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In defence of geometrical algebra

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Abstract The geometrical algebra hypothesis was once the received interpretation of Greek mathematics. In recent decades, however, it has become anathema to many. I give a critical review of all arguments against it and offer a consistent rebuttal case against the modern consensus. Consequently, I find that the geometrical algebra interpretation should be reinstated as a viable historical hypothesis.

Certain parts of classical Greek mathematics have traditionally been interpreted as "geometrical algebra"—meaning that although the surface form of expression is geometric, nevertheless the underlying ideas are essentially algebraic. This was for a long time a commonplace view among mathematically inclined historians such as Zeuthen, Tannery, Neugebauer, van der Waerden, etc. In the last generation or two, however, the tide has turned very drastically. The geometrical algebra hypothesis is now forcefully rejected and indeed seen as a symbol of the historiographical naiveté of these earlier generations of mathematician–historians. Unguru (1975) was an early focal point of this crusade, and although his critique initially drew replies from the old guard (Van der Waerden 1975; Freudenthal 1977; Weil 1978), such opposition has since silenced. In fact, "Unguru's position could now be regarded as the accepted orthodoxy", as (Rowe 2012, p. 37) accurately reports. A recent survey of research on the history of Greek mathematics reaches the same conclusion: "It is clear that the old historiography has been overcome. . . . There are very few who still believe in such historiographical artefacts as . . . geometric algebra" (Sidoli 2013, pp. 43, 25).

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This overwhelming consensus means that most of the arguments against geometrical algebra have never been challenged. From this state of affairs one could easily get the impression that the modern view simply had the stronger case and won by the weight of the evidence. However, there is reason to be cautious before jumping to such conclusions. Since geometrical algebra was always a symbol for a now antiquated mode of historical scholarship, we must be careful not to judge it by association only. Modern advances in historiographical standards have much to commend them, but specific historical hypotheses should be evaluated on the basis of the specific evidence pertaining thereto, not on the basis of degree of congeniality with one's general ideology.

In this paper, therefore, I shall attempt to defend the traditional geometrical algebra hypothesis against its modern attacks. My goal is to discuss, and attempt to refute, all arguments against geometrical algebra that I have been able to find in the literature, from general historiographical ones to specific internalist ones. My contention is that none of these arguments are convincing, and that hence the modern consensus against geometrical algebra is ill-founded.

1 Definition of geometrical algebra

I shall refer to the geometrical algebra hypothesis as GA for short, and I shall take it to consist in the following two claims, both of which have constituted the essence of the "geometrical algebra" hypothesis since at least as early as when Zeuthen (1885, p. 7), coined the phrase in his study of Apollonius's *Conics*.

- GA1. The Greeks possessed a mode of reasoning analogous to our algebra, in the sense of a standardised and abstract way of dealing with the kinds of relations we would express using high school algebra. By and large, whenever we find it natural to interpret Greek mathematics in algebraic terms, the Greeks were capable of a functionally equivalent line of reasoning. If an algebraic interpretation of a Greek mathematical work suggests to us certain connections, strategies of proof, etc., then the Greeks could reach the same insights into a similarly routine fashion. This was an abstract, quantitative-relational mode of thought that was not confined to concrete geometrical configurations and not dependent on geometrical visualisation or formulation; in particular, it was obvious to the Greeks that the exact same kind of reasoning could just as well be applied to numerical relations as geometrical ones.
- GA2. The Greeks were well aware of methods for solving quadratic problems (such as those exhibited in the Babylonian tradition). Books II and VI of the *Elements* contain propositions intended as a formalisation of the theoretical foundations of such methods.

Note that I have taken care to define GA as a historical hypothesis whose content is independent of whether you want to call it algebra or not. The GA debate is not (or should not be) a matter of semantics. The appellation "geometrical algebra" could be regarded as a shorthand only. GA, as defined above, is a concrete, factual hypothesis and should be evaluated as such.

2 The case for geometrical algebra

It is not my aim in the present paper to argue *for* geometrical algebra, but rather to argue against the arguments against it. It will be useful to indicate for reference, however, the main basic arguments for GA.

One argument for GA1 is that the most advanced Greek treatises are so intricate that it is very hard to imagine that they were conceived in this form. As Wallis (1685) puts it:

> It is to me a thing unquestionable, That the Ancients had somewhat of like nature with our Algebra; from whence many of their prolix and intricate Demonstrations were derived. (p. 3)

"And I find other modern Writers of the same opinion therein", Wallis adds, and indeed this position was more or less taken for granted as a matter of common sense among mathematicians from the seventeenth century into the twentieth century. Zeuthen (1885, ch. 1, esp. p. 7), may be considered the culmination and mature expression of this point of view.

Another basic argument is that GA1 and GA2 give purpose and sense to numerous propositions in Euclid that are otherwise very difficult to imagine any motivation for. As Van der Waerden (1975) puts it:

> We were not able to find any interesting geometrical problem that would give rise to theorems like II 1–4. On the other hand, we found that the explanation of these theorems as arising from algebra worked well. Therefore we adopted the latter explanation. (pp. 203–204)

This interpretation too is found in Zeuthen (1885), ch. 1. An accessible explication of it is given by Heath (1908) in the commentary to his translation of Euclid's *Elements* (esp. Book II and Propositions 27–29 of Book VI). The case was further strengthened when Neugebauer (1936), following great advances in the understanding of Babylonian mathematics in the early twentieth century, noted that this view harmonises well with the Babylonian tradition. An accessible overview taking this perspective into account is found in Van der Waerden (1950, pp. 118–126).

3 The critiques

3.1 Szabó (1969)

Szabó (1969) was an early opponent of GA. He offers no arguments against it, however. He does offer assertions of the following type:

> Although [II.5] is equivalent to 'the solution of an algebraic equation', it should not be interpreted in this way. Such an interpretation is misleading because it obscures the true geometric meaning of the proposition and suggests the false historical idea that the Greeks actually operated with algebraic equations in pre-Euclidean times. (p. 352)

But this is a statement of an opinion, not an argument. And so are statements of the following type, I would say:

> No traces of genuine algebraic ideas have yet been discovered in the mathematical tradition which culminated in Euclid's *Elements*. ... The claim that the 'geometrical algebra ...' resulted from the Greeks either taking over or developing further an idea of the Babylonians has no basis in fact. No connection has ever been established between this branch of mathematics and 'Babylonian science'. (p. 353)

Statements like these are very common among GA opponents. I find them unreasonable. The fact that a large number of propositions in the *Elements* can be interpreted in a coherent way as an algebraic theory surely constitutes at least *some* sort of evidence that they were conceived in this way, and its many striking parallels with the Babylonian tradition surely constitute at least *some* sort of evidence for a connection. The evidence is far from conclusive, to be sure, but it is something. It needs to be countered as such, not simply flatly denied.

Besides these kinds of assertions, the main point of Szabo's discussion is proving that "propositions which are usually regarded as part of '... geometrical algebra' can also be given a purely geometrical explanation" (p. 353). This, however, does nothing to disprove GA. The fact that that some of these propositions serve a perfectly credible geometrical purpose as well is perfectly compatible with GA, and indeed quite what one would expect on this hypothesis. After all, the theorems are stated in geometrical form and occur in a work written in a geometrical paradigm. Furthermore algebra and geometry are closely related, so many algebraically important theorems are bound to have geometrical relevance as well. The remarkable thing is not that some of them can be motivated geometrically, but that not all of them can, at least not straightforwardly. As we saw in Sect. 2, Van der Waerden (1975) cited II.1–4 as prototypical examples of theorems from Euclid that are hard to motivate from a purely geometric point of view. Szabo, meanwhile, bases his analysis on II.5 and also claims briefly that a similar point holds for II.6 and II.10. This would be an argument against the claim that *all* the propositions which GA proponents claim are algebraic lack any kind of geometric motivation. But no GA proponents ever claimed this, nor is it what one would expect if the GA hypothesis were true. The GA hypothesis is strengthened by the fact that *some* of the algebraic propositions lack clear geometric motivation. To refute this, therefore, one would need to prove that *all* the algebraic propositions have a geometric motivation. Proving merely that *some* of the algebraic propositions have such a motivation, which is what Szabo does, therefore does not impact the case for GA, especially since no one had claimed that these particular propositions completely lacked geometric motivation in the first place.

3.2 Unguru (1975)

The paper that set off the modern firestorm against GA is Unguru (1975). Unguru's claim s that it is "impossible" to think in one way and write in another, which would indeed mean that the GA hypothesis was misguided:

> Different ways of thinking imply different ways of expression. It is, therefore, impossible for a system of mathematical thought (like Greek mathematics) to display such a discrepancy between its alleged underlying algebraic character and its purely geometric mode of expression (p. 80).

However, such a discrepancy is obviously not "impossible"—if anything it is commonplace. For example in calculus we often use infinitesimal reasoning as a behind-the-scenes heuristic and then write up our findings in the completely different language of epsilon-delta formalism.

But even if this is admitted, Unguru argues, GA proponents still need to explain why Greek mathematicians, if they really could think algebraically, nevertheless restricted themselves to their rather awkward geometrical mode of expression:

> [The geometrical algebra hypothesis] fails to answer the most stringent and manifest question, viz., why did Greek mathematics stick throughout its development to the 'cumbersome', 'awkward', 'highly difficult' method of 'geometric algebra' with its application of areas, transformation of proportions by means of geometrical figures, etc.? This question gains even more in acuity when one keeps in mind that the perpetrators of the view embodied in the concept of 'geometric algebra' presume without any qualms (and rest assured) that there has been an underlying algebraic edifice to Greek geometry throughout its development. Why, then, did this algebraic framework remain all the time in the background, hidden, camouflaged, concealed? . . . So the question remains unanswered: If thinking algebraically simplifies things, as everybody would agree, and if the great Greek mathematical geniuses were algebraists at heart, then why did they put their relatively simple algebraic reasonings in the clumsy and unwieldy molds of geometrical form? (p. 75)

> If [the Greeks] thought algebraically, . . . then why did they systematically fail to use any algebraic symbolism whatever in their writings? How can one reasonably explain such a failure? Is the unwarranted assumption of such mathematical schizophrenia accountable in any convincing historico-rational manner? (pp. 75–76)

One reply to this is that ancient mathematicians did indeed purposefully obscure their published arguments. Reasons for this seem to have included a desire to claim the title of master of the field for oneself rather than giving away one's tricks for free. See Sect. 3.11 for further discussion of this point. In addition, there were also compelling philosophical reasons for insisting on expressing everything geometrically, as this gave all of mathematics a uniform ontology grounded in reality. Indeed, the rise of the analytic-algebraic conception of mathematics in the seventeenth century was vigorously resisted along precisely these lines, and the need was felt, still in the late seventeenth century, to validate algebra from within a geometrical paradigm (see, e.g. Bos 2001).

