NOTES & DÉBATS

IT'S NOT THAT THEY COULDN'T

Reviel Netz (*)

It's not that she couldn't, It's not that she wouldn't, And you know—it's not that she shouldn't: It's just that she is The laziest qal in town.

Cole Porter

ABSTRACT. — The article offers a critique of the notion of 'concepts' in the history of mathematics. Authors in the field sometimes assume an argument from conceptual impossibility: that certain authors could not do X because they did not have concept Y. The case of the divide between Greek and modern mathematics is discussed in detail, showing that the argument from conceptual impossibility is empirically as well as theoretically flawed. An alternative account of historical diversity is offered, based on self-sustaining practices, as well as on divergence being understood not in terms of intellectual values themselves (which may well be universal) but in terms of their rankings within different cultures and epochs.

Résumé. — Ce n'est pas qu'ils n'auralent pas pu. — Cet article offre une critique de la notion de "concepts" en histoire des mathématiques. Certains historiens s'appuient parfois sur un argument mettant en avant une impossibilité conceptuelle, du style: certains auteurs ne pouvaient pas faire X, parce qu'ils n'avaient pas le concept Y. Nous discutons en détail ce que cela signifie dans le cas de la différence entre mathématiques grecques et mathématiques modernes. Nous montrons que l'argument de l'impossibilité conceptuelle est empiriquement et théoriquement peu solide. Pour rendre compte de la diversité historique, l'article offre une alternative fondée sur des pratiques qui s'auto-entretiennent et sur la notion de divergence interprétée non en termes des valeurs intellectuelles elles-mêmes (qui pourraient bien être universelles), mais des rangs que ces valeurs occupent dans différentes cultures et époques.

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Is mathematics always the same? If not, why? Historians of mathematics keep returning to these fundamental questions. The very question of what is 'the same' in mathematics is not easy to answer. After all, mathematicians have always shown the surprising fact that things that appear different are truly—seen under the appropriate perspective—the same: not only in the twentieth century's hunt for isomorphism, but starting with such observations that the squares on the two sides of the right-angled triangle are, in some sense, the same as the square on the hypotenuse...¹ It is thus natural, faced with an alien piece of mathematics, to show that it is 'the same', in some mathematical sense, with a certain subset of contemporary mathematics. This gives rise to the following set of objects: OPM—an Old Piece of Mathematics,

CPM-a Contemporary Piece of Mathematics (to which OPM is equivalent),

CM-the entirety of Contemporary Mathematics (of which CPM is no more than a subset).

At this stage, the historian who wishes to say that mathematics is not always the same has two related routes open. One is to argue that while, from a certain perspective, OPM and CPM are indeed equivalent, they are also different enough from each other to merit the label 'different'.² This however seems weak on its own: no one ever denied the difference, but the question is, why should this difference matter once the basic equivalence is perceived? After all—is this not a mere matter of notation? Hence the second route: to argue that CPM is a subset of CM for a good reason: the way in which the mathematics of OPM was done made it impossible to do any mathematics but OPM, and so the modern equivalent to OPM can be CPM alone, and not CM as a whole. Mathematics is not always the same because, at different periods, different kinds of mathematics were possible. Transforming OPM into its contemporary equivalent, CPM, is

 $^{^{1}}$ [Goldstein 1995] is a fundamental study of 'the same' in mathematics, dedicated to the question: when are different mathematical proofs and propositions 'the same'?

² It is in fact difficult to define the 'equivalence' operative in this case. The standard example—the equivalence of Euclid's *Elements* II with algebraic equations—seems to suggest a meaning of 'equivalence' along the following lines: historians of mathematics often take two theorems to be equivalent when, from the perspective of the modern mathematician, the proof of any of the theorems serves to show, simultaneously, the truth of the other.

misleading: it obscures the idiosyncratic features of OPM that blocked it from becoming CM. No mere matter of notation, then: the difference between OPM and CPM is historically explanatory.

