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EUDOXOS AND DEDEKIND: ON THE ANCIENT GREEK THEORY OF RATIOS AND ITS RELATION TO MODERN MATHEMATICS*

1. THE PHILOSOPHICAL GRAMMAR OF THE CATEGORY OF QUANTITY

According to Aristotle, the objects studied by mathematics have no independent existence, but are separated in thought from the substrate in which they exist, and treated as separable – i.e., are "abstracted" by the mathematician.[1] In particular, numerical attributives or predicates (which answer the question 'how many?') have for "substrate" multitudes with a designated unit. 'How many pairs of socks?' has a different answer from 'how many socks?'. (Cf. *Metaph*. XIV i 1088ᵃ5ff.: "One[1a] signifies that it is a measure of a multitude, and number[1b] that it is a measured multitude and a multitude of measures".) It is reasonable to see in this notion of a "measured multitude" or a "multitude of measures" just that of a (finite) set: the measures or units are what we should call the elements of the set; the requirement that such units be distinguished is precisely what differentiates a set from a mere accumulation or mass. There is perhaps some ambiguity in the quoted passage: the statement, "Number signifies that it is a measured multitude", might be taken either to identify numbers with finite sets, or to imply that the subjects numbers are predicated of are finite sets. Euclid's definition – "a number is a multitude composed of units" – points to the former reading (which implies, for example, that there are many *two*'s – a particular knife and fork being one of them). Number-words, on this interpretation, would be strictly construed as denoting *infimae species* of numbers. It is clearly in accord with this conception that Aristotle says, for example (in illustrating the "discreteness", as opposed to continuity, of number): "The parts of a number have no common boundary at which they join together. For example, if five is

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a part of ten the two fives do not join together at any common boundary but are separate; nor do the three and the seven join together at any common boundary" (*Cat.* vi 4^b28-9). The fundamental operation of the addition of numbers is then just that of the *union of finite sets* – which sets must, however, be supposed disjoint. This requirement, and the corresponding one for subtraction (namely, that the number – i.e., set – subtracted must be contained in the one it is subtracted from), would lead to some awkwardness in the formulation of arithmetical relationships, and much awkwardness in the arrangement of proofs. Such requirements are never stated or accommodated in practice. One therefore has to conclude either that the requirements and the procedures for fulfilling them are tacitly understood, or, as seems more likely, that the strict distinction between number as substrate and number as species is ignored by the mathematicians. Perhaps, indeed, this is what Aristotle means when he says that the mathematicians consider their objects "*qua* separable from the substrate".

Corresponding to the question 'how many?', which asks about multitude, is the question 'how much?', which asks about magnitude (distinguished from number, according to Aristotle, as "continuous", in contrast to "discrete", quantity).[2] But this question requires fuller specification: that of a "respect", or a kind of magnitude – what one now calls, in the physicist's terminology, a dimension: 'how long?'; 'how much area?'; 'how capacious [or 'how much volume']?'; 'how heavy?', etc. As in the case of number, there is reason to think that the primary reference of magnitude terms is to the substrate – the bearer of magnitude. For example, Aristotle remarks[3] that "some quantities consist of parts having position relative to one another, others not of parts having position"; and he instances, as of the former kind, lines, planes, solids, and places – which latter, therefore, are all by him taken to be "quantities", and more specifically "magnitudes". It is clear that in speaking of these as constituted of parts having position relative to one another, he must have in mind particular spatial figures in each case. This is strikingly confirmed by another passage (*Metaph.* V 13 1020^a7–14):

We call a quantity that which is divisible into constituent parts of which each is by nature a one and a "this". A quantity is a *multitude* if it is numerable, a *magnitude* if it is measurable. We call a multitude that which is divisible potentially into non-continuous parts, a magnitude that which is divisible into continuous parts; in magnitude, that which is continuous in one dimension is length, in two breadth, in three depth. Of these, limited multitude is number, limited length is a line, breadth a surface, depth a solid.

We have here not only the same identification of specific magnitudes with actual spatial configurations, but the striking parallel of "multitude" (or "plurality") with "length, breadth, and depth" as genera, and of "number" with "line, surface, and solid" as species within those respective genera – suggesting once again that if the particular magnitude of the kind *length* (the 'limited length') is a *line* (in the sense, of course, of line-segment), the corresponding particular 'numerable quantity' (the 'limited multitude') is a set.

One more point seems worth calling attention to in connection with this primarily concrete notion of quantities. We are told by Aristotle (*Cat.* vi 6^a27) that what is most characteristic of quantities is the attribution to them of equality and inequality – that these relations are predicated of quantities and of nothing else. And indeed one finds, in Euclid's arithmetic and geometry, that "sameness" is never predicated of numbers, lengths, areas, volumes, or angles: ratios, for example, of two areas on the one hand, two lengths on the other, are (in appropriate circumstances) said to be "the same" – but never "equal";[4] on the other hand, the areas of two figures are said to be "equal", but never "the same" (indeed, most often it is simply said that "the two figures are equal" – that area is the appropriate magnitude-kind is taken to be understood). This difference is quite alien to our present way of thinking about such matters: for us, to say that two distinct triangles are equal in area is to say that they have "the same area". But on the suggested reading of the Greek terminology, it would be incorrect to speak of "the area of this triangle": a triangle does not have an area, it *is* an area – that is, a finite surface; *this area* means *this figure*, and the two distinct triangles are two different, but equal, areas. On exactly the same principle, then, two different "numbers" – that is, two different finite sets – may be "equal" (cf. Aristotle's reference, above cited, to the "two separate fives" that compose a ten). Thus, we may say that each species of quantity (whether discrete or continuous) is distinguished in Greek mathematics by its own proper equivalence-relation, called in each case just "equality"; and that where our own practice is to proceed to the corresponding equivalence classes, regarding these as particulars (numbers, lengths, etc.), the Greeks did not, in principle, make this abstraction. (On the other hand, as already remarked in connection with numbers, the exigencies of mathematical discourse tended to lead to compromises in practice.)

2. QUANTITIES AND THEIR RATIOS: EUDOXOS

The Eudoxean–Euclidean theory of ratio and proportion involves three distinct (interrelated) notions: *number*, *magnitude*, and *ratio*. The notion of magnitude is just presupposed in Euclid's exposition: neither definitions nor explicit assumptions are formulated concerning it, and number, although it is made the subject of a definition, is also in effect simply taken to be understood (for the definition does not provide a basis for arithmetical reasoning); but ratio is defined in a remarkably precise and adequate way. The phrase "theory of proportion" is used because the notion of sameness of ratio is crucial (both to the development of the theory, and to the very definition of ratio); and two pairs of magnitudes that have the same ratio are said to be "proportional" (or "in proportion").

The notion of "kind (or genus) of magnitude" is explicitly invoked by Euclid (Bk. V, Def. 3). No definition and no postulates are given by him for this notion, but he seems to take it for granted that in each magnitude-kind there is an appropriate "combining" operation on the substrates – for length, e.g., on line-segments; for area, on figures; for volume, on solids – analogous to the joining of (disjoint) multitudes, that leads to an "addition" of the magnitudes of that kind (this assumption characterizes the traditional philosophical notion of extensive magnitude).[5] (It seems quite in accord with this point of view that the word used for the operation of addition, whether of numbers or of magnitudes, is simply *και* – that is, the conjunction "and".)

It is not difficult to extract from Euclid's procedures a statement of the properties that must be presupposed, for any given magnitude-kind, or, more generally, for any species of quantity (whether discrete or continuous), in order to apply to it the general Eudoxean theory of proportion. In doing this, it is convenient to take that step in abstraction which, as we have seen, the Greeks evidently did not take in principle, although in some degree they did in practice – to abstract upon the equivalence-relation called "equality" in any species of quantity, so that the "objects" of that species correspond to the equivalence-classes, and equality becomes identity. Accordingly, we postulate a combining operation to be called "addition", not upon the substrates (a notion that is hard to axiomatize in a manageable way, and is problematic in any case for the theory of magnitude – e.g., how "combine" two bodies whose masses are each equal to that of the galaxy?), but directly upon these more abstract objects. Any species of quantity **Q**, under its operation of addition, is required to be an *ordered commutative semigroup*, in which (moreover) subtraction (of the lesser from the greater) is always possible; that is, the following conditions must be satisfied:

(1) $a + (b + c) = (a + b) + c$;
(2) $a + b = b + a$;
(3) for any a and b in **Q**, *exactly one* of the following alternatives holds:
  (a) for some c in **Q**, $a = b + c$;
  (b) for some c in **Q**, $b = a + c$;
  (c) $a = b$.

It easily follows from these conditions that the cancellation law (uniqueness of the result of subtraction) holds: if $a + b = a + c$, then $b = c$. As to the ordering, we introduce it by defining: "$a < b$" (or, equivalently, "$b > a$") means that there is a c such that $a + c = b$. It is easily established from our stipulations that if $a < b$ and $b < c$, then $a < c$ (the relation $<$ is transitive); that we never have both $a < b$ and $b < a$ (the relation $<$ is asymmetric); and that for any elements a, b, of **Q**, exactly one of the following three conditions holds: $a < b$, $a = b$, or $a > b$ ("law of trichotomy"). These properties characterize $<$ as a strict total ordering of **Q** ("strict" because asymmetric – i.e., analogous to "strictly less than", not to "less than or equal to"; "total" because the relation $<$ holds, in one direction or the other, between any two distinct elements of **Q**). We also have the important proposition: if $a < b$, then $a + c < b + c$ (the ordering is "compatible with the semigroup structure"). It is worth noting that the procedure of defining the ordering with the help of the relation of addition has a certain correspondence with the last of the "Common Notions" at the beginning of Book I of the received text of Euclid: "The whole is greater than the part". To be sure, the authenticity of this common notion has been questioned (for that matter, Tannery challenged all of them); but even if the passage is an interpolation in the original text, it provides evidence that the traditional concept of the "greater" was just this: that the greater is what is composed of the lesser and something besides.

Addition gives rise in an obvious way to the operation of multiplying a quantity of kind **Q** by a positive integer: na means "the sum of n terms, each equal to a". This operation is central to the definition of the fundamental notion of ratio.

Euclid's characterization of the concept of ratio is contained in Definitions 3, 4, and 5 of Book V of the *Elements*. The contents of these definitions may be paraphrased as follows – clauses (a), (b), (c), corresponding roughly to Euclid's three "Definitions" (although (b) really contains more than does Def. 4):

(a) A ratio ρ is a binary relation, of the following general character:
if ρ is a ratio, then given any species of quantity, **Q**, and any pair (a, b) of elements of **Q**, it makes sense to affirm (or deny) that ρ "holds" between a and b (in that order) – which we may symbolize (tentatively) by: "$\rho(a, b, \mathbf{Q})$": "a and b, taken in that order, *as* elements of **Q**, *have the ratio* ρ".

(b) It is conceivable that a pair (a, b) of quantities of kind **Q** "have no ratio at all" – i.e., that all statements of the form $\rho(a, b, \mathbf{Q})$ are *false* for this pair (a, b) and this kind **Q**. If a and b do "have a ratio", it is unique; and we shall symbolize it by "$(a{:}b)_{\mathbf{Q}}$" – "the ratio of a to b in **Q**". (In fact, the context usually makes it clear what **Q** is, and we shall actually therefore drop the subscript and just write "$a{:}b$".) The necessary and sufficient condition for a and b to have a ratio in **Q** is that for some positive integer m, $ma > b$, and for some positive integer n, $nb > a$. (Note that this condition is symmetric as between a and b; it guarantees the existence of both ratios, $a{:}b$ and $b{:}a$.)[6]

(c) If a and b are quantities of kind **Q** that have a ratio $\rho = (a{:}b)_{\mathbf{Q}}$, and if **Q**′ is any species of quantity, and c, d, any elements of **Q**′, then $\rho(c, d, Q')$ is true if and only if the following holds:

For each given pair of positive integers m, n:

either	both $na > mb$ and $nc > md$,
or	both $na = mb$ and $nc = md$,
or	both $na < mb$ and $nc < md$.

Under these conditions, we say that (a, b) and (c, d) are proportional, or have the same ratio: $(a{:}b)_Q = (c{:}d)_{Q'}$.