But even these points aside Unguru's argument is misguided in that it equates, it seems, algebraic thought with algebraic symbolism. In reality neither GA1 nor GA2 requires algebraic symbolism as such, nor was the existence of a secret algebraic symbolism ever a contention of any GA advocate. As GA advocates have always maintained, algebra is algebra whether the variable is called x, "the width", "the thing", "the root" or whatever. Consequently GA does not imply any "assumption of schizophrenia".

Unguru's emphasis on mode of expression also leads him to stress emphatically that Greek and Babylonian mathematics are "two alien cultures" (p. 73), since, he maintains, "the reasoning [in Babylonian mathematics] is largely that of elementary arithmetic or based on empirically paradigmatic rules derived from successful trials taken as a prototype" (p. 78), which is supposedly worlds apart from the geometry of the Greeks. In addition to our points above that one should not attach too much importance to surface form, one could argue that this characterisation of Babylonian mathematics doesn't even get the surface form right. Recent scholarship on Babylonian mathematics has shown that, far from being mere "empirical arithmetic", Babylonian techniques for solving quadratic problems had a strong geometrical component that was in many ways closely analogous to the type of geometrical treatment of such relations found in Book II of the *Elements*. See Høyrup (2002).

As another point of historical context Unguru also makes the following charge:

> The fact is that ... there has never been an algebra in the pre-Christian era. Consequently, there could not have been any 'geometric algebra' either. (p. 78)

Insofar as this is meant as an argument against GA, it clearly assumes what is to be proved. To GA proponents, of course, there *was* a kind of algebra in the pre-Christian era. This hypothesis cannot be disproved simply by asserting its opposite to be a "fact".

Unguru also mistakenly believes that certain algebraic insights are somehow built into the notation itself. Consider for example this proposition from the *Elements*:

> IX.8. If as many numbers as we please beginning from a unit are in continued proportion, then the third from the unit is square as are also those which successively leave out one ...

That is to say, in the geometric sequence $1, a, a^2, a^3, a^4, a^5, a^6, \ldots$, the terms a^2, a^4, a^6, etc. are squares. According to Unguru, "if we use modern algebraic symbolism, this ceases altogether to be a proposition and its truthfulness is an immediate and trivial application of the definition of a geometric progression"; in other words the proposition "becomes a trivial commonplace, which is an immediate outgrowth, a trite after-effect of our symbolic notation" (p. 99). But this is not so. The fact that, for example, a^4 is a square is not by any means implied by the symbolic notation itself. The fact that $a^{xy} = (a^x)^y$ is a contingent fact, a result that needs proving. It is not at all obvious from the very notation itself, as anyone who has taught an algebra class knows. If $a^{xy} = (a^x)^y$ is an "after-effect of our symbolic notation", then why isn't the rule

$$\frac{a}{b} + \frac{c}{d} = \frac{a+c}{b+d}$$

for adding fractions, or the rule $(fg)' = f'g'$ for differentiating a product? As (Weil 1978, p. 92) observes, "one who thinks that the rules governing the use of the exponential notation are trivial must be lacking, not only in mathematical understanding, but also in historical sense".

Unguru commits the same fallacy again in dismissing the algebraic interpretation of a lemma to X.22:

> Compare the above proof with the algebraic content of the lemma, which says that $a/b = a^2/ab$. In its algebraic form, the triviality of the entire enterprise becomes striking. The lemma becomes nothing but an inane, vapid, banal illustration of the simplification of fractions! (p. 102)

Again, the correct rules for manipulating fractions are not "trivial, inane, vapid, banal"—they are serious insights that require proof. Anyone who doubts this just has to stick his head into any algebra classroom where he will find students constantly confused about what is and what is not allowed when working with fractions.

Furthermore, if this shows that Euclid did not know algebra, then neither did, say, Viète. In Viète one may read such things as for example: "Theorem. The sum of two magnitudes plus their difference is equal to twice the greater magnitude". (Viète 2006, p. 37) By Unguru's logic this would "show beyond any reasonable doubt that what [Viète] is doing is not algebra" (p. 105; I have replaced Euclid by Viète in this quotation but the logic remains the same), for in its algebraic form $A + B + (A - B) = 2A$ the theorem is "inane, vapid, banal" and "ceases altogether to be a proposition".

Unguru also offers an argument based on the following propositions from the *Elements*.

> X.112. The square on a rational straight line applied to the binomial straight line produces as breadth an apotome the terms of which are commensurable with the terms of the binomial straight line and moreover in the same ratio; and further the apotome so arising has the same order as the binomial straight line.
>
> X.113. The square on a rational straight line, if applied to an apotome, produces as breadth the binomial straight line the terms of which are commensurable with the terms of the apotome and in the same ratio; and further the binomial so arising has the same order as the apotome.

In algebraic terms, these propositions say the following. A binomial means an expression of the form $a + \sqrt{b}$ (or $\sqrt{a} + \sqrt{b}$, but we can leave this aside for ease of writing), where a and b are rational and b non-square; an apotome is the same thing with a minus in place of the plus. X.112 says, then, that the square R^2 of a rational number divided by $a + \sqrt{b}$ gives a result of the form $k(a - \sqrt{b})$; X.113 says that the square R^2 of a rational number divided by $a - \sqrt{b}$ gives a result of the form $k(a + \sqrt{b})$, where k is rational.

Unguru's argument is as follows:

> If Euclid's lines were general algebraic symbols (which they are not), which could be manipulated like such symbols, then the essence of X.112 could be expressed as follows:

> If $R^2 = B \cdot A$, where R is a rational line and B is a binomial, then A is a corresponding apotome.

Under such circumstances, X.113 would follow immediately and trivially from X.112, as a consequence of the unicity of algebraic operations and the commutativity of multiplication, since X.113 states only that

> If $R^2 = A \cdot B$, where R is rational and A an apotome, then B is a corresponding binomial.

In such a setting, all of Euclid's efforts to prove X.113 would have been in vain, and therefore incomprehensible. Indeed, under such circumstances, no proof at all of X.113 would have been necessary and X.113 would have become, at best, a Porism and not an independent proposition. But this is certainly not the case in the Elements, and this is, I believe, a beautiful substantiation and corroboration of my view: Greek geometry is geometry! (p. 108)

The rhetoric ("incomprehensible") aside, insofar as these propositions are algebraically equivalent, they are so also geometrically. The first proposition puts the area R^2 in the form of a rectangle where one side is a given binomial and find that the other side is k times the corresponding apotome. If this side is cut into k pieces, and the pieces of the area are stacked on top of each other, then we have rearranged the area R^2 in the form of a rectangle with the given apotome as base. We also see that the other side will be k times the corresponding binomial. And this is precisely the statement of the second proposition. Clearly, then, the equivalence of these propositions is very evident already geometrically, not something that magically emerges only when one grasps "the commutativity of multiplication", as Unguru's argument assumes.

3.3 Unguru (1979)

When discussing number theory, Euclid states and proves various theorems which are, from a GA point of view, essentially just a reduction to the special case of whole-number quantities of theorems already proved elsewhere for quantities in general. Unguru (1979) takes this to be a blow against GA:

As Freudenthal would have it, *Elements* V is 'algebra and nothing else'; it is, moreover, 'a general theory of magnitude ... independent of dimension or any characteristic of specific magnitudes'. The problem with such a characterization is the existence of *Elements* VII, in which many of the things dealt with in Book V are repeated and applied specifically to numbers (integers). In the presence of a general theory of magnitude, such a procedure would not have been just repetitious and superfluous but outright senseless. Numbers, after all, are specific instances of magnitude, and what is true of magnitudes in general is also true of numbers. (p. 559)

We must keep in mind, however, that GA1 is a hypothesis about Greek *thought*, not its formal exposition. It is obviously true that Euclid wrote things up in a highly formalised fashion that does not support GA1 by its surface form. But the whole point is whether this was due to *cognitive* limitations or not. Euclid's reasons for his chosen mode of writing could easily have been due to expository and foundational considerations, and thus the formal exposition cannot be taken as indicative of cognitive limitations in the manner Unguru's argument assumes. Indeed, who honestly believes that Euclid did not see the analogies between the theorems of Books V and VII in question? Surely the most likely interpretation by far is that he understood perfectly well that some of the number theory propositions could be seen as parallel to those for magnitudes, but that he nevertheless preferred a separate exposition perhaps for the sake of following tradition or giving a self-contained exposition not assuming more advanced material than needed. The reason for the separate expositions being non-cognitive, then, it says nothing about GA1.

3.4 Unguru and Rowe (1981)

In his reply to Unguru, van der Waerden (1975, p. 201) gave an example of a Babylonian solution of a quadratic problem and pointed out that the solution given is "the same method of solution we learn at school" today. Unguru and Rowe (1981) deny that this is so. Instead, they say, "the mere knowledge of how 'to complete the square' is enough to understand fully, step by step, the scribe's procedure" (p. 6), whereas van der Waerden's account allegedly "assumes, against the textual evidence, the availability of the quadratic formula to the Babylonian scribe" (p. 7). This argument assumes that a meaningful distinction can be drawn between completing the square and using the quadratic formula. But this is a very dubious assumption. Completing the square *is* the method we learn at school today. And this is how the "quadratic formula" is always derived. The latter is nothing but a kind of recipe shorthand for the former. The two are computationally equivalent.