Such was the form of the most famous twentieth century debate in the historiography of mathematics. Unguru [1975] argued that Greek mathematics differs from its modern equivalents; Freudenthal [1977] and Weil [1978] had argued that this is a matter of notation only; Unguru wrapped up the discussion in Unguru [1979] with wide-ranging historiographical and indeed philosophical comments (more recently re-considered and expanded in [Fried and Unguru 2001]). At the heart of Unguru's reply—which has now become, to varying degrees, the established view in the community of historians of mathematics-lies the fundamental work by Jacob Klein [1934/1936, 1968], Greek Mathematical Thought and the Origins of Algebra. Klein's thesis was that Greek mathematics, for deep conceptual reasons, just could not become the same as modern mathematics, and must have had the form of dealing with the synthesis of isolated geometrical problems (instead of systematic algebraic analyses). Why? Because the Greeks did not possess the right kind of concepts: for algebra, one needs second-order concepts that refer to other concepts, but the Greeks had only first-order concepts, referring directly to reality. But let us leave aside the details of Klein's thesis and concentrate on the form of the argument. Klein's claim—the foundation of Unguru's critique—was that the difference in form between Greek mathematics and its modern counterpart was historically explanatory: to wit, it explained why Greek mathematics could not be modern. Why? Because modern mathematics, in the Greek context, was conceptually impossible.

Once again: my interest in this article is not in the detail of Klein's historical thesis.³ I am interested in the form of the argument. I shall call this the argument from conceptual impossibility. Its shape is: 'for conceptual reasons, X could not do Y'. In an important recent article, 'Conceptual Divergence—Canons and Taboos—and Critique: Reflections on Explanatory Categories', Jens Hoyrup [forthcoming] had challenged the very argument from conceptual impossibility. According to Hoyrup, we

³ I have discussed Klein's thesis in detail in [Netz forthcominga], where I argue that the difference Klein had noticed—between a more 'isolated' and 'qualitative' approach in Greek mathematics as opposed to a more 'systematic' and 'quantitative' approach in modern mathematics can be explained in terms of changing mathematical practice.

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are too hasty to speak of 'possibility' and 'impossibility', and we tend to draw the border between them too neatly. This article is written so as to support, qualify and I hope to complement Hoyrup's. In the first section I shall give several examples for what is typically taken to be the fundamental divide between ancient and modern mathematics: the more 'algebraical' or 'arithmetical' nature of modern mathematics. I shall show that it's not that they couldn't: Greek mathematicians could, and did on occasion, produce a more 'arithmetical' kind of mathematics. In the second section I shall consider together the examples from the first section, showing how, even absent the argument from conceptual impossibility, the difference between ancient and modern mathematics remains important. I shall also return to set out in more detail Hoyrup's account as well as my own, complementary historiographical approach.

1. THE NON-ARITHMETICAL CHARACTER OF GREEK MATHEMATICS

In what follows I draw upon several recent studies on Greek mathematics that, taken together, show the inadequacy of the argument from conceptual impossibility: wherever we look, we find exceptions to the rule of the non-arithmetical character of Greek mathematics. The moral, however, is not that we should give up the picture of Greek mathematics as non-arithmetical, but that we should give up the argument from conceptual impossibility.

What do we mean by the 'non-arithmetical character of Greek mathematics'? Several different things: arithmetical and numerical questions are less important than they are in other mathematical traditions; geometrical objects (which are the focus of interest) are understood in a non-quantitative way. Finally, the arithmetical system itself is patchy. It completely lacks the coherent structure of its modern counterpart, both in mathematical structure (where we have the well-understood logical sequence from integers through positive rationals and reals, and through negatives, to complex numbers) and in symbolism (where we use the decimal positional system). To the Greek, numbers are mysterious and clumsy to handle; to us, they are fully brought under the control of logic and are easy to deal with. Let us begin to note some exceptions to this picture.

1.1. Fractions

One central perceived difference between Greek and modern conceptions of number has to do with fractions. It has been argued in recent studies that the Greeks did not possess the concept of a common fraction, using instead either unit-fractions or ratios ([Knorr 1982], [Fowler 1992]). What we refer to as 'three over five', a numerical value, would be for them either unit fractions, that is, 'a half and a tenth' (a sum of numerical values) or a ratio, that is 'the ratio of three to five' (not a numerical value at all, but a relation). There is a mass of evidence where Greek mathematicians treat fractions in just this way—an evidence which seems to go beyond notational differences into mathematical practice itself: common fractions, unlike other representations of fractions, allow direct calculation with fractions of the form 'the nth of m multiplied by the qth of p gives nqth of mp'. (This direct calculation serves to put 'fractions' on a par with integers and in this way opens the way for the contemporary clear logical structure.)

