of his book, Riedlberger first collects and assesses all of the evidence that we have concerning Domninus' life, presenting this in the context of a description of the school of Athens in the fifth century, an account of the teaching of mathematics which, in this period, was part of the philosophical curriculum, and a full examination of what we know about Domninus' life (his family background in Syria, his studies in Athens,

[1] Riedlberger [13–14] shows how almost all references to Domninus in modern works repeat Tannery's judgement, an edifying example of how scholars repeat each other and rarely take the trouble to look for themselves at the primary sources. Riedlberger addresses Romano 2000 on page 16.

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his conflict with Proclus and other anecdotes, his later life). In this introductory part, Riedlberger not only demonstrates a very good knowledge of the secondary literature, he also checks, and sometimes translates anew, the ancient sources for what we know about Domninus. However, he does not always himself escape the influence of the judgements of earlier scholars. Thus, he repeats Dodds' view of the philosophy of Late Antiquity as veering to the 'irrational' [28] and depends on Lewy for the question of theurgy. The considerable growth in research in more recent times allows for a more differentiated view of these themes. Of more importance, perhaps, to the subject of Domninus is the account given by the Patriarch Photius (ninth century) of Damascius' *Philosophical History* (or *Life of Isidore*), one of the most important ancient sources for our knowledge of Domninus: Riedlberger [57, with 26] accepts too readily the Patriarch's hostile and biased account of Damascius' work [on this, see O'Meara 2006, 88].

The works attributed to Domninus are surveyed next. Riedlberger provides a full demonstration that two manuscripts of a commentary by Domninus on Aristotle's *Sophistical Refutations* survived in the Renaissance period but have since been lost. This evidence is important in that it shows that Domninus worked on Aristotelian logic as well as on mathematics. And we know from Proclus that Domninus suggested interpretations of Plato's *Timaeus*. This shows, as Riedlberger indicates, that Domninus was not a 'pure' mathematician in the sense that he had no interest in the philosophical disciplines also taught in the school of Athens, but that he had rather a profile corresponding to that of a member of that school. Riedlberger also discusses an *Elements of Arithmetic* to which Domninus refers in his *Encheiridion* but which is not extant. Domninus' indications as to the content of this work show that a major impulse for the study of mathematics was the need to understand better the difficult mathematical passages to be read in Plato (in particular in the *Republic* and *Timaeus*), a need to which Domninus, Proclus and, before them, other Platonists such as Theon of Smyrna [see now Petrucci 2012] sought to respond.

The *Encheiridion*, the only work of Domninus that survives, is a very short summary (seven pages of Greek in Riedlberger's edition) of basic notions of ancient number theory. Riedlberger shows that there is no reason to doubt the attribution of the work to Domninus and proposes an interesting and plausible explanation of its title, «ἐγχειρίδιον ἀριθμητικῆς εἰσαγωγῆς», as referring in fact to Nicomachus of Gerasa's *Arithmetical Introduction*. The title should thus be read 'Encheiridion of [Nicomachus'] *Arithmetical Introduction*'.[2] Ancient *Encheiridia*, short manuals, could be produced on the basis of a larger text, a well known example of this being the *Encheiridion* of Epictetus (a manual familiar to late antique Platonists) which Arrian produced on the basis of the *Discourses* of Epictetus. Riedlberger's interpretation of the title has, of course, further implications: far from being a rejection of Nicomachus in favour of Euclid (as Tannery's story has it), Domninus is in fact basing his work on Nicomachus' manual. Riedlberger compares Domninus with Nicomachus in detail [74–75, and in his commentary on the text], showing that Domninus largely follows Nicomachus and uses him, while sometimes using Euclid, who was also read in the school of Athens. Domninus is not, then, a maverick mathematician who rejected the mediocre arithmetic of Nicomachus so admired by Proclus and the other members of the school in favor of the scientific Euclid. These modern evaluations, Riedlberger shows, are inappropriate and unfounded. But what can the *Encheiridion* tell us about Domninus as a mathematician? Riedlberger poses this question and answers:

> Virtually nothing, actually. The few original traits listed above do not suffice to single out Domninus as an unusual arithmetician, and if so bare a list of definitions as the *Encheiridion* contains little metaphysical speculation, this does not need to be explained by the 'scientific' stance of the author, but could simple be due to its brevity.[3] [77]

Riedlberger then goes on to survey works sometimes associated with Domninus but for which there are no good grounds for attributing them to him. In one manuscript (Parisinus graecus 2531 = **S**), the *Encheiridion* is followed by a short work *How to Remove a Ratio from a Ratio*. This seems to be the reason why the latter work came to be associated with Domninus; but, as Riedlberger shows [79], this does not justify an attribution of the work to him. However, Riedlberger suggests that the work may come from a *milieu* similar to that of the *Encheiridion* and may date to the 5th/6th century

2 The English version of the title given by Riedlberger ('*Encheiridion* of "Arithmetical Introduction"') will hardly do: I think a definite article is required ('Encheiridion of the "Arithmetical Introduction"') or, as I suggest above, '[Nicomachus']' could be inserted. The Greek title of the *Encheiridion* might also require the insertion of a definite article («τῆς»), if it does indeed refer to Nicomachus' book.

3 I came myself to a similar conclusion in O'Meara 1989, 145.

[82-83]. In manuscript S, the work *How to Remove a Ratio from a Ratio* is followed in turn by anonymous scholia on Nicomachus' *Arithmetical Introduction*. Here also, Riedlberger finds no grounds for attributing the scholia to Domninus [83], while locating them again in the same *milieu* as that of the previous two texts. However, as he notes, so much of the corpus of ancient scholia on Nicomachus remains unpublished that it is not possible at present to locate these scholia more precisely. Finally, Riedlberger discusses certain *Summaries of the Principles of Optic*s by a 'Damianus of Heliodorus of Larissa'. He shows that there is no good reason for identifying this 'Damianus' as Domninus [83]. 'Of Heliodorus' could be a patronymic: this is not as rare as Riedlberger thinks in late Antiquity. To his example of Ammonius, son 'of Hermias' we could add Syrianus, son 'of Philoxenos'. Although rejecting an attribution of *How to Remove a Ration from a Ratio* and the anonymous scholia on Nicomachus to Domninus, Riedlberger undertakes to provide an edition and translation of these texts[4] along with that of the *Encheiridion*, since a new critical edition of these texts is relevant to the question of Domninus. The edition of the anonymous scholia is the first ever. However, Riedlberger does not include the *Summaries of the Principles of Optics*, since an edition of this is due to be published by Fabio Acerbi.

A critical edition of Domninus' *Encheiridion*, of the anonymous *How to Remove a Ratio from a Ratio*, and scholia to Nicomachus follows. Riedlberger gives a detailed description of the manuscripts, of which two have been added by him to those already known for the text *How to Remove a Ratio from a Ratio*. He has examined the manuscripts and taken note of the corrections, conjectures (and errors!) of previous editors, reporting fully on all of this in the *apparatus criticus*. I believe that this work has been done thoroughly and carefully, and that it can be used as a basis for future work on Domninus. The English translation also seems to be reliable on the whole, clear, and accurate. On some points there may be disagreement or difficulty. For example, it might be wiser not to give the term «θεωρία» in English (transliterated) as 'theory' (for example at 110.11, translated as 'for the myriads [10,000's] have the same theory'), since this might be a source of confusion. Riedlberger relates the Greek work to its verb, which he translates as 'to consider' [153–154]. However, the word can have a stronger meaning,

4 They are not printed by Romano [2000], who provides just the Greek text of the *Encheiridion* together with an Italian translation.

that of knowledge or doctrine, and would mean here 'the same doctrine concerns the myriads'. It would have been better to translate «φάσιν» at 116.9, 21 and 28 as 'they say' (rather than 'is said'), since Domninus is probably referring to the 'ancients' (Plato, Aristotle, Pythagoreans) who start to figure more prominently at the end of the *Encheiridion*. «παραδώσομεν» at 120.18 probably has more the sense of 'teaching' than of 'presenting'.

However these are minor points. The commentary which follows the edited texts is extremely developed, sometimes a bit verbosely, and provides the non-specialist reader with the necessary information about ancient number theory along with detailed comparisons with Nicomachus, Euclid, Theon of Smyrna, and other ancient mathematicians which confirm Riedlberger's general assessment of Domninus' work.

The book ends with a full bibliography, indexes of texts and of Greek terms, and a general index. All in all, Riedlberger has provided us with a very complete and thorough basis for dealing with Domninus. The quality of his work is in general very good and it will also be of great use to those who wish to work on the teaching of elementary arithmetic in the philosophical schools of Late Antiquity. The book is beautifully produced and is a pleasure to see and to read.

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Thomas Harriot and His World: Mathematics, Exploration, and Natural Philosophy in Early Modern England edited by Robert Fox

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Thomas Harriot is an enticing figure for many historians of science. He seems to have done everything and did these things (novel insights in the theory of equations, optics, ballistics, and astronomy to name a few) well before those who achieved recognition for their discoveries. He trained men in navigation, was on board on voyages of exploration, and took part in establishing an English colony in the New World. He made a lexicon of Algonquian and published one of the earliest accounts of America in English. He watched his patron, Ralegh, be executed and performed experiments with his next patron, Henry Percy, the Earl of Northumberland, in the Tower of London, where Percy was imprisoned. He appears in the poetry of his friend George Chapman and seems to have associated with the same people as Christopher Marlowe. Harriot lived a fascinating life. He entices because he is intrinsically interesting and brilliant. But there is more that has drawn the historian to Harriot.

Harriot is an underdog. Although his mathematics was excellent and novel, although he discovered the sine law of refraction before Descartes and Snell, although he observed the Moon through a telescope prior to Galileo, although he worked through significant problems in mechanics, he gained almost no recognition. He was largely forgotten and for many centuries assumed to be rather insignificant. Harriot excites a desire among some historians to restore honorably and dutifully his reputation—to give his works 'the recognition they deserve'. This characterizes aspects of Jon V. Pepper's chapter in *Thomas Harriot and His World*, 'Thomas Harriot and the Great Mathematical Tradition' [11–26], as well as the chapter by Jacqueline Stedall.

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Harriot is a puzzle. The historian finds in his manuscripts algebraic symbols, numbers, and diagrams, with little continuous prose.[1] Harriot inspires historians motivated by puzzles to piece together an intricate and difficult jigsaw as seen, for example, in Jacqueline Stedall's chapter, 'Reconstructing Thomas Harriot's Treatise on Equations' [53–64].[2]

Harriot seems to invite speculation. Since there is relatively little prose in his manuscripts and since he famously published very little, Harriot's ideas are not always apparent. Harriot's unpublished documents take the form of many tantalizing pieces. Some historians have produced compelling narratives and have attempted to provide meaning and coherence to the traces and fragments, e.g., Robert Goulding in his chapter, '*Chymicorum in morem*: Refraction, Matter Theory, and Secrecy in the Harriot-Kepler Correspondence' [27–51].[3]

Harriot is also a locus of reform. Harriot prompts other historians to challenge the many and sometimes entrenched speculative narratives and inferences that have been drawn, which they see as unlicensed stories that have unnecessarily and falsely embellished an individual whose work can stand on its own. Harriot's extensive manuscripts have proven to be an area for historians to uncover carefully what they may see as the 'real Harriot' or at least to state confidently that there is not enough evidence to know this or that claim that has been made. This theme can be found in John Henry's chapter, 'Why Thomas Harriot Was *Not* the English Galileo' [113–137] as well as in Ian Maclean's 'Harriot on Combinations' [65–87].[4]

1 The Harriot manuscripts from the British Library have been put online through the Max Planck Institute for the History of Science and European Cultural Heritage Online (ECHO): http://echo.mpiwg-berlin.mpg.de/content/scientific_revolution/harriot. As Fox notes in his introductory essay [3], contributors Jacqueline Stedall and Matthias Schemmel are planning a digital edition of all of Harriot's manuscripts.

2 I have borrowed the puzzle metaphor from Stedall, who explains that

> [engaging with the history of mathematics] is like trying to fit together a very large jigsaw in which most of the pieces are missing and one is not allowed to look at the picture on the box. One always hopes, of course, that some new and vital piece will turn up, but one knows all too well that it may not. [53]

3 Some past examples include Gatti 2000, Rukeyser 1970, and Yates 1936.

4 See also, for example, Clucas 2000.

The status and reputation of Harriot is a compelling *explanandum* that defies a strict intellectual history. Harriot's work seems to have been just as good if not better than the best of his contemporaries; and yet Harriot's status was minor and, until fairly recently, he was largely forgotten. This invites the social historian of science to explain why Harriot was not recognized and why he was not as influential as his contemporaries. A great many of the chapters take up this issue in some form but Stephen Pumfrey takes it on in a novel and sustained manner in 'Patronizing, Publishing and Perishing: Harriot's Lost Opportunities and His Lost Work "Arcticon"' [139–163].

Thomas Harriot, Renaissance man, explored many worlds and provides topics of interest to a variety of historians of early modern science. He is an enduring figure in the modern study of the history of science, who has proved to be irresistible to a great many. Some devote the majority of their waking lives to him;[5] others happily call themselves 'Harrioteers' and, as the dedication of Fox's edited volume [v] indicates, many see themselves not as specialists in a historical figure but as themselves students of Harriot.

Thomas Harriot and His World: Mathematics, Exploration, and Natural Philosophy in Early Modern England is the latest compilation of Thomas Harriot Lectures given from 2001–2009 at Oriel College, Oxford. The first set of lectures dating from 1990 has been published as *Thomas Harriot: An Elizabethan Man of Science*, which is also edited by Fox [2000]. Oriel is a fitting location since Harriot himself spent time at St Mary Hall, which is now part of Oriel, earning a BA in 1580. Modern historians of science have been meeting in Oxford since at least 1967 under the promotion of such scholars as David Quinn, Alistair Crombie, John North, and John Roche, to discuss their shared interest in Harriot. In 1977, Dr R. C. H. [Cecily] Tanner financed a biennial meeting in Durham chaired by Gordon Batho which continues to meet to the present as the Thomas Harriot Seminar (THS). The volumes produced from the Thomas Harriot Lectures, together with the publications

[5] As Shirley remarks:

> It has been more than thirty-five years since I began seriously to study Thomas Harriot. ...[T]his fascinating man has occupied a large portion of my waking thoughts during this period. Following his elusive genius has taken me to most of the places that Harriot himself visited during his sixty years.... [1983, v]

arising from the Thomas Harriot Seminars,[6] are joined by the previous, *Thomas Harriot: Renaissance Scientist* [Shirley 1974]. This volume is composed of papers presented at the Thomas Harriot Symposium organized by John Shirley and held at the University of Delaware in April 1971.

In the late 1940s, Shirley gained access to a significant set of Harriot's manuscripts which had been kept at Petworth House after the majority of them had been sent to the British Museum as a gift around 1810 [1983, 20].[7] With the succession of John Edward Reginald Wyndham as Sixth Baron Leconfield, and First Baron Egremont in 1967, the Petworth collection was opened further to Harriot scholars. This tradition has continued and expanded with his son, (John) Max Scawen Wyndham, the current Lord Egremont and Leconfield, who provides financial support for the Harriot Lectures and to whom *Thomas Harriot and His World* is dedicated.

The chapters in *Thomas Harriot and His World* were written over nine years by historians from several different countries and scholarly backgrounds. Nevertheless, there are several themes running throughout the volume. In his eloquent introduction, Fox has summarized the chapters and tied them together as 'The Many Worlds of Thomas Harriot' [1–10]. In this review, we will focus on the discussions of Harriot's mathematics, its possible influence, and its worth as a window into the shared knowledge of the time. The notion of mathematics as a style of thinking and occupation will be reviewed as well as the distinctions between mathematics and natural philosophy. We will briefly survey the persisting disagreements regarding whether Harriot had a natural philosophy. In addition to mathematics and natural philosophy, we find a more complex and robust portrait of Harriot's character presented by various chapters. This will inform a discussion of the perennial topic of Harriot's lack of publications and subsequent status and culpability.

Jon V. Pepper has been studying Harriot's mathematics and science at least since his PhD thesis on the topic in 1979. In his contribution to *Thomas Harriot and His World*, Pepper focuses on the mathematical work of Harriot in a way that is self-consciously independent of the social context of Harriot's

6 http://www.bbk.ac.uk/english/our-research/research_seminars/thomas-harriot-seminar/ths-publications.

7 Harriot's manuscripts have had a colorful history: see Shirley 1983, 1–33.

time and place [11].[8] In Pepper's assessment, '[Harriot] belongs to the great tradition of mathematicians' alongside the likes of 'Eudoxus…Viète, Newton, Gauss, Maxwell, [and] Poincaré' [25]. This is due to the virtues of Harriot's work itself, namely, that

> he applied not only the existing mathematics of his time to various problems …but also created new ideas, new notations, techniques and theories. [25]

To illustrate this claim, Pepper dedicates the majority of his chapter to summarizing several examples of Harriot's mathematical work and notes previous scholarly studies that have examined his work more thoroughly. In addition to some of the more well known discoveries of Harriot,[9] Pepper devotes several pages to discussing in some detail Harriot's work 'rectifying' the plane equiangular spiral and the twisted loxodromic spiral, in other words, Harriot's efforts to find the exact length of these curves. This is notable since, as Pepper indicates, 'no-one before Harriot had found the exact length of any curve' [17] and since Descartes after him thought such lines did not have determinable length at all but were 'beyond human knowledge' [16].

In addition to Pepper's evaluation that Harriot should belong to the 'great tradition of mathematicians' based on the independent merits of Harriot's mathematical work, Pepper makes another claim regarding the *influence* of Harriot in the larger trends of the history of mathematics:

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8 Chapter 1, 'Thomas Harriot and the Great Mathematical Tradition', was originally presented as the 2000 Thomas Harriot Lecture.

9 Pepper notes Harriot's discoveries in algebra, particularly his work developing *methods* (such as the interpolation and area methods) to solve problems expressed in 'algebraic symbolisms' [14]. He discusses Harriot's discovery of the sine law of refraction (prior to both Descartes and Snell) and his applications of it to 'questions of dispersion and the height of the primary rainbow' [13, 15]. He briefly covers Harriot's work on the theory of impacts, his work as a calculator, his work in navigation and astronomy, and his work in calculating 'the extended meridian lines necessary to construct a Mercator mapping' [16]. He mentions that because of Harriot's use of the binary decomposition of integers to help calculate exponentials, Harriot 'forms part of a continuing chain' extending from 'Ah'med (Rind Papyrus c. 1650 BCE) to modern computer work' [23]. And he notes that Harriot applied 'quite sophisticated mathematics' to shipbuilding and design, and ends by mentioning that Harriot correctly described parabolic trajectories in ballistics which he obtained by 'combining uniform motion with orthogonal uniform acceleration, one of Galileo's best-known results (1638)', although this was done, of course, independently of Galileo [25].

> If we have to look in general terms, what is most notable...is his contribution to the move away from geometrical to algebraic formulations. This move is often regarded as an eighteenth-century development, but it was in fact a late sixteenth-century development, first by Viète and then by Harriot, and it has been the dominant movement of the four centuries since that time. [25]

This is an intriguing claim and may well be true. However, it is unsupported in the chapter. Pepper provides no account of how Harriot's algebraic work may have been influential in the larger movements of mathematics, particularly since, in his own words, he does not discuss the 'intellectual ambience or climate of [Harriot's] place and times' [11]. He also does not provide an account of the possible ways in which the mathematical work in Harriot's manuscripts may have had any impact at all. Harriot was proficient and original in his work in algebra, as Pepper's chapter makes clear. But, since Harriot famously did not publish—which Pepper himself notes and attempts to explain—and since much of his reputation was based on a posthumous publication that did not present his work as favorably as it may have (*Artis analyticae praxis*, 1631), an explanation is needed for how Harriot *contributed* to the movement of mathematics rather than independently working in relative isolation in areas that may have become characteristic of broad trends.