Whether the Babylonians knew "the quadratic formula" or not is not a very meaningful question. Certainly no one has ever claimed that the Babylonians ever had it written down in the form of a literal formula, with symbolic placeholders for the various numbers needed, as in a modern algebra textbook. On the other hand it is beyond dispute that they understood the method of completing the square very well and were able to use it to solve quadratic problems in a systematic fashion which was so heavily standardised that it could be carried out mechanically even by someone who did not have any deep conceptual understanding of the method of completing the square. Thus the Babylonian method is identical to that of modern school algebra in its numerical steps, in its theoretical foundation in the method of completing the square and in its being useable as a mechanical recipe without much understanding. In all these senses van der Waerden's claim is obviously correct. He did not say, as Unguru and Rowe allege, that the Babylonians had "the quadratic formula" and even if he had said that, what would that even mean? The unequivocal points just enumerated amount to a functional knowledge of the quadratic formula for almost all intents and purposes; all that is lacking is a literal, typographical formula, and of course no one have ever claimed that the Babylonians had that.

More generally Unguru and Rowe maintain that:

> Under any suitable, historically reasonable definition of algebra, ancient Babylonian and classical Greek mathematical texts are not algebraic in character. In the Babylonian case they are arithmetical, while in the Greek they are geometrical. (p. 4)

We have already seen above that the claim that Babylonian mathematics was exclusively "arithmetical" is untenable. But that aside, what is the "historically reasonable definition of algebra" that Unguru and Rowe have in mind? They do not give a definition in so many words, but they often speak of "symbolic manipulations" as "the very hallmark of an algebraic system" (p. 17). Thus:

> There are important distinctions between algebra and the concrete arithmetical relationships appearing in Babylonian and some Greek materials. For there is a vast mathematical gap involved between having a general knowledge of concrete number facts on the one hand, and being able to abstract that knowledge and manipulate it symbolically without any reference to the concrete, on the other. (pp. 11–12)

> It is precisely the inability of the Babylonian mathematician 'to describe relations and solving procedures, and the techniques involved *in a general way*' that warrants his disqualification as algebraist. What the Babylonian mathematician lacks is precisely the ability to dispense with specific, definite numbers, and it is this deficiency that dictates the particular form of his approach. What he can produce is recipes, not general formulas. (Unguru 1979, p. 561)

In my opinion it is not all "historically reasonable" to define algebra along such lines. We may ask ourselves the question: What would an abstract, symbolic formulation have added to the Babylonian method of solving quadratic problems? Arguably nothing. They already mastered the solutions of such problems using a general, systematic method. This method is very clearly expressed in their texts: they use numerical examples, it is true, but it is not difficult to see in them the general method, as was clearly the intention. What purpose would it serve, then, to express the matter in purely symbolic notation? Arguably it would accomplish nothing except making the matter more pretentious and abstruse. We can still observe this phenomenon in modern classrooms, where students often stare blankly at abstract formulas but grasp their general meaning perfectly well from one or two worked numerical examples.

I maintain, therefore, that Unguru's insistence on symbolic formulations is historically *un*reasonable. The modes of thought and expression of the Babylonians were perfectly adequate for the goals they set themselves. Their mastery of quadratic problems is general and thorough and not in any way hampered by a lack of symbolism. The introduction of abstract symbolism would not have resolved any problem they were concerned with. Suppose they had made up arbitrary symbols and written down a formulation of their methods in terms of them. This would have been a huge conceptual leap according to Unguru's standards, but it is unlikely to have led, in and of itself, to any changes in the remainder of their mathematical corpus, just as today many students gloss past the gibberish abstract formulas in their textbooks and instead infer their content from specific examples. It is unreasonable, therefore, to use the presence or absence of abstract symbolism as the *sine qua non* of algebraic thought, since it could easily have been included or excluded at various stages in history without altering the actual mathematical substance at hand. Of course the abstract, symbolic formulations serve a purpose in later stages of the development of mathematical thought, but it would be an anachronism of the kind Unguru castigates to insist that it is essential also for a complete "algebraic" understanding of the earlier stages such as the solutions of quadratic problems.

We proceed to the Greeks. Regarding Greek "geometrical arithmetic", Unguru and Rowe (1981) maintain that:

> Although addition and subtraction are employed for general magnitudes in the Euclidean tradition, the dependence of these operations on a geometric formulation imposes a limitation that makes these operations qualitatively different from their modern counterparts. The modern notion of real number transcends this limitation, making it possible to equate and compare figures of differing dimensions, equating these in turn with angles or anything whatsoever capable of being measured. When number reigns supreme, everything can be related numerically to everything else. This the Greeks could not do. (p. 17)

> Throughout classical Greek mathematics, there is a strict adherence to the principle that only magnitudes of like species can be added or subtracted. In particular, this means that there was no generalized concept of number underlying Greek magnitude, and, hence, no idea of combining magnitudes of different dimensions. (p. 24)

Unguru is here violating his own dictum not to infer anything that is not supported by the sources. The sources do not show that the Greeks *could* not add any magnitudes, but that they *did* not do so. Only the former, unsupported reading helps Unguru's thesis. But why should we believe this as opposed to the latter, supported reading? Unguru doesn't tell us; he simply asserts the former reading as if it were historical fact.

In reality it makes perfect sense that the Greeks would not add any magnitudes simply because they had no reason to do so. Adding magnitudes of different types is very often nonsensical and useless. A person could know very well that 10 dollars plus 3 apples is in some sense 13, and yet choose never to carry out a calculation of this sort, not because of some conceptual obstacle but simply because it is pointless. Likewise I would suggest that the Greeks consciously decided that they had no interest in adding magnitudes of different kinds and that they therefore chose to set up their formal theory in this manner. In other words, I would suggest that the fact that the formal Greek theory of magnitudes does not allow for inhomogenous magnitudes to be added did not *preclude* them from doing so; rather it was the *consequence* of their conscious desire not to do so.

Unguru and Rowe go on to give the following variant formulation of their dimensionality thesis:

> The operations of rectangle formation and ordinary multiplication, as explicitly performed throughout the *Elements*, are in fact incompatible with one another,

> i.e., rectangle formation cannot be 'generalised multiplication' without producing inconsistency in the system of operations that we know the Greeks utilized. (p. 24)

Their argument for this thesis is as follows:

> Addition, subtraction, and multiplication (viewed as repeated addition) all preserve dimension, and it is absolutely essential that they do so. For, as we have seen, addition, subtraction and ratio formation all require that the dimensions of the magnitudes involved be equal. It follows that the introduction by Heath and others of the operation of rectangle formation as generalized multiplication represents a radical break with the intrinsic principles underlying the operations explicitly performed in the *Elements*. The representation of products via rectangle formation, which is the very cornerstone of 'geometric arithmetic', overlooks precisely the fundamental tenet of homogeneity that governs the entire Greek treatment of magnitude. (p. 30)

This argument assumes what it is trying to prove, namely that rectangle formation is not part of "the entire Greek treatment of magnitude". It is true that seeing rectangle formation as multiplication is inconsistent with *some other* ways of thinking about multiplication, but whether those other ways constitute "the entire Greek treatment of magnitude", as Unguru and Rowe assert, is precisely the issue at hand. A proponent of geometrical algebra would obviously dispute this assertion and point out that there is no problem in having several different theoretical representations of the concept of multiplication to answer to different purposes, just as in modern mathematics we may define real numbers by Dedekind cuts for foundational purposes while also assuming that real numbers correspond to physical lengths, time intervals, etc. when dealing with physical applications. This does not mean that our notion of real numbers is impossibly "inconsistent", but rather that it is flexible and that different representations of it are useful in different contexts.

3.5 Unguru and Rowe (1982)

Unguru and Rowe (1982) continue their argument by turning to "the real litmus test for the historical efficacy of the 'geometric algebra' concept", namely whether "the Greeks solved quadratic equations by utilizing geometry" (p. 1). Their contention here is that "the attempt to understand Greek mathematics as algebraically motivated leads to paradoxical conclusions that make nonsense out of what we find in the Greek texts themselves" (p. 2). To prove this, then, they assume the GA perspective and attempt to deduce a contradiction from it, as it were.

Their attack begins with a peculiar argument regarding *Elements* II.11. They argue that, assuming GA thinking, this proposition can be construed as a solution to the quadratic equation $x^2 + ax = c^2$. They then declare that the desired contradiction has been reached:

> Of course, this is pure fantasy, and neither Heath nor anyone else would blunder so badly as to mistake this for a Greek solution to a quadratic equation. The point,

> however, is (and it is a point worth emphasis) that such a solution is perfectly plausible once we take the assumptions of 'geometrical algebra' seriously. (p. 20)

Here Unguru and Rowe are mistaken about what GA advocates maintain. II.11 *is* the equivalent of solving this quadratic equation. This is the standard GA view. Zeuthen (1885, pp. 15–18) explains this very clearly and explicitly. Unguru and Rowe have simply reproduced a standard GA argument and followed it with the completely unsubstantiated allegation that it is "of course" a "blunder". Clearly this does not amount to an argument against GA in any way.

Unguru and Rowe also give various arguments as to how a general quadratic equation can be solved by simpler means than those employed by Euclid in VI.28–29. One such argument concludes triumphantly that

> We have thus "solved" the general quadratic by using nothing more than Book II-style techniques, i.e., bypassing altogether Greek proportion theory. Now that is geometric algebra! (p. 21)

Another has it that

> it is absolutely child's play to produce a solution for the general quadratic, a solution which the Greeks could not have missed, had they been doing "3-D geometrical algebra" . . .! (p. 40)

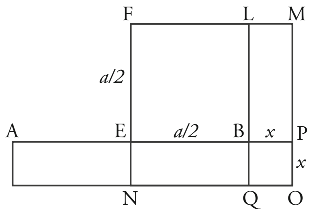
On the basis of these alternative approaches to quadratic equations Unguru and Rowe conclude:

> These arguments, we believe, conclusively show that the claim that propositions like VI.28, 29 were motivated by the desire to obtain a solution to the general quadratic equation is a historically empty claim. . . . Willingness to use just a few light assumptions drawn from "geometric arithmetic" suffices to produce relatively simple solutions to quadratics that allow one to bypass entirely the proportion theory of Book VI. (p. 42)

The logic of this argument is confused. The point that it would have been easy for the Greeks to solve quadratic equations is precisely what GA says. After all, the Babylonians did it 1500 years before Euclid, and of course they did so with "Book II-style techniques" and without Greek proportion theory. Far from being arguments against the GA hypothesis, this is precisely what GA proponents maintain. The only way these facts would constitute counter-evidence to GA would be if GA involved the premiss that solving quadratics was a very complicated business that required a sophisticated proportion theory. But no GA proponent has ever maintained such a thing. Zeuthen (1885, p. 22), for instance, is perfectly clear that Euclid postpones his full treatment of quadratics to Book VI only because he wants to employ certain generalisations and formal niceties, which, however, are not necessary for solving quadratic equations as such. Indeed, since solution methods for quadratics had long been known, it is perhaps not surprising that Euclid is eager to pursue a more sophisticated take on the theory.