Note, then, that clause (c) (or Euclid's Definition 5), which is the heart of Eudoxos's construction, characterizes the ratios by introducing the relation of sameness of ratio. It is of course crucial for such a characterization that the relation defining "sameness" be an equivalence. Reflexivity and symmetry of the Eudoxean relation are immediately obvious from the definition. As for transitivity, Euclid takes the pains to prove explicitly that it holds (Book V, Proposition 11: "Ratios which are the same with the same ratio are also the same with one another"). It is quite remarkable that this step in abstraction (the analogue of which, as we have seen, appears not to have been made in the essentially simpler case of numbers and of magnitudes) here is taken explicitly and completely. The exigencies of the problem of characterizing ratios and proportions for not necessarily commensurable quantities led to the development of a technique of "mathematical abstraction", whose fully explicit general recognition and exploitation (if we make an exception of a remark of Leibniz's) was achieved only in the course of the great transformation of mathematics in the nineteenth century.[7]

3. PRELIMINARY COMPARISON WITH DEDEKIND

The relation of Eudoxos's explication of the notion of ratio to Dedekind's well-known construction of the real numbers is easy to see. Let a and b be quantities (of some kind **Q**) that "have a ratio" in the sense laid down in Euclid's Definition V.4. Consider all pairs (m, n) of positive integers for which $mb \leqslant na$. For each of these pairs (m, n) consider the rational number m/n. It is easy to show that if (m', n') is another pair of positive integers, and if $m/n = m'/n'$, then $mb \leqslant na$ implies $m'b \leqslant n'a$; therefore we may speak of a well-defined *partition* of the set of all positive rational numbers into two subsets, "upper" and "lower", S^* and S_*, characterized by: m/n belongs to S_* just in case $mb \leqslant na$; otherwise – i.e., just in case $mb > na$ – m/n belongs to S^*. Note that clause (b), or Euclid's Def. 4, guarantees that if a and b have a ratio, neither S_* nor S^* is empty. It is also easy to see that – still for a given pair, (a, b), of quantities of kind **Q** having a ratio – *each rational number in* S_* *is smaller than each rational number in*

S^*. Thus the partition into lower and upper sets determined by a given pair of quantities that have a ratio is precisely a "Dedekind cut" in the system of positive rational numbers; and therefore defines in its turn a positive real number in the sense of Dedekind. (That Dedekind himself considered cuts in the system of all rational numbers – positive, negative, or zero – is obviously of no great importance.)

It is clear that two pairs of quantities, each pair having a ratio in the sense of Euclid's Definition V.4, which moreover have the *same* ratio in the sense of Definition V.5, determine by the above construction the same Dedekind cut, and therefore the same positive real number. We have thus a well-defined mapping of the system of all Eudoxean ratios into the system of positive real numbers.

However, it is not the case that the mapping we have constructed is (necessarily) one-to-one. In proceeding to the partition of the positive rationals, we have, in fact, discarded some Eudoxean information. Eudoxos's criterion gives a partition into three sets of rationals, one of which (the "middle one") may be empty, and (as one easily sees) contains at most a single element. (When the middle set is non-empty, our construction throws its element into the lower set). Let us consider under what conditions this can lead to the assignment of the same real number to more than one Eudoxean ratio.

Suppose that we have quantities a, b, of kind **Q**, possessing a ratio, and quantities c, d, of kind **Q**$'$, possessing a ratio and determining the same upper set as the former pair; thus, for arbitrary positive integers m, n, we have: $mb > na \Leftrightarrow md > nc$. Can it be that, at the same time, there are positive integers j, k, such that jb and ka are unequal but jd and kc equal? For this to be so, in view of the former condition, we must have $jb < ka$. Let the difference, $ka - jb$, be called o.

Now, since $jd = kc$, for every positive integer N we have $Njd = Nkc$, hence $(Nj + 1)d > Nkc$, hence $(Nj + 1)b > Nka$; and from this it follows that $N(ka - jb) < b$: *every multiple of the quantity o is smaller than b* – we may say that o is "infinitesimal" in relation to b. In particular, o and b do not have a ratio.

Conversely, let us now suppose given two quantities, o and a, of the same kind **Q**, with the first infinitesimal in relation to the second; then it is easily seen that the phenomenon under consideration does actually occur within **Q**. For suppose that m and n are positive integers with $ma > na$ – which, of course, simply means that $m > n$. By the infinitesimality of o in relation to b, we shall then have that $(m - n)a > no$, i.e., $ma > n(a + o)$. Since, on the other hand, the last inequality obviously entails $m > n$, we see that the ratios $a{:}a$ and $a + o{:}a$ determine the same upper class, and therefore the same real number. But these ratios are not the same in the sense of Eudoxos, because we have, for any positive integer n, $na = na$ but not $n(a + o) = na$.

We have therefore seen that a necessary and sufficient condition for the mapping we have defined, from the set of Eudoxean ratios into the set of real numbers, to be one-to-one, is that *every pair of quantities of any given kind have a ratio*.

This result shows that the Eudoxean theory is in a sense stronger and more refined than that of real numbers: it allows the discrimination of ratios that are not distinguished by the real numbers they determine. But in fact this refinement is of no use, and even clouds the theory. The trouble is this: It is of central importance to the theory of proportion that the ratio of b to a determines b – more precisely, that given a quantity a of kind **Q** and a ratio ρ, there is *at most one* quantity b of the same kind such that $b : a = \rho$. Indeed, Euclid proves a proposition to this effect (Book V, Proposition 9: "Magnitudes which have the same ratio to the same are equal to one another; and magnitudes to which the same has the same ratio are equal.") But Euclid's proposition is false in any domain in which infinitesimals exist; for if o is infinitesimal in relation to a – under which circumstance, as we have just seen, $a : a$ and $a + o : a$ are distinct but determine the same real number – it is easy to show that, for any positive integer n, the Eudoxean ratios $a + no : a$ and $a + o : a$ are the same. In fact, the argument given above essentially shows this: it shows that for any o infinitesimal relative to a, the ratio $a + o : a$ determines the same upper set as $a : a$ but differs from the latter in having an empty "middle set"; since the quantities no are obviously all infinitesimal in relation to a, the ratios $a + no : a$ all determine the same upper set and, having empty middle sets, determine also the same lower set. Thus, by the Eudoxean criterion, they are all the same.

Conversely, suppose the envisaged situation occurs – that $a : c$ and $b : c$ are the same, but a and b are unequal – say $a > b$. Since a and c have a ratio, there is an integer whose product with a is $>c$; let n be any such integer. Again, there is an integer (necessarily >1) whose product with c is $\geqslant na$; let the smallest such be $m + 1$. Then

$(m+1)c \geqslant na > mc$, and, by the equality of the ratios, it follows that nb satisfies the same double inequality; therefore $n(a-b) < c$. Since n can be as large as one wants, this shows that $a-b$ is infinitesimal in relation to c.

We are thus led to the same necessary and sufficient condition as before: the non-existence of infinitesimals, in a given species of quantity **Q**, is necessary and sufficient for the non-existence in **Q** of different quantities having the same ratio to the same quantity. Clearly, Euclid's proof of Proposition V.9 must have made tacit use of the assumption that all pairs of magnitudes of a given kind have ratios; and indeed we find that his proof of Proposition V.8, upon which the proof of V.9 depends, does do so.[8]

The need for this assumption was pointed out (presumably for the first time) by Archimedes,[9] in connection not with the theory of ratios, but with the closely related "method of exhaustion" – in effect, the method of limits: another great mathematical creation of Eudoxos. The assumption has come to be known as the Axiom of Archimedes; accordingly, we shall characterize as "Archimedean" any species of quantity each pair of whose elements has a ratio, and shall take the Eudoxean theory of proportion to deal in principle only with Archimedean species. With this stipulation, we are in possession of a well-defined one-to-one mapping of the system of all possible Eudoxean ratios into the system of positive real numbers.

4. DISCRETE QUANTITY AND MAGNITUDE; OPERATIONS UPON RATIOS

In the foregoing, the theory of proportion has been developed uniformly for all Archimedean species of quantity, whether discrete or continuous; in particular, therefore, for numbers. Of course, it is clear that all our conditions, including the Archimedean condition, are satisfied by the positive integers (provided that we, unlike the Greeks, count 1 as a number – for otherwise condition (5) of Section 2 above will not be met).

Euclid does not in fact proceed in this way. After developing the Eudoxean theory for magnitudes in Book V, he gives an independent treatment for numbers, in terms of "multiples" and "parts" (i.e., submultiples), in Book VII (see Definition VII.20). It has often been noted, however, that this leads to an incoherence in Euclid's exposition, when he speaks of the identity of some ratio of magnitudes with a ratio of numbers. And it is quite remarkable (this too is a well-known circumstance) that Aristotle already speaks of a unified theory of proportion for all species of quantity; he says (*Posterior Analytics* I v 74^a18-25):

That proportionals alternate [i.e., that $a{:}b = c{:}d$ implies $a{:}c = b{:}d$ – assuming that all the quantities involved are of the same kind] might seem to hold for the terms *qua* numbers and *qua* lines and *qua* solids and *qua* times; since it used to be proved separately, although it is possible to prove it of all by a single demonstration. But because there was no one name for all these – numbers, lengths, times, solids – and they differ in species from one another, they were taken up separately. But now it is proved universally; for the property indeed did not belong to them *qua* lines or *qua* numbers, but *qua* this [unnamed attribute] which is supposed to belong [to them universally].

If we consider an arbitrary Archimedean species of quantity **Q**, a number of interesting questions arise concerning the ratios of quantities of the kind **Q**. (Let us call these, for short, simply "ratios on Q".) We may, for instance, ask: Onto what subset of the system of the real numbers are these ratios mapped by the correspondence we have defined? Again, we know that each ratio on **Q** is a one-to-one relation from some subset of **Q** to some subset of **Q**; we may ask, Is each of these subsets, for each such ratio, identical with **Q**? In other words: Does each ratio on **Q** define a function on **Q**? (For it is easy to see that if each ratio on **Q** does define a function on **Q**, these functions must map **Q** onto itself; indeed, each ratio on **Q** has an inverse that is also a ratio on **Q**.) A third question: Given quantities a, b, c, d, all in **Q**, do there exist quantities e, f, g in **Q**, such that $a : b = e : g$ and $c : d = f : g$ – i.e.: Do the ratios $a : b$ and $c : d$ admit a common denominator in **Q**?

Of these three questions, the first and last may be asked "absolutely" as well – that is to say, may be asked of the system of *all* ratios that ever occur between quantities of any domain whatsoever. In other words, we may ask which real numbers correspond to Eudoxean ratios; and whether every pair of Eudoxean ratios can be expressed (in some species of quantity) with a common denominator.

Now, questions of this "absolute" kind were never raised by the Greeks – the first obviously not, since the concept of real number was lacking, but the last also not. The question can, as we have just seen, be posed in the terminology of Greek mathematics, but considerations of "all possible species of quantity" were alien to the subject as far as the Greeks developed it. Aristotle offers an enumeration of species of quantity – discrete and continuous. The discrete are: *number* and *language* (the latter with reference to prosody). The continuous are: *line*, *surface*, *solid*, *time*, and *place*. He further remarks that these are, taken strictly, the only quantities – that whatever else is quantitative is so in some way "derivatively".[10] However, one cannot assume that the exhaustiveness of this list was generally accepted, even by Aristotle himself. In the first place, there certainly are "derivative" magnitudes that Aristotle refers to elsewhere, namely velocity and density (cf. note 5). And in the second place, of these, although one – velocity – may be taken to be "derivative" from length and time (not, to be sure, in the sense of the term "derivative" that Aristotle indicates when discussing his enumeration), the other – density – can only be referred back to volume and weight; which leaves weight itself as a presumed addendum to the list. But however this may be, the clear fact is that Greek mathematics always presupposes, quite in the spirit of the Aristotelian view of the objects of mathematics as "existing in" *things*, that such kinds of quantity as there are are simply *given* in the natural world; that it is, one may say, the business of the mathematician to study those he finds, not to speculate about what others are possible. More precisely: since certain conditions are assumed to hold of every kind of magnitude (or, more generally, of quantity), simple *universal* assertions about magnitude-kinds can be warranted; but no more complex sorts of generality than this – that is, than the generality expressible by free variables ranging over magnitude-kinds – are accessible.

The issue of the "functionality" of the ratio-relations takes the form, in Greek mathematics, of the question of the existence of a "fourth proportional" to three given quantities: Given three quantities a, b, c, of which the first two are of the same kind, does there exist a quantity d, of the same kind as c, such that $a : b = c : d$? In Book IX, Proposition 19, Euclid discusses the question when this is and when it is not true in case the quantities involved are all numbers.[11] In Book VI, Proposition 12, he shows how to construct a fourth proportional to three given line-segments. But in the general theory of proportion in Book V (proof of Proposition 18), he makes tacit use of the assumption that a fourth proportional to three given magnitudes always exists.

It seems not unreasonable, in the light of this, to suppose that the existence of a fourth proportional was taken to be a property of magnitudes in general – that is, of "continuous", as opposed to "discrete", quantity.

It is easy to see that the functionality of ratios for a given species of quantity guarantees the existence of common denominators for that species. (The converse, of course, is false, as the example of the natural numbers shows.) This property is of particular interest in connection with what we may call (in the modern sense) the "algebra" of ratios: that is, ratios as a domain upon which *operations* are defined.

Here what one finds in Euclid is quite interesting. He does indeed define six such operations: *inversion*, *composition*, *separation*, and *conversion*, which take $a:b$ to $b:a$, $(a+b):b$, $(a-b):b$, and $a:(a-b)$, respectively (the last two, of course, under the assumption that $a>b$) (Defs. V.13–16); and *duplication* and *triplication*, which we should describe as the "square" and the "cube" of a ratio (Defs. V.9–10). In each of these cases, there is a theorem to be proved: namely, that if one substitutes for the quantities a, b, two others, c, d, having the same ratio, then the result of the operation expressed in terms of the second pair will be the same as the result expressed in terms of the first pair. In the case of inversion, this is immediately obvious from the definition of sameness of ratios; for composition and separation, Euclid proves what is required in Propositions V.17 and 18. For conversion, the required theorem is not given by Euclid; but it is clear that this case is essentially like that of separation.