I move on to discuss a new study of this question by Jean Christianidis [forthcoming]. Christianidis sets out from a quotation from David Fowler that is very relevant to our concerns:

"Just one example of some operation such as the addition, subtraction, multiplication, or division of two fractional quantities, expressed directly as something like 'the nth of m multiplied by the qth of p gives nqth of mp', and clearly unrelated, by context, to any conception in terms of simple and compound parts, could be fatal to my thesis that we have no good evidence for the Greek use or conception of common fractions. I know of no such example" [Fowler 1987, pp. 264–265].

Christianidis then observes that Diophantus' problem IV.36 contains just that. Not indeed in numerical terms, but in terms of Diophantus' 'syncopated algebra'. Still: Diophantus shows a clear sense of multiplication of fractions where the numerator is multiplied by the numerator and the denominator—by the denominator. Transcribing Diophantus' syncopated algebra into symbolic algebra, the essence of Christianidis' argument is that Diophantus, in IV.36, directly derives from the multiplication of '(3x)/(x-3)'4 by '(4x)/(x-4)' the form ' $(12x^2)/(x^2+12-7x)$ '. While

⁴ The original for (3x)/(x-3) was 'number, three, in the part of: number, one, lacking monads, three', or perhaps (depending on how syncopated Diophantus' original papyrus

this is not quite the example Fowler was looking for (the terms in question are general, 'number' and 'monads', rather than direct numerical terms), it is clear that Diophantus *could*. If he could multiply a numerator by numerator here, he certainly could do it elsewhere.

Notice how the entire discussion between Fowler and Christianidis is framed by the argument from conceptual impossibility. To Fowler, a *single* counter-example would be *fatal*. Why? Because his argument is that the Greeks did not have the *conception* of a common fraction. To Christianidis, a single example, even if not precisely the one Fowler was looking for, is sufficient, because it shows the *possibility* of the practice.

Christianidis had clearly shown an important result: it's not that they couldn't. To at least one Greek mathematician, in at least one context, it was quite possible to multiply fractions by the separate multiplication of denominators and numerators. Fowler was wrong, if indeed he did conceive the issue in terms of the argument from conceptual impossibility. In reality, I doubt that he did: but, even so, it is telling that his formulation was in the radical terms of conceptual impossibility. For what would be the alternative to conceptual impossibility? Chrsitianidis concludes as follows: "This provides good evidence that Diophantus had knowledge of the concept of common fraction as well as of the elementary rules of its arithmetical treatment". In other words, the alternative to conceptual impossibility is, to Christianidis, its opposite-conceptual possibilityoffered as the conclusion of the discussion. But this depends on the very argument from conceptual impossibility. If conceptual blocking is not a crucial historical process, so that people in general can do more than they usually do, then it is predictable that the difference between the 'done' and the 'not done' should be quantitative rather than qualitative: the 'done' is a matter of tendency. It would certainly be methodologically wrong, to throw away the mass of evidence for the Greek treatment of fractions in terms of unit fractions or ratios, in light of Christianidis' example: tendencies are what we should look for, and the tendency remains standing. Christianidis has done an enormously valuable service in helping us to delineate the borders of this tendency, and in blowing a hole in the argument from conceptual impossibility: yet the discoveries

was): 'Ar, 3, in the part of: Ar, 1, L Mo, 3'.

of Knorr and Fowler should still serve as basis for further analysis.⁵ The task is to re-frame them without their foundation in the argument from conceptual impossibility. I return to this task in section 2 below.

1.2. Arithmetized geometrical objects

In analytic geometry since Descartes, geometrical objects are understood in explicit arithmetized terms: lines carry a value of length, which we may represent as a variable but which is in principle a numerical value. Thus it is possible to speak of the multiplication of lines as if it were the multiplication of two numbers so that the parabola, for instance, becomes the locus of the points satisfying an equation of the form $y = ax^2 + b$. This is the essence of Zeuthen's interpretation of Greek mathematics as geometrical algebra, [Zeuthen 1886], and the essence of Unguru's critique of geometrical algebra is that Greek geometrical objects are not like that. Lines are elements within geometrical configuration, and they are not associated with numerical values. It is typical that Greek mathematicians do not say that 'the length of this line is equal to the length of that line' but instead say 'this line is equal to that', specifying a concrete relation in geometrical space. As a consequence, geometrical objects are not subject to arithmetical operations: Greek mathematics does not refer to the multiplication of lines but, concretely, to the rectangle contained by the two lines. Thus conic sections are not the locus satisfying an equation, but are a concrete spatial object, the result of cutting a cone by a plane, which also happens to satisfy a relation between certain other geometrical objects-concrete lines and squares. This is the argument of Fried and Unguru [2001].