Thus, once again, Unguru and Rowe's argument that quadratics can be solved by simpler means is by no means an argument against GA, as they imagine it to be, but rather a simple fact that GA proponents have always agreed with.

Fig. 1 Simplified case of the figure for *Elements* VI.29 with algebraic significations added



Unguru and Rowe also present an argument regarding VI.29 itself. We must first give a short account of this proposition. The Euclidean formulation is very general and in terms of parallelograms, but all of the relevant aspects for our discussion are present already in the special case of the quadratic $x^2 + ax = c^2$ solved in II.11 when all parallelograms are taken to be rectangles.

Restricted to this case, Euclid's VI.29 construction goes as follows (see Fig. 1). Given are the line segment $a = AB$ and the area c^2. Sought is a line segment x such that $x^2 + ax = c^2$. Find the midpoint E of the segment AB and erect the square $BEFL$. This square will have area $\frac{a^2}{4}$. Now construct the square $FMON$ in such a way that its area is $\frac{a^2}{4} + c^2$. Such a construction is given in proposition VI.25. The side FM of this square is $\sqrt{\frac{a^2}{4} + c^2}$. Let x be the amount by which this side length exceeds that of the small square, $\frac{a}{2}$. Then the L-shaped figure $EBLMPOQNE$ has area $x^2 + ax$. But by construction this area is also equal to c^2. Thus the constructed segment $BP = x$ satisfies the desired property $x^2 + ax = c^2$ and we have constructed the (positive) solution to the equation.

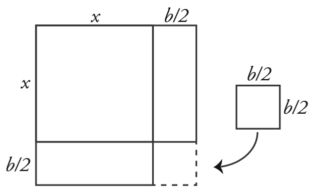
The solution corresponds to the modern algebraic one in terms of completing the square. With this method, to solve $x^2 + ax = c^2$ we would first add $\frac{a^2}{4}$ to both sides, just as Euclid starts by constructing a square with this area. This makes the left-hand side a square and gives $(x + \frac{a}{2})^2 = c^2 + \frac{a^2}{4}$. The right-hand side here corresponds to Euclid's big square $FMON$. Next we take the square root of both sides: $x + \frac{a}{2} = \sqrt{c^2 + \frac{a^2}{4}}$. In terms of Euclid's construction this corresponds to finding the side FM. Finally, to isolate x, we subtract $\frac{a}{2}$ from both sides, just as Euclid subtracts EB from EP to get $BP = x$.

Unguru and Rowe, however, deny such a correspondence:

> In algebra, the technique of completing the square has a definite object, namely to factor the equation in the form $(x + a)^2 = b$, whereupon taking square roots of both sides, a solution is obtained. Thus completing the square ... has as its only raison d'être, the possibility of extracting square roots as the next step in the procedure. What we find in the proof of VI.29, needless to say, is nothing of the kind! ... The whole idea behind completing the square is totally foreign to the method of proof found in Euclid. (p. 27)

Perhaps the best way to make our point is to give an illustration of what "completing the square" really looks like geometrically. In doing so, another interesting question arises, namely, why is there nothing comparable to the following simple

Fig. 2 "Completing the square" figure as in Unguru and Rowe (1982, p. 28)



solution of a quadratic equation in Greek mathematics, if, as claimed, the Greeks solved equations geometrically?

To solve $x^2 + bx = C$, first apply II.14 to get $d^2 = C$. Next "complete the square" as follows: $x^2 + bx = x^2 + 2(\frac{b}{2}x) = d^2$, hence, by II.4, adding $(\frac{b}{2})^2 = \frac{b^2}{2}$ to both sides produces a perfect square, i.e., $x^2 + 2(\frac{b}{2}x) + \frac{b^2}{4} = (x + \frac{b}{2})^2 = d^2 + \frac{b^2}{2}$. Geometrically this amounts to completing the diagram (of Fig. 2). Using I.47, we can find s such that $d^2 + (\frac{b}{2})^2 = s^2$ simply by constructing a right triangle with d and $b/2$ as sides. It follows that $(x + \frac{b}{2})^2 = s^2$, hence $x + \frac{b}{2} = s$, and $x = s - \frac{b}{2}$. (p. 28)

Completing the square in this sense is indeed "totally foreign to the method of proof found in Euclid", but not because it is algebraic but because it is analytic rather than synthetic. That is to say, it assumes that what is sought is already known, namely the line segment x. Completing the square in the sense of Fig. 2 *starts* with the line segment x whereas Euclid's construction *ends* with it. Euclid is, here as ever, adhering to a strictly constructivist paradigm: he uses nothing he has not first constructed. That is why in his solution of the quadratic equation $x^2 + ax = c^2$ he starts with the given a and c^2 and constructs x from them.

Thus the argument by Unguru and Rowe doesn't show, as they claim, that Euclid did not use the method of completing the square. It shows only that he used the constructive version of it rather than the analytic one. In every other way his method is the same. In terms of Fig. 1, if one used an analytic approach like Unguru and Rowe do, one would start with the rectangle $AO = ax + x^2$, then break the ax part in half and place it on top of the square x^2 to get the L-shaped figure $EBLMPOQNE$. Then one would complete the square by adding the square $FLBE = \frac{a^2}{4}$. This is exactly what is shown in Fig. 2, which Unguru and Rowe claim is so fundamentally different from anything in Euclid. In reality it is simply Euclid's construction read backwards. Reading constructions both forwards and backwards in this fashion was of course par for the course in Greek times. Typically one works analytically—that is, one assumes what is sought and reasons towards what is known—in a discovery phase, and then one reverses the steps for the synthesis, i.e. the formal presentation from what is known to what is sought.

So the answer to Unguru and Rowe's question—why is nothing like this found in Greek mathematics?—is that something exactly like that *is* found, except, of course, translated into a constructivist paradigm.

3.6 Fried and Unguru (2001)

Fried and Unguru (2001) continue Unguru's case against GA with the following accusation that GA is "incoherent":

> Why does algebra need the geometrical garb? This is not at all clear and the various attempts at an answer strike us as incoherent on various levels. They amount to the view that, of the discovery of the 'irrational' by the Pythagoreans, algebra could appear only in geometric apparel, for the sake of rigor. We are not told where Greek algebra was before that fateful discovery, nor is the secret divulged to the curious student under what attire, if any, it was hiding then. (p. 19)

This is a puzzling argument. A key tenet of the geometrical algebra hypothesis is that the Babylonian tradition provides a model of where Greek algebra was before its Euclid-style geometrisation. So, far from being some far-fetched and insufficiently specified fantasy, this pre-geometrical state of algebra is thoroughly documented in ample historical sources, which "the curious student" is free to consult.

The authors do not explain what the alleged "incoherence" is supposed to be exactly, but in any case their caricature of the opposing view is misleadingly simplistic. The matter of irrationality is just one of the many good reasons for Euclid to place all mathematics on a geometrical basis. Additional reasons include the fact that geometry provided a unified framework for virtually all known mathematics at the time and that geometry endows algebraic concepts with a concrete meaning and ontology.

Furthermore, what the authors allege to be "incoherent" is unequivocally what happened soon thereafter. That is to say, geometrical propositions like those of Euclid were used with the explicit purpose of providing the theoretical foundations for algebraic algorithms like those of the Babylonians.

The authors themselves admit that Khwarizmi's treatment of quadratics does constitute "geometrical algebra", since one finds there

> a metrical geometrical argument ... advanced specifically for the sake of justifying the mechanical, recipe-like solution of an algebraic equation. ... It is not geometry for its own sake that we face, but rather geometry for the sake of algebra. And it is impure geometry at that, since, unlike Greek geometry, metrical considerations have crept in, for ulterior motives, so to speak, namely, to vindicate a purely algebraic procedure for solving quadratics of a certain type. ... This can, then, rightly be seen as a certain brand of geometrical algebra and be designated as such, unlike those parts of Greek geometry ... wrongly baptized with the same name, which they carry illegitimately, since they embody geometrical truths for their own sake. (p. 24)
>
> This conception, legitimizing, perhaps, talk of an Islamic geometrical algebra, is, needless to say, foreign to the Greek mathematician. (p. 26)

Again, this is not argument but assertion. The claim that Euclid's propositions "embody geometrical truths for their own sake" is precisely what is at issue; it is something one needs to provide evidence for, not merely assert as the authors do.

And why would it be "incoherent" to think that Euclid intended the propositions in question for the same purpose as Khwarizmi, rather than that he had completely different motivations but nevertheless happened to supply exactly the theorems needed for Khwarizmi's purpose? The main difference between them is that Euclid omitted the trifling numerical applications of these techniques, which are perfectly understandable since those had already been known for a long time and the *Elements* is a theoretical, foundational work, not a handbook of practical calculation.

3.7 Mueller (1981)

Mueller (1981) is opposed to GA: “the geometric interpretation is, at least *prima facie*, sufficiently plausible to render the interpretation of algebra unnecessary” (p. 44), he writes. Of course this is not an argument against GA as such since no one ever claimed that a geometrical interpretation is impossible or claimed that the algebraic interpretation was strictly “necessary”, but let us put that aside and consider Mueller’s arguments for his interpretation.

Mueller sometimes speaks as if the GA question hinges on abstract methodological principles. Thus he writes:

> The point of view adopted here is that, if there are no independent grounds for choosing between an algebraic and a geometric interpretation in connection with the *Elements*, the latter is preferable because it does not use concepts which are not explicitly in the work. (p. 168)

This may sound like a principled and reasonable stance, but in reality it amounts to nothing as far as the GA debate is concerned. No GA proponent would disagree with this principle, I’m sure. It cannot be used as a deciding principle in any of these debates since both sides would happily accept it. The crux of the matter is whether there are “independent grounds” or not for deciding between the two interpretations. GA proponents do not take their view because they think it is unproblematic to go beyond what is explicitly in the work, but precisely because they find that there *are* sufficient “independent grounds” for doing so.