For duplication and triplication, the situation is rather more complicated. In the first place, the theorem that these operations do indeed depend upon the given ratios, not upon the particular magnitudes by which they are represented, is not itself stated by Euclid. In the second place, however, he does state a much more general theorem, which concerns in effect a much more general operation – namely, the operation called "composition" in more modern terminology; but composition of an arbitrary number of ratios (in a given order).

First a point of terminological clarification: Euclid actually uses the same verb, *συντίθημι*, for the operation he defines as "composition", and for a second operation – the one we now call by that name; that is, if we think of ratios as functions, the operation that takes two or more functions in given order to the function that results from the successive application of those functions in that order. In deference to what has become, since Heath, the custom among English-writing commentators on Euclid, I shall call the latter operation "compounding" rather than composition.

Now, although Euclid does not formally define this second use of his *term σύνθεσις*, he does define the *operation* of compounding an arbitrary sequence of ratios. He calls the result δι' ἴσου λόγος – "a ratio *ex aequali*", in the standard rendering; and defines it as follows (Def. V.17): "A ratio *ex aequali* arises when, there being several magnitudes, and others equal to them in multitude, which taken two and two are in the same proportion, as the first is to the last among the first magnitudes, so is the first to the last among the second magnitudes; or, in other words, it means taking the extreme terms by virtue of the removal of the intermediate terms".

This, it should be noted, is a bit odd. It seems almost to confuse the notions of ratio and of proportion; for no "ratio *ex aequali*" is actually defined. The definition in fact introduces a term that serves Euclid essentially as an abbreviated reference to a theorem – namely to Proposition V.22: "If there be any number of magnitudes whatever, and others equal to them in multitude, which taken two and two together are in the same ratio, they will also be in the same ratio *ex aequali*". (The term "*ex aequali*" in the enunciation of the theorem itself plainly serves no purpose, except as an abbreviation or tag to describe the situation considered.) But the ratio that plays the central role here – that of the first to the last of the magnitudes – is just the ratio compounded of the ratios of the successive terms in each series, and the asserted result, "sameness of the ratio *ex aequali*", is nothing but the required independence of the operation of compounding from the particular magnitudes in terms of which the compounded ratios are presented. Finally, it should be noted that for the existence, in general, within a given magnitude-kind, of compounds of arbitrary ratios in that kind, what is required is the existence of common denominators;[12] thus, if we proceed on the view that discrete quantity is represented just by number, and continuous quantity by magnitude-kinds for which fourth proportionals always exist, compounding of arbitrary ratios will always be possible within a given species of quantity. (The possibility has not so far been ruled out that there are pairs of ratios that never hold respectively for two pairs of magnitudes all of the same kind; the result of compounding two such ratios would then be undefined.) It should be noted, in connection with the operation of compounding, that Proposition V.23 establishes the commutativity of this operation (a result that is by no means trivial; it is of course not true in general for the composition of relations or functions).

The compounding of ratios corresponds, of course, to what we call multiplication of real numbers. The question arises, did the Greeks have an operation upon ratios that corresponds to our "addition"?

The answer to this is a little ambiguous. The standard answer would be a simple negative. Now, it is certainly true that the Greek mathematicians never speak of adding ratios: *quantities* are added; and ratios are not, for them, quantities (see, however, the remark just below on Archimedes). But it is equally true that these mathematicians never speak of "multiplying" ratios; yet they do have the operation *we* call by that name, and they have a name for it. (The terminology of Archimedes here deviates from Euclid's; he avoids the ambiguity of using the same verb and its derivatives for two operations. Archimedes has more than one term for "compounding". It is somewhat curious to note that one of his expressions for "the ratio compounded of that of a to b and that of b to c" is simply "the ratio of a to b *and* [in Greek: καί] that of b to c" – which tempts one to think of compounding as a kind of "addition" of ratios. This notion fits with the terms "duplicate, triplicate, [etc.], ratio", and also with that, still current for us, of the "*mean* proportional".)[13]

For the addition of ratios in our sense, the Greeks have no name at all, and this justifies what I have called the standard negative answer to our question. Nevertheless, the operation of taking the ratio, to a given magnitude as "denominator", of a sum of magnitudes taken as "numerator", is of frequent occurrence; and one finds in Euclid just the theorem required to establish that the result depends only upon the respective ratios, to the common denominator, of the magnitudes summed to form the numerator – Proposition V.24: "If a first magnitude have to a second the same ratio that a third has to a fourth, and also a fifth have to the second the same ratio as a sixth to the fourth, the first and fifth added together will have to the second the same ratio that the third and sixth have to the fourth". In this not unimportant sense, then, the Greeks did have available the operation on ratios that we know as addition.

5. DEDEKIND ONCE AGAIN; THE CONTINUITY OF THE STRAIGHT LINE

To be able to answer the remaining question raised in the preceding section, we need something new.

It is possible to argue – I think convincingly – that one way to arrive at a definite answer is already implicit in Greek mathematics. I have suggested that 'continuous quantity' or 'magnitude' may be characterized for the Greeks by the existence of the fourth proportional; but although it is plausible to regard this as a distinguishing mark, for them, of magnitude in contrast to number, it cannot be considered as expressing what they meant by the "continuity" of magnitude (as opposed to the "discreteness" of number). It will be convenient, before considering the Greek conception further, to digress briefly to the modern conception.

In the year 1872 there appeared both the well-known little monograph of Dedekind on continuity and irrational numbers,[14] and a work, perhaps less well known to a philosophical audience, in which Georg Cantor sketched a treatment of essentially the same subject from a different point of view but with equivalent results.[15] In particular, both Cantor and Dedekind pointed out the necessity, for a complete theory of classical geometry, of introducing an axiom to assert what Dedekind called "the continuity of the straight line"[16] – namely, taking the line as ordered by the relation "to the left of", the principle that if the line is in any way divided into two parts, of which each point of one is to the left of each point of the other, then either the "lefthand" part has a rightmost point, or the "righthand" part has a leftmost point.

Now, such an axiom is by no means necessary for the geometry contained in Euclid's *Elements* – a fact already emphasized by Dedekind himself in the preface to the first edition of his monograph on the natural numbers.[17] Indeed, if **E** is the smallest subfield of the field of real numbers that is closed under the operation of taking the positive square root of a positive quantity, then analytic geometry over the field **E** admits all Euclidean constructions, and in it all the theorems of the *Elements* are true; but (of course) this geometry does not satisfy Dedekind's axiom of continuity. **E** itself, one should note, cannot be the subset of the reals corresponding to the full domain of ratios for

Euclid's geometry (even if it be taken to correspond to the full domain of ratios of straight line-segments) – for the ratio of the area of a circle to that of the square on its radius can be proved to exist (strictly on Euclid's principles); and this ratio does not belong to E, as Lindemann demonstrated in 1882. If, furthermore, we admit the existence of the straight line-segments aimed at (or implied by) the famous construction problems of classical geometry – implied, that is, by the existence of a cube double in volume to a given cube; an angle one-third of a given angle; (or more generally, an angle equal to an arbitrary "part" of a given angle); a square equal in area to a given circle (or, equivalently, a line whose length is equal to the circumference of a given circle) – one gets, in the first two cases, new ratios altogether, and in the third, new ratios of straight line-segments. It is unclear where this process should end.

On the other hand, there are reasons – suggestive, and, I think, plausible, even if not conclusive – for believing that the Greek geometers would have accepted Dedekind's axiom, just as they did that of Archimedes, once it had been stated. For instance, in the treatise *Measure of the Circle*, Archimedes admits the existence of a straight line-segment equal to the circumference of a given circle; and in the treatise *On Spirals*, he *proves* the existence of such a line. Namely, he proves the beautiful theorem (Proposition 18) that the tangent to his spiral at the endpoint A of its first turn meets the line through the origin O of the spiral and perpendicular to OA in a point whose distance from O is equal to the circumference of the circle of radius OA. But such a line does not exist in the geometry based upon the field **E**; so it is clear that Archimedes must make use, in his proof, of some mode of argument that transgresses the framework of the *Elements*. Now, the only point in the proof that can possibly be challenged is the assumption that the tangent in question exists. And it is hard to think of a natural principle upon which the proof of its existence can be based that is not equivalent to Dedekind's principle of continuity.

This geometrical consideration can be supplemented by a philosophical one, if we are willing to take Aristotle as authoritative here. Aristotle offers the following definition of the "continuous":[18] First, he defines "contiguous" as "next in succession, and touching". Then he declares continuity to be a kind of contiguity: "I call [contiguous things] *continuous* when the extremes that touch and hold them together become one and the same". This is undeniably a little vague, but it does suggest a warrant for arguments of the following type: "Let a half-line with origin O be divided into two parts, so that all the points of the first have a distance from O no greater than the circumference of a given circle, and all the points of the second have a distance from O no smaller than that circumference. Clearly, nothing stands between these parts; therefore they are contiguous. But the line is a continuous magnitude; therefore these parts have a common extremity".

To be sure, there is a weak point in this argument if we base it upon Aristotle's very words: one might object that the fact that "nothing stands between" the two parts does not establish their contiguity, since it does not establish that the two "touch". But it is hard to believe that Aristotle would countenance this objection. (On another count, Aristotle does reject the argument: in *Phys.* VII iv $248^{a}18$-$^{b}7$, he contends that a circular arc cannot be greater or smaller than a line segment, on the grounds that if it could be greater or smaller, it could also be equal. This, however, denies, not the validity of arguments of the type suggested above, but the truth of the premises in the example. Moreover, Aristotle's intimation that the possibility of "greater" and "less" implies that of "equal" can even be taken to support the view that the reasoning in the example is sound: that if the premises were correct, the conclusion would follow.)

It is worth putting on record here an ingenious argument, suggested to me orally by W. Tait, for Dedekind's principle as implicit in Greek geometry – an argument drawn after all from the *Elements* itself, namely from Definition 3 of Book I: "The extremities of a line are points". Clearly, this in no sense defines anything; but on Tait's reading, the "definition" is in effect a formulation of Dedekind's principle. In the argument given above, for instance, one considers the part of the line that is "closer" to O, and reasons thus: "That part is a *finite line*" (this is Euclid's only term for a "line-segment"; indeed, he ordinarily refers to such simply as a "line"); "therefore its extremities are points: namely, O, and a second point which can only be at a distance from O equal to the circumference of the circle".

I do not offer any of the foregoing considerations as more than plausible; but if the conclusion from any of them is accepted, it does follow that every real number corresponds to a Eudoxean ratio, indeed to a ratio of straight line-segments.

6. MAGNITUDE RECONSIDERED: FROM THE ANCIENT TO THE MODERN MODE

Dedekind, of course, aimed to make the theory of the real numbers, and of continuity and limits, fully independent of geometric intuition (or "geometric evidence").[19] It seems of interest to examine how far the Eudoxean theory itself can be made to serve this end.[20]

If it is to do so, it is absolutely necessary that the concept of magnitude be freed from dependence upon anything empirically given. Thus, in the spirit of modern mathematics (with explicit acknowledgment that we here take a step that is foreign to the Greek view), we make of the conditions earlier laid down for a species of quantity a *definition* of the notion of such a species. By a species of quantity, then, we henceforth understand any domain and binary operation on that domain satisfying the conditions (1)–(5) of Section 2 above together with the Axiom of Archimedes.

It has already been pointed out that the Eudoxean ratios instantiated in any species of quantity for which common denominators exist allow the mode of composition we now call "addition"; and even that this mode of composition was known (in effect) by the Greek mathematicians. It is trivial to verify that this operation on any such ratio-domain satisfies all our conditions for a species of quantity. (Once again it should be emphasized that the Greeks did not regard ratios as quantities. Nevertheless, their ratios formed, in the indicated sense, a domain – or domains, when we consider various classes of ratios – of quantities, in our present sense of the term.)

In particular, then, the ratios of numbers form (with the indicated operation) a species of quantity; and in accordance with modern usage, we call this the domain of "positive rational numbers". The ratios of such rational numbers are of course themselves already ratios of whole numbers, hence are themselves rational numbers.

Now we may, following Dedekind (lightly modified), consider the domain of all *cuts* in the system of positive rational numbers. This domain can be given the structure of a species of quantity (indeed, of magnitude in the strongest possible sense of the term), by introducing a suitable operation of "addition of cuts": the "upper set" of the sum of two cuts is, by definition, the set of sums of elements belonging respectively to the upper sets of the summands. It is easy to see that this procedure does indeed define a new Dedekind cut (in the sense of the discussion in Section 3 above, including the provision that the upper set contain no smallest element); and further, that the system of all these cuts, under this operation, constitutes an Archimedean species of quantity or magnitude-kind. Let us call this magnitude-domain $\mathbf{D}^+$ (the 'positive Dedekind domain'). If, now, we consider a particular magnitude d in $\mathbf{D}^+$, and we let $\mathbf{1}$ be the Dedekind cut whose lower set consists of all the positive rational numbers ≤ 1, then it turns out that the Dedekind cut that corresponds (in the sense of Section 3 above) to the ratio $d:\mathbf{1}$ is just d itself; so indeed every cut in the system of positive rational numbers "is" (that is, corresponds to, or "determines") a ratio – and beyond this, corresponds to a ratio of elements of one particular magnitude-kind, fixed once for all: the domain $\mathbf{D}^+$. On the other hand, because the elements of $\mathbf{D}^+$, by virtue of what we have now established, correspond one-to-one to the system of all Eudoxean ratios, and by a correspondence that is easily seen to take sums to sums, we are led to the conclusion that the domain of all Eudoxean ratios, with its "natural" operation of addition, itself constitutes an Archimedean magnitude-kind possessing Dedekindian continuity; all Eudoxean ratios of quantities of any Archimedean species occur as ratios of magnitudes of this one kind.