This lacuna is filled, at least partially, by Jacqueline Stedall. She notes in her contribution[10] that publishing mathematical works was actually 'the exception rather than the rule' in England in the first half of the 17th century [61], a point that is supported by Stephen Pumfrey's arguments[11] from the studies of patronage included in the volume, which will be discussed more below. For Stedall,

> Mathematical ideas were exchanged freely amongst...[those] who were interested in them by means of letters, manuscripts and conversations. There is evidence that Harriot's manuscripts remained in circulation for up to 30 years after his death. [61]

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[10] Chapter 3, 'Reconstructing Thomas Harriot's Treatise on Equations' was originally presented as the 2002 Harriot Lecture as 'The Greate Invention of Algebra: Thomas Harriot's Treatise on Equations'.

[11] See Pumfrey's argument in chapter 7 [139–164] as well as his argument in Pumfrey 2003, which Henry has conveniently summarized in chapter 6 [115–117].

She cites Beery and Stedall 2009 for an argument that Harriot's ideas persisted by 'word of mouth' and catalogues later mathematicians in England such as Walter Warner, Thomas Alesbury, John Pell, Charles Cavendish, and the Savilian Professor of Geometry at Oxford, John Wallis, who all knew of and appreciated Harriot's work on algebra.

However, Stedall's purpose is not to argue the extent to which Harriot was responsible for a shift away from geometric to algebraic formulations. Instead she documents the existence of a significant 'Treatise on Equations' written by Harriot, inspired by Viète's *De numerosa potestatum resolutione*, but scattered through Harriot's manuscripts. Although the manuscripts were divided between the British Library and Petworth House, Stedall has reassembled the treatise which contains '[Harriot's] reworking in his own notation of Viète's *De resolutione*', and which goes beyond Viète to include his own method of solving quadratic equations by factorization, an idea of 'profound significance because it enables mathematicians not just to solve equations but also to look inside their structure', and utilized a method of comparison with 'canonical equations' [56]. This treatise, uncovered and reassembled by Stedall, is more thoroughly argued for and examined in a publication subsequent to her Harriot Lecture in 2002 [see Stedall 2003].

Like Pepper, Stedall is an open advocate of Harriot's reputation as a great mathematician: as she writes,

> In attempting to restore Harriot's original 'Treatise on equations', I see myself as but the latest in a long line of people who have hoped that Harriot and his algebra would eventually get the recognition they deserve. In the seventeenth century alone, William Lower, Nathaniel Torperley, Thomas Aylesbury, Walter Warner, John Pell and John Wallis all tried to see justice done to Harriot and his mathematics. [63–64]

Much of Harriot's reputation has been based on the posthumous publication known as *Artis analyticae praxis* (1631), which was put together from manuscripts, likely by Harriot's friend Walter Warner, although Harriot had named Nathaniel Torperley in his will to oversee and publish his mathematical writings. According to Stedall, the *Praxis* was 'in many ways a travesty of [Harriot's] original intentions' [60]. Relying on corroborating evidence from an unfinished manuscript by Torperley entitled 'corrector analyticus', which was in his own words, 'An Analytic Correction of the Posthumous Work of

Thomas Harriot', Stedall argues that Harriot's original intentions consist of a document much like the 'Treatise on Equations' that she has restored.

Even if it was the case—contrary to Pepper and Stedall—that Harriot effectively had no influence in the main developments of what would become modern science, the particulars of his work are still important to understand. Whereas Pepper examined the worth of Harriot's ideas in abstraction and Stedall has both reconstructed his ideas and indicated that they were championed by his peers and some of his immediate successors, Matthias Schemmel has taken quite a different approach.[12] Rather than imply that the force of Harriot's good ideas must have provided a 'link in the chain', and rather than claim that Harriot was nonetheless influential contrary to what one might think due to his lack of publications, Schemmel is interested in Harriot for the very fact that he was not influential, at least when compared with someone like Galileo. Here is why: Schemmel emphasizes that 'the thinking of an individual is governed to a large degree by knowledge that is shared with his or her contemporaries, or certain specialized groups of contemporaries' [90]. A study of a rather obscure individual's work on motion, for example, and a comparison of it with Galileo's work on motion could provide insight into this 'shared knowledge' of early modern mechanics—the loose set of ideas stemming from a variety of sources and experiences such as Aristotelian physics, techniques of medieval calculation, and the 'practical knowledge of engineers and gunners' [90]—which interested individuals would have had available to them in beginning to think about motion.[13]

Schemmel has since extended this argument and provided a 'comprehensive reconstruction, analysis and interpretation of Harriot's work on motion' [90] in *The English Galileo: Thomas Harriot's Work on Motion as an Example of Preclassical Mechanics* [2008]. In doing so, Schemmel manages to provide an excellent intellectual history of Harriot's and Galileo's similar studies of projectile trajectories, while providing a means to tackle much larger questions regarding the nature of scientific development such as 'To what extent do the peculiarities of an individual scientist's work influence its outcome?' and 'Do the peculiarities of an individual scientist's work lead to diverging develop-

12 Chapter 5, 'Thomas Harriot as an English Galileo: The Force of Shared Knowledge in Early Modern Mechanics' was originally presented as the 2004 Harriot Lecture.

13 Schemmel uses the term 'pre-classical mechanics' to refer to this loose collection of ideas and practices. In doing so, he self consciously follows Damerow *et al.* 2004.

ments in science? Would we have a completely different physics today had there been no Galileo? Or do alternative developments converge?' [91] At least in the case of Harriot and Galileo, whose 'inferential pathways' proceeded in opposite directions from each other, evidence suggests the latter.

Several of the other contributions also take up the topic of mathematics; but rather than engage with the mathematics itself, they address it in terms of a style of thinking, a title, or as an occupational category distinct from others such as natural philosophy. Perhaps because Harriot's achievements have been sung so strongly, some scholars now find it necessary to explain why, despite 'all of his astonishing genius' [115], Harriot did not rise to the level of Galileo, Kepler, or Descartes. Henry, in his contribution,[14] notes a tradition of frustration among Harriot scholars who want to champion his reputation, lament his lack of recognition, and wish that he had fully articulated a philosophy of nature [125].[15] Henry argues that what makes Harriot distinct from the reputable Galileo and thus not an 'English Galileo' so to speak, is that Galileo was a mathematician who strove also to be a natural philosopher. Not only did Galileo achieve both titles, he transformed natural philosophy into something new in the process, combining speculative philosophy with mathematics and experimentation. Harriot, on the other hand, was an excellent mathematician but he never endeavored to be a natural philosopher or, as Henry would put it, he *refused* to be one [125].

According to Henry, Harriot was essentially a mathematician and approached the world intellectually as a mathematician. He did not seek natural explanation with causal narratives as a natural philosopher would. Instead, he measured and reported and solved problems: and Henry claims that there is little evidence that Harriot ever speculated or drew conclusions from these activities [128]. To support this view, Henry points to Harriot's pictures of the Moon, noting the lack of explanation or speculation, and contrasts this with Galileo's *Sidereus nuncius* (1610), which draws conclusions from Galileo's pictures of the Moon. For instance, the Moon is not a perfect sphere—the patterns seen on the Moon through the telescope are not just patterns to be drawn and recorded but are to be understood as mountains and craters. According to Henry, it is likely that 'Harriot would still have seen mathematics

[14] Chapter 6, 'Why Thomas Harriot Was *Not* the English Galileo' was originally presented as the 2005 Harriot Lecture.

[15] Particularly, he cites Shirley 1983, North 1974, Gatti 2000, and Jacquot 1974.

and natural philosophy as separate and distinct enterprises' [134]. He likely respected this separation and 'was always thinking as a mathematician' [128].

An important plausible exception to Henry's thesis is Harriot's alleged matter theory, specifically his variety of atomism, which has been discussed at length by modern historians of science since at least 1966 with the publication of Robert Kargon's *Atomism in England from Harriot to Newton*.[16] However, there is not agreement among historians on the particulars of his theory or if it is even appropriate to say that Harriot had a theory.[17] (This will be discussed more at length below.) Henry cites some of the evidence that scholars have typically drawn on to argue that Harriot was an atomist: Torperley's criticism of Harriot for being an atomist and a reference to the topic in Harriot's correspondence with Kepler. But in response, Henry attempts to provide a deflationary account. He claims that it may have been nothing more than a debate between friends (Harriot and Torperley) or, if it was something more, Harriot's position was so weak that Henry finds it 'hard to believe that Harriot could have had much confidence in his own position' and, more generally, that 'it seems hard to imagine that he could have developed confidence in natural philosophizing by drawing upon atomism' [130, 131]. Despite Henry's excellent point regarding the differences between Galileo the natural philosopher and Harriot the mathematician, this argument is fairly unsatisfying. Even if it is true that Harriot's atomism was unpolished and did not rise beyond a debate between friends, this does not exclude it from being an example of natural philosophizing. Moreover, the claim that Harriot must have lacked confidence in his ideas simply because they are, in the assessment of the historian, weak is quite tenuous. And anyway, bad natural philosophizing would still be an example of philosophizing. Henry goes on to compare Harriot to Descartes, who brought together geometrical optics, an account of colliding bodies, and 'a matter theory that was closely modeled on atomism', and concludes that he was not an 'English

16 Kargon's discussion of Harriot has been repeatedly criticized. For example, see Clucas 2000, 102–103 and Bennett 2000, 139–140.

17 A nice overview of Harriot's place on the 'field of knowledge' according to several historians of science can be found in Stephen Clucas' contribution to the previous volume of Harriot Lectures [2000], particularly pages 94–106. Elsewhere, Henry has questioned whether Harriot should even be identified as an atomist [1982, 2010]. On the other hand, Hillary Gatti [2000] has argued that Harriot did have a natural philosophy which included a form of atomism.

Descartes'. This too, if true, only establishes that Harriot was different than Descartes. It says nothing about the existence of Harriot's efforts in 'natural philosophy'. Henry argues against the *hypothetical* position claiming that, since Harriot also studied optics, colliding bodies, and atomism, he must have been 'involved (before Descartes!) in trying to develop a new system of mechanical philosophy' [131]. But Henry cites no one who has supported this position. And more importantly, this is a different argument from one about whether Harriot pursued any form of natural philosophy to some extent.

Robert Goulding is also interested in the mathematical and natural philosophical occupations of Harriot and uses the above-mentioned correspondence with Kepler to draw some very different conclusions than does Henry. For instance, Goulding claims that Harriot was confident about his ideas based in atomism—so confident in their importance in fact that he felt it necessary to protect these ideas from being 'robd' by taking on the *persona* not of a mathematician but an alchemist. As a great many of the contributors to this volume note, even Harriot's contemporaries wished that Harriot had published his ideas and thought that he was continually being robbed of his inventions and glories when others published or presented ideas that he had discovered first. At least four of the contributors to the volume quote from the same passage from a letter that William Lower wrote to Harriot in 1610:[18]

> Do you not here startle, to see every day some of your inventions taken from you ...and yet to[o] great reservednesse had robd you of these glories...Onlie let this remember you, that it is possible by too much procrastination to be prevented in the honor of some of your rarest inventions and speculations. Let your Countrie and friends injoye the comforts they would have in the true and great honor you would purchase your selfe by publishing some of your choise workes. [Shirley 1983, 1–2, 400]

Through a close reading of Harriot's correspondence with Kepler, Goulding infers that Harriot tried, unsuccessfully, at least once, to inform the wider world about one of his 'inventions', namely, his results regarding refraction, understood according to a corpuscularian account of matter and light. By his reading of the letters, Goulding claims that Harriot wanted to stake his claim to superiority against his main rival in optics, Kepler, who at the time thought there was some 'mathematical regularity' to refraction but did not

[18] Goulding, Stedall, Henry, and Pumfrey all quote various portions of this passage (some more than what I have here reproduced).

yet have it [35]. Harriot wanted to do so without revealing his hand and thus without having yet another of his 'inventions taken'.[19] To accomplish this, responding to language used by Kepler to describe him, Harriot portrayed himself as 'a mysterious "initiate of nature"...revealing his discoveries only to shroud them in deeper obscurity' [39]. In response, a frustrated Kepler, '[who] saw himself as a model of openness' [38] wrote to Harriot that he was acting *chymicorum in morem*, which Goulding translates as 'just like an alchemist'. This, Goulding claims, was not only a mere *persona* that Harriot deliberately donned as a strategy, since alchemy was also an important part of Harriot's intellectual activity.

It may be the case that Harriot was motivated to attain recognition for this result and took on the *persona* of an alchemist to protect it. Goulding's explanation fits together quite nicely and he is very familiar with the source material. However, attributing motivations to individuals who lived 400 years ago on fairly sparse evidence is challenging. Such a motivation might explain why Harriot wrote in the obfuscating manner that he did to Kepler, but it also seems plausible that Harriot may not have been driven to seek any recognition at all. After all, as Goulding acknowledges, this would be the only outstanding case where Harriot might have sought recognition. Perhaps Harriot was satisfied in the roles made possible for him by his patron and was not interested in seeking personal recognition.[20] Nevertheless, Goulding's account is compelling.

Henry acknowledges that historians have wanted to attribute a natural philosophy to Harriot but he draws a sharp distinction between natural philosophy and mathematics, and argues that Harriot was interested strictly in the latter. Goulding, on the other hand, assumes that Harriot had a natural philosophy in which his optics, his account of collision, and his atomism were all a

[19] Goulding writes,

> Perhaps [Harriot] concluded that if he were to reveal his results 'freely and frankly', as Kepler exhorted him, he would surely see yet another one of his 'choise works' claimed as the invention of another. [39]

[20] See Pumfrey's argument regarding patronly manuscript culture *versus* commercial print culture [139–164], and the brief comments on the topic by Pepper [12] and Stedall [61].

part of it.[21] He then argues that Harriot was acting like an alchemist on purpose and that 'his study of refraction really was closely connected to his studies and experiments in alchemy' [29]. Optics, atomism, and alchemy are drawn into an intelligible whole to shed light on the correspondence with Kepler, which in turn reinforces the links that Goulding has made. What Henry argues to be separate and to bear no evidence of natural philosophy, Goulding assumes to be a natural philosophy. This dual tendency to infer the existence of a natural philosophy and to deny that Harriot had one is fairly longstanding and only one of many areas of disagreement about how to place Harriot in the 'field of knowledge', a topic addressed explicitly in several of the Harriot Lectures collected in the previous volume, *Thomas Harriot, An Elizabethan Man of Science* [Fox 2000].[22]

Ian Maclean also weighs in on the topic of natural philosophy in his contribution to the present volume.[23] Maclean is primarily concerned with the extent to which Harriot's combinatorial ideas were influenced by the 'social, political and religious context' (he claims that they were scarcely influenced at all by context), and the extent to which Harriot was marked by a 'scientific' or 'occult' mentality. But he is also interested in the relationship between natural philosophy (specifically, its connection to the contemplation of the godhead) and mathematics. He concludes that Harriot 'was capable of compartmentalizing his mind and of according different modes and degrees of commitment to different areas of his mental universe' [87].

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21 According to Goulding, Harriot's optics was based in a theory of the structure of matter. Since light is partially reflected and partially refracted by some materials, Harriot concluded that matter is particulate. If matter was structured as a regular array of atoms, when light hits it, some would be deflected and some would enter and pass through the spaces between the atoms, deflecting off each atom in a zigzag fashion. Although the zigzag path would be too small to see, the overall path of light through the array of atoms would be visible, which accounts for refracted light. Although Harriot does not explicitly express this theory in his writings, Goulding infers it from the critical writings of Harriot's friend and sometime critic, Torperley, as well as the diagrams in Harriot's manuscripts and Harriot's initially perplexing comments to Kepler which when interpreted in this light become more intelligible.

22 See Bennett 2000, Clucas 2000, Gatti 2000, and North 2000.

23 Chapter 4, 'Harriot on Combinations' was presented as 'Thomas Harriot on Combinations' as the 2003 Harriot Lecture.

Several of the contributions to the present volume, as well as past works, have noted Harriot's propensity for mathematics as well as for what appears in the manuscripts to be a preference to record, measure, and calculate, with relatively little speculation or explanation. This has been interpreted in a variety of ways. On one extreme, it is claimed that he was strictly a mathematician who was uninterested in natural philosophy and refused to participate in it. On the other, it has been claimed that although he did not always write out his thoughts, there is enough evidence to reconstruct what his natural philosophical positions likely were. Judging by this volume, the disagreements regarding Harriot's natural philosophy remain unresolved. The chapters of reworked lectures here provide a more robust and complex portrait of Harriot's character. In addition to a mathematical style of thinking and a mind apparently capable of compartmentalization, we find what appears to have been a streak of competitiveness, as seen in Harriot's interaction with Kepler in Goulding's chapter.

Pascal Brioist presents Harriot meticulously observing, recording, ordering, and learning the practices and technical language of the officers and crew on board transatlantic voyages.[24] Harriot comes across as curious, a man with a 'special capacity to absorb all sorts of practical knowledge', who was inventive in his ability to 'imagine original solutions', and who clearly had what Brioist calls a 'restless intelligence' [200].

In the rare cases where Harriot did write in Latin, for example, in the explanatory notes to the engravings in the de Bry Latin edition of Harriot's *A Briefe and True Report* (Harriot's account of America and his only publication), it is the assessment of Charles Fantazzi that Harriot wrote in fluent, elegant humanistic Latin [232]. Harriot's Latin also 'demonstrate[s] his familiarity with the classical authors' [236].[25]

In Mark Nicholls' fascinating biographic portrait of Sir Walter Ralegh's life in three acts with special attention on the 'final act' (his trial and execution) and its place in the public's imagination, we find a Harriot who is 'no fair-

[24] Chapter 9, 'Thomas Harriot and the Mariner's Culture: On Board a Transatlantic Ship in 1585', was presented as 'Thomas Harriot and the Worlds of Practice: Learning from Seamen and Soldiers' as the 2009 Harriot Lecture.

[25] Charles Fantazzi develops this argument in his contribution 'Harriot's Latin', which appears as appendix B.

weather friend', a Harriot who is congenial, 'extremely sociable', and loyal to the end [175].[26]

In the assessment of many, Harriot's status suffered because he did not publish. It is commonplace to claim that if Harriot had published his various works, they inevitably would have had an enormous impact on the development of mathematics and science. As Stephen Pumfrey notes [141], for 400 years people have been asking this same question, 'Why did Harriot not publish and secure his reputation?' As we have seen in the much quoted letter from Harriot's friend Sir William Lower in February 1610, Lower seems to imply that Harriot may have been 'prevented in the honor of some of [his] rarest inventions and speculations' simply because of 'to[o] much procrastination'. In the current volume, Goulding fastens on this alleged personal foible and elaborates: Harriot may have been prevented from publishing by 'excessive caution' and 'insecurity'. Relying on evidence from Harriot's will, Goulding claims that if Harriot had intended to publish his works during his lifetime, 'even the task of discovering which were of any significance was, it seems, beyond him' [28]. This is essentially the received view. Pumfrey calls it the 'traditional and obvious explanation', namely, that 'Harriot was simply incapable of bringing...[his works]...to the level of completion that he desired and printers demanded' [155].

Jon V. Pepper modifies the received view by presenting six reasons for Harriot's 'non-publication', all of which lift much of the responsibility from Harriot. Pepper claims they are 'easy to see' but acknowledges that they are 'only conjecture'.