It is necessary, therefore, to turn to the internalist arguments. In terms of Euclid’s Book II, Mueller argues that the proofs of the first few propositions suggest Euclid’s ignorance of algebra. The first two propositions may be expressed thus in algebraic terms, as Mueller notes:

> II.1. If there are two straight lines, and one of them is cut into any number of segments whatever, then the rectangle contained by the two straight lines equals the sum of the rectangles contained by the uncut straight line and each of the segments.
>
> $$xy_1 + xy_2 + \cdots + xy_n = x(y_1 + y_2 + \cdots + y_n) \qquad \text{(II,1a)}$$
>
> II.2. If a straight line is cut at random, then the sum of the rectangles contained by the whole and each of the segments equals the square on the whole.
>
> $$(x + y)x + (x + y)y = (x + y)^2 \qquad \text{(II,2a)}$$

Mueller notes that II,2a can easily be derived from II,1a:

$$(x + y)^2 = (x + y)(x + y) = \text{ [by II,1a] } (x + y)x + (x + y)y$$

But Euclid does not prove II.2 in this manner (he instead proves it from scratch, without reference to II.1). According to Mueller:

> The fact that he does not do so is an indication that he does not perceive the relation between these propositions in the way in which a modern algebraist would. For Euclid each of II,1–3 states an independent geometric fact. (p. 46)

On the basis of this and similar examples Mueller generalises thus:

> However one wishes to describe the results proved in book II, the proofs themselves show no sense of the connection between the propositions involved. This fact suggests strongly that Euclid is approaching his subject by looking at the geometrical properties of particular spatial configurations and not by considering abstract relations between quantities or formal relations between expressions. (p. 52)

In my opinion there is no basis for such a conclusion. Mueller's argument rests on the implicit assumption that an algebraic treatment would invoke "the connection between the propositions involved", whereas a geometric treatment would not. But this assumption is not a reasonable one. The fact that the proofs are independent is just as conspicuous on either interpretation. II.2 is obviously the special case of II.1 where the "any number of segments whatever" is taken to be two segments, and "the uncut straight line" is taken to be equal to "the whole" line segment that we started with. This is obvious already geometrically. There is surely no way Euclid could have failed to see this and thus been aware of the possibility of proving II.2 from II.1, no matter whether he thought algebraically or not.

Thus whereas Mueller's argument seems to be:

> Algebraically the propositions are connected; Euclid's proofs show no sign of being cognisant of this; therefore he did not think algebraically.

One could just as well argue, with equal justification, that:

> Geometrically the propositions are connected; Euclid's proofs show no sign of being cognisant of this; therefore he did not think geometrically.

Because of this symmetry I am of the opinion that the independence of these proofs provides no evidence one way or the other regarding GA.

Another of Mueller's arguments concerns the fact that a number of "geometrical algebra" propositions in the *Elements* are stated in terms of parallelograms even though only rectangles are of interest from the algebraic point of view. Mueller discusses for example:

> I.45. To construct a parallelogram equal to a given rectilinear figure in a given rectilinear angle.

As Mueller notes, this proposition "is on the algebraic interpretation the solution of $ax = b$, or the division of a by b" (p. 45). But for this purpose only the special case of a rectangle in place of a parallelogram is needed. Thus Mueller argues:

> On the algebraic interpretation the use of the parallelogram must be read as a pointless geometric generalization, and the failure to prove the algebraically most interesting form of I,45 as the leaving out of a trivial consequence despite its fundamental importance. (p. 45)

Euclid has indeed generalised beyond the case of primary interest, as mathematicians often do. Mueller tries to construe this as a blow to GA:

> Since Euclid often bothers to prove trivial consequences whether or not they are fundamental, the more reasonable conclusion would seem to be that his primary, if not exclusive, motivation is geometric . . . (p. 45)

Mueller does not provide any further indication of what other special cases he is thinking of, or why they would be comparable to the one at hand. In particular the latter seems far from obvious: surely mathematicians typically point out special cases not because they have a generic blanket policy to always note special cases, but rather for reasons specific to the matter at hand and its intended use. Without supporting evidence and explication, therefore, I do not see how the claim that Euclid would have noted the special case if he had intended the theorem algebraically can be considered anything more than an unsubstantiated assertion.

Another of Mueller's arguments against GA concerns the compounding of ratios, which algebraically speaking corresponds to the multiplication of fractions. In the *Elements* this notion occurs only in two propositions: VI.23 and its arithmetical counterpart VIII.5, the former of which reads:

> VI.23. Equiangular parallelograms have to one another the ratio compounded of the ratios of their sides.

The notion of a compounded ratio is not used elsewhere in the *Elements*, and in fact is not even defined. Consequently the interpretation of the compounding of ratios as multiplication of fractions has never been central to any GA reading of Euclid. Nevertheless it is quite reasonable to infer that if Euclid was a skilled algebraist as GA advocates maintain then he ought also to have seen the algebraic aspects of the compounding of ratios.

But Mueller maintains that Euclid shows no awareness of its algebraic aspects and uses this to argue against GA as follows:

> In general, the geometric books confirm the impression gained from the arithmetic ones that Euclid does not construe compounding as multiplication. VI,23 itself is, in a sense, evidence of this fact, since the product of the lengths of two sides of a parallelogram does not produce a value of any mathematical significance. (p. 154)

I disagree. The proposition shows that this value does indeed have a clear mathematical significance, namely that it is proportional to the area of the parallelogram. As far as proportionality of areas of similar or closely related figures is concerned, which is all Euclid discusses, it is just as good as the absolute area itself. So it makes perfect sense for Euclid to use it in place of the absolute area, since this alleviates the need to introduce an auxiliary quantity (the height) and thereby enables the presentation to be cleaner and more streamlined.

Mueller also notes that this proposition could have been used to prove:

> VI.14. In equal and equiangular parallelograms the sides about the equal angles are reciprocally proportional; and equiangular parallelograms in which the sides about the equal angles are reciprocally proportional are equal.

Thus Mueller argues:

> [VI.]14 itself is a direct consequence of VI,23 and VJ [a proposition discussed by Mueller which does not occur in the *Elements*; algebraically: if $\frac{z}{w} \cdot \frac{u}{v} = 1$ then $\frac{z}{w} = \frac{v}{u}$], which, as I pointed out, is obvious on the fractional interpretation of ratios. Euclid seems totally unaware of the connection between 23 and 14. (p. 161)
>
> Euclid's failure to reduce VI,14 to 23 is one piece of evidence that he does not view compounding of ratios as a kind of multiplication. (p. 162)

I disagree that such a conclusion follows. As we have noted, the notion of the compounding of ratios clearly plays a very marginal role in the *Elements* to say the least. It can certainly be considered inessential for following the work. It would clearly be a radical change of course, therefore, for Euclid to start basing other propositions on this notion, as Mueller suggests he would have done had he realised the connection.

So Euclid's motivation for not proving VI.14 from VI.23 could simply be that he did not want to introduce another abstract notion needlessly when VI.14 can be proved straightforwardly already without it. Teachers of mathematics are often faced with the choice of explaining something, on the one hand, in a concrete way that uses only what the student already knows, or, on the other hand, with the aid of more abstract concepts which may make the connections clearer and reduce the need for brute-force work while also demanding a greater conceptual penetration on the part of the student. Faced with such a choice, many teachers will chose the former option, even though they understand the latter perfectly well. Euclid's reasons for not deriving VI.14 from VI.23 could very well be of this kind.

On this interpretation, it is still reasonable that Euclid would want to include VI.23 as a nod to those in the know. The use of compounding here is in any case a rather harmless matter of formulating the theorem and thus not as conceptually demanding as the rewriting of the *Elements* envisioned by Mueller. In particular, Mueller's VJ mentioned above would be far from "obvious" to a reader who did not have a good conceptual understanding of such manipulations already. By avoiding relying on such concepts, Euclid frees himself from the burden of having to explicate it from scratch—this, however, would no longer be the case if he followed the path suggested by Mueller.

In the quotation above we saw Mueller claiming that his interpretations in these instances "confirm the impression gained from the arithmetic ones". However, this is not an indication that I have omitted one of the Mueller's key arguments against GA. On the contrary, in his discussion of the arithmetic books Mueller explicitly postpones all discussion of this point:

> The serious question for interpretation is whether compounding should be viewed as a representation of multiplying, i.e., as a device for representing the multiplication of fractions in the language of proportionality. I shall be arguing that compounding should not be viewed in this way, but shall postpone this argument until later chapters covering propositions in which Euclid employs the notion of compounding. (p. 88)

The arithmetical side of the matter is instead discussed in connection with Book VIII, where Euclid proves the arithmetical analog of the above proposition:

> VIII.5. Plane numbers have to one another the ratio compounded of the ratios of their sides.

The relevant definition is:

> VII. Definition 16. And, when two numbers having multiplied one another make some number, the number so produced be called plane, and its sides are the numbers which have multiplied one another.

The following proposition is obviously a special case, even though Euclid proves it separately:

> VIII.11. Between two square numbers . . . the square has to the square the duplicate ratio of that which the side has to the side.

> VII. Definition 18. A square number is equal multiplied by equal, or a number which is contained by two equal numbers.

Mueller sees the fact that Euclid makes no connection between VIII.5 and VIII.11 as evidence against GA:

> [Euclid's] failure to exploit VIII,5 [in his proof of VIII.11] is a good indication that he does not construe compounding as a representation of multiplying. (p. 92)

It is not clear to me what the argument is supposed to be here exactly. Are we to believe that Euclid did not realise that VIII.11 is a special case of VIII.5? Surely it is utterly inconceivable that Euclid would have failed to see this. The fact that VIII.11 is a special case of VIII.5 is blatantly obvious on *any* interpretation of these propositions, not just the algebraic one. And if we agree to this, then how could Euclid's decision to prove VIII.11 from scratch possibly serve as an indicator of whether he adopted one point of view rather than the other?