In effect, therefore, with the help both of our more abstract and general notion of a species of quantity, and of Dedekind's construction applied to the rational numbers, we have established the identity of the system of Eudoxean ratios with the system $\mathbf{R}^+$ of the positive real numbers; for since the operation of "compounding" is available for the ratios, alongside the operation of addition, the structure obtained is not only that of a positive ordered semigroup, as required for a species of quantity, but that of a positive ordered "semifield": that is, it is the structure of the positive elements of an ordered field, and moreover of a "complete" ordered field in the modern algebraic sense.[21]

7. REFLECTIONS ON THE DECLINE OF GREEK MATHEMATICS

Certain comments about the gap between Greek and modern mathematics – in particular, about aspects of the Greek point of view that made developments like those that took place in the seventeenth century essentially impossible for the Greeks – occur in the literature frequently enough to have become clichés. It seems worthwhile here to reconsider some of these.

Perhaps the central issue is whether the Greek distinction between number and magnitude, and the associated fact that whereas numbers can be both added and multiplied, magnitudes can only be added, constituted an essential handicap – overcome in the late Renaissance by the more casual, one might say more "Babylonian" attitude that then prevailed.

Now it is important, in discussing a matter of this kind, to keep clearly in mind a certain peculiarity of the question. It is a peculiarity related to the topic of anachronism – itself one on which I believe that historians of science and mathematics often make too hasty judgments. In respect of anachronism, the issue becomes joined when the suggestion that some investigator possessed a certain conception or arrived at a certain result (characterized, perhaps, in the terminology of a later period) is dismissed with the argument that that is impossible, because that conception or result was unknown at the time. I hope it is clear to the reader, when it is stated thus baldly and abstractly, that to argue so is just to beg the question: if, for instance (as I happen to have argued myself, and to believe firmly), Newton possessed a conception that can be characterized in our own terms as that of a "field of force", then the objection that that is impossible because such a conception was unknown at the time will be simply false. An analogue in the present case is presented by the question whether the Greeks possessed a notion equivalent to the concept of a certain subsystem of the positive reals that contains the positive integers and is closed under addition, multiplication, and extraction of square roots. (This is, of course, not the same as possessing the conception of such a system *as* a subsystem of the positive real numbers: the latter, but not the former, presupposes the conception of the totality of positive real numbers.) *This* question, it seems to me, can be answered decisively in the affirmative: for the considerations already presented show that the Eudoxean ratios, with the operations on such ratios known to (although not always emphasized, or even named, by) the Greek mathematicians, constitute such a system.

But suppose we now raise the question, "*Could* Greek mathematics possibly have attained to the conception of the full system of real numbers (or of positive real numbers)?" In this case, there is no doubt that Greek mathematics did not in fact attain to that conception.

Therefore, in this case, the objection of anachronism does not beg the question, but is quasi-irrelevant to it – this is the peculiarity, and the somewhat delicate point. What we are interested in here is something like "accessibility": an estimate of how far the Greeks were from a certain conception, how much they would have had to do to attain it. And here the negative argument that one encounters, related to that against anachronism, is an appeal to what may be called the "spirit" of Greek mathematics: e.g., that the allegedly anti-calculational bias of the Greek mathematicians, in contrast to that of the Babylonians or that of the Renaissance, makes such a step impossible for them.

The point is a genuinely delicate one, and in its nature not susceptible of definitive factual resolution, on the basis of either empirical or logical evidence. I am concerned at this point to urge the view that such an issue is to be treated deliberately *as* delicate, not as simple; to plead therefore on behalf of caution with regard to judgments either way, and of openminded willingness to reflect upon alternatives; bearing always in mind that we are speaking of possibilities, not of what may have been, but only of what *might* have been.

The question of the possibilities inherent in the Greek mathematical tradition is closely tied to the question of the reasons for the decline of that tradition. The reigning view, since the great work of Zeuthen on Greek mathematics, has been that that decline was in part the consequence of inherent limitations, which Zeuthen himself summarizes as follows: (1) The care for unassailable rigor tended to conceal "whatever might facilitate the initial approach to questions, permit them to be grasped at a glance, or make clearer the aim of each operation". (2) (a) The ingenious geometrical form in which the Greek mathematicians represented what we describe as algebraic relationships was limited to relationships of at most the third degree; moreover, (b) although this "geometric algebra" is (within its limits) a perfectly clear and convenient instrument for anyone who has mastered it, the pedagogical problem of communicating its principles to a pupil is very difficult except in face-to-face oral presentation – its written representation is very difficult for the neophyte to follow. (3) Besides these two "defects that chiefly concern the form [of exposition]", there was another "related to the very foundation"; namely, "the Greek mathematicians had so high an idea of their scientific dignity that they excluded from their classic works whatever did not seem to them perfectly rigorous", and "in consequence actual numerical calculations that could not regularly furnish more than an approximation were excluded, and relegated to a less esteemed science, logistic".[22] Heath does no more than echo Zeuthen's view, with less subtlety:

[T]he further progress of geometry on general lines was practically barred by the restrictions of method and form which were inseparable from the classical Greek geometry .... [T]he Greeks could not get very far ... in the absence of some system of coordinates and without freer means of manipulation such as are afforded by modern algebra, in contrast to the geometrical algebra, which could only deal with equations connecting lines, areas, and volumes, but involving no higher dimensions than three, except in so far as the use of proportions allowed a very partial exemption from this limitation .... The restriction then of the algebra employed by geometers to the geometrical form of algebra operated as an insuperable obstacle to any really new departure in theoretical geometry.[23]

We have already seen, however, that the Greek theory of proportion in fact contained the means by which quite arbitrary algebraic relationships could have been represented: for products and powers could be (and were) represented by compounding of ratios, and sums could equally have been represented by an operation on ratios (whatever terminology might have been introduced for this operation). Moreover, that such operations stood in clear correspondence with those of the geometric algebra, and also with those of ordinary arithmetic, was certainly not unknown to the Greeks. It is true that they distinguished the operations of multiplication of numbers, of forming the rectangle of two lines, and of compounding two ratios; but they knew perfectly well the connections among these operations. This is even reflected in their numerical terminology: for a product of two numbers is called a "plane" number, and its factors are called its "sides".[24] It is true, as Zeuthen remarks,[25] that the terminology of the theory of proportion makes the general representation of algebraic relationships awkward; but he adds, with his usual good sense, that this was in itself no "insuperable obstacle": what would have been required was a suitable symbolism (or possibly some perspicuous alternative), and there was no objection in principle by the Greek mathematicians to the use of symbols.[26] Zeuthen concludes that "to understand why no such extension of the Greek algebra occurred, we must envisage what the Greeks had gained in respect of theory by attaching algebra to a geometric representation and to the theory of proportion"; and goes on to discuss the fundamental difference, for the Greeks, between numerical – which is to say, rational – relationships, and operations upon incommensurable quantities. But this seems to me off the point. The issue is not whether one ought to *confuse* numerical (that is to say, integral or rational) operations and relations, on the one hand, with those involving incommensurable magnitudes, on the other – of course, one should not! – but whether the Greeks were in a position to develop a general theory that would *subsume* all such relations and operations. As I have tried to make clear, the Eudoxean theory of proportion, with the Archimedean complement, is in fact such a theory. What has to be explained is why the Greeks failed to exploit this potentiality of a theory they actually possessed.

As to Zeuthen's remark that "actual numerical calculations that could not regularly furnish more than an approximation" were excluded, by the Greek mathematicians, "from their classic works", it has to be taken with some caution. In the first place, construed quite literally, it is simply not true. Among the "classic works" of Greek geometry at least two concern themselves explicitly with approximations: Archimedes' *Sand-reckoner* and his *Measure of the Circle*. If the approximation in question in the former work is fanciful, and has moreover the character of a *tour de force* (the problem Archimedes claims to solve being that of actually naming a number greater than the number of grains of sand that would fill the universe), the work nevertheless contains not only an explicit treatment of the problem of devising a systematic notation capable of representing extremely large numbers in a modest space, but also a discussion of an *experimental, instrumental* method of determining upper and lower estimates of a quantity of great astronomical importance – namely, the apparent diameter of the sun. As to the *Measure of the Circle*, its whole aim is to give a serviceable rational approximation, again in the form of upper and lower estimates, to the ratio of the circumference of a circle to its diameter; thus it consists precisely in "numerical calculations", leading to a result useful in further calculations, that "could not furnish . . . more than an approximation". Moreover, the calculations carried out proceed by using analogous (upper and lower, rational) estimates of the square roots of integers (including rather large integers).

The one sense in which Zeuthen's statement may be regarded as correct is that, although Archimedes uses numerical approximations, and with the help of a geometrical construction and argument derives from them the approximation he is seeking to a fundamental geometric ratio, he does not explain the methods by which the estimates employed along the way – the approximations to square roots – were actually obtained. In other words, it may fairly be claimed, not that numerical calculations or approximations do not occur in the"classical" mathematical works, but that those works do not systematically discuss numerical methods. This, however, serves to prove the weakness of the point; for what decayed, after the great age that terminated with Apollonios, was the creative tradition of Greek *geometry* – the command of numerical methods was not in fact lost, as the work of the great Greek astronomers down through Ptolemy shows conclusively.

Of the considerations adduced by Zeuthen, the one that seems most convincing as an obstacle to the continuation of the mathematical tradition is the difficulty confronting any student who attempts to master the Greek "algebra" on the basis of the written expositions alone, without the direct guidance of a teacher who is himself a master of the subject.[27] There are two reasons, however, why this cannot be regarded as a primary explanation for the decline of the tradition – one, so to speak, intrinsic, and one a matter of historical fact. The first is implicit in a statement of Zeuthen's own. Speaking of the limitation of the geometric algebra to representation of small exponents, he remarks:[28] "We cannot rest upon the fact that this limit did exist and that there was no particular prospect of surmounting it, *after* the time of decline had begun: we require positive grounds to explain why that limit was not surmounted already in the flourishing time, whether by the development of a symbolism (even a rudimentary one), or perhaps in some other way". Clearly the difficulty of learning without a master became a fundamental problem only when masters were no longer available: while the tradition flourished, it did so in spite of that difficulty.

The second reason why the explanation cannot be regarded as a basic one lies in the simple fact that for centuries after Apollonios of Perga – called "the Great Geometer", and by common consent the last of the truly great Greek geometers – the geometric algebra was quite competently handled by generation after generation of "epigones".[29] On the other hand, the great new flourishing of mathematics and its application to the physical sciences that eventually occurred in Europe after a long period of total eclipse of the tradition, was closely connected with the recovery and mastery of the great documents of Greek geometry: the texts of Euclid, Archimedes, and Apollonios.[30] Thus neither the actual historical circumstances of the decline nor those of the subsequent renascence of the creative development of mathematics support the view that the sheer technical difficulty of the Greek methods was of primary importance.

I should like, however, to suggest another aspect of the point about personal contact with a master that I think has some claim to be considered as an essential factor in this connection – not, to be sure, as a "cause" of the decline, but as a condition that contributed to the likelihood of such a thing. Even in the post-Renaissance period – even in our own time, with its unprecedented flourishing of mathematics – it has been extremely rare for a creative mathematical talent to develop without the stimulus of first-rate teachers, and of teachers who themselves are productive mathematicians: we have not had many Ramanujans. One ought to consider how much more this would have been so in a period when there was not only nothing like the system of university careers we now have, but when written treatises themselves could have been available only to very few. And then one should consider the question, What are the conditions required for the stable, reliable propagation through time of a productive mathematical tradition? It seems clear enough that a necessary condition for this is the existence of a mathematical community – by which term I mean a sufficient number (I do not say how many is sufficient) of creative mathematicians, in communication with one another and interested in one another's work; and a considerably larger audience, also interested in the subject, who will provide, in some way or other, a base of economic support for the enterprise and a pool of students large enough to make probable a community of the same complexion in the next generation.

Now, it is evident that these conditions were realized for a period of perhaps two and a half, perhaps as long as nearly four centuries, among the Greeks.[31] That in itself is very remarkable – the development *ab ovo* of such a mathematical culture is, indeed, a unique occurrence in human history, and stands in the development of human knowledge in something like the position of the appearance of a new species in biological evolution. (This claim may seem to ignore the fact that mathematical knowledge existed in other, and earlier, cultures – for instance, in that of Mesopotamia, whose direct influence upon the Greeks in stimulating their mathematics now seems highly probable. I shall return to this point presently.) But it is well known to evolutionary biologists that the ordinary fate of a species is extinction. So – to pursue the analogy – we have to ask both how substantial was the breeding population (the mathematical community I have referred to above), and how stable was its ecological niche.

Having suggested a series of questions about both the historical and the theoretical sociology of mathematics – What are the conditions for a stable creative community, and to what extent were these conditions realized among the Greeks? – I have to confess myself unqualified to offer expert answers to them (although I shall offer a tentative guess). But I wish now to present some reflections, first upon the mathematics of the Babylonians, and then upon the works of Archimedes, that bear upon these questions.

