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Ancient Rhetoric and Greek Mathematics: A Response to a Modern Historiographical Dilemma

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To the memory of three days in the Negev

Argument

In this article, I compare Sabetai Unguru's and Wilbur Knorr's views on the historiography of ancient Greek mathematics. Although they share the same concern for avoiding anachronisms, they take very different stands on the role mathematical readings should have in the interpretation of ancient mathematics. While Unguru refuses any intrusion of mathematical practice into history, Knorr believes this practice to be a key tool for understanding the ancient tradition of geometry. Thus modern historians have to find their way between these opposing views while avoiding an unsatisfactory compromise. One approach to this, I propose, is to take ancient rhetoric into account. I illustrate this proposal by showing how rhetorical categories can help us to analyze mathematical texts. I finally show that such an approach accommodates Knorr's concern about ancient mathematical practice as well as the standards for modern historical research set by Unguru 25 years ago.

Introduction

The title of the present paper indicates that this work concerns the relationship between ancient rhetoric and ancient Greek mathematics. Such a title obviously raises a simple question: Is there such a relationship? The usual appreciation of ancient science and philosophy is at odds with such an idea. This appreciation is rooted in the pregnant categorization that ranks rhetoric and science at very different levels. While the philosophical tradition has criticized rhetoric as it was initially practiced (by fifth-century BC Sophists in particular), it has attributed to mathematics a close affinity to science in general. As far as sophistry and rhetoric are concerned, the *locus classicus* is Plato's *Gorgias*, in which the initial discussion with Gorgias brings Socrates to make a sharp distinction between mere cookery (sophistry) and the true knowledge of the art of speaking. As for mathematics, it appears in the *Republic* as an intermediary stage necessary in the education of the best rulers of the City, again by opposition to the practitioners of a debased political oratory trained by the Sophists. These initial

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distinctions were further elaborated, particularly by Aristotle, and became usual standards in modern historiography.[1] Nevertheless, I want to argue that more recent trends in the history of Greek mathematics point, on the contrary, toward such a relationship, and my purpose is to explain *why* and *how*.

The idea of bringing together mathematics and rhetoric is not foreign to historians of pre-modern mathematics. Indeed, Giovanna Cifoletti's recent studies on sixteenth-century French mathematicians have shown that the rise of algebra as a full discipline included in the mathematical curriculum during this period is deeply correlated to the way in which knowledge was reorganized in the frame of the humanist *Encyclopaedia*.[2] Since the "rediscovery" of classical rhetoric was crucial to this reorganization, it appears that the fundamental shift that occurred in mathematics with the introduction of symbolic algebra is correlated to the no less fundamental shift that occurred in the way rhetorical concepts were taken up in order to reconsider the general organization of knowledge. This shift included a profound criticism of certain Aristotelian distinctions between scientific reasoning and dialectic, notably by Peletier and Ramus. In particular, the understanding of mathematical reasoning through rhetoric, which may seem surprising to us, had actually become a natural idea at the end of the sixteenth century (see Cifoletti 1992, introd. and ch. 5).

The development of late sixteenth-century mathematics also explains why so much attention was given to Greek texts that spoke about *problems* and *analysis*, namely the texts of Pappus, Diophantus, and Proclus. Indeed, *problem*, with the traditional meaning it had from medieval algebra, was progressively understood through, and finally confounded with, the rhetorical notion of *quaestio*. As for the Greek notion of *analysis*, the way it was understood in the sixteenth century was dependent partly on the aforementioned texts, and partly on the way it was progressively confounded with the rhetorical *ars inveniendi* and with symbolic algebra.[3]

Consequently, the study of this intricate process logically requires that we deepen our historical understanding of what ancient Greek *analysis* was in order to delineate the innovations and modifications that have created the modern understanding of it.[4] In the endeavor to understand the nature of ancient analysis, there can be no question

[1] Those views are reflected in Marrou's famous history of education in Antiquity (Marrou 1948), in which a sharp (but artificial) contrast is made between "Platonic" and "Isocratean" education.

[2] By the sixteenth century, this word had kept its original Greek meaning of *enkuklos paideia*, the "general" or "common" knowledge on which basic education (*paideia*) is rooted (for Cifoletti's studies, see Cifoletti 1992 and Cifoletti 1995).

[3] This confusion appears most clearly at the end of this period, in Viète's *Introduction in the art of analysis* (1591).

[4] Such was probably the purpose of Michael Mahoney when he wrote his famous article about ancient Greek analysis (Mahoney 1968). He had to understand ancient analysis in order to clarify the nature of the "reconstruction" of ancient Geometry undertaken by modern geometers like Pierre de Fermat. This was already Jacob Klein's project (Klein [1968] 1992, 154 ff.).

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of using modern algebra, as Sabetai Unguru convincingly has shown in his 1975 groundbreaking article (Unguru 1975). Nevertheless, the previously mentioned approach to sixteenth-century mathematics suggests another possible way to explore this issue: through the study of ancient rhetoric. Since the "rediscovery" of ancient analysis is correlated to the "rediscovery" of ancient rhetoric, we may ask whether both were correlated *from the outset*, i.e., in Antiquity. After all, ancient analysis, understood in a loose sense, is meant to provide a concrete way to invent solutions for problems. But invention (*heuresis*) is a well-known part of ancient rhetorical theory.

I myself have devoted a detailed study to this question in the context of Late Antiquity (Bernard 2003a). I have shown that a difficult passage of Pappus' *Mathematical Collection* can be better understood if one takes into account the techniques developed by orators and dialecticians that had become dominant in Late Antiquity. I am also preparing two other case studies: the first treats Eutocius' commentary on Archimedes; the second examines Proclus' commentary on Euclid.

My present purpose is not to dwell on these particular studies but rather to show how recent trends in the history of mathematics point toward ancient rhetoric. This objective parallels the general theme of the international workshop in Tel-Aviv in May 2001 organized by Leo Corry and Sabetai Unguru, which focused on recent trends in the history of mathematics.[5] In other words, I want to defend the view that whereas there has not been, to my knowledge at least, any explicit comparison made between ancient rhetoric and Greek mathematics other than mine, this comparison stands behind some dilemmas of modern historiography. In other words, I believe that modern historians have been turning around this idea for some years without necessarily knowing it.

In the first part of this study, I will discuss the nature of the "dilemma" to which I have alluded. The issue at stake is the role that has been given (or denied) to mathematical practice in historical interpretations of Greek mathematics. I will illustrate this point by comparing the opposing views of Unguru and Knorr on this subject. In the second part, I will focus on the specific details of the controversy in order to show that the main question is the nature of ancient *analysis*. This will afford me the opportunity to list certain important questions related to this issue. In the third and concluding part, I will more fully develop how the recourse to ancient rhetoric could partly "solve" the previously described dilemma. Specifically, I will emphasize the reasons that make the choice of rhetoric as a historical context for Greek mathematics relevant and important for modern historians.