I deny none of this. But what to make of Archimedes' usage in *Sphere and Cylinder*, Book II? Within an appendix to the treatise (lost from the main line of transmission of Archimedes' work and reported in Eutocius' commentary [Heiberg 1915, pp. 132–144]) as well as in an alternative proof to the penultimate proposition, Archimedes' text contains many occurrences of expressions in which original wording is, *e.g.* 'to apo tês *AB epi tên GD*' or, in more general:

to apo tês {two Greek letters} epi tên {two Greek letters}

⁵ Christianidis, indeed, does not deny this: his argument relates to Diophantus alone and not to Greek arithmetic and logistic as a whole.

'The $\langle \text{square} \rangle$ on the $\langle \text{line} \rangle$ {two Greek letters} epi the $\langle \text{line} \rangle$ {two Greek letters}'.

In other words this is an expression composed of three constituents:

- 1. the Greek mathematical formula for a square ('the on the $\{\text{two letters}\}$ ')⁶, followed by
 - 2. the preposition epi, followed by
 - 3. the Greek mathematical formula for a line ('the {two letters}').

(Expression 1 is not always that of a 'square' and may sometimes be a 'rectangle': it is therefore used for areas in general.)

I have discussed this expression in [Netz 1999b] and herein I repeat my conclusions: it is impossible, given the context and the phrasing, to read the expression other than in terms of the arithmetical operation of multiplication. That is: the preposition epi, in this context, means precisely the same as what we mean by the expression 'multiplied by' so that Archimedes repeatedly speaks of 'the area multiplied by the line'. I also find it very unlikely to ascribe the presence of the expression here to any textual corruption. In other words, we find a clear case where a Greek mathematician—indeed, the greatest of Greek mathematicians, active in the heyday of Hellenistic mathematics—explicitly makes geometrical objects subject to arithmetical manipulation.

Once again, however, I do not believe such a counter-example should make us discard Unguru and subscribe, finally, to Zeuthen's interpretation: Unguru did clearly identify an important tendency in Greek mathematics; Fried and Unguru offer a much more sensitive analysis of the ancient theory of conic sections than Zeuthen did. After all, the epi, at least in Hellenistic mathematics, is indeed exceptional. This, in my view, explains its very use by Archimedes. My argument in [Netz 1999b] was simple: Archimedes deliberately had used in this passage—the very end of *Sphere and Cylinder*, Book II—a strikingly different language, one that served further to mark a remarkably complex stage of his treatise.

In other words: I suggest that Greek mathematicians were always very well aware of the correspondence between the operation of constructing an area or a solid out of given lines, and the operation of multiplying numbers. Thus there was nothing impossible, conceptually, in referring to

⁶ Note that this formula (like most Greek mathematical formulae) is elliptic: the main noun, 'square', is elided, and is understood (in the Greek) from the article.

a solid as the 'multiplication' of an area by a line. While possible, however, such an expression would also be exceptional and therefore would be read off, by its audience, as carrying some special, extra-meaning; hence Greek mathematicians would normally refrain from using this expression, unless they wanted to make some special point with it—which Archimedes did at the very end of *Sphere and Cylinder*, Book II.

The above account, as stated, might appear unsatisfactory. After all, why would Greek mathematicians, to start with, use such expressions only exceptionally? And am I saying anything different than 'the Greeks did not usually multiply areas by lines, because they did not usually do that'? I return to discuss such general methodological questions in section 2 below and, for the time being, I notice the similarity in structure between the example seen in sections 1.1 and 1.2: a practice is ascribed to the Greeks by Fowler or Unguru, based on some fundamental conceptual grounds; a counter-example is provided by Christianidis or Netz; the argument from conceptual impossibility dictates that, given the counter-example, Fowler or Unguru's overall account should be discarded. However, since the account is obviously important and informative, it seems better to discard the argument from conceptual impossibility and to look for alternative methodologies.