There seems considerable likelihood, as I have remarked above, that the interest of the Greeks in mathematics was stimulated by contact with the Mesopotamian tradition, which, as we now know, included not only a quite remarkable mastery of algebraic techniques, but also a fund of geometrical knowledge – including, for instance, knowledge of what we call the "Pythagorean Theorem".[32] There are, in point of our present concerns, three salient features of that tradition. First, the literature of the tradition gives us no information whatever concerning the methods by which this knowledge was obtained. Neugebauer divides this literature into two classes – "table texts" (e.g., multiplication tables and tables of reciprocals) and "problem texts" (containing either lists of numerically posed problems alone, or also instruction in the procedures for solving those problems). The problem texts in particular, insofar as they contain methods for solving the problems, are purely prescriptive – "recipe" or "cookbook" methods. Thus, for instance, we do not know whether the Pythagorean Theorem was known on the basis of an argument from more evident principles, or whether it was inferred empirically from the measurement of simple right triangles; we do know, on the other hand, that at least those pedagogical materials that have been preserved and studied by our historians reveal no interest at all in communicating the evidence for the correctness of the procedures inculcated. Second, the knowledge itself seems to have been cultivated entirely for some sort or other of practical application (priestly, commercial, agricultural). Theoretical notions such as from very early times fascinated the Greeks – e.g., the distinction of prime and composite numbers, or that of commensurable and incommensurable magnitudes – are entirely lacking. The Babylonians possessed a very good approximation to the value of the square root of 2; for π they ordinarily used the very crude value 3, but they also had the better one 25/8; in neither case, however, is there shown any interest in the nature of the precise value that is thus "approximated".

The third salient feature of the Babylonian tradition has, I think, an evident connection with the preceding two. I quote Neugebauer:[33]

> . . . [T]he texts on which our study is based belong to two sharply limited and widely separated periods. The great majority of mathematical texts are "Old-Babylonian"; that is to say, they are contemporary with the Hammurapi dynasty, thus roughly belonging to the period from 1800 to 1600 B.C. The second, and much smaller, group is "Seleucid", i.e. datable to the last three centuries B.C. . . . The more than one thousand intervening years influenced the form of signs and the language to such a degree that one is safe in assigning a text to either one of the two periods.
>
> So far as the contents are concerned, little change can be observed from one group to the other. The only essential progress which was made consists in the use of the "zero" sign in the Seleucid texts . . . . It is further noticeable that numerical tables . . . were computed to a much larger extent than known from the earlier period, though no new principle is involved which would not have been fully available to the Old-Babylonian scribes. It seems plausible that the expansion of numerical procedures is related to the development of a mathematical astronomy in this latest phase of Mesopotamian science.

These characteristics of Babylonian mathematics may be summed up in the statement that, for the Babylonians, mathematics was not an *enterprise*, but a lore and a skill possessed by the priestly or scribal class, for use in essentially administrative functions, and passed on from generation to generation much as were the techniques of the craft guilds in the middle ages. Of course, the techniques had to have been discovered; and innovations might occur from time to time; but invention or discovery was no more the business of those trained in the lore, than it was the business of the medieval master of a craft. That the tradition preserved no historical record of discoveries should, I think, be seen as a correlate of the fact that it showed no interest in new discoveries.[34]

It is sufficiently evident that the situation of Greek mathematics was radically different. It is this unprecedented difference that I have earlier characterized as a unique occurrence in human history (for of course the mathematical tradition of the modern period built upon the recovered knowledge of that of the Greeks). But to see vividly just how different it was, some consideration of the works of Archimedes will be useful. It is above all the prefatory epistles to Archimedes's treatises that are most instructive here. These prefaces seem to me to have received less attention than they deserve in the historical literature – and, when attended to, to have been inadequately appreciated, even misunderstood. I believe that they provide precious information about the nature of the community of Greek geometers at the peak of its achievements, just before the decline set in.

Note first that, of the great original documents of Greek mathematics preserved to us – the *Elements* of Euclid, the works of Archimedes, and the *Conics* of Apollonios – the works of Archimedes alone are primarily expositions of original investigations (for although Apollonios was unquestionably a geometer of great originality, who contributed profoundly to the subject he expounds, the intention of his treatise is encyclopedic: it is a *summa* of conics, incorporating the results of more than a century of research).

Most of the works of Archimedes that have been preserved take the form of epistles to other mathematicians, introduced by general remarks about their contents; remarks that relate those contents in some way to the wider context of inquiry.[35] A most striking characteristic of these introductory remarks is the strong desire they manifest on Archimedes's part not merely to contribute to mathematical knowledge, but to stimulate investigation by others. This is evident above all in the famous *Method*, whose whole purpose is to explain a heuristic technique which Archimedes has found valuable in his own researches. It is worth quoting at some length from the introduction to that work, addressed to Eratosthenes (I use here the version of T. L. Heath; passages in brackets correspond to restorations of the Greek text by Heiberg, its discoverer, in places where the palimpsest manuscript was illegible):[36]

> Seeing . . . in you . . . an earnest student, a man of considerable eminence in philosophy, and an admirer [of mathematical inquiry], I thought fit to write out for you and explain in detail . . . the peculiarity of a certain method, by which it will be possible for you to get a start to enable you to investigate some of the problems in mathematics by means of mechanics. This procedure is, I am persuaded, no less useful even for the proof of the theorems themselves; for certain things first became clear to me by a mechanical method, although they had to be investigated by geometry afterwards because their investigation by the said method did not furnish an actual demonstration. But it is of course easier, when we have previously acquired, by the method, some knowledge of the questions, to supply the proof than it is to find it without any previous knowledge. This is the reason why, in the case of the theorems the proof of which Eudoxus was the first to discover, namely that the cone is a third part of the cylinder, and the pyramid of the prism, having the same base and equal height, we should give no small share of the credit to Democritus who was the first to make the assertion with regard to the said figure though he did not prove it. I am myself in the position of having first made the discovery of the theorem now to be published [by the method indicated], and I deem it necessary to expound the method partly because I have already spoken of it and I do not want to be thought to have uttered vain words, but equally because I am persuaded that it will be of no little service to mathematics; for I apprehend that some, either of my contemporaries or of my successors, will, by means of the method when once

established, be able to discover other theorems in addition, which have not yet occurred to me.

This passage beautifully illustrates the counterpart of a point made above concerning the absence of any interest in the history of the subject in the Babylonian literature: Archimedes, clearly, is interested in the past *because* he is interested in the future – the relation of Demokritos's contributions to those of Eudoxos illustrates, he says, an important feature of mathematical progress. The passage shows, moreover, that Zeuthen (writing before the discovery of this treatise) was not entirely right when he spoke of the Greeks' concern for rigor as leading them "to conceal whatever might facilitate the initial approach to questions". Of course, Zeuthen's comment remains true, insofar as the main body of Greek mathematical exposition does not address heuristic concerns. But the existence of Archimedes's treatise not only constitutes an exception to that general rule – it also shows that the Greek spirit of rigor was not incompatible with an impulse toward heuristic exposition; so that (one may say) if we had only had another Archimedes, we might have had more such works. And when one considers that the method of Archimedes involved not only considerations about centers of gravity (which is what makes it "mechanical"), but also the analysis of areas into "sums" of *lines* and volumes into "sums" of *planes* – in short, the techniques codified in the seventeenth century by Cavaliere as the "method of indivisibles" – the potential fruits of the systematic elaboration of these ideas can be seen to be rich indeed.

The compatibility between the heuristic and the rigorous, clear enough in the passage quoted, is expressed again, rather poignantly, after the "mechanical" discussion of the first proposition, where Archimedes comments[37] that the proposition "is not actually demonstrated by what has been said; but [the latter] has created a certain impression [or "appearance"] that the conclusion is true. We, therefore, seeing that it has not been demonstrated, but surmising the conclusion to be true, shall arrange[38] the geometrical demonstration that we ourselves have discovered and published". This surely displays a very clear – and sophisticated – appreciation, on the one hand of the power and importance of heuristic procedures, and on the other of the difference between a well-motivated conjecture and a proof.

Both that awareness, and the concern to stimulate mathematical investigation (most especially the latter), are shown not only in the

Method, but in a number of the epistles already mentioned. In these epistles, the name of Konon several times occurs: Archimedes laments his early death, and pays repeated tribute to his mathematical talent in general, and in particular to the progress that might have been expected if he had lived. It seems worthwhile to quote from two of these; and first, from the introduction to *On the Sphere and the Cylinder*:[39]

Archimedes to Dositheos greeting.

I have previously sent you, of the propositions that had been examined by me, the following, written down together with its proof: that any segment bounded by a straight line and a section of a right-angled cone[40] is four-thirds of the triangle which has the same base with the segment and equal height. Subsequently, certain important[41] theorems have occurred to me, and I have worked out their proofs... [*Here follows a statement of the principal results of the present treatise – author.*] These properties pre-existed by nature in the figures mentioned, but remained unknown to those who before us were occupied with the study of geometry – none of them having perceived that these figures have a common measure; I therefore have not hesitated to place [my results] side by side with those which have been treated by other geometers, and with those that appear by far the more important of the ones concerning solids examined by Eudoxos, namely, that any pyramid is the third part of the prism which has the same base with the pyramid and equal height, and that any cone is the third part of the cylinder which has the same base with the cone and equal height. For these properties having pre-existed by nature in these figures, it yet happened that they were unknown to all the many geometers worthy of mention who lived before Eudoxos, nor had been perceived by one of them. Now, however, it will be open to those with ability to examine these [propositions of mine]. They ought to have been published while Konon was still alive, for I should conceive that he would best have been able to grasp them and to pronounce upon them the appropriate verdict; but, as I judge it well to communicate them to those who are conversant with mathematics, I send you the proofs I have written, which it will be open to those versed in mathematics to examine. Farewell.

I want to call attention to two features of this passage. First, it makes evident the importance to Archimedes of the existence of a community of mathematicians, competent to study his results and to judge them; a judgment he seeks with wonderful simplicity and openness. (A student in one of my classes commented, I think very justly, when I read this passage aloud, "But that's amazing – he's asking for peer review!") Second, there is the striking, reiterated phrase about properties that "pre-existed by nature in the figures" but that remained long unknown. Dijksterhuis, commenting on this phrase, says:[42]

Archimedes here seems to voice his astonishment that geometrical figures may have remarkable properties inherent in them, i.e. without their being stated in the definition we give of them, which properties may long remain unnoticed, in spite of their simplicity.

It is the typical mathematician's astonishment at the unsuspected intrinsic wealth of his own definition which is being expressed here.

It seems to me that Dijksterhuis has this almost upside down. In the first place, there is no indication on Archimedes's part that the figures themselves *depend*, in some way, upon their definition; on the contrary, he takes their own objective existence for granted: the properties *pre-existed in the figures by nature*. And in the second place, it seems evident to me that what Archimedes is calling attention to is not how astonishing it is that these properties could ever have been unknown, but is, on the contrary, the remarkable fact that such properties, not immediately evident, can be (have been) *made* evident (and open to public scrutiny and judgment) by mathematical demonstration. Thus, although the passage does not explicitly repeat the reference to discoveries yet to be made (by "some, either of my contemporaries or my successors") that we have seen in the *Method*, I think the same idea is implicit in the passage: "the properties have always been there; some that were hidden were made manifest by our forebears; some were previously made manifest by me; since then I have succeeded in discovering more . . ." – the implication for the future seems clear.

That implication is explicit, together with Archimedes's characteristic desire to stimulate research, in the remaining passage I wish to quote, which is taken from the opening of the treatise *On Spirals* (again addressed to Dositheos):[43]

Archimedes to Dositheos greeting.

Of the theorems which I sent to Konon, of which you continually ask me to send you the proofs, most are already before you in the books brought to you by Herakleides; I send you certain others written out in the present book. Do not be surprised at my having long delayed to publish the proofs of these theorems; this has been owing to my wish to present them first to persons engaged in mathematical studies who prefer to investigate them for themselves. In fact, how many theorems in geometry which have at first seemed inaccessible are in time successfully worked out! Now Konon died before he had sufficient time to investigate the theorems referred to; otherwise he would have discovered and made evident all of them, and would have advanced geometry by many other discoveries besides. For we know that it was no common ability that he brought to bear on mathematics, and that his industry was extraordinary. But, though many years have elapsed since Konon's death, I do not find that any one of the problems has been attacked by a single person. I wish now to put them in review one by one, for it happens that there are two included among them that I myself have been unable to bring to a satisfactory conclusion; so that those who claim to discover everything but produce no proofs of the same may be confuted as having actually pretended to discover the impossible.