[5] The general title was "History of Mathematics in the Last 25 Years: New Departures, New Questions, New Ideas."

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I

In a 1984 article on the recent history of Greek mathematics, Len Berggren remarks that interest in this field lies in the fact that "it is an area where there is still controversy over some of the main features and where issues of considerable historical importance are still unsettled" (Berggren 1984, 395).

This judgment has been true until now. Indeed, there are still debates about the correct understanding of quite basic features of Greek mathematics such as its synthetic or analytical character. The Tel-Aviv workshop actually reflected this atmosphere since many talks explicitly referred to Unguru's strong contribution to this debate – specifically, the 1975 article *On the Need to Rewrite the History of Greek Mathematics* (Unguru 1975). More recently, Michael Fried and Sabetai Unguru have published the important study they were preparing on Apollonius' *Conics* (Fried and Unguru 2001). The book presents itself as an explicit illustration of the 1975 thesis and eventuates from other articles Unguru has published in the past 25 years. It also presents itself as the counterpart of Zeuthen's famous book about Apollonius' work (Zeuthen 1886).[6]

Let us summarize Unguru's statements about the way most of the history of Greek mathematics has been written. He argues that it has been spoiled, so to speak, by an abusive and illegitimate use of algebra or algebraic transcriptions of Greek mathematical works. Following Jacob Klein and Michael Mahoney (among others), Unguru says that the use of algebra in historical interpretations is not just a matter of external *form* that would leave the *content* of what is interpreted untouched. It also entails a basic misunderstanding of ancient methods because modern notation and conceptualization inherently carry a modern "intentionality" which is foreign to the ancient thought. The notion of "intentionality" is borrowed from Klein who follows Husserl on this point. To illustrate this general view, Unguru directs heavy attacks against the concept of "geometrical algebra," first invented by Tannery and then adopted and extensively used by Zeuthen in his commentary on Apollonius. Unguru also argues in his study of Apollonius' *Conics* that this idea of geometrical algebra, although it appeared in historiography in the nineteenth century, has its roots in the works of sixteenth- and seventeenth-century algebraists (Fried and Unguru 2001, 20ff). The idea of an *art* that the Greeks would have used but concealed in cloudy formulations leads us back at least to Descartes' *Regulæ*.[7] Unguru actually goes so far as to generally deny the legitimacy of any kind of mathematical approach to the *truly* historical study of ancient Greek mathematics. Mathematics and historical accounts are for him completely incompatible and he explains how so:

[6] "The purpose of this book, then, is to present a new interpretation of the *Conica*, an alternative to Zeuthen's reading of the text" (Fried and Unguru 2001, 1).

[7] It actually leads us back to Vieta. Cf. Klein's close scrutiny of this idea in (Klein [1968] 1992, 153, 170, 272, n.245).

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> Faced with an ancient mathematical text, the modern interpreter has an initial choice. First is the mathematical approach. It consists of two steps: (1) try to find out how s/he would do it (solve the problem, prove the proposition, perform the construction, etc.) and then (2) attempt to understand the ancient procedure in light of his/her answer to step (1). Instead of this pre-eminently mathematical approach, however, the modern interpreter can refuse to decipher the text by appealing to modern methods, using for its understanding only ancient methods available to the text's author. This is the historical approach. Needless to repeat, the spoils of interpretation differ according to the two approaches followed. (Fried and Unguru 2001, 406)[8]

Let us contrast these views with those of Wilbur Knorr. Knorr is another major historian of Greek mathematics. Not only has he made an impressively large contribution in the past decades to the field of the history of Greek mathematics, he has also repeatedly proposed new and enlightening views about the chronology and the very nature of the Greek mathematical tradition. One obvious example can be found in his study on the evolution of Euclid's *Elements* (Knorr 1975). Another can be seen in the emphasis he has put on the study of the late commentators for themselves (Knorr 1989, first chapters). The interesting fact about Knorr's methodology is that his basic belief about Greek mathematics is diametrically opposed to Unguru's. Namely, Knorr has taken up Viète's and Descartes' idea of a *hidden art* by which the Ancients made their discoveries, although the synthetic mode of exposition that is found most often in the texts testifies only indirectly about what this way of discovery was. This idea has become the major concept on which Knorr has built his principal work on the ancient tradition of geometric problems (Knorr [1986] 1993), as can be seen from the following passage taken from the first chapter:

> [T]he essential mathematical aspects of the different technical contributions will provide the principal instrument for revealing their historical relations. A difficulty here is that the ancient writers (especially those in the later editorial tradition) preferred the synthetic mode of exposition in their formal treatments of geometry . . . . To mathematicians of the seventeenth century, indeed, even to some of the ancients, this format was notorious for obscuring the essential line of thought . . . . But when the discovery of solutions for geometric problems was at issue, the ancients exploited an alternative method: that of *analysis* . . . . [T]he analysis of a problem exposes in a natural and well motivated way the rationale behind each step of the construction. By contrast, when only the synthesis is given, these steps often appear arbitrary and without clear motivation until much later in the proof. Thus, in my accounts I will prefer analytic presentations to synthetic ones. In cases where only the synthesis is extant, I will fashion the analysis corresponding to it . . . . My intent is not to paraphrase or otherwise reproduce what is already available in the extant primary literature, but rather to bring forward the essential, usually simple, geometric idea underlying each result and thus to

[8] The quotation goes on: "The longstanding traditional approach has been the mathematical and we have encountered in this book numerous instances of it. Zeuthen is one such illustrious instance. Toomer is another. This is obvious when reading Apollonius and Toomer on Apollonius side by side, as his edition *cum* translation makes it easily possible."

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> provide an appropriate introduction for further investigation of that literature. (Ibid., 9; Knorr's emphasis)

Thus, just where Unguru sees the ultimate foundation of the special kind of anachronism made by most modern historians of mathematics up to the twentieth century, Knorr sees the best foundation of a faithful account of the Greek mathematical tradition. Accordingly, just as Unguru would not hesitate to dismiss most modern interpretations inspired by modern mathematics as the perfect antinomy of a historical study in the proper sense, Knorr, in his most polemical passages, does not hesitate to dismiss most modern historians like Heath, Tannery, or Szabó because they are not truly mathematicians but *only* philologists. Therefore, Knorr claims, they do not have even the beginnings of a true understanding of the mathematical tradition precisely because they do not *belong* to it (ibid., 87f).

It becomes obvious from this set of quotations that confronted with such opposite views, modern historians of mathematics face a true dilemma: should they use their own mathematical practice as a key tool for the understanding of ancient mathematical texts, or should they avoid it, on the contrary, as the worst (historical) sin they could commit?