We have now discussed the only uses of compounded ratios in the *Elements*. But Mueller also counts its absence in the later books as further evidence against GA:

> Euclid . . . does not even use the notion of compounding in the solid books. His failure to do so is perhaps the strongest evidence that he does not construe compounding ratios as multiplication. (p. 221)

The reason why this constitutes such "strong evidence", according to Mueller, is that the theory could have been greatly simplified if the method of compounding ratios had been used in a full-fledged fashion. But this argument works only if we assume that

Euclid must have preferred such a presentation if he had realised that it was possible. Only then does his choice not to do so say anything about his understanding of the algebraic aspect of compounding ratios. But this seems to me to be a very dubious assumption. As we all know, it is a common-place state of affairs in mathematics that a certain theory can be presented in two ways: either very concretely, which often means relying on long computations and brute-force methods, or very abstractly, which often means much more sleek proofs but much greater conceptual sophistication. It would be much too simplistic to say that the latter is obviously superior. Each method has its advantages. The former is often more accessible in principle to a less sophisticated reader; it has a certain methodological purity by staying close to the source and not introducing concepts far removed from the immediate matter at hand; it raises fewer ontological questions by working on a more tangible level of concreteness; etc. It is perfectly plausible that Euclid avoided the method of compounding ratios for reasons such as these. Therefore his choice not to use this method says nothing about whether or not he understood this method algebraically.

This point of view seems to agree with Mueller's own conclusion:

> The general situation might perhaps be summarized by saying that Euclid will use the theory of proportion in an elementary way to avoid a complex solid-geometrical argument, but he will not use it in an abstract computational way to substitute for geometrical argument. (p. 226)

This is just what one would expect on the hypothesis that Euclid desires to use the most concrete methods in his proofs. Though Euclid uses the language of compounded ratios on a few isolated occasions, he does so only in the most elementary way; it is a basic shorthand expression rather than a method of reasoning. This is perfectly consistent with the hypothesis that he is fully aware of the algebraic power of compounding ratios but has decided to keep his presentation as concrete as possible, just as many mathematicians today write books for a certain audience in a concrete form even when they know a more abstract and streamlined theory.

It would have been different if Euclid had used the method of compounded ratios in his proofs, but used it in a clumsy way. Then his presentation could be faulted on its own terms—not on the basis that it could favourably be replaced by a different approach altogether but on the basis that the very method he choses could have been carried out far more perfectly by someone who had greater understanding of those very methods. *That* would have been evidence that he did not fully understand the method of compounded ratios in an algebraic manner. But that is not what we find in the *Elements*.

3.8 Saito (1985)

Zeuthen formulated the GA1 hypothesis in his work on Apollonius's *Conics*. The theory of conics is of course replete with quadratic relations at every turn and therefore relies on the kind of relations found it Book II of the *Elements*. Zeuthen found that something like GA1 was the best way to characterise the role these propositions played in the *Conics*. But Saito (1985) argues that, on the contrary, "Apollonius's thought can be better understood if we assume that crucial steps of his argument depend on geometric intuitions" (p. 32) rather than algebraic reasoning. This leads him to view the role played by the results of Book II of the *Elements* in a different light. The basis for Saito's new interpretation is that "the process of visualization is a necessary element in the reliance on geometric intuition", and that "the propositions in *Elem.* II play an important role in this process [of visualization]" (p. 42). Thus those results

> are not methods of treating the lines and areas as general quantities in a way similar to modern algebra, but they are the means for transformation between 'visible' and 'invisible' forms of areas. The former is indispensable because it makes geometric intuition available, while the latter is adapted to the formal statement of results as propositions . . . (p. 43)

> The main feature of this formal expression is the 'invisibility' of the figures involved, such as 'the square of the ordinate' and 'the rectangle contained by two line segments which lie in a line'. (p. 46)

Thus, in Saito's view, propositions are most efficiently stated in terms of "invisible" areas, as this allows for certain abstraction and generality. At the beginning of each proof, however, these abstractions must be "unpacked", as it were, into visible areas. This is done using results from Book II of the *Elements*, which express "invisible" squares and rectangles as a combination of other figures, which may be visually represented.

However, it would be easy to construct the square on the ordinate or to raise one of the two sides of a "flat" rectangle in a direction perpendicular to the other. In this way one could easily make Saito's "invisible" areas visible with only the simplest of tools. Book II of the *Elements* would not at all be needed to do this, which speaks heavily against Saito's interpretation of the purpose of Book II. In fact, it seems much more natural to interpret the "invisibility" of these figures as an indication that they express relations thought of abstractly, just as GA1 says.

Saito also goes on to interpret Euclid's *Elements* in terms of the notion of "invisible" areas. Consider

> II.1. If there be two straight lines, and one of them be cut into any number of segments whatever, the rectangle contained by the two straight lines is equal to the rectangles contained by the uncut straight line and each of the segments.

which is the geometrical equivalent of

$$x(a + b + c + \cdots) = ax + bx + cx + \cdots$$

This proposition at first "appears quite strange" and "seems to be a tautology" (p. 54), since the proof simply points out the very evident fact that a rectangle cut into so many pieces has the same area as the sum of the pieces. This seems too trivial to even admit of a "proof" in any meaningful sense, and in fact, as Saito notes (p. 55), Euclid himself seems to have used this theorem implicitly already earlier in his proof of the Pythagorean theorem I.47 (where the square on the hypothenuse is decomposed into two rectangles corresponding to the squares on the legs). To resolve this puzzling state of affairs “the notion of ‘visible’ and ‘invisible’ figures seems to be useful”, according to Saito. II.1, from this point of view, is in itself is a statement about “invisible” figures, since it speaks in abstract terms without any reference to a specific geometrical configurations. The proof, then, which does little more than merely restate the proposition in terms of a specific, lettered figure, does from this point of view accomplish something:

> What Euclid has done is to reduce the equality between ‘invisible’ figures to that of ‘visible’ ones. The latter equality is evident by geometric intuition and it can be safely conjectured that Euclid thought it a sound basis for the proof of the former. As a result, I claim that II 1 is by no means trivial, for it extends the ‘visible’ equality to the ‘invisible’. (p. 55)

This also explains why Euclid has committed no fallacy in his proof of 1.47: there he uses only the “visible” form of the theorem, which is self-evident. An example of a converse type occurs in XIII.10, “the sole explicit example of the use of II 1–3 in the *Elements*” (p. 55). There Euclid applies II.2 in the abstract, without having the geometrical configuration of that theorem visually represented in the figure, thus suggesting once again that the essence of the propositions of Book II is their abstract formulation.

But again none of this is an argument against the geometrical algebra hypothesis. On the contrary, the GA hypothesis seems to account for the above in a natural way without the need for a recourse to the rather contrived notion of “invisible figures”. The essence of abstraction in the formulations of the propositions in Book II, their saying something more abstract than the obvious geometrical facts on which their proofs are based, their being applied in abstract rather than visual form—all these things are in perfect accord with the geometrical algebra hypothesis. Saito replies to this as follows:

> A criticism might be raised that the invisible figures and their sides are substantially the same as quantities in general, and thus that my interpretation is at bottom algebraic. But it is a mistake to interpret invisible figures in this way. They retain their geometric properties, since depending on the arrangement of the figures, one of the pair of twin-propositions is necessary. *Elem.* II contains the propositions concerning the ‘invisible’ figures for the solution of geometric problems, and these propositions are usually stated in pairs, the two propositions being used in mutually complementary way [sic] to solve a problem. And this mutually complementary use of a pair of propositions is evidence that Euclid did not regard geometric magnitudes (areas and lengths of lines) as general quantities. (p. 56)

> For example, both II 5 and II 6 can be expressed by the equality $(a+b)(a-b) = a^2 - b^2$ and some other pairs of propositions can likewise be represented by a single algebraic equality. (p. 47)

Saito’s argument here assumes that such double forms would not occur if the treatment was intended algebraically. I do not find this a reasonable assumption. For example, a modern algebra book may include both $(a+b)^2 = a^2 + 2ab + b^2$ and $(a-b)^2 = a^2 - 2ab + b^2$ when a single formula would have been enough if a and b were intended as general magnitudes, which in fact they are. Of course modern algebra books include both of these formulas because it is convenient in practice to have both forms for easy reference, even though of course they are ultimately equivalent.

Furthermore, Saito's algebraic formulation of II.5–6 is extremely liberal. van der Waerden (1950, p. 120) also uses this liberal formulation, though he has his own hypothesis as to how the two theorems differ. Using a more literal interpretation one would obtain (as Zeuthen 1885, p. 12 does) the following algebraic meanings:

> II.5. If a straight line $[a]$ is cut into equal $[\frac{1}{2}a + \frac{1}{2}a]$ and unequal $[(a - b) + b]$ segments, then the rectangle contained by the unequal segments of the whole $[(a - b)b]$ together with the square on the straight line between the points of section $[(\frac{1}{2}a - b)^2]$ equals the square on the half $[(\frac{1}{2}a)^2]$.
>
> $$(a - b)b + \left(\tfrac{1}{2}a - b\right)^2 = \left(\tfrac{1}{2}a\right)^2$$
>
> II.6. If a straight line $[a]$ is bisected and a straight line $[b]$ is added to it in a straight line, then the rectangle contained by the whole with the added straight line and the added straight line $[(a + b)b]$ together with the square on the half $[(\frac{1}{2}a)^2]$ equals the square on the straight line made up of the half and the added straight line $[(\frac{1}{2}a + b)^2]$.
>
> $$(a + b)b + \left(\tfrac{1}{2}a\right)^2 = \left(\tfrac{1}{2}a + b\right)^2$$

Thus it is highly misleading to claim that these propositions are algebraically the same. They are no more the same algebraically than they are geometrically, so it makes no sense to use the existence of both as evidence that Euclid did not think algebraically. According to Saito:

> The double form i.e. the existence of twin-propositions, can be explained in the context of their application to the geometric arguments. They are used in mutually complementary ways according to the arrangements of points and lines in the problems and theorems to which they are applied. (p. 59)

This is of course true, but it proves nothing unless one makes the tacit assumption that this cannot be explained equally well algebraically. And Saito has given no argument for this assumption, except the mere assertion that the propositions are algebraically identical. But to Zeuthen they are far from identical, and to van der Waerden, who does see them as in a way corresponding to the same formula, there is nevertheless an *algebraic* distinction between them in terms of the problems they are designed to solve. Altogether, then, Saito's argument about the "twin" propositions II.5–6 consists in showing that they are geometrically differentiable and assuming that they are algebraically indistinguishable. But since there is no reason to believe the latter assumption, the former proves absolutely nothing one way or the other as far as GA is concerned.