What is most fascinating here is the remark about the false propositions included among those originally, and long since, communicated. Those commentators I have encountered who discuss this point take the view that these propositions were deliberately circulated by Archimedes as a trap to catch pretenders. Thus Zeuthen refers to these propositions as *Vexieraufgaben* (that is, "trick questions").[44] Again, Dijksterhuis, after noting Archimedes's statement that he "preferred to leave it to mathematicians to find out things for themselves", adds: "There was also some malignant design in this, for on the same occasion he reveals that two of the propositions on the sphere formerly enunciated by him are incorrect, and that he had added them in order to entice those who are always saying of everything that they have found it, without ever giving proofs, into saying that they had discovered something impossible".[45] In the same vein, van der Waerden tells us that Archimedes, "in order to trip up his conceited Alexandrian colleagues ["*um seinen eingebildeten alexandrinischen Kollegen ein Bein zu stellen*" – literally, "to put a leg out" to them] would intersperse false theorems here and there, 'so that those who claim to have discovered everything themselves, but without supplying the proofs, might for once be caught in a trap, by claiming to have found something impossible'".[46]

Now, it seems to me that this interpretation of the passage is by no means the only possible one; and indeed, if we accept Heiberg's reading of the text itself, it even seems an impossible one. For on that reading, Archimedes calls the false propositions ones that *he himself* was unable to bring to a satisfactory conclusion (i.e., to prove) – pointing out in the sequel that they can in fact be refuted by appeal to other results of his own. But this seems, on the face of it, to mean that Archimedes had circulated propositions, at least some of which were as yet uncertain conjectures; that he had later discovered the incorrectness of some of these; and that he wants this to serve as an object lesson that rigorous proofs alone can establish mathematical results, and that those who make assertions without supplying proofs run the risk of being exposed as charlatans.

The alternative I have just proposed is, however, far from certain; for Heiberg's reading of the crucial words is itself conjectural.[47] Nonetheless, and even if Archimedes did set a deliberate trap, the implication of malice or spite seems to me quite out of keeping with the whole spirit of the remarks in all the passages I have quoted expressing Archimedes's interest in encouraging others to new discoveries. And a further suggestion of van der Waerden's on this point appears quite astonishing. He asks, immediately after the passage quoted at the end of the paragraph before last, "Might this gibe [that is, about being tripped up, or caught in a trap] be aimed at Eratosthenes? One would be inclined to believe so, when one reads the ironic-admiring introduction to the *Method*, which is addressed to Eratosthenes." I myself see no evidence of irony in the words quoted earlier from the introduction to the *Method*: in the first place, the words do not of themselves betray irony (so that if such were intended, it would have somehow to be inferred from the circumstances of the communication); and beyond this absence of positive evidence for van der Waerden's interpretation, it is very hard to understand why Archimedes would take the trouble to write out a whole treatise on methods of discovery, and then send it to a man he did not in fact regard as a capable investigator. (The mathematicians of the seventeenth century, by contrast, when they wished to score off their competitors, would conceal not only their methods of discovery, but their very theorems, by communicating only counts of the letters occurring in the formulation of their solutions to problems, and leaving it to their correspondents to solve these strange anagrams.)

Let me summarize the main point of what has grown to a rather lengthy discussion of texts. Not only does Archimedes's actual work disclose mathematical inventiveness of the highest order; the way in which he presents that work, the way in which he distributes it, shows an intense interest in soliciting both the judgment of a mathematical community, and the participation of that community in a continuing endeavor to advance the subject. His attitude is the very opposite of one that seeks to *complete* – to round off and put an end to – the subject. To make this point even clearer, it would be tempting to discuss in some detail the kind of new invention that Archimedes's works display; but that would be the subject of another paper. Let me, however, at least mention that in his treatment of the area of curved surfaces, in the treatise *On the Sphere and the Cylinder*, Archimedes introduces a set of *new postulates*, which serve in effect – for the first time – to *define* a notion of area for a class of such surfaces (namely such as are "concave always toward the same side" – or, put in fairly modern terms, such as have everywhere non-negative Gaussian curvature). In fact, it is also in this place that there are introduced – again for the first time – principles that determine a concept of *length* for a class of curved *lines* (once more, such as are concave towards the same side).[48] It clearly follows that Archimedes did not regard the foundations of geometry as having been closed by Euclid's codification of the "elements", but saw both new conceptions and new assumptions as within the competence of mathematical investigators to propose.

Now, this intense spirit of research, and the desire to communicate that spirit, is just what I have earlier noted as in all ages of creative mathematics that we know of, an essential influence – and one that operates preponderantly through personal contact. But how rare this must have been in the time of Archimedes himself is clear from the fact that so few names occur as eligible correspondents or members of his audience; and from the fact, mentioned by him, that none of the problems he had circulated had, to his knowledge, even been attacked by anyone else. (This last circumstance, by the way, seems to tell against the assumption that there was a flock of braggarts who always claimed to have solved every problem, and whom, personally, Arhimedes aimed to trip up: for if there had been, one would surely expect that some of these would have taken the bait.) Thus, finally, I return to the point that the stability of an environment conducive to the flourishing of mathematics in antiquity must perforce have been slight; that one need not seek – and has not in fact found – "internal" causes of the decline of the tradition. The wonder is rather that such a spark was once struck, that the flame burned with great splendor through several generations – and that, having died down, it was never fully extinguished, but was rekindled again after more than a millennium and a half.

8. ON THE TREATMENT OF RATIO AND PROPORTION BY ARCHIMEDES

While ruminating upon the subject of the last section – originally intended as a set of brief concluding remarks – I happened upon an instance both of scholarly failure, and of what seems to me a characteristic and fundamental misunderstanding of the relation between ancient Greek and modern mathematical conceptions; between, so to speak, Eudoxos and Dedekind. Since the author of the mistake in question is the eminent scholar E. J. Dijksterhuis, and its subject is Archimedes, the point calls for some detailed attention.

In the course of a chapter on 'The Elements of the Work of Archimedes', Dijksterhuis discusses in a single section a series of lemmas from several of Archimedes's treatises; and, in particular, the following – Proposition 1 of *On Conoids and Spheroids*:[49]

Given four series of magnitudes, all equal in number – e.g.:

> A, B, Γ, Δ, E, Z
> H, Θ, I, K, Λ, M
> N, Ξ, O, Π, P, Σ
> T, Υ, Φ, X, Ψ, Ω

> – if the magnitudes of the first series have among themselves, two by two, the same ratios as do the correspondingly placed magnitudes of the second series; and if the magnitudes of the first series have, to the correspondingly placed magnitudes of the third, the same ratios as do those of the second, respectively, to those of the fourth; then the sum of the magnitudes of the first series will have to the sum of those of the third the same ratio as the sum of the magnitudes of the second series to the sum of those of the fourth.

In other words: given the series of magnitudes named above, if A:B = H:Θ, B:Γ = Θ:I, etc.; and if A:N = H:T, B:Ξ = Θ:Υ, etc.; then the sum of the magnitudes of the first row has to the sum of those of the third the same ratio as the sum of the magnitudes of the second row to the sum of those of the fourth.

The argument of Archimedes runs thus: By reasoning *ex aequali* – i.e., by compounding of ratios – we have N:Ξ = (N:A)(A:B)(B:Ξ) = (T:H)(H:Θ)(Θ:Υ) = T:Υ, and similarly Ξ:O = Υ;Φ, etc. Then we have the following: the ratio of the sum of the magnitudes of the first row to its first member, A, is the same as that of the sum of the magnitudes of the second row to its first member H (Archimedes states this conclusion without citing any grounds); whereas the ratio of A to N is the same as that of H to T and the ratio of N to the sum of the magnitudes of the third row is the same as that of T to the sum of the magnitudes of the fourth row. (Note that this last assertion stands in the same relation to the conclusion of the first step above – the argument *ex aequali* – as the conclusion about the ratio of the sum of the first row to its first member stands to the first series of proportionalities posited by the hypothesis of the proposition.) The conclusion of the proposition now follows directly, *ex aequali*.

That is the argument, exactly as Archimedes gives it.[50] Dijksterhuis, however, gives us something significantly different. I shall quote the main part directly from his book. To follow this, one must know that he uses the notation "(x, y)" – rather than "x:y" – for the ratio of magnitudes x and y; and that he has labeled "(1)" the first part of the hypothesis of the proposition – namely, that (in his notation) (A, B) = (H, Θ), (B, Γ) = (Θ, I), etc; by "(2)" the second part of the hypothesis – that (A, N) = (H, T), (B, Ξ) = (Θ, Υ), etc.; and by "(3)" the first conclusion drawn *ex aequali* – that (N, Ξ) = (T, Υ), etc.

After stating the first of the arguments *ex aequali* in the proof given by Archimedes, Dijksterhuis proceeds as follows:

We therefore have on the one hand, by (1):

$$(\mathrm{A}, \mathrm{H}) = (\mathrm{B}, \Theta) = (\Gamma, \mathrm{I}) \ldots = (\mathrm{A} + \mathrm{B} + \ldots \mathrm{Z},\ \mathrm{H} + \Theta + \ldots . \mathrm{M})$$

on the other hand, by (2) and (3):

$$(\mathrm{A}, \mathrm{H}) = (\mathrm{N}, \mathrm{T}) = (\Xi, \Upsilon) = (\mathrm{O}, \Phi) \ldots = (\mathrm{N} + \ldots \Sigma,\ \mathrm{T} + \ldots + \Omega),$$

from which follows that which it was required to prove.

Then, after treating the second (more general) case of the proposition, which I have here omitted, Dijksterhuis makes this comment:

In the proof it has been assumed that the magnitudes of the four series are all homogeneous; in fact, reference is made to the ratios (A, N), (A, H), and (N, T), and this implies that A is homogeneous with N and with H, N with T, and consequently also A with T.

Of this restriction, however, Archimedes does not take the slightest notice in the applications. We shall find him using the proposition in the case where the magnitudes of the series I and III are volumes, those of the series II and IV lengths, in which case the ratio (A, H) makes no sense. It seems probable that this is a sign of slackening in the strictness of the Euclidean theory of proportions, due to the fact that in applying the propositions of the theory of proportions it was never necessary to take account of the definition of proportion (which explicitly stipulates homogeneity as condition for two magnitudes being in any ratio to each other) and of the way in which these propositions had been derived from the definition. In addition, the custom of representing any magnitudes, of whatever nature, diagrammatically by line segments was bound to conduce to an increasing neglect of the difference in dimension between volumes, areas, and lengths, and to the gradual reaching of a conception which was equivalent to that of positive real numbers.

It is very hard to understand how Dijksterhuis can have so misrepresented the argument actually given by Archimedes. The proof Dijksterhuis gives is the same (up to notation) as that in Heath.[51] But in the preface to his book, Dijksterhuis remarks as a fundamental difference between his versions of Archimedes's arguments and those of Heath that the latter "represents Archimedes' argument in modern notation" – a proceeding in which "it is often the most characteristic qualities of the classical argument which are lost" – whereas in his own exposition "the proofs are set forth in a symbolical notation specially devised for the purpose, which makes it possible to follow the line of reasoning step by step".[52] One would surely expect him to have taken particular care about a passage that leads him to attribute to Archimedes a lapse from Euclidean standards of rigor.

I was so surprised to read that Archimedes treated a fundamental point in this loose way that in spite of my own lack of schooling in Greek, I made it my business to check the original (with the aid of primer, lexicon, and grammar), and to compare it with other translations. Those of Ver Eecke and Mugler are entirely accurate here. The former is cited by Dijksterhuis as a "very reliable, absolutely literal translation"[53] (Mugler's version, which appears to be strongly indebted to Ver Eecke's, was published later than the book of Dijksterhuis). It is plain from the text that the only homogeneity assumptions required by the argument as Archimedes gives it are: homogeneity within each of the four series; between the first series and the third; and between the second series and the fourth. These assumptions are never violated in the applications Archimedes makes.

What further complicates the puzzle of how Dijksterhuis could have made such an error is that he proceeds to give substantially the argument of Archimedes himself, as an alternative that Archimedes *could* have used. His words are as follows:

> For the rest it is easy to see that according to the strict conception of the theory of proportions the proposition remains true for the case where the magnitudes of series I are homogeneous only with those of series III, those of series II only with those of series IV.
>
> In fact, from (A, B) = (H, Θ); (B, Γ) = (Υ, I) etc. it follows, by application of the definition of proportion, that
>
> $$(\mathrm{A} + \mathrm{B} \ldots + \mathrm{Z}, \mathrm{A}) = (\mathrm{H} + \ldots \mathrm{M}, \mathrm{H}).$$
>
> From (3) it follows likewise that
>
> $$(\mathrm{N}, \mathrm{N} + \Xi + \ldots \Sigma) = (\mathrm{T}, \mathrm{T} + \Upsilon + \ldots \Omega),$$
>
> from which *via*

$$(A, N) = (H, T),$$

it follows *ex aequali* that

$$(A + B \ldots + Z, N + \Xi \ldots + \Sigma) = (H + \ldots M, T + \ldots \Omega).$$

But this is exactly the argument of Archimedes – except that Dijksterhuis supplies, cryptically, the reason omitted by Archimedes for the asserted identity of the ratios of the sums of the first and second rows to their respective first members. There is, however, a very interesting point to be made here. Archimedes often fails to cite grounds for inferences based upon well-known principles. In the present case, the argument *ab ovo*, from "the definition of proportion", is unnecessary, because the conclusion follows from elementary considerations: (i) from the data of the proposition, by inversion (which, it may be noted, Archimedes has performed at one point of his explicit argument without mentioning the fact) and compounding of ratios, we have $B : A = \Theta : H$, $\Gamma : A = I : H$, etc.; therefore (ii) by Proposition 24 of Book V of Euclid – i.e., by what we have already seen, at the end of Section 4 above, to amount to the addition of ratios (only generalizing Euclid's statement to arbitrarily many ratios) – the ratio of the sum of the second through the last members of the first row to its first member is the same as the corresponding ratio for the second row; and, finally, (iii) by "composition" of ratios – i.e., by what we should call addition of 1 (or more exactly: of the ratio 1:1) – the ratio of the sum of all the members of the first row to its first member is the same as the corresponding ratio for the second row. (The extra step of composition is of course required only because of the Greek reluctance to assert such a proportionality as $A:A = H:H$.)