1.3. Numerical geometrical objects

In the previous section we have seen an example where, surprising to Unguru's interpretation of Greek mathematics, geometrical objects are brought under arithmetical operations. Even so, those geometrical objects are not yet given numerical values: the square is multiplied by the line, but this is not based on such statements as 'let the area of the square be sixteen monads, and the length of the line be four monads; so their multiplication is 64 monads'. Such association of numerical values with geometrical objects is indeed absent from Hellenistic mathematics.

That is—depending on whom we choose to define as 'Hellenistic mathematician', and whom we then choose to read. In a recent Ph.D. thesis, [Tybjerg 2000]), Karin Tybjerg had finally took the trouble of reading Hero as a mathematician. She points out a systematic duality in the *Metrica*—Hero's main work in 'geometry' (if this is indeed the appropriate term). On the one hand, Hero appeals to the prestige of geometrical proof,

invoking the authority of Archimedes and using terms such as 'demonstration' (apodeixis), clearly suggesting the authority of geometrical proof in the Euclidean-Archimedean tradition. On the other hand, his approach is based, throughout, on specific numerical values. (Tybjerg's example is Metr. I.6, 14.18-20: "Let ABG be an obtuse angled triangle having 13 units, BG 11 units and AG 20 units".) These values are either assigned the geometrical objects at the outset, or are assigned them mid-way as a kind of further check on the validity of an otherwise geometrical argument. Mostly the arguments are indeed fundamentally geometrical: the measurements are proved using the tools we know from Euclid's *Elements*. At the ends of the first two books, however, Hero adds rules for the measurement of 'irregular' objects: for instance, a statue may be measured by covering it in cloth which is then taken off and measured as a surface... In other words, Hero deliberately extends the scope of geometrical proof. I quote from Tybjerg's summary of this issue, in section 3.2: "Hero introduces the Metrica as a continuation of Eudoxus' and Archimedes' work ... [yet] he offers a different form of demonstration that involves both a geometrical part and an arithmetical procedure, where the result for a specific example is calculated. These demonstrations are evaluated on the basis of their usefulness and ease and are extended to include purely physical methods of measurement." Tybjerg associates this with Hero's larger project, in her interpretation: carving a domain for mechanics, distinct from that of pure geometry on the one hand and the banausic arts on the other hand.

In other words, we see here the same mechanism as suggested in [Netz 1999b] for a small stretch of text in Archimedes, applied in this case to an entire œuvre. Hero deliberately sets himself apart from Greek mathematics in the Euclidean-Archimedean tradition, by making a choice that, systematically, was not made by traditional geometrical authors. Of course it was not conceptually impossible for Greek mathematicians to conceive of geometrical objects as carrying numerical values: we see that one author did just this, systematically. But it is surely significant that this author is also different in several other ways. Assigning numerical values, then, was not impossible but simply different. However, even difference in itself is significant: it carries meaning and so would be used only in the right context, where that difference is what the author wishes to convey. Archimedes wished to be different at the end of Sphere and Cylinder,

Book II; Hero wished to be different throughout.

1.4. A complete series of integers

The evidence usually given for the 'patchy' nature of the Greek numerical system is that the Greeks not only failed to articulate our 'rational' numbers (in that they did not use our common fractions), but also failed to articulate our *integers*. This is because the Greek sequence of integers was apparently incomplete: the upshot of Euclid's definitions of number in *Elements* VII.1-2 is that 1 is a principle of all number, but is not a number itself. Thus the sequence of integers is $2, 3, 4, \ldots$ I quote from Hoyrup [forthcoming] whose treatment I am following:

"Here, it is often claimed (names and exact quotations are omitted for reasons of charity) that the Greeks *could not* think otherwise. Since they understood number as 'collection of units', they 'failed to understand' that 1 is a number."