In exploring this dilemma, I will refrain from resorting to any modern historians other than Unguru and Knorr. There are three reasons for this. First of all, Unguru and Knorr represent the extreme ends of the historiographical landscape concerning Greek mathematics and are therefore in some way representative of it. Secondly, they express their methodological claims very clearly. Lastly, I myself have been strongly influenced and inspired by both. One could object that focusing as I do on extreme and polemical positions implies getting involved in a tiring, old-fashioned, and therefore unproductive fray. But I cannot agree with this because if we leave aside the most polemical overtones of the conflicts that Knorr's and Unguru's positions have entailed, we must insist that there still remains a *real* dilemma. The polemic is not the core, but rather the most external manifestation of a deeper and real historical problem. Indeed, Unguru and Knorr both have successfully demonstrated, at least in my opinion, that they have made valuable contributions to the historical study of Greek mathematics. Their respective points of view on Greek mathematics are both *possible* and *enlightening* points of view. On the other hand, these views clearly seem incompatible. My view is that the study of rhetoric in Antiquity provides some possible keys to solve this dilemma in a way that does not seem like an unsatisfactory compromise. To understand this, we have to enter a little further into Knorr's and Unguru's arguments.

II

Taken on a large scale, the picture I have described above is faithful. Unguru dismisses any mathematical reading in historical studies as *historically* baseless since, he says, it is

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from the outset a source of anachronism. Knorr, on the contrary, rejects any attempt to avoid a *mathematical* involvement in historical interpretations of mathematical texts, for he deems the essential lines of development of Greek geometry must be judged *from the inside*.

Let us enter into the details of this controversy. The target of Unguru's attacks centers on the question of algebra. The reason for this rejection is that, for Unguru, algebra and algebraic methods stand at the core of modern mathematical thinking.[9] But on this point, Knorr clearly shares the same feelings as Unguru since he carefully avoids any interpretation that cannot be put into Greek *form*. Furthermore, he seeks a thorough correspondence between his reconstructed analyses and the actual synthetic presentation that is most often found in the texts. We have seen, for example, that he speaks in his book about the ancient traditions of geometric problems (ibid.), about "the essential, usually simple *geometrical* idea underlying each result."[10] To take an example from the thousands that can be found in Knorr's work and especially in Knorr 1989, here is what Knorr writes in the introduction to his commentary on the beginning of the Third Book of Pappus' *Collection*:

> [M]odern scholars have judged the method [commented on by Pappus] to have merit, not as an actual construction of the solution, but as an approximation procedure. Their discussion invariably adopts an algebraic mode; rather than paraphrase them, I will present a geometric version that may better capture the heuristic base of the method. The key, in my conception of it, lies in its affiliation with the method of Eratosthenes. (Knorr 1989, 64)[11]

This passage testifies that Knorr is at least sensitive to the objections to algebraic interpretations raised by Unguru. In this case, Knorr explicitly refuses to resort to algebra to build his commentary on the construction, even if in the criticism he makes about Pappus' comments, he again resorts to some algebraic speculations (ibid. 79, n.12–13). In a way similar to Unguru, Knorr has distanced himself early on from Zeuthen's concept of "geometrical algebra," as the first chapter of his study of Euclid's *Elements* makes clear:

> While I shall continue to use the term "geometric algebra," I will never mean by it anything other than "Book-II-type-geometry." (Knorr 1975, 11)

In fact, Knorr accepts the use of Zeuthen's expression but with a much more restricted meaning. Thus, Knorr does not go as far as Unguru in his criticism of

[9] In this respect, he follows the analyses of Jacob Klein and Edmund Husserl before him, as we have said before.
[10] Cf. (Knorr 1986, 9) quoted above (emphasis added).
[11] This is the passage I have commented upon in my own study on Pappus (Bernard 2003a).

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Zeuthen's concept, but he nevertheless explains that neither Book II nor Book X of the *Elements* can be viewed as algebraic treatises (ibid., 10–11).

In summary, while taking into consideration the non-algebraic *form* to be given to his speculations, Knorr allows himself to propose reconstructed analyses (in the Greek sense) in order to show the way in which mathematical results were *found*, not just demonstrated, by Greek mathematicians. At the same time, he carefully avoids, as much as he can, giving any algebraic expression to these speculations. Now, insofar as he makes such a reconstruction, he is to some extent in line with the Cartesian idea that the Ancients must have had some kind of very simple art by which they found their results. Here, he strays from Viète and Descartes, who share the belief that the Ancients hid this art afterwards within cloudy formulations. The following quotation is the famous passage in which Descartes, speaking about the "true mathematics" of which we find remains in Pappus and Diophantus, explains why the Ancients hid their art by a certain injurious cunning (*pernicioso quadam astutia*):

> For they perhaps feared, just as many inventors have been found to have done in the case of their discoveries, that because the true mathematics was *easy and simple* it would become cheapened in becoming popularised, and they preferred to exhibit to us in its place as the results of *their art* certain sterile truths, very acutely demonstrated by deduction, so that we might admire those, instead of *teaching us the art itself*, which would have quite subverted our admiration.[12]

It seems to me hard to deny that Knorr makes a valid point here since the idea of a different, yet correlated, way to that of demonstration is just the point that Archimedes makes in his famous *Ephodos* that Heiberg retrieved in a palimpsest. Archimedes speaks about a *tropos*, a way of doing things, by which you can eventually *find* certain results but not *demonstrate* them – although this *tropos* may also appear useful for demonstrations:

> [C]ertain things first appeared to me mechanically and had to be demonstrated by geometry afterwards because their investigation by the same way [*tropos*] is without demonstration. For it is easier, when we have previously acquired, by this way [*tropos*], some knowledge [*gnôsin*] of the questions, to supply the demonstration than it is to find it without any previous knowledge [*medenos egnosmenou*]. This is a reason why, in the case of the theorems that the volume of a cone and a pyramid are one-third of the volumes of the cylinder and prism respectively having the same base and equal height, the proofs of which Eudoxus was the first to discover, no small share of the credit should be given to Democritus, who was the first to state the fact though without proof.[13]

[12] Rule IV AT X 376, Klein's translation (Klein [1968] 1992, 272). Emphasis added on the ideas "adopted" by Knorr.

[13] Translation Heath revised. The Greek text is based on Heiberg's partly conjectured version of the text. Since the palimpsest is now being examined with modern techniques by Reviel Netz and others (Netz et al. 2001/2), we should obtain in a short while a better reading of this section.