This can be put in a slightly different way. What Archimedes in effect does is to take, for each magnitude in a given row, its ratio to the first member: i.e., he expresses those magnitudes in terms of the first of them, as what we should call a "unit of measure"; and then – again in our own terminology – he adds those representative ratios. That is, given the identification of Eudoxean ratios with real numbers that I have argued for in this paper, *he adds the real numbers that represent the magnitudes when that unit of measure is specified.* This is of course precisely what we do when we represent magnitudes by real numbers.

In short, Dijksterhuis is in the first place demonstrably wrong in attributing to Archimedes a slackening of Euclidean rigor here; and in the second place he is wrong in associating such a slackening of rigor with a move towards modern conceptions – in connecting "an increasing neglect of the difference in dimension between volumes, areas, and lengths" with "the gradual reaching of a conception which was equivalent to that of positive real numbers". This latter conception in no way entails a neglect of differences in the dimensions of magnitudes; on the contrary, attention to such differences is quite essential for the coherence of arguments in our own mathematics and physics. And – as I hope we have now amply seen – the rigorous conceptions of Greek mathematics were no bar, in principle, to reasoning about ratios fully equivalent to our own reasoning about real numbers.

There is one more point of at least incidental interest to be made here about Archimedes in particular. We have seen that in the statement and proof of the proposition under discussion Archimedes speaks always in terms of ratios and identity of ratios, in preference to the language of "proportion". It is tempting to see in this choice of language, which seems to emphasize the ratios themselves as objects of mathematical consideration, what Dijksterhuis thought he saw in the (illusory) lapse from rigor: namely, a move in the direction of our own point of view toward the real numbers. That such a tendency may indeed have characterized Archimedes's mathematical outlook is suggested by another peculiarity of his style. In his work on the ancient theory of the conic sections, Zeuthen remarks as a distinctive stylistic contrast between Archimedes and Apollonios that, where the latter generally employs the terms and conceptions of the so-called "application of areas", and the "geometric algebra" associated therewith, the former prefers to deal with proportions.[54] In other words: Apollonios represents *geometrically* what we call algebraic operations – products by an operation that associates to a pair of lines an area, quotients by one that associates to an area and a line a second line; Archimedes prefers to represent the corresponding relations as ones *between ratios*. And Zeuthen concludes his chapter on the treatment of conics by Archimedes with these words:[55]

If moreover, as in the example just cited, Archimedes by preference employs the theory of proportion in investigations, in which Apollonios prefers the area-operations that are more closely related to our algebra, I am most inclined to believe that the one who here manifests his personal peculiarities is rather Archimedes than Apollonios, who is generally the more exactly adherent to his Alexandrian predecessors. For this assumption there speaks the circumstance, that the use of the application of areas in the second Book of Euclid is appreciably older than the Euclidean theory of proportion, and therefore was to a greater extent at the command of the geometers who were responsible for the first development of the theory of the conic sections.

Note that Zeuthen is here concerned to establish the close affiliation of Apollonios to the older treatment of conic sections. He regards Archimedes's peculiarity of style as simply an eccentricity; indeed, he takes the algebra of lines and areas to be more closely related to our own than the reasoning in terms of proportions – i.e., ratios – of Archimedes. It is of course the thesis of the present paper that exactly the reverse is the case; and the evidence presented in this section may be summed up as tending to show, in my own opinion, that if we had only been blessed by another Archimedes or two in antiquity, the mathematics of the seventeenth century might have begun – and on firmer and clearer foundations than when it did in fact begin – more than a millennium and a half earlier.

NOTES

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1 Aristotle, *Metaph.* VI i 1026ᵃB–12, 15; XI iii 1061ᵃ28-ᵇ4, iv 1061ᵇ 22–3, vii 1064ᵃ33–4; *Nich. Eth.* VI viii 1142ᵃ17.

1a τὸ ‘ἑν: literally, “the one”.

1b ‘ο αριθμός: literally, “the number”. Cf. also Euclid VII, Defs. 1 and 2.

2 Aristotle, *Cat.* vi 4ᵇ20ff.

3 Aristotle, *Cat.* vi 4ᵇ20ff.

4 But it is extraordinary, in the light of this, that in one passage (*Nich. Eth.* V vi 1131ᵃ31) Aristotle himself says that “proportion is equality of ratios [ἡ γάρ ἀναλογία ισότης ἐστὶ λόγων]”.

5 Two points are worth noting, however: (1) Aristotle asserts a relation of proportionality for magnitudes – namely, *speeds* and *densities* – that are not ‘extensive’. (2) Euclid treats angles as magnitudes; but since he does not admit any angles greater than or equal to two right angles, he cannot in fact join or compose arbitrary angles – i.e., arbitrary substrates for this genus of magnitude. It was suggested, rather persuasively, by C. L. Dodgson (Lewis Carroll) that Euclid would regard values of this magnitude-kind exceeding (or equal to) two right angles not as themselves ‘angles’, but in effect as what we should now describe as ‘formal sums’ of angles; see Dodgson (1879, pp. 192–93). To carry out this idea explicitly would go some distance in the direction of the abstract structural notion to be developed in the text immediately below.

6 The wording of Euclid's Definition V.4 is susceptible of two different literal interpretations. The definition reads: “Magnitudes are said to have a ratio to one another which are capable, when multiplied, of exceeding one another.” The defining clause can be taken to mean either, as in the text above, that some multiple of each exceeds the

other, or that some multiple of each exceeds some multiple of the other. Since each of these conditions follows trivially from the other, however, the difference is of no mathematical importance.

[7] Cf. Hermann Weyl, (1949, pp. 9–11).

[8] T. L. Heath, in his edition of Euclid, cites Definition V.4 as justifying the step in question; see Heath (1925, vol. III, p. 14). But this is inconsequent: the definition merely states the condition for two magnitudes of the same kind to have a ratio; what is required is assurance that that condition is always satisfied.

[9] See Heath (1912, pp. 233–34) (the introduction to 'Quadrature of the Parabola', which contains what is presumably the first explicit occurrence of the assumption); p. 155 (in the introduction to 'On Spirals'); and pp. 1–2 (the introduction to 'On the Sphere and Cylinder', Book I: this passage does not mention the assumption – or "lemma", as Archimedes calls it – in question; but it is here that Archimedes attributes to Eudoxos the first rigorous proofs of those propositions which, in the first cited passage, he says have been established by his predecessors with the help of that lemma).

[10] Aristotle, *Cat.* vi $4^{b}23$–26, $5^{a}39$–$^{b}10$.

[11] The discussion itself is, in fact, defective; but (whether this is a fault in the original, or – as T. L. Heath assumes in his comment on the proposition – results from a corruption of the text) the defect is of no importance here.

[12] More precisely: to compose the ratios $a{:}b$ and $c{:}d$, taken in that order, one needs to represent them respectively in the forms $e{:}f$ and $f{:}g$; but this is just to represent the ratios $a{:}b$ and $d{:}c$ with a common denominator f.

[13] See Heath (1912, p. clxxix). (Heath actually renders Archimedes's terms *καί* [*ὁ*] and *προσλαβὼν* [*τὸν*] as 'multiplied by'; but this has no linguistic justification, as the first means simply 'and', the second 'taken besides'.

[14] Dedekind (1872, pp. 315–34); English translation, Dedekind (1872a, pp. 1–27).

[15] Cantor's considerations bearing on our subject appeared in one of his papers on the theory of trigonometric series; see Cantor (1872, pp. 92–102).

[16] See Section III of Dedekind's monograph (1872, 1872a). The property that Dedekind called "continuity" is formally identical with what, in current topological terminology, is called the *connectedness* of the line. Cantor's construction, which relies upon the metric structure of the line (whereas Dedekind's makes use only of its ordering), leads formally to the property now called (metric, or, more generally, 'uniform') *completeness*; but for a uniform space with a dense subset isomorphic to the rational numbers, the two concepts are equivalent.

[17] Dedekind (1888, pp. 339–40; 1888a, pp. 37–38).

[18] *Phys.* V iii $227^{a}9$–13; *Metaph.* XI xii $1069^{a}2$–8.

[19] See the introductory remarks to Dedekind (1872, 1872a); and, for some reflections on the motivation of Dedekind's concern, Stein (1988, pp. 242–49).

[20] The discussion that follows is to a certain extent adumbrated by Frege, in the second volume of his *Grundgesetze der Arithmetik*. Just as Frege based his theory of the natural numbers on the demand that the intrinsic structure of such a number reflects its use in the representation of the sizes of sets, he wanted to construct a logical concept of the real numbers to reflect their use in representing the measures of magnitudes; and, in his characteristically ponderous and thorough way, he moves toward such a construction through most of the course of that volume (without actually attaining its formal com-

pletion – this was to have been given in the third volume, which was never written). See Frege (1903), pp. 69–162 (in which Frege criticizes numerous attempts at a foundation of the theory of the real numbers, and finds a clue to the way he proposes to follow); and pp. 163–243 (in which he begins his own construction).

[21] In his very careful examination of the deductive structure of Euclid's *Elements*, Ian Mueller makes two remarks bearing upon the notions of magnitude and ratio that conflict with the discussion I have given; these deserve comment. The first is concerned with the relationship of the 'lemma' whose necessity for the theory of Proportion was pointed out by Archimedes – that is, of the Axiom of Archimedes, in the form in which the latter states it – to Proposition I of Book X of Euclid, and to the assumption tacitly made by Euclid in its proof. The assumption required by Euclid is formulated thus by Mueller (1981, p. 139, assumption Vd; I render Mueller's symbolic statement into prose like that of Euclid): *Of two magnitudes of a given kind, the larger is exceeded by some multiple of the smaller.* The assumption of Archimedes he quotes as follows: *Of unequal areas, the excess by which the greater exceeds the less, if added to itself, can exceed any given finite area* (Mueller, p. 142, principle QP; and cf. Mueller, principle SCI,L5, which generalizes this assumption to 'lines' and 'solids' in addition to areas – so that one may plausibly extend it to magnitudes in general). Mueller's comment on the relation of these two assumptions is: "Archimedes' *lēmma* is equivalent to the form of X,1 which Euclid uses in proving the results referred to by Archimedes; but unless one assumes $y - x < y$, it is weaker than the principle explicitly proved by Euclid, X,1, and the assumption he uses in proving this principle, Vd" – for "if x is infinitesimal, no multiple of x will exceed y even though a multiple of $y - x$ does" (Mueller, p. 143). But on my reading, that $y - x < y$ follows immediately from the definition of the ordering (or from the principle that 'the whole is greater than the part'). From this it follows that Archimedes' assumption is equivalent to that required by Euclid, and suffices to exclude infinitesimals. Explicitly: if y exceeds x, then y also exceeds $y - x$; hence, since x is the amount by which y exceeds $y - x$, Archimedes' 'lemma' gives us that y exceeds some multiple of x, i.e., Archimedes' assumption entails Euclid's. Perhaps it is worth adding that, even aside from the 'common notion' concerning whole and part, it appears odd to argue that if x is infinitesimal, y will not exceed $y - x$; surely it is more natural to say, in this case, that y exceeds $y - x$ by an infinitesimal magnitude. And in fact Heath (1925, vol. I, p. 182) cites Proklos to the effect that the 'angle of a semicircle' – that is, the angle between a semicircular arc and its diameter (which, in the Greek terminology, differs from a right angle by the 'hornlike' angle between the arc and the tangent) – although itself not an acute angle, is less than a right angle.

The second remark of Mueller's that calls for comment is the following (Mueller, p. 121): "If my account of Euclidean arithmetic is correct, there can be no doubt that the *Elements* do not contain the basis for the development of real number theory. For numerical ratios do not form a system of objects ordered by a relation of being less than; indeed, ratios are not objects at all." Now, that ratios are not 'objects' is a somewhat delicate point, for it might be argued that quantities themselves (for Aristotle, e.g.) are not 'objects' – if by that term one means what is capable (in Aristotle's terminology) of 'separate existence'. But quantities are, for the mathematician, 'objects of study'; and so, one may plausibly contend, are ratios. To be sure, as I have been concerned to emphasize, for the Greek mathematicians, ratios are not themselves quantities. Nevertheless, and in some degree contradicting what Mueller's statement seems to imply (or at least suggest), ratios – in Euclid, Book V – do form a single system (whether or not 'of objects') among whose members relations not only of sameness and difference, but also of greater and less obtain (see Euclid V Definition 7); and (as we have seen) they are subject to a number of well-defined operations. What is most directly pertinent, of course, is that an operation analogous to addition can be defined on ratios – in effect, as again we have seen (or, at least, as I have argued; cf. also the further discussion in the Appendix below) an operation that was known to, and employed by, the Greek geometers – satisfying all the conditions needed both to allow the application of Eudoxean ratios *to* Eudoxean ratios, and to support the construction made by Dedekind. In this sense, therefore, I believe that Mueller is mistaken, and that Euclid's *Elements* do "contain the basis for the development of real number theory".