This then is yet another example of the argument from conceptual impossibility. As Hoyrup shows, however, it is clear that Euclid himself had occasionally thought of 1 as a number: a perfect number is defined as a number equal to its parts and so, for six to be a perfect number (as Euclid proves in *Elements* IX.36), 1 must be a number. This argument by Hoyrup has deep consequences for the historiography of mathematics. Just because historians cared so much for the questions of conceptual possibility and impossibility, they also tended to pay the greatest attention to the passages that promised insight into the conceptual foundations of science: definitions and philosophical discussions. It turns out that buildings do not necessary follow their foundations. To be precise: it appears that Euclid has specific, extra-mathematical concerns governing his definitions VII.1-2. It is typical of Greek mathematical definitions that they act as a kind of shop-window where Greek mathematics interacts with the wider intellectual world and heeds its concerns. Thus definitions are offered even beyond mathematical necessity: they serve not merely mathematical, but also wider philosophical goals. No one needs, within mathematics, to have a definition of a 'point': but the prospective audience of *Elements* I would feel cheated without a discussion of what 'points' are, and so Euclid puts the mathematically inert definition, 'a point is that

which has no part', at the very start of his work.⁷ The same might be the case with *Elements* VII.1–2. Before proceeding with the mathematical discussion, Euclid needs to answer the question 'what is number', and he answers it in a way that makes sense to a philosophical audience. The definition serves primarily a philosophical, not a mathematical goal, and for this reason, indeed, can be safely ignored within the mathematical practice itself (where, indeed, no explicit definition of number is required at all—and where the reference to the sequence of integers 1, 2, 3, ... may be taken for granted as it makes the simplest arithmetical sense).

I shall return to discuss in section 2 below Hoyrup's own interpretation of this gap. For my immediate purposes I note the following. (1) Once again, we find that there was no conceptual impossibility: the Greek could think of 1 as a number (and as a consequence could envisage the entire series of integers $1, 2, 3, \ldots$): apparently, this is what they standardly did. (2) The choice whether to consider 1 as a number or not was significant: one would think of 1 as a number in an arithmetical, but not in a philosophical context. In other words, the choice was goal-dependent: for philosophical purposes (e.g. for answering the question 'what is number?') 1 was not a number, for arithmetical purposes (e.g. for finding perfect numbers) 1 was. (3) There also appears to be a certain hierarchy between the goals: this may be seen in a context where both goals operate. The introduction to *Elements* VII serves simultaneously a philosophical goal (satisfying a philosophically educated audience) and an arithmetical goal (preparing the ground for *Elements VII–IX*). For the definition of number, the two goals diverge: the definition called for by philosophy differs from that called for by arithmetic. It appears that the philosophical goal has higher precedence: which is indeed to be expected given everything else we know about the relative positions of philosophy and mathematics in antiquity.

1.5. A decimal positional system

One of the major tools of modern mathematics—and one that makes

⁷ I have argued for this interpretation of Greek definitions in [Netz 1999a, chap. 3]. This is of course not to deny that some Greek mathematical definitions, e.g. the definition of 'being in the same ratio' in *Elements* V, are motivated by mathematical concerns and effectively serve as the axiomatic foundation for later arguments; for this axiomatic function of Greek definitions see [Mueller 1991].

its arithmetization all that much easier—is the decimal positional system. The system 'carries on its sleeve' its infinite extendibility and in this way serves to present to the eye, in a very clear way, the nature of enumerable infinity; above all, it is a superb tool for calculation. Its absence from Greek mathematical practice may therefore be correlated with the overall non-arithmetical character of Greek mathematics.

In subsections 1.1–1.4 above I gave examples where an established position, based on the argument from conceptual impossibility, claims that the Greeks did not have X, in the face of a counter-example. This is not the structure of the example in this subsection. There is no standard argument showing why the Greeks could not have a decimal positional numerical symbolism. To the contrary: it is often suggested that Archimedes in the Sand-Reckoner had come close to producing such a system.8 In a paper [Netz forthcomingb] I argue that this was not the case: Archimedes' Sand-Reckoner does not set forth a decimal positional system so that we can say in general that Greek mathematics never did produce anything resembling such a system. The thrust of this subsection is in my argument why this did not take place. Since the Sand-Reckoner is the closest Greek mathematics come, in the extant sources, to putting forward a decimal positional system, its absence there may tell us about the deeper reasons for its absence elsewhere. Once again, I argue, it's not that they couldn't: Archimedes was capable of developing a decimal positional system and preferred not to.