(1) Since Harriot had a generous patron, publication was less important.
(2) Some of his work was of a 'restricted' nature, 'classified' so to speak, and specifically not to be shared with others.[27]
(3) Printing the notations that Harriot developed would have been troublesome.
(4) Printers may have been sceptical that there would be an audience which could understand his mathematical works.

[26] Chapter 8, 'Last Act? 1618 and the Shaping of Sir Walter Ralegh's Reputation', was presented as the 2008 Harriot Lecture.

[27] Pepper's first two explanations, although only conjectures on his part, are corroborated by Pumfrey's argument as we will see.

(5) At times Harriot suffered from ill-health and was generally an extremely busy person, and
(6) due to his supposed unorthodox religious views, he may have feared attracting controversy [12].

Stephen Pumfrey turns the 400-year-old question around, which also, and even more so than Pepper's explanations, removes 'blame' from Harriot.[28] As we have seen, Henry, while responding to historians' frustration that Harriot was not clearer about his natural philosophy, has joined others in claiming that this frustration is at least in part the historian's fault: we should stop projecting our desire to find a natural philosophy in Harriot.[29] In a similar way, Pumfrey suggests that historians should stop projecting our norms of publication onto Harriot. Rather than ask why Harriot did *not* publish, we should ask why *should* he have published at all [143]. Pumfrey situates this new question in the context of his studies of early modern patronage. He notes that Harriot's life spanned a time of transition from the circulation of ideas in a private patronly manuscript culture to the circulation of ideas in a public commercial print culture. Through this change, the manuscript culture remained significant—the gift of a manuscript was individual and intimate and did not have the 'ungentlemanly' connotation of the personal pursuit of fame. A similar shift was also occurring between the notion of private 'secrets of nature' offered to one's patron to the notion of 'science' offered to the international public.[30] Pumfrey claims that

> historians of science have overlooked the extent to which Harriot was content with manuscript circulation, and the obstacles that prevented moving his work from manuscript culture to print culture. [143]

[28] Chapter 7, 'Patronizing, Publishing and Perishing: Harriot's Lost Opportunities and His Lost Work "Arcticon"' was presented as 'Patronage, Protection, and Publication of Scientists in the Renaissance: The Strange Case of Thomas Harriot' as the 2006 Harriot Lecture.

[29] Henry quotes both chapters by Bennett and Clucas in the previous collection of Harriot lectures [Fox 2000].

[30] Pumfrey cites Eamon 1994, which has further developed this notion.

To illustrate this point Pumfrey contrasts the case of Harriot's lost work on navigation known as the 'Arcticon', which was never published,[31] with Harriot's contemporary Edward Wright's *Certaine errors in navigation*, which was printed in London in 1599. According to Pumfrey's well argued chapter, the publication of Wright's *Certaine Errors* was the exception and Harriot's 'Arcticon' was the norm.[32]

The evidence that Pumfrey uses to defend this position is quite interesting. He has studied the conventions of printing at the beginning of the 17th century and has found, after reading the front matter of more than 1000 early modern English specialist books, that the dedicatory letters follow a regular formula. From this, he has drawn several conclusions which support his theses.

(1) 'The specialist works were routinely first produced and circulated for a manuscript culture' [146].

(2) A printed work was only one of several that the author had composed, 'and that if the patron and readers approve, there are more and better books to come' [147].

(3) The patron played a 'crucial role...in authorizing a book to move fully into the public sphere as a printed edition' [149]. The patron had 'rights over its use and distribution', especially 'when the work conferred a clear advantage, for example, in military or economic terms'. And,

> the authority of a book, especially an innovative one, was established by the letter of dedication which made clear the involvement of a patron, whose supposed honour and discernment provided the Renaissance equivalent of the authority conferred today by a peer-reviewed academic journal. [149]

So for instance, Harriot's patron, Ralegh, may not have supported publication of Harriot's works on navigation because, if these results were made public, Ralegh may have lost his competitive edge in

[31] J. J. Roche has claimed that if it had been published 'it would have had an immediate impact on western navigation and established Harriot internationally as a navigation expert' [139] (quoted in Pumfrey's chapter).

[32] This point is also supported, although not explained, in Stedall's chapter:

> it must be borne in mind that in the first half of the seventeenth century, mathematical publication in England was the exception rather than the rule. [61]

some of his pursuits. Also, Ralegh dramatically fell from grace just as did Harriot's second patron, Henry Percy, who ended up imprisoned in the Tower of London. Consequently, neither had 'viable honour to lend to Harriot and his work' [149]; and yet, as we have seen in Nicholls' comments in his biographic portrait of Ralegh, Harriot remained loyal.

(4) Although they had stylized, formulaic references to critics, the critics mentioned in the dedicatory letters were not always mere tropes but sometimes in fact quite serious. Pumfrey has made a close analysis of the various kinds of criticisms that were acknowledged and combated in the dedicatory letters, as well as a particular analysis of Wright's situation. He finds that because of accusations of plagiarism (to which many texts were vulnerable since 'many works circulated for years in manuscript form' [153]), Wright was *forced* to publish to clear his name as well as that of his patron. Wright, in this difficult situation, unlike Harriot, had the honorable reputation of the Earl of Cumberland to 'authorize' his text.

Thus, there were few reasons why Harriot should have published: he may well have been comfortable in the patronly manuscript culture. But even if he had wanted to publish, there were several obstacles in his way due to the political situation of his patrons. In the case of Wright, there were exceptional reasons, stemming from accusations of plagiarism, that compelled him to publish and he had the support of a reputable patron to do so.

Fox's *Thomas Harriot and His World* continues to develop a more complete and nuanced portrait of Harriot's manner of thinking and of the quality of his work, particularly in mathematics. The contributions to this volume also demonstrate that the further examination of Harriot sheds light on many other aspects of early modern science, perhaps because Harriot thrived in so many diverse worlds. By learning more about Harriot, one comes to understand the importance of systems of patronage for the communication of ideas in a time of transition between manuscript and print culture in England. The study of Harriot provides insight into the 'shared knowledge' of the mechanics of motion at the time as well as into the social and intellectual importance of occupational divisions between natural philosophy, mathematics, and alchemy. A close look at Harriot reveals a keen 'outsider's' perspective on the vast technical and practical knowledge required of men on transatlantic voyages of exploration. Fox's volume also continues the tradition of striving for comprehensiveness in scope. Multiple methods are employed by an international cast of scholars spanning a decade of work in the history of science. And the tradition of keeping an up-to-date and extensive bibliography of works published on Harriot, which one finds in Shirley's edited volume [1974, 166–174] and updated by Katherine D. Watson in Fox's previous edited volume [2000, 298–303], is continued in this volume [243–247] by Daniel Jon Mitchell.[33]

In addition, three appendices are included,[34] the last of which follows up in a fascinating way on an appendix to the previous volume, which discussed several possible painted portraits of Harriot [Batho 2000]. A portrait of an unknown man hangs in the President's Room of Trinity College, Oxford, which is possibly that of Harriot. Diccon Swan has painted a copy of this portrait, which now hangs in the Hall of Oriel College, and has included his description [238–241] for Fox's current volume.[35] Swan explains that the painting is fairly modest and ends his piece, which also closes the volume, writing:

> Copying it was a fascinating job and, since it probably took me 10 times longer to copy than it took the artist to paint, I feel I probably know the painting better now than the artist himself ever did. [241]

The textual historical portrait of Harriot produced by the contributors to *Thomas Harriot and His World* no doubt also surpasses the original painted portrait hanging at Trinity College in talent, time, and care.

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[33] As Mitchell notes, the website of the Thomas Harriot Seminar contains an up-to-date bibliography of publications on Thomas Harriot, which is maintained by Stephen Clucas.

[34] 'Appendix A: The 'Perfect' Harriot/de Bry: Cautionary Notes on Identifying an Authentic Copy of the de Bry Edition of Thomas Harriot's *A Briefe and True Report* (1590)' [201–229]; 'Appendix B: Harriot's Latin' [231–236]; and 'Appendix C: The Portrait of Thomas Harriot' [239–241].

[35] The account was reproduced from the *Oriel College Record* [2007].

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Communities of Learned Experience: Epistolary Medicine in the Renaissance by Nancy G. Siraisi

Singleton Center Books in Premodern Europe. Baltimore: The Johns Hopkins University Press, 2013. Pp. xii + 163. ISBN 978–1–4214–0749–4. Cloth $45.00

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In this pithy volume, Nancy Siraisi reminds us how far the history of science has come from early 20th-century models of geniuses working in isolation. *Communities of Learned Experience* puts the theme of networks center stage, making useful connections to current research on communities of knowledge and republics of letters both humanistic and scientific even as it contributes more particularly to the history of medicine. Through this book, readers gain vicarious enjoyment of the three inaugural lectures that Siraisi recently delivered at John Hopkins University's new Singleton Center for the Study of Premodern Europe. In 87 pages, she offers a distillation of the encyclopedic learning, rigorously forensic analysis, elegant argumentation, and wry humor that are the hallmarks of her career of research and teaching in the history of medicine, especially but not exclusively within the context of 15th-century Italy. So, too, readers get a taste of Siraisi's recent interest in 16th-century physicians' humanistic predilections for history, antiquarianism, and other forms of literary and archaeological study well outside their occupational remit [Siraisi 2007]. Rather than a targeted argumentative intervention, then, this book is an expert introduction to the world of early modern medical inquiry. Siraisi surveys the macro-historical fields of science, medicine, anatomy, and botany even as she analyzes individual practitioners, circumstances, and networks micro-historically.

At the heart of this book, we find minute scrutiny of the *epistolae medicinales* of two 16th-century physicians. The letters of Siraisi's first protagonist, Johann Lange (1485–1565), reveal predominantly courtly and humanistic cultural priorities. Lange was personal physician to the Elector Palatine of Heidelberg and lived during the early decades of the 16th century, when the expansive tendencies of literary humanism had not yet confronted the

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conservative climate of the Counter-Reformation. The epistles of her second protagonist, Orazio Augenio (1527–1603), take us into the medical marketplace of Italy's urban centers during the more fraught decades spanning the late 16th and early 17th centuries. Beyond telling us about two interesting and comparatively understudied physicians, Siraisi issues an amiable manifesto for historians of medicine to attend as carefully to physicians' collections of letters as they have traditionally done to their *consilia* (compilations of medical advice) and treatises. Indeed, *Communities of Learned Experience* demonstrates throughout the rewards of epistolary evidence, from which Siraisi recovers and connects her protagonists' broadly intellectual, specifically scientific, and densely social worlds.

The organization of this volume maximizes its utility for specialists and nonspecialists alike. A brief introduction welcomes readers into the Republic of Letters at large—embodied by luminaries such as Erasmus—and the republic of medicine as a suburb of that larger literary polity. Thereafter, an initial chapter charts medical 'contexts and communication' across Europe, rooting interpretive paradigms that might otherwise be quite abstract in the lives and careers of a few paradigmatic physician-networkers. Scholars interested in Venetian medicine and fans of historian Richard Palmer's *oeuvre* will be cheered to see Nicolò Massa used as a model in this first chapter alongside the more famous cases of Girolemo Mercuriale and Conrad Gesner.[1] The first chapter having established the basic state of play in European medical theory, practice and epistolary conventions, the two following chapters then focus, respectively, on Lange and Augenio.

Both of the epistolary collections that Siraisi examines in detail offer portraits of physicians who were learned and well-connected beneficiaries of the cultural ferment associated with so-called 'medical humanism'. The recovery and emendation of the ancient medical canon has been well documented by historians of medicine, with Siraisi herself at the vanguard.[2] Yet Siraisi emphasizes that Lange and Augenio shared more than merely their training. Both physicians, like so many of their profession, were members of families with considerable track records in the world of medicine. (Albeit it does become important for Lange's rather smoother career path that his progenitors included more university-trained men than Augenio's had.) So, too,

[1] On Massa, see Palmer 1981.

[2] Wear, French, and Loni 1985 is a compendium on the topic.

these physicians shared some career strategies, above all the cunning use of epistles to form and navigate social, cultural, and professional networks and to enhance their reputations. Accordingly, while Siraisi emphasizes that an important part of Augenio's self-promotional repertoire was his insistence on being more 'modern' than colleagues such as Lange—that is, more inclined to dispense with the ancients and risk new methods of healing and new interpretive models, even Paracelsus (to a point)—she does not ultimately see Augenio as decisively more cutting-edge than the ostensibly more traditional Lange. Rather than being antipodal or even starkly contrasting, these two physicians appear more as points not too terribly distant from each other on a continuum of intellectual and professional possibilities. The distinctions between these two men seem more matters of degree than kind.

Early modern historians working in many different patches of the field will be interested in Siraisi's analysis of the effects of the Counter-Reformation, especially the years of the Council of Trent (1545–1563) on both physicians' range of intellectual and socio-professional motion. While by no means stopping the cross-pollination of medical ideas north and south of the Alps that was so notable a feature of Lange's early career, the greater geographic and confessional restrictions were certainly much in evidence in his later letters. Augenio came to his professional peak when letters to or from Protestant lands were at best career liabilities (at worst, invitations of denunciation for heresy). Nor surprisingly, then, Augenio's letters evince throughout a greater weight toward Italian circles than international networks, and toward considerations of immediate practical healing rather than abstract theories of body and spirit that could so quickly tip into heterodoxy. Still, even here Siraisi resists categorical statements. Counter-Reformation constraints surely hampered the sharing of information between Catholic and Protestant practitioners but Siraisi also shows evidence of continued exchange, for instance, the post-Tridentine letters between Girolemo Mercuriale and the Calvinist Theodore Swinger of Basel. And she makes the intriguing point that group solidarity in the medical world may in any case have coalesced not around confessional allegiances but instead around the major 'camps' of Galenists *versus* Paracelsians [27–36].

Historians of science have long debated the periodization of different branches of knowledge and practice. While not putting too fine a point on it, Siraisi situates Augenio (late16th century) as possible evidence of a new phase in the professionalization of medicine. Augenio's evident concern for tangible and logistical problems of treatment contrasts with Lange's generally greater emphasis on philosophical or theoretical problems relating to medical 'truth' or ideals and thus hints at the waning of medical humanism and the concomitant rise of something closer to medical empiricism.

Given the breadth of the topics and problems it engages, *Communities of Learned Experience* should have diverse audiences. In addition to its utility for historians of medicine, this book will serve intellectual historians (and their graduate students) very well. Naturally, those focused on the fortunes of the ancient medical canon will be the most obvious beneficiaries but those interested in any form of early science or for that matter the production and circulation of any sort of 'learned' knowledge will benefit from watching Siraisi interrogate her epistolary sources. The circles that she brings to life also offer interesting parallels to other scholars' recent studies of ostensibly very different intellectual communities [*inter alia* Campbell 2006, Grafton 2009, Pal 2012]. Readers will also find this book's critical apparatus phenomenally helpful. Even within the space constraints, Siraisi surveys essential scholarship in several subfields and languages—in fact, the endnotes form almost a second short book, running 65 pages in their own right and include (*mirabile dictu*, in these lean times) original language quotations, predominantly in Latin.

Embedded within this volume are also useful spurs to further research. Augenio's case, for instance, raises questions in my mind about the ways in which a medical career served as an avenue for social or cultural advancement—or, perhaps even more than a medical career specifically, a facility with literary epistles that formed part of physicians' training. Lacking any famous or even especially well-connected family at the start, Augenio managed by the end of his life to achieve a prominence (at least in Italy) roughly equivalent to that achieved by the initially better-positioned Lange. According to Siraisi, Augenio managed this, 'chiefly through his carefully maintained personal correspondence networks' [83]. Were letters themselves, then, the primary engine of mobility for other categories of cultural aspirant? Along the same lines, the connections between 'medical humanism' and what I suppose we should call 'literary' humanism are drawn loosely. It is taken as given that physicians participated in the broader literary cultures of their time but we might have heard something more about why this participation was so important, even in the later 16th century. Siraisi mentions the cachet attached to humanism [44 esp.] but this might be connected more closely to the difficulties that physicians still confronted in asserting their membership in 'high culture'. After all, contemporary writers may not have been as acerbic as Petrarch or Dante but they still lampooned the profession as a haven for mercenary quacks and social climbers. We may see in physicians' use of 'the literary', then, at least a measure of professional anxiety and vulnerability.

At all events, Siraisi's latest contribution draws two finely etched portraits of medical men navigating their sometimes similar, sometimes distinctive careers at a moment of profound epistemological shifts. For its wealth of information and important call for more attention to medical epistles, *Communities of Learned Experience* takes a more than worthy place in Siraisi's *oeuvre* and should occupy an important space in the history of science section of early modernists' collections.

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Médicine, astrologie et magie entre Moyen Âge et Renaissance: autour de Pietro d'Abano edited by Jean-Patrice Boudet, Franck Collard, and Nicolas Weill-Parot

Micrologus' Library 50. Florence: SISMEL—Edizioni del Galluzzo, 2013. Pp. xvi + 341. ISBN 978–88–8450–468–5. Paper €54.00

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This volume brings together essays arising from a colloquium held in Paris in 2006 devoted to the celebrated physician, philosopher, and astrologer Pietro d'Abano (1250/1257–1315/1316). Best known today for his massive collection of medical *quaestiones*, the *Conciliator differentiarum philosophorum et precipue medicorum*, Pietro also authored treatises on poison (*De venenis*), physiognomy (*Liber compilationis phisionomie*), astronomy/astrology (*Lucidator dubitalium astronomie*, *De motu octave sphere*, *De imaginibus*), and Aristotle's *Problemata* (*Expositio problematum*). His works have inspired a number of studies and critical editions in recent years [e.g., Paschetto 1984, Seller 2009, Cadden 2013, Federici Vescovoni 1988], and the papers presented here amply demonstrate the depth and breadth of recent scholarship on the Paduan physician and, more broadly, on the history of science and magic in the later Middle Ages. As the enlightening introduction by Jean-Patrice Boudet points out, Pietro enjoyed quite a reputation in the Renaissance and early modern period not just for his medical and astrological learning—his major works were widely printed in the 15th and 16th centuries—but also for his supposed expertise as a magician (and necromancer). He was, after all, twice summoned before inquisitors in Paris and Padua, and was been burned posthumously in effigy as a heretic. Together, the scholars represented here all seek in some way to unpack the subsequent image of Pietro as famous scholar or as heretical magician (or rationalist martyr to the church), whether through intensive study of Pietro's own works, consideration of his reception by contemporaries and later readers, or analysis of works spuriously attributed to Pietro.

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As several of the studies presented here make clear, Pietro d'Abano's own understanding of astrology would not have seen him performing necromantic rituals. In her essay 'L'astrologie comme science théorique, rationnelle et autorisée dans le *Lucidator* de Pietro d'Abano', Graziella Federici Vescovini demonstrates that Pietro presented astrology (or more properly, astronomy/astrology) not simply as a rational and theoretical science (*scientia*), but also, in fact, as the most important of the sciences, prior to all others, and certainly not equivalent to magic. Pietro d'Abano rejected both the distinction between art (*ars*) and science (*scientia*), and the subordination of all sciences to metaphysics, since all sciences, in his reckoning, had the same subject, namely, being. But whereas metaphysics considered being as being, physics (of which astrology and medicine were branches) regarded being through movement, whether future (astrology) or past (medicine). Pietro's schema in fact placed astrology as privileged among sciences in being necessary and indispensable to both philosophy and theology. Since God is not knowable except through effects produced by mediation of the movements of the heavenly bodies, astrology, whose object is the study of those movements, is in effect the science of the knowledge of God's actions. Furthermore, by insisting that the stars and planets were neither minor gods, nor demons, nor celestial intelligences, Pietro retained for astrology its character as a mathematical science, effectively denying that it was a form of magic. In fact, in the *Lucidator*, he came down hard against practices that smacked of necromancy or of the 'detestable' astrological images decried in the *Speculum astronomie*, although he perhaps softened that pose in the *Conciliator*. Even there, however, by insisting that the planets acted not through an occult substantial form but rather by means of a medical-sounding *complexio*, Pietro d'Abano pulled the theoretical rug out from under the practice of astral magic.