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In a little exchange I had with Unguru on this point, he wrote to me that "quite often, Wilbur Knorr's 'analysis' is a disguise for algebra" (personal communication). Here, he is applying to Knorr the exact argument he denied to other historians, namely that parts of Greek geometry are just "disguised algebra" or "algebra in geometrical garb" (Unguru 1975). To such a judgment, one can apply precisely the same criticism Unguru himself applied to the tenet of algebra disguised in Greek geometry: if we do not find any symbolic algebra in Knorr's reconstruction, but only an *analysis* in the form that could be expected from a Greek mathematician, how can we maintain that it is not simply what it seems to be? Or shall we permit the general axiom that *any* modern historian making use of mathematics is disguising in geometrical garb his or her basic thoughts which can be only algebraic by nature? This clearly cannot be the case, since the basic problems underlying Unguru's "suspicions" are more intricate. Jacob Klein summarizes them suggestively:

> It is clear, to be sure, that the feasibility of an interpretation not based on modern presuppositions must always be limited; even if we succeed in ridding ourselves completely of present-day scientific terminology, it remains immensely difficult to leave that medium of ordinary intentionality which corresponds to our way of thinking, a mode essentially established in the last four centuries. On the other hand, the ancient mode of thinking and conceiving is, after all, not totally "strange" or closed to us. Rather, the relation of our concepts to those of the ancients if oddly "ruptured" . . . we must always keep in mind the difference in the circumstances surrounding our own science and that of the Greeks. (Klein [1968] 1992, 118)[14]

This quotation highlights the fundamental and subtle idea that while Greek mathematics is not inaccessible to us, we are obviously and irremediably cut off from the "circumstances" that surrounded ancient science. Therefore, any access to ancient mathematics requires envisioning the *missing* circumstances in which it was created. Our access to it is, therefore, necessarily mediated through a partial historical reconstruction of these circumstances.

The debate concerning the ancient "way of discovery" in mathematics is obviously related to the historical questions surrounding ancient analysis. In this respect, an interesting touchstone is the series of modern commentaries made on Archimedes' famous 4th proposition of Book II of his treatise *Sphere and Cylinder*. It is the following proposition that is stated as a problem: *to cut a given sphere in two segments that have a given ratio to one another* (fig. 1).

This proposition is famous among historians of Greek mathematics because it belongs to one of the rare texts of *early* Hellenistic Antiquity in which one finds explicit and complex *analyses* of difficult problems like the one we have just cited. Therefore, it is usually chosen as the example *par excellence* of Greek ancient

[14] Quoted by Unguru and Rowe in Unguru 1981, 43–4.

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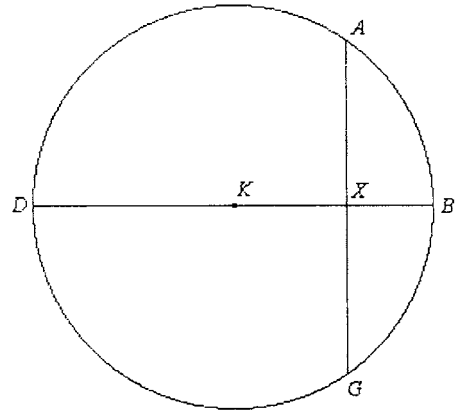


Fig. 1. Archimedes, *Sphere and Cylinder* II.4.

mathematical analysis.[15] For example, in his famous 1968 article about ancient analysis, M. Mahoney takes this passage as an illustration of what he calls "advanced analysis in use" (Mahoney 1968, 337 II.C).[16] Knorr also describes in detail the problem and its solution and compares it to the analyses of two other problems:[17] (1) a *neusis* used in the lemmas of his treatise about spirals and (2) another *neusis* used for the inscription of a regular heptagon in a circle – this second problem and its treatment are known only through an Arabic translation by Thabit ibn Qurra. These problems have interested him not only because of the technique common to both, as well as to the treatment of the problem by Archimedes or by his Greek and Arabic followers, but also because they illustrate his general thesis about the centrality of analysis in the ancient tradition of problem solving:

[15] The analysis of the problem is actually known to us through Eutocius' reconstitution of it on the basis of "an old text" (*Arch.* III 130.29) that he declares to have retrieved. In Archimedes' text as we have it, there is only an allusion to the solution of the problem and a reference to "the end [of the text]" (for a detailed analysis and synthesis of it, see *Arch.* I 192.5). But the end of the treatise does not actually contain them. The reconstitution of Eutocius is precisely meant to *prove* that the text he retrieved is indeed the missing end promised by Archimedes (*Arch.* III 150.23ff). Those questions are discussed, among others, in Reviel Netz's detailed study of an important part of this text (Netz 1999). Here I follow Netz in his judgment that the basis on which Eutocius worked out his reconstitution is indeed due to Archimedes, or at least to some competent mathematician contemporary to Archimedes (ibid., 24ff). Eutocius also quotes two other solutions of the above-mentioned problem by Dionysodorus and Diocles, both of whom belong to the early Hellenistic period (*Arch.* III 152.27ff).

[16] To summarize, Mahoney claims that analysis has usually been considered as a philosophical and/or logical problem, so little attention has been paid to it as a *historical* problem. Moreover, he insists that the logical interpretation of it (analysis as "reverse or backward inference") obscures the fact that to Greek mathematicians, it served as a "body of techniques" that was available to them for their problem-solving activity. Thus, Mahoney is naturally led to give concrete examples of how ancient analysis was *used* (and not just considered or presented). Hence, the aforementioned passage about Archimedes.

[17] Cf. the fifth chapter of his study about the tradition of geometrical problems (Knorr [1986] 1993), which is devoted to Archimedes' techniques ("Archimedes: the Perfect Eudoxean Geometer").

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> Archimedes' treatment of this problem is a nice example of how the process of generalizing stimulates the field of geometric studies by raising new questions for solution not posed in the special problems initially. (Knorr [1986] 1993, 173)

Finally, Archimedes' proposition is quoted by Unguru in his 1975 article (Unguru 1975) in the frame of his criticism of Mahoney's claim that the Ancients used "an informal, but subtle and penetrating, algebra of line lengths" in their analyses (Mahoney 1968, 337).[18] Unguru says, on the contrary, that in this passage Archimedes is much closer to Euclid than to any algebraist.[19] Unguru adds that we can at best speak about "a freer manipulation of lines" in this passage than in other ones in Greek geometry.

It is not surprising then to see that Knorr has inserted a long note (Knorr [1986] 1993, 203–4, n.94) after his reconstructed analysis of the construction of the rectangular heptagon.[20] This note is of course important for us because it is a rare case in which Knorr explains his position in the dispute initiated by Unguru. Knorr first says that it is indeed absurd to claim that there is any algebra in Archimedes or in "the ancient geometry" in general:

> [C]ertainly, the ancient geometry never had access to the special advantages afforded by algebraic notations and conceptions in the full modern sense. (Ibid.)