[22] The quoted passages are from Zeuthen (1902, pp. 201–3). The original (Danish) version of this work appeared in 1893. Zeuthen had earlier discussed the reasons for the decay of Greek mathematics in his seminal work, *Die Lehre von den Kegelschnitten im Altertum*; see Zeuthen (1886, pp. 469–75). It is in this earlier discussion that the limitation of the geometric algebra to relationships of lower than the fourth degree is mentioned explicitly; the later summary omits this point. B. L. van der Waerden, in the first volume of his *Erwachende Wissenschaft*, claims in his introduction (see van der Waerden (1966, pp. 19–20) to give a new analysis of the causes of the decline of Greek mathematics; however, at the relevant place (pp. 439–41), he cites Zeuthen – and mentions no cause that Zeuthen had not already discussed.

[23] Heath (1921, vol. II, pp. 198–99).

[24] Euclid, Bk. VII, Def. 16.

[25] Zeuthen (1886, p. 471).

[26] Zeuthen (1886, p. 472–3).

[27] Cf. Zeuthen (1902, p. 202): "[W]hoever is familiar with this mode of representation, and understands the signification of the figures, can manipulate them as easily as one does nowadays with literal expressions; he can, moreover, in pointing to these figures, explain orally to his pupils the operations effected. And, in time of peace, as long as oral instruction was practiced at Alexandria, the result was that mathematical intelligence could be perfectly maintained; but as soon as the peace was disturbed, and the tradition conserved by that instruction was lost, one could only have recourse to the study of meticulously elaborated treatises – and a fatal regression became evident."

[28] Zeuthen (1886, p. 472).

[29] Cf. Zeuthen (1886, p. 470). Apollonios wrote in the second half of the second century B.C.; Pappos of Alexandria in the early fourth century A.D. – thus about five and a half centuries later. (For Pappos's date, somewhat later than that given by Heath, cf. van der Waerden, 1966, p. 470.)

[30] Cf., e.g., Zeuthen (1903, pp. 1–2). When Galileo comes to demonstrate his theorem that the path of a projectile is a parabolic arc, not only does he refer to Apollonios for the crucial properties of the parabola, but he makes a particular point of the handicap faced by a student who has not "gone so deeply into geometry as to make a study of" that author – see Galileo (1638; 1974 trans., pp. 217–19, corresponding to pp. 269–70 of the standard Italian edition of 1898).

[31] Two and a half centuries from the generation before Hippokrates of Chios, Demo-

kritos, and Theodoros, to the time of Appolonios of Perga; nearly four centuries if we date the origin of the tradition as early as Thales. But it ought to be noted that even if the later tradition of the Greeks is correct in attributing interest in (and contributions to) mathematics to Thales and his immediate successors, and to the earliest of the Pythagoreans (a generation after Thales), one would be dealing at this early date with at most the tentative beginnings of a tradition of research – to employ the biological analogy suggested in the main text below: with the first mutant potential ancestors of a new species, rather than with an actually established species.

[32] A very instructive brief sketch is to be found in Otto Neugebauer, (1969, ch. ii).

[33] Neugebauer, (1969, p. 29).

[34] It is perhaps worth noting that in point of the features that Zeuthen has identified as contributing to the vulnerability of the Greek tradition, that of the Babylonians is almost precisely contrary: no "care for unassailable rigor"; algebra rather than geometry as the characteristic mode of procedure; great attention to numerical calculation. (Of course, the stagnation of the Babylonian tradition despite these facts does not of itself refute Zeuthen's contention, since he has not argued that the features he finds lacking among the Greeks are sufficient conditions for a flourishing creative mathematics, but only that they are necessary – or rather, one should probably say, crucially facilitating – conditions.)

[35] The chief exceptions are the two physical treatises – *On the Equilibrium of Planes* and *On Floating Bodies*. The *Measure of the Circle* is also an exception; but what we possess of this work is clearly only an extract (and a somewhat garbled one) from the original. The (so-called) *Sand-reckoner* (in the Greek, simply *Sand*) is a partial exception, inasmuch as it is addressed not to a mathematician, but a king.

[36] Heath (1912). The passage quoted occurs shortly after the beginning of the treatise, which starts on p.12 of the Supplement; it is to be found on pp. 13–14 of the latter. (Note that the Supplement appears at the end of the volume, with pagination starting over.)

[37] Heath (1912, pp. 17–18). I have here deviated from Heath's words, guided by several other translations (the German of Heiberg, the French of Mugler, the English of Dijksterhuis) and by the Greek text. In particular, I have preferred, in place of Heath's phrase "a sort of indication" (that the conclusion is true), the one I have used – "a certain impression" – both as closer to the Greek (ἔμφασίν τινα) and as more expressive of the subjective state implied. (See also n. 38 below.)

[38] Heath remarks that the word τάξομεν, which I have rendered as "shall arrange", is a doubtful reading and difficult to translate. I take the meaning to be, not (as Heath puts it) "shall have recourse to", nor (as Heiberg, Dijksterhuis, and the French translators understand it) "shall give below" or "shall mention", but rather this: that "having formed a certain impression that the conclusion is true, we shall [seek to] institute [or "arrange"] the geometrical demonstration [which in fact] we ourselves have discovered and published". In other words, I take Archimedes' "we . . . ourselves" to mean, on the one hand, the inquirer(s) in process of searching for the proof, and on the other Archimedes himself who has already discovered and previously published that proof. (The proposition in question is that giving the area of a parabolic segment, published in the *Quadrature of the Parabola*.)

[39] I am guided here by the French versions of Ver Eecke (1921, pp. 3–4) and Mugler

(1970, pp. 8–9), both of whom use the revised Greek text of Heiberg's second edition (1910); and by the Greek text itself, as Mugler prints it, in preference to the translation of Heath (1912, pp. 1–2), which is based upon Heiberg's edition of 1880 (Heath's version was published originally in 1897); cf. n. 41 below.

[40] That is, a parabola. Archimedes' phrase – the standard designation of the parabola before Apollonios – derives from the fact that the conic sections were originally defined as sections of a cone by a plane perpendicular to a generator of the cone. If the vertex angle of the cone is acute, of course, such a Section will be an ellipse; if the angle is right, a parabola; if the angle is obtuse, a hyperbola.

[41] Heath reads "not yet demonstrated", and places in parentheses the Greek word (*ἀνελέγκτων*). But the Greek text (as given by Mugler) has *ἀξίων λόγου.* Mugler's French translation agrees with his reading of the Greek, and so does the generally very accurate and reliable one of Ver Eecke. This proves to reflect a difference between Heiberg's first edition of Archimedes (1880) and his second (1910). The reading of the former was a conjectural emendation of *αντιλεγον*, which occurs in the only Greek manuscript of the beginning of this treatise then available; that of the latter is based upon the agreement of the medieval Latin version of Moerbeke with the then newly discovered palimpsest – the one that has given us the only known text of the *Method.* (Oddly enough, the Latin translation in Heiberg's 1910 edition continues to reflect his earlier version of the Greek.) Although the matter is of no importance for us, I have preferred to follow what seems to be the better, and now standard, reading (see n. 39 above).

[42] Dijksterhuis (1956, p. 143).

[43] Mugler, (1971, pp. 8–9); Ver Eecke (1921, p. 239); Heath (1912, pp. 151–52).

[44] See Zeuthen's commentary, attached to Heiberg's first report on and translation of the *Method* of Archimedes: J. L. Heiberg and H. G. Zeuthen (1906–7, p. 419).

[45] Dijksterhuis (1956, p. 34).

[46] Van der Waerden (1966, p. 345).

[47] Heiberg in his second edition proposes, for a collection of divergent readings of the various manuscripts, the emendation *ἐμαντῷ μήπω πεπερασμένων διὰ τέλους ποτιτεθῆμεν* – literally: "[there happen] to have been added [a certain two] not yet accomplished thoroughly by me" (*ποτιτεθῆμεν* is the Doric form of the aorist passive infinitive of *ποτιτίθημι*, itself Doric for *προστίθημι*, "adjoin to"); see Heiberg (1913, p. 3, notes to l. 23 of p. 2).

[48] A typical example of the misunderstanding prevalent among recent commentators about the relation of the ancient to the modern theories, and especially about the role of the concept of real number, is connected with this point. In a discussion of Galileo as a mathematician, Carl B. Boyer mentions Galileo's scorn for the doctrine of Aristotle that "a straight line does not bear any ratio to a curve"; mentions also "Archimedes' theorem in the work *On Spirals*, in which a straight line is found which is equal in length to the circumference of a circle"; and proceeds to explain a sense in which, taking account of "the finer points in the comparisons of straight and curved", Aristotle was in the right: "In the first place, there was as yet no definition of what is meant by the *length* of a curve – nor even of a prime requisite for that definition, *real number*. More importantly, in the ancient comparisons of straight and curved, the problem had been to construct with Euclidean tools alone a straight line equal to the circumference of a circle (or other

given curve)" (Boyer 1967, p. 239). Now, the second of these points is arguable; but there is good reason to think it incorrect: thus Zeuthen (1886, p. 262) expresses the view "that a more exact investigation would lead to the result, that the mechanical execution of [what Zeuthen calls "Einschiebungen" – "insertions"; namely the class of "mechanical" constructions called in Greek νεύσεις] was not only employed practically, in the earlier time, *but was also acknowledged theoretically as a device that one might properly use, if a problem could not be solved by means of circle and straight-edge*, and that only in a later time did one feel obliged, whenever it was possible, to use conic sections to effect those insertions that could not be transformed into constructions by means of circle and straight-edge" (emphases in the original). (The issue here becomes a little involved: Zeuthen implies (a) that constructions by means other than those with "Euclidean tools" were *always* admitted as theoretically justified, but (b) that in the "later time" a stricter principle had developed of using only the simplest means that would suffice to resolve a problem. In any event, "constructions" with the help of conic sections – themselves not attainable by "Euclidean tools" – were certainly allowed; and Euclid's own restriction to circle and straight line can be seen as characterizing the "elements", in contrast with the higher branches of the subject.)

But it is Boyer's first remark that more directly concerns us – that there was available no definition of the length of a curve, "nor even of a prime requisite for that definition, *real number*". I have said that what Archimedes postulates serves in effect as such a definition: what we should now describe as an axiomatic characterization of a concept. An explicit definition, to be sure, was not given until well into the nineteenth century; and the conditions of legitimacy for an "axiomatic characterization" were certainly not investigated until even later. But that comment applies to a great deal that is central in Greek geometry; for instance, the definition of ratio itself, with which we have been primarily concerned, is not explicit in Euclid: as we have seen, he defines "having a ratio" and "having the same ratio" – and this can be regarded as an "axiomatic characterization" of a certain relation, to be justified by a series of theorems that establish the existence of a unique relation satisfying the conditions laid down.

The crucial point, however, is simply that Boyer is wrong in claiming that the concept of real number – in contrast with concepts already available in Greek mathematics – is a prerequisite for the definition of the length of a curve. The characterization given by Archimedes can easily be transformed into an explicit definition of the relation of *equality* ("of length") *between a straight line, and a curved line concave toward one side* (namely, such equality holds if and only if the given straight line exceeds in length all simple polygonal paths inscribed in the curve, and is exceeded by all such paths circumscribed about the curve); and that is all one needs.

[49] In what follows, all quotations are from Dijksterhuis (1956, ch. iii, 7.20). For the sake of greater clarity, I have here taken the liberty to paraphrase the lemma of Archimedes, instead of giving it in the form of a verbally exact translation. I have also restricted myself to the first case of the lemma, whose treatment contains all that is essential for us. (On the other hand, in order that my notation fit with that of Dijksterhuis, I have followed him in using the same Greek letters to designate the magnitudes in the example as Archimedes himself does.)

[50] As in the formulation of the proposition, I have allowed myself to paraphrase, rather than translate, the words of Archimedes, but I have followed exactly the steps of his

proof. I have also been faithful to his terminology. In particular – a point that will come up subsequently – I have phrased the entire argument in terms of ratios, and sameness of ratios; and so does Archimedes. The word "proportional" occurs nowhere in his exposition of this proposition or its proof.

[51] Heath (1912, p. 106).

[52] Dijksterhuis (1956, pp. 7-8).

[53] Dijksterhuis (1956, p. 7).

[54] Zeuthen (1886, p. 55): "[O]bwohl Archimedes in der Regel seine Bestimmung der Kegelschnitte nicht an die bei der Flächenanlegung gebrauchten Kunstausdrücke anschliesst, ist der praktische geometrische Gebrauch der Flächenanlegung ebenso genau mit dieser letzteren Bestimmung, die vermutlich auch diejenige Euklids ist, verbunden gewesen wie mit der des Apollonius"; p. 61: "Bei Apollonius werden wir in der Regel nicht wie hier [viz., in Archimedes] die Gleichungen der Kegelschnitte als Proportionen dargestellt finden, sondern als Gleichungen ersten Grades zwischen Flächen."

[55] Zeuthen (1886, p. 63) – I have been constrained by the difference between German and English syntax to render somewhat freely the phrase, "der sich and die alexandrinischen Vorgänger genauer anschliessende Apollonius".

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