In Nicolas Weill-Parot's 'Pietro d'Abano et l'occulte dans la nature: Galien, Avicenne, Albert le Grand et la *differentia* 71 du *Conciliator*', Pietro d'Abano appears, again, not as a magician, even in one of the most extensive scholastic musings on occult virtues, difference 71 of his *Conciliator*. As Weill-Parot notes, 'occult' in Pietro's parlance referred not to the supernatural but to natural phenomena whose causes were unknown and which could be explained by hidden properties in things. Pietro's experience as a physician was certainly helpful in his understanding of occult virtues, as Avicenna (following Galen) had discussed medicines that worked not because of their elemental qualities (cold, hot, wet, dry) but thanks to their 'specific form'. Weill-Parot demonstrates the ways in which Pietro d'Abano's treatment of occult virtues (in a question devoted to the nature of the specific form) was particularly indebted to the discussion of the specific form in Albertus Magnus' *De mineralibus* (although Albert is not mentioned in Pietro's text). According to Weill-Parot, Pietro d'Abano delineated a purely natural occult (such as the effects of certain stones) and a magical occult (involving human operation, such as astrological images). Pietro's concern in the *Conciliator* was with both of these sorts of occult virtues (he was one of the great proponents of medical astrological images). Weill-Parot also notes Pietro's insistence that the specific form can be known neither by reason nor by the senses, but only by its effects. This assertion of the limits of human knowledge, according to the author, is precisely what allowed scholastic authors to offer a rational explanation of such *mirabilia* as the magnet's attraction of iron. For Pietro d'Abano, in fact, the effects of occult specific forms were so 'normal' that one would do better to marvel at the properties of fire than to wonder at the powers of a magnet.

Béatrice Delaurenti's 'Pietro d'Abano et les incantations. Présentation, édition et traduction de la *differentia* 156 du *Conciliator*' examines one of the most seemingly 'magical' topics treated in Pietro's great medical compendium: incantations, or verbal formulas, designed to produce a definitive effect and which were not infrequently utilized in medical practice. Again, however, it is difficult to discover the *magus* of later legend in Pietro's authentic writings. Rejecting the hypothesis that the words of the incantation themselves had some intrinsic force, Pietro named a number of different possible causes by which incantations might work, ranging from the qualities of the human soul itself to the actions of God, angels, demons, stars, or the agent intellect. He took pains to argue against the conclusions of William of Auvergne and Augustine that all incantations involved an implicit pact with a demon. Rather, for Pietro, demonic intervention took place only when incantations were uttered by the unlearned (such as the inevitable *vetula*): in the hands of a learner practitioner, particularly one experienced in astrology, incantations worked through an entirely natural process, whether because of the patient's own sense of hope and trust in the physician or by the cooperation of astral influences. Finally, Pietro also implied—with a touch of comic scepticism—that an incantation might work simply by accident, as in the case of a noble whose outburst of laughter upon an old woman's pronouncing the ridiculous incantation 'two and three make five; three and two also' expelled the fishbone stuck in his throat about which he had consulted the *vetula*. What Pietro did in *differentia* 156, urges Delaurenti, was to minimize the notion of supernatural causes behind the force of incantations in favor of emphasizing their natural causes: the soul and the stars. Delaurenti stresses the audacity of Pietro's position, coming as it did in the opening years of the 14th century, just as the preoccupation with demons was sharply increasing in Europe; but she also underscores the ultimately tentative character of his writing. She concludes the essay by looking at the subsequent fate of *differentia* 156, which could be repurposed to argue for the role of demons in incantations or to link more closely medical incantations to works of magic and necromancy. Pietro's own ambiguities and hesitations, she suggests, themselves left open the possibility of alternate (more sinister) interpretations of his writings and his career.

As a number of essays in this collection demonstrate, even if Pietro d'Abano was not himself a necromancer, he did assign astrology a greater role in medical theory and practice than did many contemporaries. But he also was aware of the practical (and theological) limits to astrological science. In this light, Giovanna Ferrari, in 'La durata della vita: *humidum radicale*, medicina e astrologia nel *Conciliator* di Pietro d'Abano', looks at the Paduan physician's treatment of the concept of radical moisture, a topic that received much discussion in 13th-century philosophy, medicine, and theology, as authors tried to clarify the origin of this substance, which, together with innate heat, was thought to play a role in the sustenance of human life. Pietro, in *Conciliator diff*. 111–113, addressed three specific questions concerning radical moisture: its origin and nature, the feasibility of its being restored or replenished through diet, and the possibility of thereby prolonging human life. In tracing radical moisture's origins in generation, Pietro placed particular emphasis on a *virtus informativa*, an agent linked to celestial influences. In order to leave physicians room for action in restoring radical moisture, however, with the possibility of thereby lengthening life, even though Pietro contended that the stars at the moment of generation determined the quality of innate heat and radical moisture, he also admitted limits to astrologers' ability to predict such details as the length of life accurately. Hence, Ferrari argues, Pietro—the great proponent of astrology—acknowledged the limits of astrological prediction in order to safeguard the physician's scope of action.

The later legends surrounding Pietro d'Abano sometimes made of him an alchemist (as was the case with many medieval authors, Pietro had an alchemical treatise spuriously attributed to him). What might have been the

Paduan physician's actual attitudes towards and knowledge of alchemy forms the subject of Chiara Crisciani's enlightening 'Pietro Abano, alchimia e alchimisti'. As Crisciani notes, physicians (and even some theologians) in the 13th century did not share the concerns of contemporary jurists about the relatively new practice of alchemy, considering it a technique with potential usefulness for medicine. Pietro d'Abano, inasmuch as he dealt with alchemy in his *Conciliator*, appears by and large to have concurred in that judgment. Three sections in the *Conciliator* touch upon alchemy, all, as Crisciani points out, drawn from work's third section which is devoted to practical medicine and pharmacology. In all three cases, alchemy appears largely as metallurgical in nature: Pietro does not portray the alchemists' elixir as a potential pharmacological agent for humans. Pietro's discussion of quicksilver in *differentia* 151 reveals his familiarity with alchemical texts—he has read the pseudo-Geber *Summa perfectionis*, for example—as he addresses the debate whether minerals originate from mercury alone or from mercury and sulfur together. In *differentia* 178, devoted to the discussion of theriac, Pietro draws an analogy between the making of theriac and the alchemists' processes (without evincing interest in any specific details of their operations, however). In both cases, he says, art and nature are seen to cooperate. In *differentia* 219, however, while discussing the preparation of a *medicina solutiva* or solutive purge, Pietro insists that art can produce only an inferior copy of nature and points to the superiority of natural gold over alchemical gold. As Crisciani reveals, these three *quaestiones* hardly present a consistent or deliberate statement of Pietro's thinking about alchemy. It is clear that he accepts alchemy's validity, as do other contemporary physicians, and considers it a subject with which he, as a physician, should keep current. Yet, as Crisciani perceptively notes, for Pietro and his contemporaries, alchemy was still primarily seen as an affair of metallurgy, not medicine. How then to explain a statement in the *Lucidator* that appears to paint alchemy in a much more negative light? Crisciani suggests that scholars have misread this puzzling passage, which may instead imply that some detestable magicians have hidden behind the respectable labels of physician and alchemist.

In 'Genèse et postérité du commentaire de Pietro d'Abano sur les *Problèmes* d'Aristote. Le succès d'un *hapax*', Maaike van der Lugt examines Pietro d'Abano's commentary on the *Problemata* attributed to Aristotle, a work that was translated between 1258 and 1266 and that treats a variety of questions with unknown or debated answers, often regarding the explanation of particular observed facts for which the cause was hidden. As van der Lugt shows, the commentary was completed in Padua in 1310 but was most likely begun in Paris in the 1290s before Pietro journeyed to Constantinople to learn Greek. In keeping with his general intellectual preoccupations, Pietro often proposes astrological explanations for Aristotle's 'problems', although he is careful to show the complexity of the disputed points. Van der Lugt devotes the final section of her essay to the reception of Pietro's commentary, which was, as her title indicates, in many respects one of a kind. It was certainly the most influential commentary on the *Problemata* and frequently Pietro's own paraphrases actually served as a substitute for the rather obscure translation itself. Yet subsequent commentaries that used Pietro's as a basis lacked the ambition and scope of the Paduan physician's work, whether by vulgarizing the text, removing any of the sense of debate from Pietro's comments, or reorganizing the commentary alphabetically into what was effectively a popularizing encyclopedia. Van der Lugt speculates on the reasons why Pietro's commentary remained a *hapax*, pointing to the unusual nature of the *Problemata*, focused as it was upon particular cases rather than upon the generalizing principles of Aristotelian *scientia*. As she notes, medical authors, by contrast, were by definition focused upon the particular; not surprisingly, the *Problemata* and commentaries on it tended to be copied with medical texts more often than philosophical ones. Finally, she suggests the very 'virtuosity' [181] of Pietro d'Abano's exhaustive commentary dampened future authors' enthusiasm for attempting to produce their own versions.

If Pietro d'Abano's own works reveal little that could substantiate his later reputation as a necromancer, his reception among Italian readers in the early 14th century similarly does not help to explain his subsequent renown as a physician. So demonstrates Joël Chandelier in his 'Pietro d'Abano et les médicins: réception et réputation du *Conciliator* en Italie dans les premières années du XIVe siècle'. As Chandelier points out, the first evidence of Pietro's fame as a physician dates only from the years 1420–1440. Chandelier's examination of medical texts from northern Italy of the first decade after the redaction of the Conciliator in 1310 yields, in fact, no explicit mention of Pietro's great medical work. When the *Conciliator* finally did appear in a 14th-century medical text, Gentile of Foligno's commentary on Avicenna's *Canon*, it came up for criticism and, as Chandelier demonstrates, Gentile's original version from the 1320s, while clearly tracking Pietro d'Abano's text, simply referred to its ideas as those held by 'quidam modernorum'. Similarly, once more in the 1320s, when Dino del Garbo (a pupil of Taddeo Alderotti's) cited the *Conciliator*, it was again to disagree with several of Pietro's conclusions, which he criticized as 'ridiculous and vain' [191], eventually opining that the author should better be known as the Corruptor than the Conciliator. And in a short treatise from the 1340s, Gentile da Foligno again criticized positions outlined in the *Conciliator*. The *Conciliator* was not often referred to in the 14th century and was copied in manuscript and reproduced in print considerably more frequently in the 15th and 16th centuries than in the 14th. How to explain Pietro d'Abano's strikingly poor reputation amongst 14th-century physicians? Chandelier suggests a certain closing of the ranks of Italian university physicians against one who stood somewhat outside that group and its norms both in his training and in the originality of his medical thought. In particular, Pietro's emphasis on astrology cut against the grain of the teaching of Italian faculties of medicine in the 14th century. As astrological medicine came more into vogue in 15th-century Italy, Chandelier comments, so too did Pietro d'Abano's fame rise.

Pietro's posthumous reputation again comes under scrutiny in Franck Collard's contribution, 'Le *De venenis* de Pietro d'Abano et sa diffusion: d'une traduction à l'autre (1402–1593)', examining two French translations of Pietro's brief treatise on poisons, a work that Collard notes could have but seems not in actuality to have played a great role in the construction of the 'black legend' of Pietro d'Abano's expertise in occult sciences. The work enjoyed a great success in manuscript and print (from the 15th century) and had a great influence on later poison treatises. Comparing the two translations, one in manuscript and dating to 1402, the other printed in Lyon in 1593, leads Collard to some interesting observations about the uses of Pietro's treatise. The translation of 1402 was made by a Carmelite friar named Philippe Oger for Jean le Meingre (Boucicaut), who, after he had recently been named governor of Genoa, clearly sensed that a plot to poison him was a real possibility and was seeking practical advice in a language that he could read. The translator in 1593, Lazar Boet, was unaware of the earlier translation but appears to have plugged into a large vogue for vernacular translations of medical treatises as well as a resurgent interest in poisons in France since the 1560s. And, again, the treatise, printed in a small, pocket-sized format, appears to have been destined for practical ends. Neither translation, however, Collard concludes, had much influence or did much to expand the diffusion of Pietro's work on poisons. In the first instance, Collard speculates that the treatise may have appeared as a work too dangerous to allow vulgarizations since it divulged information about poisonous substances. In the later translation, the author suggests that the publication simply came too late, the vogue for treatises on poisons having subsided after the 1580s.

Some of the possible reasons for Pietro d'Abano's brushes with ecclesiastical authorities become apparent in Danielle Jacquart's 'Autour de la *Compilatio phisionomiae* de Pietro d'Abano'. She examines the oldest copy of Pietro's commentary on the *Compilatio phisionomiae*, bearing the date 1295 and contained in BNF MS Lat. 16089, a collection of texts that includes a significant number of treatises concerned with prophecy, astrology, and magic, some of which raise issues condemned in Paris in 1277. As Jacquart notes, towards the end of Pietro's treatise, he laments that a copy of the text had fallen into the hands of a certain scoundrel in Paris, forcing him to recompose the treatise in a longer and better redaction. Pietro d'Abano, like other university authors of the late 13th century, sought to endow physiognomy with the character of *scientia*. For Pietro, that meant explaining how physiognomy could function as a sign by reference both to theories about generation and to astrological causes. The difficulty was that physiognomy was supposed to give clues about the soul (permitting an astute observer to ascertain his true friends, for example), and Pietro took some pains to circumscribe the science to the 'natural' and not to humans' actions owing to the use of reason and free will. Key to this balance was Pietro's description of generation of the soul and its relation to the body. Given certain statements in the later *Conciliator*, Jacquart suggests quite convincingly that these passages of the *Compilatio phisionomiae* in which Pietro relied heavily on the Aristotelian notion of the *intellectus vocatus* were in fact those that raised the eyebrows of Parisian Dominican friars, one of whom would then be the 'scoundrel' to whom he alluded near the text's end.

The two final essays in the volume by Jean-Patrice Boudet and Julien Véronèse directly confront the Paduan physician's later reputation as a *magus* by examining two overtly magical treatises attributed to Pietro d'Abano during the Renaissance. In 'Magie et illusionnisme entre Moyen Âge et Renaissance: les *Annulorum experimenta* attribués à Pietro d'Abano', Jean-Patrice Boudet discusses a text largely devoted to creating illusions. The *Annulorum experimenta*, known in at least six manuscripts from the 15th and 16th centuries, appears in a list of *libri de magia suspecti* compiled by Johannes Trithemius in 1508. The *Annulorum experimenta* certainly partakes of the tradition of astral magic, basing its *experimenta* on the 28 lunar mansions, although Boudet points out that the author has made a number of errors which reveal his rather low competence in astrology. Boudet summarizes the general procedure for the 40 experiments in the treatise, which involved a number of rituals and invocations. While the majority (27/40) of the *experimenta* in the treatise are aimed at producing illusions and thus served largely for entertainment, Boudet points out that the instructions that seem to have raised the greatest interest were those for summoning one's own private demon, who would respond to any question put to him. As Boudet notes, contemporary legal sources show people being brought to trial for just such demonic magical practices.

In 'Pietro d'Abano magicien à la Renaissance: le cas de l'*Elucidarius magice* (ou *Lucidarium artis nigromantice*)', Julien Véronèse looks at another text attributed to Pietro d'Abano, one known as the *Elucidarius magice* and by other similar names, which Trithemius labeled 'vain and superstitious'. Trithemius's judgment about this treatise, which seems to date from the latter part of the 15th century, was a response to its overt orientation towards conjuring spirits. Véronèse nicely reveals the differences between the multiple versions of the text (two in 16th-century manuscripts and one from the initial printing in 1565). The operations described in the text, which have a heavy overlay of astral magic, involved an operator who had been spiritual purified, a number of sacramentals (such as holy water), the construction of various circles in which to operate the ritual, and finally a set of invocations and suffumigations. Although the spirits invoked are not labeled as demons in the text, the fact that they are somewhat unreliable indicates that they are in fact demons. Tracing the sources of the *Elucidarius magice*, Véronèse discovers a fascinating interpenetration of various ritual magic texts: the famous Munich Clm 849 studied by Richard Kieckhefer [1988], the *Clavicula Salomonis*, the *De quatuor annulis*, and the *Liber juratus* of Honorius. Finally, Véronèse notes that, since Trithemius asserted that there were many fables recounted about Pietro d'Abano, it seems quite plausible that Trithemius viewed the attribution of this text of spiritual magic to the Paduan philosopher to be one of those myths.

A number of appendices to individual entries greatly enhance the works presented here. To begin there are editions of the important Difference 156 of the *Conciliator* (edited by Béatrice Delaurenti), the *Annulorum experimenta* (edited by Jean-Patrice Boudet), an Italian version of the same (the *Trattato degli anelli*, edited by Stefano Rapisarda), and the version of the *Elucidarius magice* found in Vat. Reg. Lat. 1115 (transcribed by Julien Véronèse). None of these works has appeared in a modern edition and to have them here is of invaluable service to scholars working on the history of magic in medieval and Renaissance Europe. The authors and the press are to be commended for making them available to other historians. Further, Delaurenti offers in addition a French translation of *Conciliator diff.* 156, accompanied by a number of extremely useful annotations. Joël Chandelier's discussion of the reception of Pietro's medical teaching is supplemented by a helpful listing of the manuscripts and printed editions of the *Conciliator*. And Jean-Patrice Boudet provides a detailed inventory of the contents of Paris, BNF MS Lat. 7337, which contains not simply the *Annulorum experimenta* discussed in his essay but also a number of astrological, medical, and magical texts, described here in enough detail to whet any researcher's appetite to see the manuscript itself.

It is truly a pleasure to read a collection of essays that are tied together in such a close thematic way. Perhaps because the contributions do all speak in one way or another to the central *problématique* of Pietro's later image in a way unusual in such volumes, the whole really is greater than the sum of its parts. One comes away, for example, with a clear sense of the importance of astrological explanations—and frequently of one going back to the central moment of conception—in a number of Pietro's medical theories, a point that recurs in many of the studies here. But a reader seeking a simple answer to the question of why this brilliant philosopher, physician, and astrologer attained a later reputation as a necromancer is likely to come away disappointed. Upon reading these essays, Pietro d'Abano's later fame as a *magus* in some ways becomes even more puzzling than before: in his authentic works, while certainly astrology (and indeed astrological images) held a central place, Pietro took pains to distinguish his practices from forbidden magic and to present astrology as a legitimate *scientia*. He noted, with a certain amount of pique, places in which the Parisian Jacobins who hounded him had clearly misunderstood his words. In the end, however, as Boudet points out in his introduction, just as with Albertus Magnus, Roger Bacon, and Arnald of Villanova, Pietro sailed close enough to the limits of the permissible to enable subsequent generations to imagine him having gone beyond safe waters. As the authors of this remarkable volume amply have demonstrated, historians still have much to learn about this brilliant and enigmatic thinker.