This quotation confirms the misgiving we have already encountered in Knorr's former works toward such concepts as "geometrical algebra." It also shows Knorr's acceptance of Unguru's (and Klein's) claim about the inseparability of form and content concerning the use of algebra in modern historical interpretations (Knorr indeed speaks about "notations and conceptions"). A few lines later, Knorr also accepts another key argument of Unguru's which is the false deduction that was and is often made when historians claim that because algebra can be successfully applied to ancient mathematics, the Ancients must have actually used some kind of algebra:

> [O]f course, it is in the strict sense fallacious to argue that these ancient techniques [the techniques of "auxiliary diagrams" in Knorr's description] *are* a form of algebra because they serve the same function of separating quantitative relations from their special contexts. (Ibid.; Knorr's emphasis)

Despite these two major concessions to Unguru, Knorr maintains that the term "geometrical algebra" remains useful in "alerting us to the fact" that, in some cases, diagrams fulfill a very special function in mathematical reasoning. Namely, they serve

[18] A few lines later, Mahoney speaks about "an algebra-like manipulation of line lengths."
[19] Cf. note 122 in Unguru 1975, in which these questions are discussed in detail.
[20] Knorr has finished the work Tropfke had begun before him (Knorr [1986] 1993, 178 and 203, n.90 to 93).

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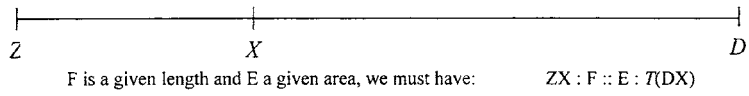


Fig. 2. The problem to which Sphere and Cylinder II.4 is reduced.

as auxiliaries to other propositions, and as such their interest lies in that they exhibit "quantitative relations" that can be used in "ostensibly unrelated diagrams" (ibid.).

For example, in the case of proposition II.4 in Archimedes' *Sphere and Cylinder*, the primary analysis of the problem leads Archimedes to a *new* problem which can be, and is, stated in a more general form than the one concerning the cutting of a sphere in a given proportion: the new problem is to cut a given length ZD at X so that the first segment ZX is to a given length F as a given area E is to the square on the other segment XD (fig. 2).

In this form, the problem is more general than the one corresponding to the cutting of the sphere, and in particular, it has in this form a *diorismos* that the initial one did not have: the cube on E and F must not be greater than the cube constructed on the third of ZD and the square on the two-thirds of it. The analysis of the problem is done through the intersection of two conic sections, a hyperbola and a parabola, and this technique can be, and actually was extended by Archimedes' followers to the other problems Knorr speaks about.[21]

Thus, if we dig into the detail of the controversy about what could or should be called the "algebra" of Greek mathematicians, we finally arrive at the question of what Mahoney calls the "*technique*" of analysis of Greek mathematicians. More generally, what was the exact nature of the *tropos* Archimedes speaks about, i.e., the way of discovering or "inventing" (*heurein*) things? Are we allowed to speculate about this, as Knorr extensively does? And specifically, are we allowed to *reconstruct* the analysis that would correspond to the extant synthesis? And if so, what kind of *rules*, in the sense of *rules of historical interpretation*, shall we follow in this endeavor?

Another thorny question that is hidden in these first problems is that of Classical versus Late Antiquity. It is a fact that most of the explicit testimonies about analysis in the extant mathematical literature must be traced back to *Late* Antiquity – with the notable exceptions of some of Archimedes' works and Euclid's *Data*. Eutocius, Pappus, the anonymous scholiast to *Elements XIII*, and Hero, speak about analysis, and much of what we know about the "treasury of analysis [*topos analuomenos*]," according to Pappus' expression, is known from these late sources. These authors obviously had a special interest in such questions. On the other hand, and beyond the aforementioned "notable exceptions," one can generally intuit that ancient analysis is deeply rooted in the early Hellenistic period.[22]

[21] A more detailed examination of this general problem is found in Netz 1999. I myself am preparing an analysis of this text from the point of view of ancient rhetoric.

[22] This actually is Knorr's basic hypothesis. Cf note 15.

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It is not surprising, then, that Unguru and Knorr take very opposite stands on this question. While Unguru insists that the scholium to XIII.1 was interpolated some 400 years at least after the classical period, Knorr, on the contrary, claims that the synthetic and formal mode of exposition was favored by writers of the "late editorial tradition" and suggests that the Ancients followed rather the analytic mode, and he explains why they may have done so:

> [T]his would doubtless owe to the appropriateness of exposing learners to the use of this method in preparation for their own research efforts later. It also has advantages for exegesis, seeing that the analysis of a problem exposes in a natural and well motivated way the rationale behind each step of the construction. (Knorr [1986] 1993, 9)

Therefore, Knorr adds, it was especially suited to "the purposes of geometrical research."

I myself have no clear answers to all the questions I have mentioned. While I must admit I share Knorr's belief that the techniques of analysis have their roots in Classical Antiquity, I cannot share his implicit idea that the synthetic mode of exposition is an offspring of the later editorial tradition, nor that the interest for analysis is an exclusive feature of the classical tradition, because the textual evidence obviously speaks against these ideas. In any case, we will not be able to solve these dilemmas as long as we lack a sufficiently coherent view of the *duality* represented by Classical "versus" Late Antiquity. The absence of a clear vision of the situation is reflected by the words themselves that we use, since the adjective "classical" itself would have no meaning if there had not been a period where the "classical" character *as such* of certain texts had not been slowly forged – and this period extends throughout the Hellenistic times at least. There is also this misleading notion of "decline" which has been imported from the political to the scientific historiography and which has to be further cleared.[23]

To conclude my commentary on the dilemma concerning the opposing views of Unguru and Knorr, I think that we have found the crossroad: it is the question of *analysis* and the historical methods that should be used to reveal the nature of the underlying techniques. At this point, the attacks delivered by Unguru against mathematical *technê* find their limits, but likewise his claim that we should not use modern algebra to explore this question must remain our guideline – all the more so since modern algebra was developed precisely for the purpose of clarifying the nature of ancient analysis. What, then, is the way to explore ancient analysis without indulging in anachronism? From another point of view, *which context* should be chosen to elucidate the way of invention of the Ancients? The following section should suggest some answers to these questions.

[23] This question is at the center of Netz's recent study on Archimedes and Eutocius (Netz 1999, 13, 43ff). He has generalized this approach by introducing the notion of *deuteronomic texts* (Netz 1998). I discuss this generalization in a forthcoming article (Bernard 2003b).