3.9 Saito (1986)

Like Mueller, Saito (1986) argues against GA1 on the basis of the ways in which the Greeks handled compounded ratios. According to Saito, Greek works betray a lack of understanding of the basic algebraic structure of such operations. If the Greeks had truly understood these things in an abstract, algebraic fashion, he argues, they could, and presumably would, have streamlined their works considerably. This argument concerns the "behind the scenes" reasoning, as it were: obviously the Greeks insisted, for one reason or another, that their finished treatises must proceed in a strictly synthetic-geometric fashion; the question is whether they mastered algebraic thought and only wrote up their results in this form for the sake of formal presentation, or whether their thought itself was limited along with their mode of expression. Saito maintains the latter. His evidence for this is the manner in which the Greeks dealt with rules such as (the equivalent in the language of compounded ratios of)

$$\frac{a}{b} = \frac{c}{d} \cdot \frac{e}{f} \quad \Rightarrow \quad \frac{c}{d} = \frac{a}{b} \cdot \frac{f}{e} \tag{F}$$

According to Saito,

> though it is self-evident to us that [(F)], it was not so to Euclid, nor for Apollonius. Later Pappus took the trouble to prove it. We will find other examples of their ignorance of or indifference to the multiplicational implication of compounding. (p. 31)

There are several things to object to here. First of all (F) is not "self-evident" to us either. It is a well-known fact, to be sure, and one that we have used so often that it has become second nature to us. But that doesn't make it "self-evident".

But even this caveat aside, the fact that Pappus proved (F) does not mean that it was not considered self-evident. Saito apparently thinks so since he goes on to promise "other" evidence, apparently implying that this is one piece of evidence. But in reality, of course, mathematics is full of proofs of self-evident theorems. To take but one example, the triangle inequality, proved by Euclid (I.20), is "evident even to an ass", as a famous phrase reported by Proclus has it.

What, then, is Saito's further evidence for his claim? First he considers the role of compounded ratios in Euclid. As noted above, this notion occurs in only proposition VI.23 and its arithmetical counterpart VIII.5. If we write P for the area and l, k for the sides of a parallelogram then VI.23 says, algebraically speaking, that

$$\frac{P_1}{P_2} = \frac{l_1}{l_2} \cdot \frac{k_1}{k_2}$$

In Euclid's *Data* a very similar result occurs as proposition 68:

> If two equiangular parallelograms have a given ratio to each other, and if one side also has a given ratio to one side, the remaining side will have a given ratio to the remaining side.

Algebraically, this amounts to the variant

$$\frac{l_1}{l_2} = \frac{P_1}{P_2} \cdot \frac{k_2}{k_1}$$

obtained from *Elements* VI.23 by applying (F). Indeed Euclid could easily have proved the theorem in this way, and as Saito notes: "this proof would not only simplify the proof in the text of the *Data*, but would also bring about a radical change in the style and object of the argument" (p. 38). Thus this might seem to suggest that Euclid did not grasp the underlying algebraic nature of these results and methods.

In my opinion this is an unconvincing argument. Saito interprets the fact that Euclid didn't prove *Data* 68 from *Elements* VI.23 as an indication that he didn't see the algebraic bridge between these results, which would have made the proof very easy. That is to say, Euclid did not have enough algebraic knowledge to take this step. It seems to me more likely that precisely to opposite is true: Euclid had *too much* algebraic knowledge to take this step, for he saw that the results are really one and the same. From this point of view, it would not make sense to "prove" *Data* 68 from *Elements* VI.23 since it is not really a separate theorem but simply a reformulation of the latter in a form more appropriate to the *Data*. Indeed, as Saito himself reports, the extant text of the *Data* also comes with an alternative proof of proposition 68, which has more in common with the algebraic proof although "the algebraic character is not so conspicuous in this alternative because it repeats the procedure in VI, 23, rather than use its conclusion" (p. 39). This, of course, is exactly what one would expect if *Data* 68 was thought of as a reformulation of *Elements* VI.23 to be proved in a self-contained manner, as I proposed.

Saito next turns to Apollonius's *Conics*. Apollonius makes extensive use of compounded ratios throughout. Saito decides to focus on propositions 41 and 43 from the first book, though, as he admits (esp. p. 54), this is a rather arbitrary sample. These propositions involve compounded ratios, but, according to Saito, "only superficially, as a means of compact enunciation of the propositions and shorter representation of the argument, not as a method of analysis" (p. 41). "This fact seems to suggest that [Apollonius] was not even aware of the possibility of the use of compounding [in the manner of algebraic analysis], which would have greatly simplified the argument" (p. 53).

This sounds compelling enough until one pursues the details. It then transpires that this alleged "great simplification" is not all that great; in fact, arguably, it is hardly even a simplification. Saito himself gives a reconstruction of the combined analysis behind the two propositions in question on pp. 47–48. This analysis consists of 24 steps. He then proceeds to give "a simpler analysis" (pp. 49–50), which utilises (F). This "simpler" analysis picks up at step 11 of the original analysis, adds four steps and one application of (F), and then coincides again with the original analysis in its last four steps. Let us, for the sake of argument, be charitable and say that the use of (F) is a single step, even though it is arguably the outsourcing of multiple steps to a lemma. With this concession, then, the "simpler" analysis consists of 20 steps, 15 of which are identical to the original 24-step analysis. Saito gives no further argument as to why the second analysis is "simpler", and yet he considers this grounds enough for a very ambitious conclusion:

> This analysis would have greatly simplified the synthesis of the propositions I, 41, 43. The complexity of the extant text testifies that this line of thought never occurred to Apollonius, though he has made use of the compounding of ratios in the same propositions. How can we explain this apparent contradiction? ... It must have been the case that the compounding of ratios had not yet been sufficiently developed ... (p. 50)

Such is the extent of Saito's argument that Apollonius did not master the algebra of the compounding of ratios. To me it seems dubious to draw such wide-ranging conclusions on the basis of the possibility of making very slight simplifications in the proofs of two cherry-picked propositions.

3.10 Grattan-Guinness (1996)

Grattan-Guinness (1996) purports to "examine the credentials and verisimilitude of geometric algebra as an interpretation of the *Elements*" and "find[s] in favor of the critics" (p. 357). To this end he gives a summary of "the principal criticisms" of GA (pp. 359–360), which I shall now quote and reply to in order.

> 2.1. The algebra is simply the wrong style: there are no equations, or letters used in an algebraic way, in the *Elements*.

The *Elements* contains many propositions about things being equal to other things. To say that these are not "equations" is to say that they are not written in the form of a symbolic equation in the modern sense. But that does not mean it is not algebra. If you take a modern algebra textbook and translate all equations into words, and all x's into "the root", then it does not cease to be algebra. This change in surface form does not alter the underlying content and line of thought of the work. Therefore the absence of typographical equations does not prove that GA is "wrong".

> 2.2. Had Euclid been thinking algebraically, he would have presented constructions corresponding to easy manipulations of [$(a + b)^2 = a^2 + 2ab + b^2$] (for example) which, in fact, are absent from the *Elements*.

I find it dubious to argue this way, as if there is only one way to write a book if you know algebra. Authors' choices whether to include or exclude particular materials in their works are complex decisions that take into account a myriad factors and considerations. Merely knowing algebra is not sufficient to dictate these choices in a deterministic manner, as this argument assumes.

Furthermore, I would argue (as would many GA advocates, I believe) that Euclid's purpose is to *incorporate* a certain body of algebraic knowledge within a geometrical paradigm, not give a self-contained introduction to algebra for its own sake. And his account is sufficient for this purpose. So why would he include more algebraic material? No detailed account of why exactly Euclid would have been obligated to do this has ever been given.

> 2.3. Information is lost when the algebra is introduced, in particular concerning shapes of regions. Thus, using "$p+q$" to denote adding, say, two rectangles does not distinguish between their being adjoined at the top, bottom, left, or right ...

What is the problem with this exactly? Why would the algebraic interpretation need to be 100 % information-preserving? Indeed, this geometrical information is often incidental. For instance, in the proof of the Pythagorean Theorem (I.47) the "arms" of the "windmill" could just as well be flipped inwards. The figure would be messier but the proof would be the same. Thus it is far from clear that discarding information about these kinds of incidental aspects of the concrete representation does any real harm to a faithful representation of the underlying ideas of the work.

> Again, theorems about parallelograms are often (mis-)written in terms of corresponding theorems about rectangles...

Indeed, some theorems in the *Elements* are expressed in terms of parallelograms where the rectangular cases are the interesting ones from an algebraic point of view. Consequently GA proponents generally focus their discussions on the latter special case. But focussing on the most interesting case is not the same thing as "miswriting" the theorem, so what's the problem?

> 2.4. Common algebra is associated with analysis in the sense of reasoning from a given result to principles already accepted. Euclidean geometry goes in the reverse, synthetic, direction. Hence, proofs may well be warped.

Like the preceding arguments, this is not an argument against GA. It seems to be an argument against the claim that algebra constitutes a perfect translation of all the contents of the *Elements*, preserving every conceivable aspect of the original. Of course no GA proponent has ever been committed to anything near such a stance, so these kinds of arguments are red herrings that have nothing to do with the serious historical question at hand.

> 2.5. Euclid never measures a geometrical magnitude of any kind. For example, there is nothing in the *Elements* directly pertaining to π, in any of its four roles for circles and spheres; apparently such mathematics was not Element-ary for him. Hence the association with algebra leads to an emphasis on arithmetic which cannot be justified.

Whether Euclid measures anything is a matter of interpretation. Theorems about the equalities of various areas can be certainly construed as having to do with measuring those areas, or even with measuring underlying lengths, as in, say, I.47, XIII.10 or XIII.12. But be that as it may, Grattan-Guinness's argument still has no bearing on GA. Again it is not an argument that has to do with historical hypotheses and the evidence for them, but rather an argument that an emphasis of algebra could lead to misunderstandings, such as the notion that Euclid was concerned with the number π. The credibility of GA as a historical hypothesis cannot be faulted on the grounds that it allegedly "leads" by "association" to various misunderstandings of Euclid that it neither entails nor endorses.

> 2.6. If the Greeks really possessed this algebraic root, why did they not bring it to light in the later phases of their civilization? Why, one might add, did that philosophically sophisticated culture not introduce a word to denote, even if informally, this important notion?