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Pluralité de l'algèbre à la Renaissance edited by Sabine Rommevaux, Maryvonne Spiesser, and Maria Rosa Massa Esteve

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One central narrative in the history of science addresses how we came to use letters, lines, and squiggles to compress dramatic mathematical and physical stories into compact, digestible phrases of algebra. For historians of mathematics, the issue is doubly pressing. First, algebraic language is simply how modern mathematics works, so its emergence is worth knowing about. The second reason follows. For easier comprehension, historians tend to translate old texts into modern algebraic notation and then deal with ancient mathematics through this algebraic translation.

Thus, the calls to respect original formulations in the history of algebra are especially crucial if we hope to understand how change came about (and not just what emerged). Reviel Netz sharpened Sabetai Unguru's charge to respect the original texture of mathematical language in his *Transformation of Mathematics in the Early Mediterranean World* [2004] by focusing on a shift from problems to equations. He argued that the genres, vocabulary, and diagrams of mathematics are not mere vestments on an algebraic skeleton but reshape the very structure of mathematics. He ended where the story of algebra begins, in Baghdad with the equations of Omar al-Khayyām and al-Khwārizmī. Students of Medieval and Renaissance mathematics are accustomed to picking up the history of algebra at this juncture, pointing to the vernacular traditions of Italy, Germany, and France (to a lesser degree), before landing in the algebra of François Viète and René Descartes around 1600. Though some scholars such as the late Michael Mahoney argued that more attention must be paid to the diversity of Renaissance algebra and its variety of genres and aims, in practice the story still tends to be told as a way to uncover the sources of Viète or Descartes, who finally disclose the 'unity of algebra'.

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In this new volume, Sabine Rommevaux, Maryvonne Spiesser, and Maria Rosa Massa Esteve have gathered together studies that demonstrate the plurality of algebra in the Renaissance. The immediate occasion for this book was their conference, 'Unity or Plurality of Algebra (12th–16th centuries)' held at Tours in May 2009 as part of a CESR project begun in 2006 under Rommevaux's direction. As a whole, the volume should reorient the study of Renaissance algebra to consider a broader range of texts and to bring the specific diversity of algebraic practices into focus.

Chronologically, these studies range across the rise of 'algebra' as a basic feature of mathematical culture in Western Europe, notably through the evolution of textbook traditions: the first essay deals with the medieval Latin translations of al-Khwārizmī and the last examines the demise of the oral culture of the *Rechenmeister* in the generation before Descartes' *Géométrie* (1637). The structure of the book reflects an interest in the diverse textures of the operations and practices that were given the name 'algebra' during this period. This specificity is balanced by attention to the big questions that have often occupied students of Renaissance algebra, such as the relationship between arithmetic and geometry vis-à-vis algebra, and the candidacy of algebra to be a 'universal' or 'great' art.

The volume is organized into four sections on:

(A) the medieval European reception of Arabic texts,
(B) the regional styles of algebra in Renaissance Europe,
(C) the relation of algebra to arithmetic and geometry, and the last and largest on
(D) the variety of Renaissance definitions of algebra.

A volume of this sort is bound to energize the study of algebra by refocusing our attention on the details, since it consciously eschews grand statements or simplifications. In keeping with this approach, I will in this review restrict myself chiefly to supplying a survey of the book's individual chapters, followed with a couple of thoughts about fruitful directions for research.

A. The medieval European reception of Arabic texts

The two chapters on the Arabic traditions offer lessons on the limits of current scholarship. Max Lejbowicz focuses on the genealogy of Arabic algebra in the European context in the light of Gerard of Cremona's Latin translation of the *Kitāb al-jabr wa l'muqābala* of al-Khwārizmī. We all know this genealogy. Or do we? Lejbowicz tells a cautionary tale in recounting modern scholarship on Latin translations of al-Khwārizmī, showing how tenuous our grasp on that history is. Early 19th-century Europeans were only passingly interested in medieval Arabic scholarship and relied largely on faulty descriptions of manuscripts instead of directly examining them. The illustrious 19th-century historians of mathematics Guillaume Libri and Baldassarre Boncompagni each edited different Latin translations of the *Kitāb al-jabr*. But Lejbowicz, retracing their steps, shows how each managed to miss correct attribution by careless editing. Libri did not recognize that the translation had been by Gerard because he did not examine the other treatises in the manuscript that indicated Gerard's authorship, so he identified the translation as by 'Anonymous'. And Boncampagni, despite codicological counter-evidence, believed that the work which he edited to be by Gerard. And then the Latin reception of al-Khwārizmī grew even more complicated with the discovery of Robert of Chester's translation. (Robert of Chester's translation has become standard, especially in an English version; yet Karpinsky's modern edition is in fact based on a 16th-century manuscript that had been corrected for Johannes Scheubel's *editio princeps*, only a distant witness to the medieval manuscripts.) Lejbowicz's chapter suggests at least three tasks for future work. First, the Renaissance manuscript, still the standard image of medieval Latin translations of the *Kitāb al-jabr*, should be recognized as a late witness. Second, the treatise which Libri edited and assigned to 'Anonymous' should be known as that by Gerard of Cremona. Finally, since the authorship of Boncampagni's edition is now uncertain, we need studies of its true authorship. (Lejbowicz wonders whether yet another author might be responsible, i.e., Guillaume de Lunis.)

In a different way, Marc Moyon's chapter suggests that algebra had a limited role in Latin mathematics, at least in the Middle Ages. Surveying three practical mathematical treatises by Abū Bakr, Fibonacci, and Jean de Murs, Moyon considers the point of such mathematics. In Latin mathematics, did algebra serve primarily theoretical or practical purposes? All three authors were familiar with the rules of algebra. Did they use such operations to solve problems in the 'science of measurement'? Moyon finds that his three authors indicate an evolution in the uses of algebra: while Abū Bakr used algebraic rules as a mere alternative to traditional geometry, after him Fibonacci and Jean de Murs increasingly used algebra as its own method of solving certain problems. To be sure, neither later author leans on algebra too far: neither turns to algebra to analyze solids, for example, and the problems that they solved by algebra alone are 'marginal' to the practical geometry in question [55]. Moyon thus raises the intriguing possibility that algebraic rules were not used out of practical necessity. Rather, even in the middle of medieval *practical* geometry, algebra was seen as an alternative to traditional methods for *theoretical* reasons.

By showing algebra as not wholly necessary to practical mathematics—at least at first—Moyon's account nuances the usual story of algebra's origins in the late medieval Italian *abaco* tradition, where local teachers in mercantile towns passed on mainly practical texts and practices, which slowly filtered into the rest of early modern Europe. For example, in 15th-century Germany, the counterpart to *abaco* was the art of *Coss* which addressed old problems of currency exchange, measurement, and distance within a new vernacular tradition.

B. The regional styles of algebra in Renaissance Europe

For France, the story often begins with the 15th-century algebraic master of southern France Nicholas Chuquet, cast in the role of a vernacular receptor of the Italian tradition. Then, the spotlight usually shifts to humanist Paris, in particular, to the court-based, literary circles of Jacques Peletier du Mans. Giovanna Cifoletti [1992] described this rhetorical project as institutionalized in university Latin by Guillaume Gosselin, working in the 1570s. The second part of the book offers an opportunity to see whether this story holds.

François Loget opens this second part by brilliantly remapping the landscape of algebra in 16th-century France. He focuses on the 1550s, when a flurry of Latin algebras issued from Parisian presses. Thus, Loget moves away from Peletier and Gosselin's court-based algebra, instead putting the university in the foreground. In particular, the market for new Latin algebras in the 1550s suggests that the charismatic pedagogue Peter Ramus was especially responsible for making algebra a standard part of the science of numbers: it is Ramus who turns out to have especially modified algebraic expressions, shortening the cossist abbreviations of his German source, Johannes Scheubel, to mere letters. Here algebra enters Latin university handbooks.

But in Spain, as Maria Rosa Massa Esteve shows, algebra was more commonly found in vernacular practical arithmetics (with the exception of Pedro

Nuñes' work of 1567, which is discussed in later chapters). Practical arithmetics by Marco Aurel, Juan Pérez de Moya, and Antic Roca share simple language aimed at solving mercantile problems—Esteve reports no proofs or geometrical constructions. But she nonetheless thinks that these three works share two distinctive practices that contribute to the development of algebra. First, these algebras try to simplify rules for solving such problems by setting unknowns ('characters') in a series of continuous proportion. Second, they have an analytical approach to the 'Rule of the Thing'. That is, these vernacular works present this algebraic rule as the construction of an equation to check problems that have been 'imagined as solved'.[1] Esteve's close reading helps one sense the distinctive mathematical texture of the Spanish *arte mayor*, seeing it as a possible source of the analytic method so often tied to Viète and his reading of Pappus. In the *arte mayor*, analysis could become an explicitly shared algebraic method in the generation before Viète.

What then of Nicholas Chuquet? He brilliantly expanded on the most sophisticated parts of the Italian tradition. At the same time, because he never published in print, it has been hard to see whom he influenced, if anyone except Estienne de la Roche, whose *Arismetique* (1520) lifted many problems straight out of Chuquet's manuscript. Albert Heefer supplies a partial answer to this puzzle. Arguing that historians have missed de la Roche's innovation (though he used Chuquet's problems, he frequently offered new solutions), Heeffer intervenes in the historiography of algebra in two ways. The first has to do with algebraic objects themselves, a point that he has aired elsewhere. That is, Heeffer suggests that our understanding of early modern algebra has been confused by different kinds of 'unknowns'. Laboring under this confusion, historians have sometimes sometimes mistakenly identified problems as dealing with *multiple* algebraic unknowns when some of the 'unknowns' were just placeholders for knowns—they were not, for example, actually operated upon to solve the problem. By clarifying this point, Heeffer isolates a tradition of problems that actually deploy two unknowns. In 1474, Chuquet began to use a second unknown, an annotation calls this 'the rule of quantity'. This terminology also shows up in de la Roche. But then Christoff Rudolff, author of the first German algebra textbook, deploys a similar phrase. Did he learn from the French tradition? Heefer compares Rudolff's use of the phrase to de la Roche's. As a result, it seems possible

[1] See Kouteynikoff's account of Gosselin in a later chapter.

that Rudolff encountered Chuquet's problems through de la Roche's work or, Heeffer suggests, that by comparing the order of problems, the German *Rechenmeister* somehow had access to Chuquet's manuscript.

Heeffer's second methodological intervention is the database of 2000 algebraic problems from before 1600, which enables him to trace influences such as these accurately. Interested readers should investigate this database at http://logica.ugent.be/albrecht/math.php.

A mere three chapters, two mostly on France, cannot offer a comprehensive picture of the transmission of the medieval algebraic heritage. But they do suggest that the traditional story needs considerable work. Latin textbooks turn out to be as important as vernacular manuals; furthermore, all three chapters show that certain styles of mathematics are only partly explained by regional traditions.

C. The relation of algebra to arithmetic and geometry

In the third part of the book, Odile Kouteynikoff and Marie-Hélène Labarthe consider one of the oldest questions concerning Renaissance algebra: 'Is it actually an arithmetical tool or rather an application of geometry? Or is it instead a more fundamental mode of mathematical reasoning prior to both arithmetic and geometry?' To answer this question, they consider the works of the Paris humanist Guillaume Gosselin (died *ca* 1590) and Pedro Nuñes (1502–1578), professor of mathematics at Coimbra, both conceptually adept and widely learned in earlier algebraic traditions.

In Paris, after Estienne de la Roche in the 1520s and after the Latin revitalization of algebra in the 1550s, there was Guillaume Gosselin. His significance lies first in his *De arte magna* (1577) and then in his French translation of Tartaglia's treatise on number and measure (1578). Gosselin is of special interest because, on the one hand, he was deeply read in the tradition of vernacular problem-solving, a tradition that includes Stifel, Cardano, the Spanish *arte mayor*, and of course the earlier French authors. On the other hand, he also was a careful reader of the new editions of ancient Greek mathematics, notably Diophantus. The result all this reading, Odile Kouteynikoff shows, was a commitment to developing better theoretical tools.

Gosselin was especially attentive to Diophantus' use of 'fictions', replacing unknowns with 'false' values to approximate a solution systematically: the

Rule of False Position (or Hypothesis). Using this case study, Kouteynikoff shows us where Gosselin fits algebra in the hierarchy of arithmetic and geometry. At times, Gosselin made algebra a sub-discipline of arithmetic: algebra was practical arithmetic, he emphasized in his translation of Tartaglia. But at other times, he alerts the reader to how algebraic rules can be applied outside of numbers to geometrical objects. For example, Gosselin first formalized the Rule of False Position in an algebraic context and then used the ancient problem of duplicating the cube to reveal the rule's geometrical use. By working in different disciplines, algebra appears to be more fundamental than either of them. On balance, Gosselin seems to have seen the Rule of False Position as a more general, even universal, tool.

That was hardly the only option. Scholars such as Henk Bos have suggested that algebra depended on the geometrical tradition for methodological respectability [e.g., 2001]. Marie-Hélène Labarthe leads us in the same direction, tracing a path through the *Libro de álgebra en arithmetica y geometria* (1567) of Pedro Nuñes. She focuses on proofs for two of the six canonical rules inherited from al-Khwārizmī. To prove these rules, Nuñes thoroughly depended on geometrical constructions. His language makes clear that his algebraic reasoning about 'sides' and 'squares' is indeed about geometrical magnitudes—*cosas*, for example, are explicitly the sides of surfaces (*centos*). At the same time, Nuñes insists that the 'numbers' marking such magnitudes are subject to arithmetic. In particular, he follows the ancient prohibition of irrational fractions. As a result, the objects of algebra are defined by the combination of the ancient rules for both disciplines. Labarthe points out that her account vindicates Jens Høyrup's account [2002] of Nuñes, in which Nuñes' potential for innovation is limited by his assumptions from classical arithmetic and geometry. But Labarthe suggests that this very limitation is valuable in reconstructing precisely how arithmetic and geometry fit together in the history of algebra [213]. Here is one of the places in the volume where the authors might have passed a little further beyond careful textual analysis. The point needs explicit unfolding. I am ready to believe that Nuñes' exposition of algebra was enriched, not bounded, by the blend of traditional arithmetic and geometry. But how, exactly?

Gosselin and Nuñes make a fascinating comparison. Gosselin apparently was stimulated by ancient arithmetic to simplify through general, abstract rules, eventually breaking the traditional rules. In contrast, Nuñes may have been limited by the tradition but he built up a more systematic account of it. In both cases, however, the exposition of algebra depends on both arithmetic and geometry but it conceptually slips back and forth between the older disciplines.

D. The variety of Renaissance definitions of algebra

The last and longest group of chapters aims more directly at the question that distinguishes this collection from older histories of Renaissance algebra: 'What is, or was, algebra?' The question is important because historians of mathematics have often thought the answer obvious: just go back to the period and check whether a given figure had achieved a passing grade on a particular algebraic concept. This collection signals an effort to dig more deeply, with greater historical sensitivity.

This sensitivity shows first by attending to the account of algebra's origins that Renaissance practitioners themselves gave. In a chapter on 'Narratives of Algebra in Early Printed European Texts', Jacqueline Stedall points out that algebra was justified to the reading public by either reputable genealogies or promises of utility. Her account of genealogies is most developed. From Pacioli to Peletier, in the first half of the century, authors often reported that algebra was founded by a shadowy Arabic figure named 'Geber'. Høyrup [1996] and Cifoletti [1996] have argued that Renaissance mathematicians systematically obscured the Arabic roots of algebra in the 16th century. Stedall pinpoints the shift to 1550, when Johannes Scheubel observed that Regiomontanus had connected Diophantus to algebra (in an oration first printed in 1537). By the 1550s—the same decade that Loget highlights as a turning point—the Greek origins of algebra threatened to eclipse the vague Arabic attribution to 'Geber'. So did this changing attribution match a different definition of algebra? Stedall suggests that with Stifel and the generation of the 1550s, algebra was 'no longer to be seen as a collection of specific techniques (i.e., as inherited from al-Khwārizmī) but as a general method encapsulated in a single rule' to be applied anywhere in arithmetic, an account that fit nicely with its new origins in the *Arithmetica* of Diophantus [234]. The new account of algebra's classical origins fit new priorities, mathematical as much as political.

Stedall finds only one author advertising algebra for its own sake, Recorde in his *Whetstone of Witte* (1557). Her authors addressed a public who needed to be convinced that algebra was worth investment; within the more restricted republic of mathematicians, however, there were lovers of algebra such as Girolamo Cardano. To measure their lack of practical interest, one must dig past public images to private obsessions, as Veronica Gavagna reveals. Gavagna reconstructs the editorial history of a text that is often overshadowed by Cardano's *Ars magna*: his *Arithmetica*. Historians have mostly ignored the *Arithmetica*. Those who have not, have simply thought it a novelty that Cardano composed after his masterpiece, around 1545. But Gavagna finds earlier vestiges of the work. In 1539, Cardano sent a letter to Tartaglia with a copy of his newly published *Practica arithmetica*, mentioning his account of book 10 of Euclid's *Elements*. He explained that it resolved a new type of algebraic equation but was too long to publish with the *Practica*. Gavagna hypothesizes—in part on the basis of Cardano's autographs—that in fact Cardano was referring to the *Arithmetica*. But if it was written in 1539, why wait until 1545 to publish it? Tracing changes in Cardano's use of specific equations, Gavagna suggests that he was reluctant to publish it because he hoped to clarify parts of the work—some clarified bits were published in the *Ars magna*. Perhaps he meant to work out the remainder (specifically the sections on Euclid) at more leisure but he may have published what he had in 1545 in a hurry to establish priority. Meanwhile, if Gavagna's reconstruction holds, the *Arithmetica* now provides a snapshot of a key stage in the earlier development of Cardano's *Ars magna*. The implication would be that Cardano developed his algebra as commentary on Euclid's *Elements*, thus bringing algebra into the realm of learned reflection on classical problems for their own sake.

The same tension between practical and theoretical uses of algebra returns in Pedro Nuñes' algebra. Maryvonne Spiesser first shows that even though it was published in the vernacular, Nuñes' work based itself not on a local, Iberian practical tradition but instead on the Italian works of Pacioli, Cardano, and Tartaglia. Furthermore, he turned to sources such as Regiomontanus' *De triangulis omnimodus* (1464, printed 1533) for problems. In other words, the problems that he posed for algebra came from the developing canon of classicizing geometry. Speisser leads the reader through several examples of how algebra served to resolve such classical problems. Interestingly, Nuñes' commitment to the rigor of algebra is somewhat ambivalent. To be sure, it serves as a kind of master discipline, a 'scientific' method for all parts of mathematics. But Nuñes does not insist that his reader follow every proof: those are just for doubters. As Spiesser elegantly sums it up:

> The nature of algebra oscillates constantly between two poles: a science, coming out of geometry and which surpasses it; an art, a technique superior for resolving mathematical problems. [285]

One of Nuñes' most accomplished mathematical readers, the Jesuit mathematician Christoph Clavius, would collapse the poles of algebra as art and science. In a chapter that addresses Clavius' definition of algebra head on, Rommevaux reveals an aged, consummate pedagogue integrating some of the previous centuries' progress in algebra—he refers to Bombelli, Cardano, Tartaglia, Maurolyco, Viète, and the medieval arithmetic of Jordanus. Chiefly, however, he uses Stifel and Nuñes. Rommevaux demonstrates in particular that Clavius mentions algebra chiefly as an 'art' for resolving problems of every kind in mathematics. This puts him in the tradition of seeing algebra as a 'great art', like Cardano and Nuñes. But Rommevaux implies that, in reordering Nuñes, Clavius shifts the conceptual foundations of algebra. Instead of setting algebra off with geometrical proofs as Nuñes does, Clavius expresses algebraic rules as a 'continuation of the rules of elementary arithmetic' [308].