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III

To step out of the dilemmas described and analyzed in the previous sections, we have to return to the question of context. This question obviously lies at the core of Unguru's main claim: Unguru wants modern historians of mathematics to avoid any recourse to modern mathematics in their interpretations. According to him, they must use instead only "Greek" tools. Correspondingly, he urges them to set Greek mathematics back into its "own" cultural context. As Unguru himself points out (Unguru 1975, 78, n.21), scholars like Árpád Szabó and Jacob Klein, both of whom Unguru utilizes extensively, have already adopted this cautious attitude. Jacob Klein, for example, is a very subtle-minded and sharp analyst of Platonic, Peripatetic, and Neoplatonist philosophy. His study of Diophantus' arithmetical work, for example, is grounded on the basic hypothesis that this work has to be set back into the context of the history of Platonism. More precisely, he claims that Diophantus' work has to be seen as the "theoretical logistic" that is implicit in Plato and that later Neoplatonists could not adopt for reasons pertaining to their own philosophical bias. Thus, Diophantus would, so to speak, fill a gap in the philosophical landscape defined by Plato's philosophy. Thus, for Klein, philosophy and its history play essential roles in the history of science. This belief can be discerned in many passages, for example in the following one:

> Neoplatonic mathematics is governed by a fundamental distinction *which is, indeed, inherent in Greek science in general*, but is here more strongly formulated. According to this distinction, one branch of mathematics participates in the contemplation of that which is in no way subject to change . . . . The other branch of mathematics, on the other hand, has for its object the treatment and manipulation of size or counting. (Klein [1968] 1992, 10; emphasis added)[24]

Unguru alludes to this when he speaks about

> the character of ancient mathematics, in which philosophical presuppositions and metaphysical commitments played a much more fundamental and decisive role than they play in modern mathematics. (Unguru 1975, 86)

We find in both cases a fundamental assumption that is actually easily accepted: there is an organic relation between Greek philosophy and Greek mathematics. But Klein goes further and eventually sees mathematical texts as the *expression* of this or that philosophy. One clear example of this position is his analysis of the philosophical "shift" from the Platonic notion conceived by Aristotle on the question of the nature and measurement of numbers:

[24] This is the first sentence of ch. 2.

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> Aristotle's ontological view of *mathematika*, especially of the "pure" numbers, soon comes to influence the development of mathematical science itself . . . . The "arithmetical" books of Euclid (VII, VIII, IX) *directly mirror* this ontological transformation. (Klein [1968] 1992, 110–1; emphasis added)

It is again interesting to compare the previously quoted views to Knorr's since Knorr was profoundly opposed to Klein's and Szabó's perspective. This can already be witnessed in his study of Euclid's *Elements* (Knorr 1975, 3–4), and after the polemical exchanges he had during the period around the 1981 Pisa Conference, he made his position very clear in different texts. For example, in the introduction and first chapter of his 1986 text, he strongly refutes the widespread idea that one has to look for "motivations" or "explanations" for the development of Greek mathematics in Greek philosophy:

> This aspect of the commentators [i.e. their philosophical commitment to metamathematical issues] helps us to understand an emphasis commonly, but, I believe, mistakenly made in discussions about the ancient geometry. One seems typically to assume that metamathematical concerns were the effective motivating force underlying the efforts of geometers, for instance, that the hallmark of their tradition was their organization of geometric findings into tight structures of deductive reasoning, as if their primary ambition was the production of treatises like the *Elements* of Euclid and the *Conics* of Apollonius. But this surely cannot be correct. The writing of textbooks is the end of mathematical research only in the sense that death is the end of life: it is the last term in a sequence, but not the purposive element which explains why one engages in the activity of progressing through the steps in this sequence. (Knorr [1986] 1993, 7)

One can even say that Knorr has emphasized the notion of problem *precisely because* he wanted to show the internal dynamic of this problem-solving activity that finds its motivation in the process of mathematical research itself and not in any external factor. Many of his studies do actually reflect this state of mind since his efforts tend to make clear the technical coherence of the geometrical tradition, sometimes at the cost of unwarranted hypotheses. This motivation is clearly expressed at the end of the first chapter of his study on geometric problems:

> [T]he most exciting aspect of [the ancient textual material], in my view, is that a closer examination reveals the general lines of *a coherent development*, and that the extant record, despite its incomplete state, actually makes sense as the remnant of *an extraordinary movement in thought whose basic outline is discernible*. If the reader . . . comes to appreciate the unsatisfactory state of current scholarship and to perceive the possibility of achieving *a coherent view of this movement*, then my effort shall have attained its principal objective. (Ibid., 11–12; emphasis added)

When Knorr resorted to the notion of *problem* to create his own reconstruction of the ancient tradition of Greek geometry, he was not aware of the fact that he was

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actually resorting to a central concept of ancient rhetoric.[25] On the other hand, scholars like Klein or Szabó, who were looking for a possible context for Greek mathematics, resorted to philosophy as the *only* available possibility. They were also unaware, though in a different way, that there is at least *another* possible context – that of ancient rhetoric. I would even be maliciously tempted to argue – although such debates quickly become sterile – that resorting to rhetoric or sophistry, in general, is more natural and justified than resorting to philosophy. Let us take, for example, the very notion of *mathêma* or "learning matter" according to Klein's translation (Klein [1968] 1992, 51). This concept obviously stands at the very heart of Plato's speculations on what learning means: the *Meno* dialogue is the *locus classicus* in this respect. But the notion of *mathêmata* stands even more obviously at the center of sophistic and rhetorical *practice* – not just speculations. This fact is usually overlooked because of some pregnant misunderstandings about sophistry that are probably due to the heavy shadow of Platonism on our historiography. The main purpose of sophistic techniques was much less to "persuade others" than to *make others learn* – or, in Greek words, to build *mathemata* in a very concrete sense.[26]

Now, how can one concretely use the categories of ancient rhetorical practice in order to study ancient mathematical texts? To illustrate this point, I will come back to a case I have studied with much care: Pappus of Alexandria. Many historians tend to consider his *Collection* as characteristic of Late Antiquity, a period regarded as generally decadent. From this perspective, late antique authors are thought to have cared less for creative invention than for classification and inventory of the past "classical" texts.[27] Unfortunately, this view is at best partial and at worst a real misunderstanding of this period. Knorr had already acknowledged, although reluctantly, that Pappus demonstrates some sensitivity to ancient heuristic procedures (Knorr [1986] 1993, 360). But we should go much further: the period of Late Antiquity is characteristically *obsessed* with the idea of invention. The Latin word *inventio* is itself the translation of the Greek word *heuresis*. Let us reconsider the example of Pappus and the beginning of Book Three of his *Collection*.[28] What is it about? The pupil of Pandrosion, who was most probably a female competitor of Pappus (Jones 1986, 9, n.4), has suggested to the latter that he should examine an

[25] Knorr only refers to Aristotle's *Topics* as a possible context for the explanation of *problêmata* and *protaseis* in "debating contexts" (Knorr [1986] 1993, 374, n.60).