The Greeks may have done all this for all we know. Our sources are very limited. And insofar as they didn't it could have been because it was too basic. After all, the old Babylonians knew how to do this kind of algebra already 1500 years before Euclid, which suggests that the "sophisticated culture" of the Greeks would have found it quite trivial (whether they learned it from the Babylonians or not). Also, the Greek mathematical tradition is not notable for revealing its motivations in other cases either. Why, for example, did they care about conic sections? Did they draw inspiration from sundial astronomy or maybe the problem of the duplication of the cube? Apollonius wrote a big treatise on conics without saying a word about it. The Greek mathematical tradition is one of the refined formal treatises, not one prone to chitchatting about its motivations and goals.

> This point is strengthened by Klein's real history of algebra from later Greek figures (especially Diophantos) through the Arabs to the Renaissance and early modern Europeans, for a gradual process in three stages is revealed: (1) using and maybe abbreviating words to denote operations and known and unknown quantities, (2) replacing these words by symbols or single letters, and (3) allowing letters also to denote variables as well as unknowns and extending notational systems for powers. The interpretation of Euclid as a geometrical algebraist requires him to have passed all three stages; and while he might have skated through them with greater ease than did his successors, the total silence over his achievement among his compatriots is indeed surprising.

It is nonsense that the GA interpretation "requires" this of Euclid. These three steps are not the notion of algebra that GA is based on. In any case, (1) is of course trivial and obviously found in the Babylonian tradition, and (2) and the second part of (3) is not more than notational conventions. The first part of (3) is unclear until the meaning of "variable" has been specified, but one thing that is clear is that it has nothing to do with either GA1 or GA2.

Grattan-Guinness also raises the following objection to GA:

> 3.1. Euclid *never* multiplies a magnitude by a magnitude ... [For example,] in Euclid's geometry *the square* **on** *the side is not the square* **of** *the side, or the side squared*; it *is* a planar *region* which has this size. (pp. 360–361)

In a later publication, Grattan-Guinness (2004) again reaffirms this view:

> Common algebra is the wrong algebra anyway [for interpreting Euclid], for Euclid never multiplied geometrical magnitudes together. (p. 300)

This question-begging argument is not really an argument at all for Grattan-Guinness's point of view but rather a mere assertion of it. According to the GA interpretation, of course, the forming of rectangles in Book II *is* a kind of multiplication of magnitudes. One cannot argue against it by asserting that Euclid did not multiply magnitudes, since this amounts to nothing but assuming what is to be proved.

Of course it is true that Euclid speaks of squares, for example, as geometrical objects and not their areas as the result of a numerical multiplication. But how is this supposed to prove that GA is "wrong"? One might as well insist that von Neumann's set-theoretic construction of the natural numbers actually has nothing to do with numbers at all since it is really only about *sets*. This may be technically true in a very narrow, dogmatic sense, but of course few would insist that common arithmetic is the "wrong" way of conceiving what von Neumann's construction is about—far from being "wrong", that is precisely the sense in which it was always intended to be understood.

3.11 Netz (2004)

Netz (2004) studies the history of one particular problem first occurring in the work of Archimedes. The problem amounts to a cubic equation, and Archimedes's solution in terms of a parabola and a hyperbola is readily interpreted in algebraic terms. For example, where we would speak of the square or square root of a quantity, the Greeks would instead speak in terms of the ordinates or abscissas of a parabola such as, in modern terms, $py = x^2$, which comes to the same thing. The question is whether or to what extent Archimedes and other ancients thought of the problem in algebraic or proto-algebraic terms, or whether their mode of thought was limited to the strict geometrical concreteness they used to express their results.

Netz's answer is ostensibly to "side with Unguru and Klein: there was a basic divide separating ancient, from later mathematics, typically seen in the transformation from a more geometrical approach to a more algebraic approach" (p. 190), but in reality he explicitly comes down on the opposite side of the crucial question. For example, he writes:

> Quite naturally, we now find that the two approaches [i.e., geometrical and algebraic] could mix in the very same proposition. This in itself is meaningful: there are no deep conceptual taboos involved (as authors such as Klein sometimes tend to suggest). The Greeks could think of objects in terms of their configuration, or in terms of their quantitative relations—and they could mix the two approaches. In all probability, they never even stopped to distinguish between the two. (p. 53)

> There does not seem to be a big conceptual divide, separating ancients from moderns, so that a certain type of mathematical understanding was inaccessible to the ancients. Greeks were perfectly capable of a quasi-algebraic treatment—but, in practice, they happened to minimize it. (pp. 54–55)

So, on this view, the Greeks were very proficient in proto-algebraic ways of thinking but chose to downplay this in their published works. The reasons for this were not cognitive but stylistic and sociological. As Netz explains:

> We should not think of the correspondence of Archimedes as a series of publications—Archimedes communicating to the world his newest ideas. Instead, a published work, of the kind we have extant today, is merely a stage in an ongoing intellectual tournament. (p. 62)

For this reason Netz argues:

> The Greek authors do not aim to allow their solutions to fit some structure of classification within which their work can be recognized. On the contrary: they attempt to blur the outline of the problem, to hide their dependence upon different approaches. Archimedes' generalized statement [of the problem mentioned above] is conjured out of nowhere, to surprise the reader (this is so that the reader would not see the general form as a mere technical tool, forced upon Archimedes to simplify the terms of the problem). Dionysodorus hides the dependence of his analysis on that of Archimedes—and precisely for this reason foregrounds the geometrical setting of his problem. (p. 57)

These preferences for geometrical obfuscation ceased making sense when the "ongoing intellectual tournament" was no longer ongoing. Medieval mathematicians grappling with the Greek mathematical heritage faced a very different task, namely that of absorbing and synthesising a vast body of mathematics. Thus they focussed on systematisation and abstract classification, and this, rather than any cognitive breakthrough, explains their more algebraic style, Netz argues:

> Hellenistic Greek mathematical practice focused on the features of the individual proof, trying to isolate it and endow it with a special aura. Thus the characteristic object of Hellenistic Greek mathematics is the particular geometrical configuration. Medieval mathematical practice focused on the features of systems of results, trying to bring them into some kind of order and completion. Thus the characteristic object of medieval mathematics is the second-order expression. In a particular geometrical configuration, the mathematician foregrounds the local, qualitative features of spatial figures. In a second-order expression, the mathematician foregrounds the global, quantitative features of mathematical relations. Thus, Hellenistic Greek mathematics—the mathematics of the aura—gave rise to the problem; medieval mathematics—the mathematics of deuteronomy—gave rise to the equation. (p. 187)

As Netz notes, the fact that no great cognitive leap was needed for this transition is especially clear in the case of Eutocius:

> No one can ascribe to Eutocius any deep originality as a thinker. To find in him, already, the characteristic features of medieval mathematics, is therefore remarkable. But once we see that those features arise not from conceptual developments, but from changes in mathematical practice, Eutocius' originality becomes clear. Eutocius' mathematics was already different, in terms of its practice, from that of Hellenistic Greek mathematics. Archimedes looked for striking results standing on their own; Eutocius looked for systematization. (p. 188)

This point about the implausibility of a second-rate mathematician like Eutocius making great conceptual advances generalises well beyond this specific case. As Mueller (1981) admits:

> Scholia which interpret propositions in book II numerically . . . and other evidence make it certain that the possibility of applying geometric algebra to arithmetic problems was an established fact by the first century A.D. (p. 50)

There are two basic ways of interpreting this fact: either this generation made profound conceptual advances beyond what Euclid had conceived, or else they merely spelled out what Euclid thought was too trivial to mention. Proponents of GA advocate the latter and its opponents the former, i.e. that Euclid somehow failed to see what Babylonian mathematicians had seen 1500 years before him, and what commentators, who on all other counts were inferior and unoriginal as mathematicians, would see 300 years after him.

Altogether Netz has provided, in my view, an excellent case against Unguru and Klein, notwithstanding the fact that he claims to argue for them, in that he has shown clearly that the Greeks were very much capable of thinking in an essentially algebraic fashion. It happens that they chose to hide it, but they did so for reasons that have next to nothing to do with the development of mathematical thought and everything to do with rather pedestrian and extra-mathematical considerations regarding publication conventions and tactics.

In order to maintain his opposition to GA despite the above, Netz also raises a few specific arguments against an algebraic reading of Archimedes. These arguments are defensive in nature, since on the face of it it seems clear that Archimedes is indeed operating essentially algebraically, as Netz admits:

> Archimedes' text, very surprisingly, makes a deliberate choice to deal with objects as if they were quantitative in nature. This choice, more than any other feature of Archimedes' text, points forwards towards a more algebraic understanding of the problem. (p. 98)

This concerns Archimedes's "multiplication" of a figure by a line: Archimedes's expression is of the form "figure epi line", which is quite clearly conceived as an abstract quantity rather than anything like a concrete area or volume in the figure. As Netz notes, "epi does not refer to the construction of a solid from an area and a line"; "In fact we have a clear sense of what it might mean, and this is because it is often used in calculations, in expressions of the form: number epi number" (p. 100), where it means "multiplied by".

To argue against the prima facie algebraic interpretation Netz maintains that the operation in question is not truly algebraic after all since it lacks certain essential algebraic properties. For example, it is not commutative: we never hear of "line epi figure" (p. 104). I find this argument thoroughly unconvincing. The fact that we always say "base times height" and never "height times base" when describing the area of a rectangle does not prove that we fail to comprehend the commutative nature of the operation. It is simply a stylistic choice to always used the preferred and standardised version, though we understand perfectly well that they are equivalent.

4 Conclusion

The geometrical algebra hypothesis has, for the past few decades, been a kind of scapegoat in a war of historiography. As the hallmark of a currently unpopular mode of scholarship, this hypothesis has been condemned with zeal by a new generation of historians. Because of its unfashionable association, the geometrical algebra hypothesis has seen objections of all sorts hurled its way. And with no one to defend it, bystanders are likely to assume that it is justified. But the geometrical algebra hypothesis deserves a fair trial. In this paper I have attempted to address every substantial argument ever raised against the geometrical algebra hypothesis. I have argued that none of them are at all compelling. I urge, therefore, that it is time to take a step back from perfunctory opposition to geometrical algebra and to look at its case afresh with an open mind.

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