The final chapter of the volume brings out a theme that lies mostly latent in this volume: the social place of mathematics and the shape that it impressed on algebra. Do, in fact, the roots of the elite advances in Viète and Descartes' algebras lie in the practical soil of merchant maths? Ivo Schneider uses the German *Rechenmeister* Johannes Faulhaber to consider how the masters of German *Coss* shaped the concept of algebra in the decades around 1600. Such masters seem to have been mostly architects and mercantile teachers—not university masters—whose livelihood depended on an oral pedagogy. Schneider evokes the world of practical 'secrets', which teachers advertised to would-be students: Faulhaber claimed that *Coss* was an 'art and science' which would lead its practitioner to all other mathematical disciplines. This context helps explain why *Rechenmeister*, wary of sharing their wares too freely, circulated new results very slowly. For example, even though Cardano's solution of cubic equations was appeared in 1545, it was not available to a German public until 1608.

Schneider's account is most fascinating for what it says about the end of this oral, practical, craft-oriented culture of mathematics. Critics such as Descartes derided the *Rechenmeister* for not only their secretiveness but also the variety of 'tricks' that they invented to solve the same, simple problem, each master patenting his own 'methods'. This criticism partly reflects the disdain of Descartes, an academically-trained amateur, for Faulhaber's status as a practitioner with economic interests. The practical algebraist was undone by print culture, Schneider implies. After a fellow *Rechenmeister* divulged Faulhaber's algebraic secrets in print, he was compelled to make his name in other mathematical domains such as surveying and architecture. Not until 1622 did he publish in algebra. What he published that year, however, belies Descartes' dismissive judgment, for it included a solution to the quartic equation (equivalent to the solution in Descartes' *Géométrie* of 1637). In narrating this exchange, Schneider reveals a striking moment in the history of algebra. With printed algebras, readers could puzzle out their own solutions to problems instead of hiring a specialist to teach them—a fascinating glimpse of the tensions between professionals and the amateur without economic interests [e.g., 326].

E. Conclusion

At the beginning of this review, I mentioned Reviel Netz' large-scale account of the development of mathematics from problems to equations. This volume suggests that this mathematical story, far from becoming easier and neater between 12th-century Baghdad and the 17th-century Dutch Republic, first becomes much messier. One reason was the very rediscovery of classical mathematics, which forms an uncertain theme throughout this volume. Netz's analysis of ancient mathematics might provide a historical analogy to help understand what this rediscovery meant for Renaissance mathematics. Netz claims that late antique authors founded new (systematic) second-order reflections on mathematics by organizing the first-order works of classical geometers such as Archimedes, thus giving rise to systematic collections of problems and eventually new techniques (such as those by al-Khayyām and al-Khwārizmī) for dealing with those problems. This sounds a bit like an old story about Renaissance mathematics. In 1975, Paul Lawrence Rose suggested that the key contribution of humanist mathematicians was to make the store of classical mathematics available in new editions, translations, and commentaries, a necessary first step towards new creative answers to old problems. Rommevaux, Spiesser, and Esteve's volume suggests that the circle could be made larger: Renaissance mathematicians such as Stifel and Nuñes synthesized and built on the earlier classical, Arabic, and vernacular traditions all together. Thus, the key insight is that both scholarly and practical traditions need to be taken into account for algebra. It is foolish to exclude one or the other. The 'humanizing' Gosselin is a fascinating instance. Certainly, Gosselin's Rule of False Position is a practical analytic tool of the sort surely deployed by masters of *Coss* and *abaco*. Yet, to generalize the rule, Gosselin turned to Diophantus and the ancients.

This brings us to the social and material contexts of these books. Syntheses were achieved in textbooks. Several chapters focus explicitly on textbooks and, as a whole, this volume offers evidence of a momentous shift away from the *abaco* and *Coss* books for merchants towards algebra within a liberal arts education. While the works of Cardano and Stifel were meant to shore up a reputation among dueling practitioners, their Latinate, scholarly trappings put them into the world of liberal learning. With the flood of Latin textbooks published in the 1550s, we see algebra inserted into the liberal studies of the upper college and the university arts course. The very debate over whether algebra is chiefly an arithmetical or geometrical art is not only conceptual but was given special urgency by the social prestige of those disciplines as parts of the (disintegrating) quadrivium. Faulhaber's published work (his oral teaching notwithstanding) can hardly be called pedagogical. But Nuñes, Clavius, and Descartes all wrestle with the problem of presenting advanced material for students of the arts.

The shift towards liberal arts textbooks raises a number of unanswered questions about social uses of these works (a concern nearly explicit in Stedall's chapter). If Clavius' algebra, for example, serviced the hundreds of new *studia* and universities throughout Europe, did it also help merchants? Did this textbook revolution entirely pass by the clientele of the *Rechenmeister* and *maestri d'abaco*? Or did those demographics now attend city schools, where mathematics teachers could belong to the university world? What needs more work is the intended and actual readership of these works. Curiously, *Pluralité de l'algèbre à la Renaissance* expends hardly a word on the material apparatus or page layout of these mathematical books of the new age of print. Typography, diagrams, and physical descriptions of books say a great deal about authorship as well as readership (as one might infer from the mistakes of Libri and Boncampagni that Lejbowicz details). Yet, not a single image is reproduced, though some are imitated in modern typography.

To think about the broader significance of algebra in this period, we might reflect on Latin. Here *Pluralité de l'algèbre à la Renaissance* offers a helpful corrective. Often historians have mathematics have generalized about the social implications of mathematics from the language in which it was published, differentiating for example, between classicists and cossists. This is an especially fascinating question with regard to algebra, since it has often been identified with the cossist mathematics of the German *Rechenmeister* or the *abaco* tradition of Italy. Clearly, the vernacular often includes some of the most practical—and operationally sophisticated—forms of algebra. Likewise, the more theoretically rigorous efforts to prove algebra, to link it especially to geometry and arithmetic, occur in Latin treatises: the turn in Paris to Latin algebras of the 1550s and Clusius' algebra (1608). But there are plenty of exceptions to the polarity of vernacular/practical and Latin/theoretical: I only mention Stifel's *Arithmetica integra* and Nuñes' *Libro de algebra*. To see the larger topography of algebra in the 16th century, then, we need to do the kind of work that Schneider undertakes, linking the content of mathematical texts with the people, communal practices, and institutions that made such texts worth publishing. That is, the greater significance of the 'Latin turn' in Renaissance algebra is in the audience. Latin brought algebra into the arts classroom—which brings us back to the question of social utility of mathematics. Few contributors to this volume dwell on the larger audiences and the social contexts of algebra, with the exception of Schneider and Loget. That is hardly a criticism: to do the history of mathematics, we first need to get straight what the texts say. But what the texts meant and did in early modern Europe requires us to take another step, one that this volume now invites.

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Ignis sacer. Una storia culturale del 'fuoco sacro' dall'antichità al Settecento by Alessandra Foscati

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Medieval ideas of disease were rational and complex but they do not yield a clear picture of the past. Historians who interpret those ideas in order to recover both the cultural and physical aspects of diseases centuries ago can only bridge the distance if they first penetrate the semantic and lexical obscurities of another thought world. Alessandra Foscati's new study of an old disease terminology reveals just how carefully that task must be approached, since medieval writers described disease in ways that were more literary and historical than what we would characterize as empirical or scientific. These writers belonged, furthermore, to a restricted literate culture trained in particular ways. Treating the ill was by no means the exclusive domain of the literate but writing histories of diseases and epidemics was. So medieval writers drew vocabulary from an already remote classical past when they described the course of diseases and their impact on their societies. Accounts of those now distant pathological realities employed Greek and Latin medical nomenclatures that are not strictly equivalent to those of modern medicine. The precise subject of Foscati's book is the evolving meaning of the Latin expression 'ignis sacer' ('Holy Fire'), which for centuries described pathologies that ulcerated the skin and resulted in gangrene, and which has been thought in the modern period to have designated epidemics of ergotism. By examining a wide set of sources that include medical treatises, chronicles, hagiographies, sermons, public edicts, and notary texts, and by comparing these with each other over time, Foscati dispels some persistent confusion regarding the nature of these accounts.

Described in a 12th-century hagiography of Saint Ilarius of Poitier, *ignis sacer* burned in the flesh with no visible flame and 'smoldered within quietly, invisibly and with a fetid stench, so that it is easy to tell that this is not our

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mundane fire, but rather infernal' [43: my translation]. In an earlier description cited by Foscati, the Frankish chronicler Rudolf of Glaber described an epidemic that raged throughout Italy and Gaul in the last decade of the 10th century. Glaber tied the terrible epidemic to an eruption of Mount Vesuvius in the months prior. The volcano ignited a fire that spread to the bodies of the sick across the peninsula and beyond. The continuity imagined between the eruption and the epidemic illustrates the deep understory of the noun 'ignis', which carried philosophical, medical, and religious meaning that can be lost easily or distorted by hasty reading. The Frankish chronicler explained that Vesuvius had previously vomited 'sulfurous fire mixed with a great multitude of stones' [35]. In the 17th century, 'sulfurous fire' identified the theory of chemical volcanic ignition used by natural philosophers such as Giovanni Alfonso Borelli and Robert Hooke to explain volcanism. Obviously, chemical ignition so envisioned could not have been the meaning of Glaber's expression, though elements of a naturalistic explanation cannot be discounted. The medieval classification of diseases (nosology) and the description of those diseases (nosography) have to be understood on their own terms as much as possible. That effort is the central thrust of the book. If the classical and medieval sources that Foscati analyzes surely record instances of infectious disease, those pathological realities are, as she suggests, historically remote, difficult to retrieve, and obscured by changes in language and meaning.

Foscati develops a crucial revision of a historical interpretation that is by now itself quite old. Ever since medical observers began to identify ergotism at the end of the early modern period, historians have equated *ignis sacer* with epidemics of that disease among European populations. We now know that people who ate rye flour contaminated with the fungal toxin *claviceps purpurea* suffered from intense burning pain, gangrene, and convulsive seizures. Doctors became aware of the link between ergot poisoning and contaminated rye as early as the 17th century but it was only in the 18th century that medical texts began to identify ergotism (*ergotisme* in the French sources) with *ignis sacer*, really because the expression was widely useful in describing gangrene. Foscati, however, disputes whether a single pathology can account for the huge body of medieval sources citing *ignis sacer* and kindred phenomena. Ergot poisoning causes gangrene but medieval people developed gangrene for many reasons, including infection.

Foscati's most immediate contribution to the historiography is readily located, as are the theoretical foundations of her analysis. She argues that disease names in historical accounts should be thought of as 'semantic vectors' that might designate very different pathologies in modern diagnosis [x]. She attributes this concept to Mirko Grmek's argument [1989, 1] that disease is a human idea, an explanatory model emanating from the thought constructs of a period. Foscati adapts Grmek's ecological concept of *pathocoenosis* to her analysis as well, specifically, to illustrate that the cultural and semantic transformations tracked in the book may also have been responses to changing ecology of disease in medieval Europe. 'Pathocoenosis' refers to the combination of pathogens present in any given population at a particular historical moment. Starting in the late 1960s, Grmek introduced the neologism in order to generate a more expansive model for explaining the frequency of diseases in given periods. He postulated that pathogens exist in communities acting in symbiosis, antagonism, or even indifference. The occurrence of diseases modulates through interaction with others and is influenced by the ecological and endogenous conditions that those pathogens inhabit in the environment and the human body [Grmek 1989, 3]. As Foscati implies in the opening lines of the book, any historical interpretation of disease must bear in mind that pathogens and their frequencies evolve, along with diagnoses of doctors [ix]. Combined, these formulations show that Foscati does not divorce her philological analysis from the natural history of medieval disease. Ultimately, as the title makes very clear, the cultural and semantic dimensions of 'ignis sacer' are the principal subject. The biological and ecological dimensions of historical pathologies would require a very different kind of investigation and should not rely on written historical sources alone.

This densely footnoted and compact book contains an introduction, four body chapters, and a brief appendix of primary source selections. As a good humanist, Foscati works back as far she can to find the fount of the expression 'ignis sacer'. She locates a point of origin in Lucretius' *De rerum natura* (first century BC) [3]. Foscati discovers considerable original variance in the Latin expression eventually adopted in the medieval medical lexicon. For the Roman authors Virgil and Columella, for instance, the expression may have denoted outbreaks of epizootic disease. For Pliny, on the other hand, 'ignes sacri'—in the plural—denoted human maladies afflicting the skin. In the first century AD, Celsus narrowed the definition to a disease that developed serpentine ulcerations on the body [6]. The Latin expression was tightly nested in the Hippocratic corpus by medical writers such as the fifth-century AD Roman African Cassius Felix. Formal inclusion in the vocabulary of Greek medicine established the semantic equivalence between 'ignis sacer' and the Latinized Greek word 'erysipelas'. Isidore of Seville observed the same correspondence two centuries later, confirming its place in the medical lexicon. That equivalence would resurface at various points, existing as part of the larger system of nested meanings that shifted over time [12].

Foscati's analysis stimulates broad considerations throughout the book, some of which perhaps deserved fuller treatment. Symptoms of disease were intensely subjective and even then rarely written down as the direct expression of a sick person. Descriptions were frequently second hand, so distortion operated on distortion well before the long chain of literary repetition occurred. Much gets refracted in the prism of a philological analysis. We can see that the medieval understanding of illness derived in part from Greek medicine and natural philosophy, as expected. The range of the ancient knowledge greatly expanded after the 12th century. Even then, medieval nosography (description of the symptoms and etiology, or cause, of disease) always observed the Christian rationale of divine castigation for the sins of man. The sacred *topos* had a rationalizing function across a breadth of texts. Natural and divine operations situated symptoms into recognizable typologies that could be structured upward to causal understanding. By virtue of the micro-macrocosmic analogy, medieval writers sought coherence in narrating portentous historical events and natural disasters. Epidemics were in that respect analogous to other kinds of cataclysms. They were preceded by signs, for instance, and manifested operations of the divine in the natural order. The misreading of such sources by 18th-century historians stemmed from ignorance. Medieval nosography was not empirical in a strictly modern sense. More accurately, descriptions of epidemics and diseased individuals were subordinated to the narration of history. They functioned in ways that we might much more aptly characterize as literary [32].

Foscati dedicates a second long chapter to the close examination of the sources that recounted outbreaks of *malattie urenti*, an Italian class of diseases associated with the sensation of burning. 'Ignis sacer' was initially only one in a constellation of expressions. Most significant, Foscati finds no explicit correlation with ergotism in the earlier material. Foscati explains the wide recurrence of the expression by suggesting that it was especially apt for characterizing maladies that appeared to be divine or infernal—hence the related expression 'ignis infernalis'. Her conclusion is perhaps a little broad, though substantively correct: the terrible, mysterious, and internal nature of the fire qualified it as supernatural rather than natural [59]. Fire was one of the four elements along with earth, water, and air, so its mundane operation was well accounted for by Aristotelian natural philosophy. The speciation of terms really occurred at the level of the adjective qualifying the noun 'ignis'. Fire could be variously occult, divine, sulfurous, infernal, or invisible (for example, *occultus, divinus, sulfureus, infernalis, invisibilis*). In the early 11th century, 'ignis sacer' emerged as a dominant noun and adjective pairing describing the terrible symptoms of gangrene, often in conjunction with 'ignis Sancti Anthonii' ('Saint Anthony's Fire').

Deftly, Foscati discerns patterns in the morass. One pattern was that chronicles and hagiographies that developed the equivalence between *ignis sacer* and gangrene frequently reinforced an association with the similar expression 'ignis sancti Anthonii'. The overlap appears, for example, in the 13th-century religious texts of the renowned preacher and chronicler of heresies Stephen of Bourbon, who blurred the distinctions between *ignis sacer*, *ignis infernalis*, and *ignis Sancti Anthonii*. Contemporary medical texts, complicating matters further, used the Greek and Roman vocabulary of disease, describing various cutaneous diseases as erysipelas [93]. The reader might easily become lost in a thicket of overlapping expressions but fortunately Foscati manages to extrapolate a useful general picture. If 'ignis sacer' seems to have described gangrene in individuals as well as epidemics that may have included ergotism, the related expression 'ignis Sancti Anthonii' was not used to describe epidemics of any sort but instead described individual cases of gangrene [119].

Foscati delves into the relationship between the conceptualization of disease and the medical charitable practices of religious orders in a well-developed third chapter. The historian considers the larger social and cultural context in which authors used the expression 'ignis Sancti Anthonii'. Her close reading medical texts as well as religious records confirms that the expression described gangrene in afflicted individuals and not epidemics of ergotism. Most significant, Foscati finds that the strict association between the Hospital Brothers of Saint Anthony and traditions of charity and treatment of those suffering from ergot poisoning was, just like the distortion of 'ignis sacer', a later error of historical interpretation generated when a new disease construct occluded the original meaning of medieval texts. Disease ecology adds to the complexity. Foscati's reintroduces *pathocoenosis* at this point, highlighting that the disease environment of the later Middle Ages was likely very different from that of previous centuries. As Grmek has argued, communities of pathogens changed but so did human social and demographic behaviors with the recovery of urban life after the 15th century. Foscati makes this consideration: few sources after the Black Death of 1348 record epidemics of *ignis* diseases, though instances of gangrene abound in the record. Any conclusion is speculative, she concludes, but it is possible that epidemics of bubonic plague and then syphilis diminished the cultural importance of previous diseases [139–140]. The book's foray into this topic is limited and a less narrowly focused book might have done more with it but it is a useful expansion of the subject.

Foscati's book does shed much light on the semantic instability of 'ignis sacer' and 'ignis Sancti Anthonii', thus putting the traditional history of ergotism into question. As the fourth and final chapter argues, a new disease construct began to develop near the end of the early modern period. Scientific empiricism shifted the foundation beneath the two long-standing expressions. In 1676, Denis Dodart, a member of the Royal Academy of the Sciences in Paris, reported on an epidemic in north-central France and, significantly, observed a connection between that epidemic and the consumption of spoiled rye [181]. Medical writers began to build the case for ergotism in the ensuing decades empirically as well as by gathering evidence from earlier medieval texts. Reading these historical sources without the tools of modern philology, 18th-century writers produced an unnaturally simple account of the disease past.

Foscati's book expands our understanding of early modern empiricism too, especially in the final chapter. There is an intriguing similarity, and difference, between the 17th-century nosography of burning diseases and the natural history observation of volcanism that was in the same period (especially by the late 1600s), a bourgeoning science. Volcano watchers recorded the fiery history of eruptions empirically but in ways structured by medieval and Renaissance epistemologies (leaving aside the debates about the variances there). *Historiae* of volcanic eruption were similar to those of disease because they shared an analogy with the body and because they were sacred, historical, and natural philosophical in nature. Not yet geologists or volcanologists, early modern naturalists delved into texts. Observers of Vesuvius, for example, read back to the great eruption of the first century AD famously described by Pliny the Younger and then worked forward through the spotty record in late antique and medieval sources, up through the growing sequence of modern eruptions (in 1631, 1660, and so forth). The approach to classical and medieval sources would not have differed significantly whether they chronicled eruptions or epidemics. The similarity of these cases, on the other hand, ends when one considers the different effect created by the collation of textual evidence and empirical observation. For early volcanology in the 1600s, still unformed within a nascent modern geology, historical sources were a useful complement to empiricism because they helped to identify the date and frequency of previous eruptions, even when modern observers deemed the empirical value of earlier accounts to be scarce. In the case of epidemics, perhaps, historical accounts were more likely to mislead. Maybe that was because those events were not the operation of a highly visible and identifiable natural phenomenon with a discernible periodicity but were rather the operation of multiple pathogens that remained invisible to even the best observers.