[26] One good example of misunderstanding entailed by the aforementioned Platonic view is the usual translation of the Greek word *meletê* into the Latin word *declamatio* (hence *declamation* in English). But *meletê* and *declamatio* are very different concepts, since *meletê* properly is the *task* that engages a speaker or sophist whose pupils have come to hear (*akouein*) speak. The emphasis is not put on the fact of "impressing" audiences that would be considered "passive lumps," according to M. Heath's clever remarks (Heath 1995, 5), but of displaying his skill as a speaker in front of an audience that is considered able to judge the performance and to *learn* something from it. Thus, the emphasis is put on *learning* rather than *persuasion* (Bernard 2003a, §1.2).

[27] For modern defenders of this point of view, see Wilbur Knorr (Knorr 1989, 225ff); Alexander Jones (Jones 1986, 1); Reviel Netz (Netz 1998). I discuss and contest it in a forthcoming article (Bernard 2003b). The following analysis of Pappus is excerpted from this discussion.

[28] This passage is more fully discussed in Knorr 1989, ch. 4; Cuomo 2000, 145ff.; and Bernard 2003a.

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original geometrical construction, which is meant to solve the classical problem of inserting two means proportional by "plane means." This construction is certainly new: Pappus, who is usually so eager to demonstrate the scope of his mathematical knowledge and even presents some solutions of the Delian problem as his own,[29] does not identify it as something known. Moreover, the criticism he sketches is precisely only sketched and again indicates that he is facing something new. We are thus in a context of research, to use a modern term, which nevertheless parallels the first words of Pappus' foreword:

> Those wishing to discriminate [*diakrinein*] more precisely [*technikôteron*] between the [things] sought in geometry, my dear Pandrosion. . . . (*Coll. Math.* I 30.3–4)

This context is additionally confirmed by the type of criticism that Pappus elaborates: it is not *apodictic* (demonstrative in the logical sense) but rather *epidictic* (demonstrative in the usual sense). The concrete concern that Pappus has in mind is indeed to show his own value as an "instructor" in the technique of analysis. He thus does not reach any conclusion in a strictly logical sense, but a "rhetorical conclusion," which is constituted in particular of new problems given in the form of lemmas (*Coll. Math.* I 48.19–52.30). Lastly, the effort made by Pandrosion's pupil to propose an original construction is mainly encouraged by Pappus. In general, Pappus displays many marks of respect toward his interlocutors. The first of them is to justify the very fact that Pandrosion's pupil has made a proposal. Pappus does it by referring to an ancient debate on the nature of mathematical investigations. It opposed those who held that such investigations were "problems" or "projections" to those who maintained that they were essentially "theoretic" or contemplative.[30] Pappus thus calls on the prestigious authority of (unnamed) Ancients to encourage the endeavor of his young competitor. But this is only an *ad hoc* erudite reference on the part of Pappus; the basic meaning of the word *problem*, as Pappus uses it in this passage, is rhetorical. It refers in general to a subject (*hypothesis*) of declamation cast (*proballomenon,* from *proballein*) by a public of listeners to the speaker, or more generally, set by circumstances, as when one has to defend a given case before a tribunal. In this particular case, it refers to the challenge set by Pandrosion's pupil to Pappus: to examine a construction, which the pupil claims solves the Delian problem.[31]

Now, is the construction proposed by Pandrosion's pupil an *original* one? Wilbur Knorr has shown, quite convincingly in my eyes, that it may be a clever variation on the construction of Eratosthenes' *mesolabe* or "mean-taker" (Knorr 1989, 64–68). Eratosthenes' device was well known in Pappus' time: this is attested in particular by the description Pappus gives of it a few pages later (*Coll. Math.* I 56.18–58.22).[32]

[29] See Cuomo's detailed discussion of this passage (Cuomo 2000, 145ff).
[30] We know the protagonists of this antique debate through Proclus' report (*In Eucl.* 77.15–78.13).
[31] For an extended discussion and justification of this interpretation, see Bernard 2003a.
[32] See also the more detailed account found in Eutocius, *Arch.* III 88–96.

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However, to conclude from this that the construction devised by the pupil of Pandrosion was not original would be a pure misunderstanding: the practice of invention, as it is known from ancient rhetoric, consists of building up an answer to a given challenge. The way to do it is to construct this answer, to the best of one's ability, on the basis of *topoi*, that is to say, of a kind of "treasury" of arguments taken from classical literature. It is probably this kind of "treasury" to which Pappus alludes at the beginning of the seventh Book of his *Collection*, as he speaks about the *analuomenos [topos]*, the "analytical topic."[33] This is one of the texts taken from this "treasury" that Pappus uses himself to initiate the pupil of Pandrosion to the technique of analysis. This is also a kind of treasury that Pappus probably intends to *constitute* when he lists, after his criticism, a series of solutions to the Delian problem. We moderns tend to have a dramatic notion, so to speak, of invention – all the more so since we most often lack the rhetorical training that would give it a concrete content. But in antiquity, invention is still conceived of and practiced most often as *a slight variation on a well-known basis of* topoi. As such, to some extent it can be compared with modern jazz music, according to a striking comparison proposed by Henri-Irénée Marrou (Marrou 1948, 275). The kind of construction proposed by the pupil of Pandrosion is therefore *perfectly inventive*, provided one conceives of invention in the very way Pappus' contemporaries understood and practiced it. In other words, there is a great disadvantage in not taking into account the (generally very subtle) categories elaborated by Greek rhetoricians in order to put into practice invention, which we now tend to relate to subjective "originality."

The passage of Pappus I have discussed is of course a very isolated case. Nevertheless, I hold that it reveals powerful means to interpret other Greek mathematical texts, not only belonging to the same period (Eutocius, Proclus) but also of earlier periods. In other words, I claim that this kind of reading has a high degree of generality and can thus be applied to many other texts, but I cannot fully discuss and justify this claim here since it requires more detailed case studies.

Let us then come back to the thorny question I have mentioned concerning Greek *analysis*. It seems fitting to me to explore the field of ancient rhetoric in order to get not only the objective *information* that is often lacking in solely *mathematical* texts, but also a relevant *point of view*. By "relevant," I mean, in perfect accordance to Unguru's criteria, "non anachronistic." As is well known, rhetorical practice is one of the most deep-seated phenomena pertaining to ancient Greek culture – and by that I am not only referring to something pregnant in the Greek and Roman ancient world, but also to something that lasted from the birth of ancient Greek civilization to the

[33] See Alexander Jones (Jones 1986, 83, line 3) and his commentary on *topos* (ibid., 378–9). Alexander Jones deems the usual translation "*treasury of analysis*" to be "*too coloured to render the bland Greek word*." From another point of view, the classical translation has for itself the authority of usage as familiar to Commandino or Newton as to those in Antiquity, in which it belonged to the traditional vocabulary of rhetoric. Newton, in one letter to David Gregory (May 1684) deems indeed *locus resolutus* (i.e., Commandino's translation) to be equivalent to *penus Analytica* or "*treasury of analysis*."