A century after Dodart described an epidemic of ergotism, A. P. Read's *Traité du seigle ergote* [1771] became one of the texts that established the scientific case for a disease caused by eating *seigle ergoté* (ergotized rye). Foscati appends a selection of the treatise at the end of her book. Read explained that

> there is every reason to believe that the different diseases that afflicted France in the tenth, eleventh, twelfth, thirteenth and sixteenth centuries, under the name of 'feu sacre','mal des ardents', 'feu infernal', 'mal de St Antoine', were caused by the use of ergotized rye. [207]

Read identified 12 epidemics of the disease between 944 and 1630. Citing the absence of scientific knowledge among the historians who chronicled these past scourges, Read explained that a terrible common set of symptoms helped to identify ergotism as the cause. Appearing on the skin, the disease spread inside the body, blackening and consuming limb and flesh. 'Not even the bones were spared this fire's furor', he wrote. It is revealing that Read described later 16th- and 17th-century epidemics as showing dry gangrene (*gangrene seche*), clearly evincing one of the expressions of early modern nosography that Foscati shows overwrote earlier sources [208–210].

The greatest use of this meticulously researched book will be by those historians of science and medicine, not few, who read Italian scholarship. Foscati's analysis manifests the best abilities of scholars with the training and proclivity to develop a philological approach that cuts across types of sources from different periods. Fellow historians so inclined will find the extensive footnotes especially helpful; these become a parallel text that locates the evidence, expands the analysis, and lays a well-marked trail to follow. Scholars of early modern epistemology will find this book useful, since Foscati's analysis delves into features of scientific empiricism already studied by Gianna Pomata and Nancy Siraisi, both of whom the author cites. Finally, this study merits reading for another reason. Foscati develops an interesting picture of how historians in the early modern period interpreted medieval sources as evidence. On that one wishes for more too but scholars interested in historiography and the construction of historical knowledge in general should mine this fine book for its insights.

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Writing Science: Medical and Mathematical Authorship in Ancient Greece edited by Markus Asper with Anna-Maria Kanthak

Science, Technology, and Medicine in Ancient Cultures 1. Berlin: De Gruyter, 2013. Pp. viii + 502. ISBN 978–0–19–991387–9. Cloth £41.99, $65.00

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In April 2009, Markus Asper assembled leading scholars of ancient Greek mathematics and medicine to participate in New York University's Ranieri Colloquium on Ancient Studies. The conference, called 'Writing Science: Medical and Mathematical Authorship in Ancient Greece,' resulted in this volume, the first in De Gruyter's series Science, Technology, and Medicine in Ancient Cultures. The volume includes several of the conference's original participants and Asper, in collaboration with Anna-Maria Kanthak, has expanded its scope to include contributions by, for instance, Karine Chemla, Apostolos Doxiadis, and Michalis Silaros, who in their papers address ancient Chinese, Mesopotamian, and Egyptian scientific texts.

Writing Science is arguably the most innovative collection of essays on ancient science to come out in recent years. The authors promote a literary/aesthetic methodology to analyze a variety of ancient Greek medical and mathematical writings and they contextualize ancient scientific texts in relation to (other) ancient Greek literature. The papers examine authorial voice, narrative, genre, literary style, and the politics of reading circumstances. The anticipated audiences of the papers vary from scholars trained in the history and philosophy of science to classicists versed in Hesiod, Homer, and Thucydides.

Paul Keyser's and Markus Asper's contributions engage to the greatest degree with studies in the history and philosophy of science. Keyser's paper, the first in the volume, addresses the nature of science in general. He posits his own definition of science as the sum of collections of effective recipes and their explanations—or practices and theories, respectively—and he argues that the social contexts that produce scientific innovations are robust ecologies of debate which, with respect to the ancient Greek sciences, disregard traditional

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authority, are open to innovation and dissent, accord a relatively high status to mercantile activity, and exhibit a low level of xenophobia [24]. Keyser tests his hypothesis of what conditions determine whether science flourishes or declines in any given social context by reference to the distribution in time of ancient scientists, distilled from Keyser's and Irby-Massie's own *Encyclopedia of Ancient Natural Scientists* [2008]. Keyser presents three case studies in the history of the ancient Greek sciences which he intends to demonstrate the progress of science: summing a series, the diagnosis and prognosis of wounds in the head, and the design of artillery.

While Keyser advances a transhistorical theory of progress, Asper examines narratives of progress in ancient Greek scientific texts. He analyzes three plot-structures: boundless accumulation, teleological completion, and circular return. Whichever plot structure a text realizes results from the author's fashioning of both his own identity as author and his position in relation to other contributors in the area of inquiry. Asper observes that different plot structures thrive in different fields. The conception of progress as a steady accumulation is predominant in ancient Greek mathematical texts, where authors present their work as part of a diachronic group-effort which, Asper boldly claims, renders ancient Greek mathematics 'normal science' in the Kuhnian sense [417]. The teleological plot and the story of return, on the other hand, frequently appear in medical writings, reflecting the high degree of competition among physicians.

In his contribution to the volume, Reviel Netz maintains that Greek mathematics 'is as competitive as any other Greek genre' [217]. Why, then, did the narrative of progress in mathematical texts differ from the narrative of medical texts? An analysis of why it is that the plot of accumulation, rather than of completion or return, was popular in mathematical texts would have contributed a more robust analysis of the culture and rhetoric of ancient Greek mathematics.

The vast majority of the volume's papers constitute authorship studies. Chemla eliminates the author from the Chinese classic *The Nine Chapters on Mathematical Procedures* and argues that the scriptural act that produced the text was not one of writing but rather of editing. Heinrich von Staden investigates the challenges of 'writing the animal' and the concomitant effects on authors' self-representation and rhetoric, as evidenced by Aristotle's, Pliny's, and Galen's zoological writings. Philip van der Eijk examines Galen's persona in *Mixtures*. Ineke Sluiter argues for the dominance and, moreover, violence of the commentator over a source text in Homeric scholia and, by extension, Galen's corpus. Reviel Netz emphasizes the textual character of Greek mathematical practice in contrast to the performative authorship of other ancient Greek literary genres. Serafina Cuomo argues that the authorship and audience of classical account inscriptions reflect political participation in the Athenian empire. Alan Bowen analyzes the techniques employed by Hellenistic authors of introductions to astronomy when establishing their authority. Brooke Holmes explains the 'structurally disembodied' character of the physician in Hippocratic texts.

Although an emphasis on authorship pervades the volume, the range of literary topics and types of scientific texts analyzed remains impressive. The collection includes nearly as many papers on ancient medicine as mathematics and the varieties of mathematics examined reflect the diversity of the ancient Greek mathematical tradition including geometry, numeracy, mechanics, and astronomy. If Asper and Kanthak meant to establish the legitimacy of the literary/aesthetic approach to ancient medical and mathematical texts as well as the fruitfulness of comparative studies in ancient science writing, then, with this cast of expert historians of ancient science, they have accomplished their goal.

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In memoriam
Allan Gotthelf
(30 December 1942–30 August 2013)*

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Allan Gotthelf passed away on August 30, 2013 in his apartment in Philadelphia under the care of close friends, finally succumbing to the cancer that he had battled successfully for many years. He has left behind a lasting legacy of distinguished scholarship on Aristotle's philosophy and science; but more than that, Allan was able, through his infectious enthusiasm, spirit of cooperation, and formidable organizational skills, to reshape the field of Aristotelian studies fundamentally. Not only through his scholarly publications but through his prodigious organization of workshops, conferences, summer institutes, and a truly astounding network of correspondence, which only became more astounding with the advent of the Internet, Allan was able to involve an ever expanding number of scholars in a systematic study of Aristotle's biological writings, encouraging us to consider the *Generation of Animals*, *Parts of Animals* and *History of Animals* as much a part of our scholarly diet as the *Organon*, *Metaphysics*, or *Nicomachean Ethics*. Indeed, he encouraged us to think of these investigations as sources of insight into these philosophical works and not merely as documents in the history of biology.

Born in Brooklyn on December 30, 1942, Allan Gotthelf came of age during the heyday of the Brooklyn Dodgers, attending games at Ebbets Field as a young boy shortly after Jackie Robinson had joined the team, an event that left a lasting impression. He attended the highly acclaimed Stuyvesant

* For Allan Gotthelf's *curriculum vitae* and list of publications, please go to http://www.ircps.org/directory/allan-s-gotthelf

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High School, where he developed a passion for mathematics and science, and, in 1959, he enrolled in Brooklyn College intent on pursuing a joint degree in mathematics and physics. His intellectual focus was redirected toward philosophy during the summer of 1961 as a consequence of reading *Atlas Shrugged*, the epic novel by American novelist and philosopher Ayn Rand that stressed the fundamental importance of philosophy in the lives of individuals and cultures.

He graduated in 1963 with a major in mathematics and a minor in philosophy and, after earning an MA in mathematics at Penn State, entered the graduate program in philosophy at Columbia University. Again under the influence of Ayn Rand, who had a profound admiration for Aristotle, Gotthelf eventually decided to write his dissertation on 'Aristotle's Conception of Final Causality', which he defended before a committee consisting of James Walsh, Paul Kristeller, and John Herman Randall Jr. He was awarded his PhD in 1975 and, a year later, an article based on his dissertation that presented its deeply original thesis—that for Aristotle, final causality, the idea that goals were somehow responsible for processes directed toward their realization, was rooted in an irreducible potential for form—was awarded the dissertation essay prize of the *Review of Metaphysics* and was published in volume 30 of that journal. This was to be the first of a series of six papers dealing with different aspects of Aristotle's commitment to teleology, which span the entirety of his career and now comprise the first six chapters in his collected papers, *Teleology, First Principles, and Scientific Method in Aristotle's Biology* (2012).

It is quite unusual for a first publication to have the sort of impact that 'Aristotle's Conception of Final Causality' did but it was not an accident. In the interests of having scholars actively engage with the interpretation that he was defending, Allan compiled a list of people in the fields of ancient philosophy, philosophy of science, and even biology who he had reason to believe would find the argument of interest and sent offprints, typically with introductory notes, to everyone on that list. This act of engagement had the desired impact: it was not long before anyone who took up the topic of Aristotle's teleology had to contend with Allan's thesis and argument, that Aristotle's teleology was a scientific claim about the causality of biological processes and rooted in his metaphysics of (formal and material) natures and (active and passive) potentials. Teleology thus implied no quasi-conscious agents, demiurgic designers, or future states affecting the past; and yet it was a distinctive form of causation not reducible to the causal interactions of the materials involved in those processes. Through reviews of their work, and occasional review articles like his 'Understanding Aristotle's Teleology' (1997), Allan engaged in an on-going discussion with those scholars who had challenged his views—on-going until his last days. In an email that he sent to me less than four months before his death, Allan expressed regrets that his health would likely not permit him to attend the Princeton Classical Philosophy Colloquium in 2013, 'Necessity and Teleology in Aristotle's Natural Philosophy'. He especially regretted having to be absent because it would have provided him an opportunity to talk to a number of younger scholars who had expressed reservations about his interpretation.

In 1979, three years after the publication of this ground-breaking article, he was awarded a prestigious Junior Fellowship at the Center for Hellenic Studies in Washington, DC and it was during his year at the Center that he began to develop a proposal for a collection of essays on Aristotle's biology. Over the next 20 years, Allan played a central role in organizing conferences, workshops, and summer institutes that encouraged scholars of Aristotle's philosophy to integrate the study of his biological works into their research. It was the first of these, a 10-day conference entitled 'Philosophical Issues in Aristotle's Biology', that would serve as the basis for a wide-ranging collection of papers highlighting Aristotle's biological writings as a valuable source for exploring central themes in Aristotle's philosophy. Organized in collaboration with David Balme, it took place at Williams College in Williamstown, MA during the summer of 1983. The resulting eponymous volume, which I was privileged to co-edit, is widely credited with moving Aristotle's animal investigations into a central place in Aristotle studies.

Allan was thrilled to collaborate with David Balme on this event. Balme's translation and commentary of sections of the *Parts and Generation of Animals* in the Clarendon Aristotle Series (1972) had been a revelation to Allan. They became friends during Balme's visit to the Institute for Advanced Study in Princeton in 1976 and interacted constantly until Balme's untimely death in 1989. Allan took great pleasure in organizing and editing the Festschrift in Balme's honor, *Aristotle on Nature and Living Things* (1985); and in 1986, Allan was awarded a three year NSF grant to carry out collaborative research on the *History of Animals* with Balme, which turned out to be the last three years of Balme's life. After Balme's death, Allan devoted a significant portion of the remainder of his life to seeing through to publication Balme's draft of the third volume of the Loeb edition of the *History of Animals* 7—10 as well as his *editio maior* of the *Historia animalium*, a draft of which Balme had completed for the series, Cambridge Classical Texts and Commentaries. Thanks to Allan's efforts, the Loeb volume appeared in 1991 and the first volume of the *editio maior* (consisting of an extensive introduction, text, apparatus, and index) in 2002. Allan was roughly halfway through preparing the second volume, the commentary, when ill health forced him to put work on it aside.

There were to be many other conference collaborations on related themes—with Sir Geoffrey Lloyd (1985, 'Aristotle's Philosophy of Biology', King's College, Cambridge), Pierre Pellegrin and Daniel Devereux (1987, 'Joint CNRS/NSF Seminar on Interconnections of Biology, Scientific Method and Metaphysics in the Scientific and Philosophical Writings of Aristotle', Oléron), John Cooper and Michael Frede (1988, 'NEH Summer Institute on Aristotle's Metaphysics, Biology and Ethics', University of New Hampshire), and Wolfgang Kullmann and Sabine Föllinger (1995, 'Symposiums über 'Aristoteles' Biologie'', Werner-Reimers-Stiftung, Bad Homburg). Through his central role in conceiving and orchestrating these events, Allan earned a reputation as an extraordinary organizer and the Oléron and Bad Homburg conferences led to the publication of important collections of essays that further contributed to moving Aristotle's biological writings to the center of Aristotle scholarship.

Allan was without doubt a major force behind the organization of conferences during this period but he was publishing ground-breaking essays as well: 'Notes towards a Study of Substance and Essence in Aristotle's *Parts of Animals* II-IV' (1985), 'First Principles in Aristotle's *Parts of Animals*' (1987), '*Historiae I: Plantarum et Animalium*' (1988), 'The Elephant's Nose: Further Reflections on the Axiomatic Structure of Biological Explanation in Aristotle' (1997), and 'Division and Explanation in Aristotle's *Parts of Animals*' (1997). All of these essays were aimed at deepening our understanding of the way in which Aristotle's metaphysics and theory of knowledge informed the logical and explanatory structure of his study of animals and, conversely, how a more detailed and systematic study of these biological investigations could deepen our understanding of his philosophy.

In the late 1990s, Allan was diagnosed with cancer and this led him to take early retirement from The College of New Jersey in 2002, where he had served as department chair from 1988 to 1997 and had helped develop a Minor in Classical Studies. In his honor, the College created the Gotthelf Prize to be awarded annually to an outstanding graduating student in Classical Studies, selected by the faculty of that program.

That award spotlights an equally important facet of Allan's professional life, his love of teaching. Allan discovered that he had a gift for teaching early on. While still in graduate school, he taught an introduction to philosophy at the Pratt Institute in Brooklyn and for the next three years—while still in graduate school—was a full time philosophy instructor at Wesleyan University. By the time he had been awarded his PhD from Columbia, he had been Assistant Professor of Philosophy at Trenton State College (now The College of New Jersey) for six years and had already been awarded tenure. Having co-taught with Allan on a number of occasions, I was able to experience at first hand his pedagogical talents. His natural warmth and benevolence helped him to forge personal, one-on-one relationships with his students. He conveyed his passion for philosophy and the role of clear and rigorous philosophical thought in the achievement of one's goals in memorable ways, often through the use of humor. Whatever subject was being taught, his classes were workshops in philosophical method and the value of philosophy. Knowing the importance of good teaching, Allan looked for ways to pass on the skills that he had acquired. From 1982—1990, he and Michael Hooker conducted APA Eastern Division sponsored weekend teaching workshops.

Though there was no graduate program in philosophy at his home institution, throughout his career, Allan sought out opportunities to teach graduate seminars often in collaboration with others: with John Ackrill at Oxford in Trinity Term 1984, at Georgetown University in 1985, with David Charles and me at Oxford in Trinity Term 1994, and at Tokyo Metropolitan University in the Summer of 1994.

After his early retirement from TCNJ, Allan spent a term as visiting professor at the University of Texas, Austin and, thanks to an Anthem Fellowship for the Study of Objectivism, subsequently joined me in the department of History and Philosophy of Science at the University of Pittsburgh. Shortly after his arrival, in 2004, Robert Bolton and I organized a conference, 'Being, Nature and Life in Aristotle', to honor Allan's contributions to the study of classical philosophy and science. The papers presented at that event, along with a number of others by people who had wanted to attend but could not, were published in a Festschrift entitled *Being, Nature and Life in Aristotle: Essays in Honor of Allan Gotthelf*, edited by Bolton and me and published by Cambridge in 2010.

One of his greatest pleasures when he joined the faculty of the University of Pittsburgh in 2003 was being able to teach and direct graduate students. Though his Anthem Fellowship did not require it, while at Pittsburgh he taught, or co-taught with me, four graduate seminars and served with me on two doctoral committees. He was also a constant presence at the reading groups of the Program in Classics, Philosophy, and Ancient Science and sat in on a number of his colleagues' graduate seminars.

Unfortunately, in 2011, Allan's status as visiting professor at Pittsburgh ran up against a bureaucratic 'statute of limitations' and the Anthem Foundation had to search for another home for his Fellowship. The Department of Philosophy at Rutgers University happily complied and, in 2012, Allan was appointed there as the Anthem Foundation Distinguished Fellow for Research and Teaching in Philosophy. He taught an advanced undergraduate class on Aristotle during his first year there and was planning on co-teaching a graduate seminar with Robert Bolton in the Fall Term of 2013.

As I noted earlier, Allan was first oriented toward philosophy and Aristotle by his early encounter with Ayn Rand's philosophy of Objectivism. Rand's admiration for Aristotle was clear to an attentive reader of *Atlas Shrugged* but far more obvious in the lectures and non-fiction essays that she began to publish in the early 1960s. Throughout his career, Allan devoted considerable energy to encouraging the study of Objectivism among academic philosophers. He was a founding member and, as its secretary, the guiding spirit of the Ayn Rand Society of the APA, which was founded in 1988. His vision for the yearly meetings of the Society was to select a topic of central importance to Objectivism on which scholars sympathetic to Rand's approach would engage prominent specialists—the goal being to familiarize both the audience and the invited specialist with the distinctive approach of Objectivism to the topic at hand.

His Anthem Foundation Fellowship was designed so that he could devote part of his research to promoting the scholarly study of Objectivism. In 2000, Allan published *On Ayn Rand* as part of the Wadsworth Philosophers Series; and soon after he joined me in Pittsburgh, we contracted with the University of Pittsburgh Press to publish a series entitled 'Ayn Rand Society

Philosophical Studies', each volume to be based on papers presented at the Society meetings over the years. Volume 1 (*Metaethics, Egoism and Virtue: Studies in Ayn Rand's Normative Theory*) was published in 2011 and volume 2 (*Concepts and Their Role in Knowledge: Reflections on Objectivist Epistemology*) appeared in 2013, just a few months before Allan passed away.

With the same goal in view, during his years in Pittsburgh, he co-organized a series of workshops that brought philosophers trained in the Analytic tradition together with philosophers sympathetic to Objectivism on themes such as 'Concepts and Objectivity', 'Normativity and Justification in Epistemology and Ethics', and 'Perception, Consciousness and Reference'. Three publications with the same objective were in the planning stages when Allan passed away: *Ayn Rand: A Companion to Her Works and Thought*, co-edited with Gregory Salmieri for Blackwell Companions to Philosophy series; *Ayn Rand as Aristotelian*, volume 3 in the Ayn Rand Society Philosophical Studies series; and *Concepts, Induction and the Growth of Scientific Knowledge*, to be co-edited with Richard Burian. This last was based on a conference that reflected Allan's deepening interest during his years in Pittsburgh in the relationship between concept formation and induction, a topic of importance both for Objectivism and for Aristotle.

As if the projects that I have just recounted were not enough, during these years in Pittsburgh Allan also organized or co-organized a series of six workshops on Aristotle's *Generation of Animals* (which helped inspire chapters 4 and 5 of his collected papers of 2012); a series of workshops on 'Discovery and Justification in Aristotle'; and (with Robert Mayhew) a workshop on the Aristotelian *Problemata*. He also began planning with Armand LeRoi, a developmental biologist and producer of the BBC documentary 'Aristotle's Lagoon', a Penguin Classics volume of selections from Aristotle's biological works, a project which will hopefully be brought to completion by LeRoi and myself.

Having read a review of Allan's life as a scholar and teacher, a reader might be left with the impression that Allan's professional life *was* his life. Those who knew Allan, however, were well aware of his many passions outside of philosophy, though he was always happy to explain how each of them related to his philosophical sense of life. From his childhood encounter with Jackie Robinson breaking the color barrier in baseball, Allan developed a lifelong interest in the history of the Negro Leagues during the time when baseball was a segregated sport. While living in the Philadelphia area, he became an avid fan of the Flyers hockey team and, when he moved to Pittsburgh, he became equally passionate about the Penguins. He had a deep love for Frank Lloyd Wright's architecture and during his life even managed to arrange to live briefly in a number of homes designed by Wright. He was an avid film buff and had deep love for many genre of music, especially romantic piano and operetta. Allan approached each of these subjects as he did everything—with an intense desire both to experience and to understand. He took great pleasure in sharing these, and other, passions with his close friends.

Among these non-academic pleasures, there was always a special place in Allan's heart for a small Inn built in 1810 at the base of Mt Snowdon in Northern Wales named Pen-y-Gwryd, where Sir Edmond Hillary, Tenzing Norgay, and their team resided while training for the first successful ascent of Mt Everest in 1953. Part of its charm lies in the fact that it is filled with rare, historic memorabilia related to that event and that team members had reunions there every 10 years. From his first visit in 1984 as a fellow at Clare Hall, Cambridge and Wolfson College, Oxford, Allan loved the vivid presence of history in the UK and his visits to Pen-y-Gwryd were among his greatest pleasures. Typically, Allan became close friends of the innkeepers and on most of his many trips to Great Britain he would find time to return to Pen-y-Gwryd.

It will come as no surprise to readers who knew Allan that he spent his remaining time, after it became clear that he had only months to live, focused on insuring that the projects which he was then working on would be carried through to completion. As he faced the imminent end of his life, Allan remained focused on the future and on the achievement of values of importance to him.

Allan Gotthelf is survived by Ronald and Cassandra Love and their sons Zach and Ian Barber, whom Allan regarded as family, and by his sister Joan Gotthelf Price. He will be remembered fondly by his many friends, colleagues, and students.

A Personal Note

Because my unique relationship with Allan no doubt colors this remembrance of his life, I will here briefly recount our 42 years of continuous collaboration and friendship. We first met in 1971, during my last year as an undergraduate philosophy major at York University in Toronto. We were introduced by a professor at York who was close friends with Allan and knew of our shared interest in both Aristotle and Ayn Rand. After our first meeting, Allan agreed to read and discuss with me my undergraduate honors thesis on Aristotle's *De anima*. He encouraged me to consider a career in philosophy and served as an unofficial advisor all through my time in graduate school at the University of Toronto. I had a strong interest in the philosophy of biology as well as in ancient philosophy and was able to combine those interests by writing a dissertation on the relationship between the concepts of matter, form, potentiality, and actuality in Aristotle's *Metaphysics* and the use of those concepts in the biological works. Thus, early on, Allan and I became jointly engaged in the project of trying to understand the ways in which a study of the biological works could inform a study of Aristotle's philosophy. During a visit to the Princeton Classical Philosophy Colloquium in 1976 (on Aristotle's biology, with lectures by Montgomery Furth, Marjorie Grene, and David Balme), Allan introduced me to David Balme, at the time a Fellow at the Institute for Advanced Study. With encouragement from Allan, I recruited Balme to be the external reader of my dissertation. From that point on, Allan and I corresponded constantly, commented on drafts of each others' papers, and often attended the same conferences. In 1982, we spent a good part of the summer together as participants in an NEH Summer Seminar on the Philosophy of Biology at Cornell University organized by Dick Burian and Marjorie Grene—and of course the following summer was the Williamstown conference organized by Allan, mentioned above.

The following year, I was a Junior Fellow at the Center for Hellenic Studies, where Allan and I read through the entirety of the *Historia animalium*, meeting regularly all through the Fall term to discuss key passages in each book. It was at that time that Allan invited me to contribute to the Balme Festschrift and to co-edit *Philosophical Issues in Aristotle's Biology*; and of course, I attended all the workshops and conferences in which Allan was involved in the 1980s and 1990s. We completed the editing of *PIAB* (as we came to call it) in the Spring of 1987, while I was a Fellow at Clare Hall—in fact, Allan, my daughter, and I resided together in an apartment that backed on Fenner's Lawn, the Cricket Pitch in Cambridge. During those years Allan introduced me to Pen-y-Gwryd. One such visit, in 1985, occurred during a tour of Wales with Allan and Cynthia Freeland. Our next joint venture in Great Britain was in 1994 when, at the invitation of David Charles, Allan and

I co-taught a graduate seminar with Charles at Oxford, once more under the title 'Philosophical Issues in Aristotle's Biology'.

It was during these years of working intensely with Allan on many different projects that I came to appreciate one of his defining character traits—his ability to keep large, long range goals in view while simultaneously focusing with great intensity on every detail that was necessary to achieving those goals. This aspect of his character was a constant, whether he was working on a philosophical problem, doing historical research, planning a seminar, editing a paper or book for publication, or planning a trip. I sometimes found working on projects with him exhausting but in the end always wonderfully rewarding.

In 2002, while the founder and then director of the Anthem Foundation, John McCaskey, was visiting in the Department of History and Philosophy of Science in Pittsburgh and working on a dissertation on the history of induction at Stanford, we discussed the idea of creating a fellowship for Allan to be held in my department at the University of Pittsburgh. As a result, from 2003–2012, Allan was a Visiting Professor of HPS and Anthem Fellow for the Study of Objectivism. As noted above, this gave Allan and me the opportunity to collaborate in a variety of ways. A wonderful case of Aristotelian τυχή occurred when one of Allan's best undergraduates during his last years at TCNJ, Greg Salmieri, was accepted into graduate school by Pittsburgh's Philosophy Department in 2001 and Allan became an Anthem Fellow at the University of Pittsburgh in 2003. Thanks to his Fellowship, Allan and I thus shared the unexpected pleasure of serving on Salmieri's dissertation committee.

At some point our relationship evolved from Allan being a teacher and mentor to a colleague and friend and, as I liked to say, co-conspirator. What never changed was Allan's benevolent spirit, which showed itself in many ways both great and small, from the many ways in which he encouraged and supported my work professionally to the unexpected postcards or gifts that would arrive in the mail.

The only other person with whom I have had as close and continuous a relationship is the person to whom I am married. The loss of someone who has been a close friend for most of your adult life creates a void that cannot be filled, of course. In a very real sense, however, Allan is still here—in his publications, in his lasting impact on our profession, in the memories of his friends and colleagues, and through the inspirational influence on students during more than four decades of teaching.

Teone di Smirne. Expositio rerum mathematicarum ad legendum Platonem utilium. Introduzione, traduzione, commento by Federico M. Petrucci

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Editorial and academic rules are a bit different on either side of the Alps and that is fortunate for researchers and scholars. Here is Theon of Smyrna smartly propelled into the foreground, thanks to a 27-year old researcher, Federico Petrucci, by the way of his Italian translation with commentary following the current text *antiquo modo*. Exhaustive research underpins references, parallelisms, contrasts, and alternative or divergent ways of interpretation: meticulous footnotes in the introduction and commentary draw just as easily on the Arabic tradition and medieval literature as on late Antiquity or modern authors. Synoptic tables [46–49, 52–53] on musical and astronomical topics, then arithmetic [542–552], and again astronomy [553–555], are precious tools showing how easily the author can read and mine ancient texts for answers.

The book consists of 609 pages printed in small and rather squeezed characters with narrow margins. In a strictly alphabetical order, the bibliography (18 pp.) displays together editions of ancient texts, books with general contents, papers on special issues, in all the 'topics concerned'. There is an index of ancient sources (20 pp.), an analytical index (13 pp.) where one can find: both general and technical terms and among them ancient authors' names (*sic*) with differentiated references to the translation or to the commentary, and lastly a brief list of Greek terms (2 pp.), i.e., a limited selection of concepts, sometimes without the translations used in the work.

The introductory presentation (53 pp.), which is very condensed, is followed by philological notes on the text and translation (38 pp.) that are argued in the same scrupulous way in order to justify or to refute the emendations put

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forward in previous editions and translations.[1] The Greek text (96 pp.) is in fact given as an undivided, continuous whole which includes the emendations that have been accepted as well as the diagrams from Hiller's Teubner edition [1878]. This text is not lineated anew: the page and line numbers (marked in fives) of Hiller's edition alone are given in the right margins. The Italian translation (86 pp.) is given too as a continuous whole without any title or even blank spaces inserted to articulate the sections of Theon's treatise (the introduction and the accounts of arithmetic, music, and astronomy). Hiller's page and line numbers (marked in tens) are given in brackets within the translation itself.

Every heading of the commentary (229 pp.) thus begins with the page and line numbers of Hiller's edition, so that it compels you to refer to the text and the corresponding translation: bookmarks or post-its required! Nevertheless, in the commentary, four subtitles indicate the sections of the Theon's book; and some underlined page and line numbers at the beginning of some paragraphs indicate (but not always) the great divisions proposed in introductory presentation: I [19], II [21–22], III [26], IV [31].

Three appendices follow and supplement the commentary. The last (5 pp.) concerns Theon's traditional mathematical style and wonders about some inaccuracies; the second (6 pp.) focuses on the quotation of Plato's myth of Er in order to show that many variants fall outside the scope of the 'Art of Misquotation' and must have an 'ideological' reason that is in keeping with the goal of 'Platonem ex Platone σαφηνίζειν' [536]. And the first (16 pp.) studies in detail the parallels to Calcidius' commentary—without any mention of Béatrice Bakhouche's works [2012]—and how Theon uses Adrastus of Aphrodisias' *Commentary on Plato's* Timaeus. Petrucci here adopts the same procedure as in his commentary, 'following the current text', and refers at last to one of his papers that is 'more complete' on this topic [530n34]. The complexity of the arguments and the thickness of their presentation probably explain why, in his translation, he gave up printing quotations of Adrastus in italics as he does for those of the other authors or forgoes marking them off with brackets following Delattre Biencourt 2010. As a result, there are occasional missteps in the commentary when Petrucci ascribes to Theon arguments explicitly imputed to Adrastus and goes so far

[1] E.g., Boulliau 1644, de Gelder 1827, Martin 1849, Hiller 1878, Dupuis 1892, Smyly 1907, Tannery 1912, Barker 1989, and even Delattre Biencourt 2010.

as to accuse Theon of discrepancy when Theon actually keeps himself aloof from the quotation just given.

However, as the author explains most pertinently, one of the main aims of Theon's astronomical writing is to help the reader to perceive what differentiates the pattern of Platonic theorizing in astronomy that he promotes (viz., that the planetary motion, both direct and retrograde, are true motions in accordance with the rotation of the circle of the Other, itself opposite to that of the Same) from Adrastus' Peripatetic view (viz., that the planetary motions which we observe are apparent motions, a φαντασία [see Delattre and Delattre 2006] or φαινόμενον to be accounted for), though Theon nevertheless uses Adrastus' account to a great extent.

The scrupulously traditional tone of this remarkable academic book will on occasion astonish or even disappoint the curious or interested reader. For example, the choice to preserve the Latin terms (which Dupuis used in his translation at the end of the 19th century) for the fractions of 'one and one part more' (as Luc Brisson [1992] translated «ἐπιμόριος») for both the intervals of tone (9:8) and the fifth (3:2) casts the text in an old fashioned style. Why not use the Greek terms, as he uses 'epitrite' for the interval of fourth (4:3) and 'epimere' for the fraction of 'one and several parts more'?

Similarly old-fashioned is the choice to preserve the Latin title of Hiller's edition without translating it on the first page—though the cover suggests Greek in that it is illustrated with an image of a splendid fragment from the frieze of the Parthenon in Athens—and of designating Theon's treatise in the following pages by 'Expositio' but no more. The author gives us the Greek text of the titles in manuscripts A and B only [9n2], and stays silent about the possibility that the title translated by 'Expositio' ('Presentation') might come from reading «χρησίμα» instead of «χρησίμων» in B [see Macadam 1969].

But the main subject of 'Platonic ire', as Amyot said in translating Plutarch, is due to the dismissal of alternative interpretations of Theon's harmonic and astronomic theories [see, e.g., Delattre 1998, Delattre and Delattre 2003] as 'completely inopportune' and labeling them 'in chiave mecanica' [43] without any kind of debate or trial.

Petrucci 's commentary goes on as if there were *no Platonic spheres* built in order to support repeated investigations about the directions (front or back) of the direct and contrary movements which might be the most appropriate to account for the observed appearances, and which might, at the same time, help research into the points at which the whole system is stabilized. He reads the musical section as though there was *no Pythagorean musical canon graduated* in order to allow sliding the bridge above or under the string, thus making it possible to complete the intervals between fixed notes with adjusted, moveable ones. Moreover, according to Petrucci, the arithmetical section needs *no table of numbers at all* which might permit direct reading [see Bertier 1978, 40ff] of the consonances, their multiples, and above all the harmonic intervals squaring with them. Petrucci manages to comment on the 'plainly arithmo-geometrical' nature of Theon's arithmetic [332 ff.] without any reference to Maurice Caveing's works [see 1997 on arithmo-geometry]. And he even draws attention to the risk of 'superinterpretation of the book' [336n139] that would claim for the Middle Platonist a deeper knowledge of the implications (and of the technical uses) of the matter dealt with. Petrucci does prefer a 'light reading'!

So we should not lay great stress on the light and surreptitious translation of the recurrent phrase in Theon's astronomical section, «κατὰ συμβεβηκός», once more translated literally by the Latin phrase 'per accidens' as medieval astronomy passed it on, without taking any notice of Martin's detailed note on this subject.[2] To pass over this last difficulty in complete silence cannot be 'accidental'! However, it produces a bit of wavering in the comments again: thus, on page 483, the author admits that

> the pattern of the eccentric circles would follow the pattern according to the epicycles *per accidente* (that is, would be necessarily implied by it)

but on page 496, he only recalls the demonstration (as a partial one) of 'the accidental coincidence of the eccentric pattern with the epicycle's one' and nothing else.

The very judicious choice of displaying in a comparative tabular form the main objects of research or ζητήματα (in music and in astronomy) according to their order of appearance in Plato's *Timaeus* and in the pages of the *Expositio*, with their 'technical' and 'exegetical' treatments face to face, underscores two other evasions. First of all, the way in which Theon tackles the different points is far from following the order in the *Timaeus* and

[2] See Note X on ch. 22 [Martin 1849, 368–370], which is translated into French in Delattre Biencourt 2010, 405–408.

the differences fit in exactly with the rewriting and the resetting of Plato's discourse by the Middle Platonist commentator, a fact which the author does not appreciate sufficiently because of his comments 'along the current text'. Then, the systematic use of the series of ζητήματα appears as a matter of fact to reorder Theon's speech in a more traditional way at the risk of overshadowing his originality. There is no doubt, of course, that Theon explicitly draws on a widely exploited tradition and on many diverse technical sources (Neopythagorean and Eratosthenian in arithmetic and harmonics, Hipparchan and Eudoxan in astronomy). But is there no chance that Theon's practice of Middle Platonic exegesis [see Dillon 1977]—while carefully taking a middle course between the commentary of Adrastus the Peripatetic, which he quotes and criticizes at greater length, and that of Dercyllides the Platonist, whom he refers to in a more expeditious yet also critical way—might claim some measure of originality? According to the author [60n178], it is a double feature of Middle Platonic exegesis that it takes up both 'the extension of the exegetical reformulation to the philosophical consequences of the Platonic writings' and 'the possible widening (without fear of falsification) that attributes to Plato the whole corpus of technical knowledge', which actually allows one to reaffirm 'paradoxically' Plato's authority [61].

On one side, the 'extension to the consequences' is rather easy to perceive, for example, in the treatment of Theon's long quotations of Plato with the metaphor of the dyers as well as in the parallel of learning the scientific disciplines and the stages of initiation into the Mysteries. On the other side, the 'falsifying widening' is far from being so evident. The most blatant example of astronomical error and hotchpotch perpetrated by Theon is most likely his effort to tell apart 'with more accuracy' [Hiller 1878, 172.22] three 'very close' measures of the periodic return of the Sun to the same point, a return that 'most of the mathematician astronomers look upon as equal to 365 days and a quarter'. The commentators ramble on about this so as to prove Theon's scientific incompetence or the 'low level' of competence in his sources. Petrucci examines all that with the greatest care [485–486nn587–588]. But why does he here confine himself to only the technical arguments instead of wondering which 'exegetic reformulation' this error might correspond to? It is hardly conceivable that these different periods for the return of the Sun to the same point fit in with the diverse astronomical appearances of its motion, nowadays better known and precisely measurable, because, in fact, the values of 365 ½ and 365 ⅛ days are more likely different values in the measure of one and the same *phenomenon*, a fact which is at the heart of the reform—still recent for Theon—of the Roman calendar. Actually, these narrow differences afford to the Middle Platonist commentator the occasion to introduce strange solar periods of two, four and eight years (2, $4 = 2^2$, $8 = 2^3$), periods that are plainly 'theoretical' at this point, so as to link them and the Sun's motion in longitude along with its 'theoretical' motions in latitude and depth 'in a more accurate way', that is to say, to link them in accordance with the progression of numbers in the right branch or first Platonic *tetraktys* (1, 2, 2^2, 2^3) of the 'Λ of seven numbers' used by Plato and all his followers to organize and calculate the fundamental harmony of the world's soul.[3] It is not so clear whether this 'theoretical reformulation' of these three different measures of the solar year in a strictly Platonic speech was only a 'falsifying widening'. But it is true that it intended to reaffirm Plato's authority 'paradoxically'.

It is not useful to go on through the other detailed comments. We must recognize unhesitatingly the meticulous quality of this significant publication, which has been brought to completion so very quickly, and the undeniable utility of this rich and well-documented, academic work provided by a young and quite learned researcher for all the connoisseurs—mathematicians, musicians, astronomers, philologists, or philosophers—of the Middle Platonist Theon of Smyrna.

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[3] The second Platonic *tetraktys* is 1, 3, 3^2, 3^3.

[4] Arithmetic and music only.

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