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expiration of Greek pagan culture.[34] As a cultural fact, ancient rhetoric actually relates the notions of *agôn* and *agonism* that pervade all Greek culture (Lloyd [1996] 1999). In short, we hold here a huge corpus of texts which has been left unexplored till now by historians of mathematics, but which could nevertheless clear up many intricate debates about the way of discovery of the Ancients. Therefore, the study of ancient rhetoric is relevant to the aforementioned dilemma.

I must insist on an important point here. To make it clear, let us begin with a simple question: what is the *core* of ancient rhetorical practice? The first and foremost answer is: *practice itself*. Otherwise stated, a central key for understanding ancient rhetoric is that it is oriented toward an accomplishment (a performance), which is the so-called "declamation" or *meletê*.[35] For *meletê*, there are by nature no rules; one can only *prepare* for it. Hence, the important notion of *progymnasta* or *preparatory exercises*. We touch here on one of the most important ideas that Isocrates, the founder of the classical *paideia* according to Marrou, tries to define in his *Antidosis*: for the accomplishment which is the supreme aim of his *paideia tôn logôn*, there is no science (in the sense of *epistêmê*) that would enable one to cope *in advance* with all possible cases. This feature of Isocrates's *paideia* is intimately related to the purpose of the rhetorical training which is to make others learn and practice rhetoric themselves. When one comes to the accomplishment, the *meletê* proper, Isocrates says one can no longer distinguish between "master" and "pupil" – at this stage one finds only competitors who are engaged in a common task.[36]

If we keep in mind the essential function of the rhetorical exercise that I have quickly sketched,[37] we are able to understand that texts that have been "influenced" themselves by such practice possess a very particular *form*. To put it simply, the kind of text that someone trained in rhetoric would write is partially built in order to induce its reader to practice rhetoric. It therefore functions as a kind of *trap* for its reader or its listener (in the case of a speech). Mathematical texts, that is, texts that are *mathemata* in the true sense, "*learning matters*," also share in this particular form. This view immediately raises a difficult question: how can one describe a text that is built to induce something, a text that for example leaves *gaps to be filled* by subsequent readers?[38] I do not have any definite answer to this subtle question, but one thing at least is sure: one cannot avoid this aspect once he or she has entered the "rhetorical

[34] On the "permanency" of ancient *paideia* in Greek culture, see the enlightening studies of Peter Brown (Brown 1992) and Robert Kaster (Kaster 1988).

[35] See note 26.

[36] See Isocrates' *Antidosis* 178ff in which he himself quotes an important passage of one of his earlier works, *Against the Sophists*, §15–18.

[37] The portrait of ancient rhetoric I draw here is of course very rough. A more faithful presentation must take into account the subtleties of the historical development of ancient rhetoric that lasted all throughout Greek history (including Late Antiquity). But those details are only understandable once some basic and stable features of ancient rhetoric have been understood (for more detailed accounts, see Bernard 2003a; Marrou 1948; and Porter 1997).

[38] For an example, see my analysis of a difficult passage of Pappus in Bernard 2003a, §2.2.

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dimension" of ancient texts. In other words, one cannot avoid the practical aspect lying *behind* or rather *inside* the *form* of the text. In this respect, I cannot agree with Unguru when he radically dismisses the Cartesian idea of an *art* carefully *hidden* in the ancient texts. That is to say, I cannot agree with his idea that

> [t]here is no historical advantage whatever growing out of the gratuitous assumption that the men of old played tricks on us by systematically hiding their line of thought. (Unguru 1979, 562)

The assumption Unguru speaks about may well be qualified "gratuitous" if it serves to justify an illegitimate resort to modern algebra. But taken separately, I think it represents for *historians* a fruitful assumption to which one should pay attention, provided one keeps an eye on ancient rhetorical practice. I think that the Cartesian idea, if it is taken as an historical idea, is not wrong in that it claims the Ancients had a hidden art, but rather in that the Ancients could have *revealed* it and turned it into an explicit *method*. The notion of secret *as such* plays an essential role in ancient science (this can be perceived in Archimedes' introduction to his *Ephodos*).

Conclusion

To conclude, we can now understand why and how the study of rhetoric appears as a kind of natural outcome of the dilemma that the extreme positions of Unguru and Knorr represent for the modern historian. To summarize, the reasons are the following: mathematical texts, in Antiquity, were not just a kind of ossified depository of classical knowledge. In the framework of rhetorical practice, classical texts in general were used to support a very specific kind of practice. *Problems* in the most general sense, i.e., "challenging questions," could be found in ancient texts – they often consisted of excerpts (*epitomai*) that were then considered *problems* insofar as they entailed some kind of challenge, or *hypotheseis* insofar as they supported the subsequent task (*meletê*) of dealing with the initial challenge. However, not only the problems, but also their "solutions," in the sense of the "work of answering" that the challenge implied, relied on classical texts. In this respect, the latter were considered a treasury of *topoi*, i.e., of elements that could nurture invention (*heuresis*) provided they were properly organized (cf. the notions of *lexis* and *taxis*). Traditionally, the strength of such a practice relied on the fact that it had to be exposed to a public of listeners or readers, so that they may learn, by the concrete example provided by skilled practitioners, how to practice themselves. *Learning* (not convincing) is the most essential scope of ancient rhetorical practice, at least in the Greek context. Now it had been long recognized by ancient rhetoricians that mathematics was the field *par excellence* in which students could devote themselves exclusively to learning. This rhetorical tradition was by far not forgotten in Late Antiquity. The purpose of late

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authors like Pappus, Eutocius, or even Proclus was to illustrate, each in his own way and by various degrees, how one could or should *practice* mathematics, on the basis of ancient texts. This point of view was fully coherent with ancient rhetorical practice and expressed a feature of mathematical texts that was present but less explicit in texts of the early Hellenistic period. This situation finally explains why, when one sets out to explore ancient mathematical practice, just as Knorr and Unguru have done each in his own way, one is naturally led to take ancient rhetorical practice into account. Surely, the *form* of ancient mathematical texts has to be carefully respected and studied: this is Unguru's claim. But since this form is particularly well adapted to a certain kind of practice based on ancient rhetoric, this study cannot exclude practice, which is Knorr's claim. Those two claims appear to contradict each other only insofar as we lack a clear idea of the context in which ancient mathematical texts were elaborated: this is, in a sense, Klein's claim.

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