Note first of all that Greek mathematicians, after all, were acquainted with a form of the decimal positional system, namely the abacus. The ancient Mediterranean abacus—the normal instrument for any calculation in Archimedes' world—simply was a decimal, positional system. We must of course bear in mind the nature of the instrument: unlike the Eastern abacus (with its beads carried along pre-determined wires) the Mediterranean abacus consists simply of small counters moved along surfaces which, in the surviving archeological evidence, are usually not marked by particular values. In other words, the instrument consists of a series of scratches dividing rows to which the calculator assigns, for the given

 $^{^8}$ E.g. [Mugler 1971, p. 129]: "Pour s'affranchir de son côté des limites imposées à la numération par le langage courant, Archimède invente un système de notations comparable à nos systèmes de puissances de 10."

calculation, values such as 'units', 'fives', 'tens' and onwards. (One typically distinguishes odd, 10^n positions, from even, $5 \cdot 10^n$ positions: this may serve perhaps so as to make the maximum number of counters on a single position no more than five—a number with important cognitive significance.) The essence of the calculation algorithms is that, when you have two counters in a $5 \cdot 10^n$ position, you may remove them and put a single counter on the position to (e.g.) their left; or, when you have five counters in a 10^n position, you may remove them and put a single counter, once again, on the position to (e.g.) their left. The crucial thing to notice is that the system is fully positional, including the central property that position is a relative, not an absolute term. Calculation is blind to the question, whether a position stands for 'fives', for 'fifties' or for 'five hundreds': it merely distinguishes the odd and even sequence of positions.

It would be preposterous to claim that Greek mathematicians—or indeed practitioners in general—had the concept of the decimal positional system as long as they dealt with counters on surfaces, but had lost that concept as soon as they had applied themselves to papyrus. It is probably best to avoid talking of 'concepts' at all; but if we wish to use the word than we should say the Greeks did have the 'concept' of the decimal positional system. However, it remains true that Greeks did not write numbers down with a decimal positional system. The most efficient Greek written numerical system—and the one common in Archimedes' own time—was that of alphabetical numerals. This uses as symbols the sequences α — θ for 10–90, ρ – γ for 100–900. M may represent 'myriad or 'ten thousands' (the largest number in Greek natural language). Combinations of such symbols (with a special tag to represent that a numeration counts

⁹ See the discussion of the cognitive history of the abacus in [Netz 2002]. The best overall treatment of the archeology as well as the mathematics of the Greek abacus is [Schärlig 2001], while the central study remains the series of publications [Lang 1957, 1964, 1965, 1968].

¹⁰ As pointed out in [Hoyrup forthcomingb], it is this remarkable semiotic property which makes algorithms so effective with positional systems.

¹¹ It should be noted that the abacus does contain a representation for zero (otherwise, indeed, the algorithms could never work)—namely, the empty slot. This representation does not become written, because the Greeks chose not to write down in a direct way the abacus. As soon as the abacus got written down, the decimal positional system we are acquainted with was invented, zero and all. That is: there was no conceptual breakthrough required in order to invent the sign 'zero', all it took was the decision, to represent the abacus in writing.

not units but thousands) can easily represent numbers up to a few millions. This was sufficient for any Greek practical purpose though obviously very limited as far as very large numbers are concerned.

When, in the Sand-Reckoner, Archimedes wishes to introduce a new system for the representation of large numbers, the most natural thing for him to do would have been to create a written correlate of the abacus-in other words, a system equivalent to our decimal positional system. 12 This however is not what he did. Instead, he developed the following system. The numbers from one to myriad myriads are called 'first numbers'. (Myriad myriads are what we call now a hundred million.) The last of these first numbers, then, is myriad myriads. We take this as a 'monad' or 'unit' of the second numbers. We may consider then the myriad myriads monads of the second numbers: this Archimedes takes as the monad of the third numbers. With this repeated, we have finally arrived at the notion of the myriad myriad numbers. Myriad myriad monads of this we consider as a monad of the first numbers of the second period. (It turns out that until now we have dealt only with numbers of the first period.) This second period, too, has first, second, third and fourth numbers... and myriad myriads monads of its myriad myriad numbers would serve, naturally, as the monad of the first numbers of the third period-continuing in such a way until we reach myriad myriad monads of the myriad myriad numbers of the myriad myriad period. This is the largest number represented in Archimedes' system.

That this is not the decimal positional system is apparent from the fact that Archimedes' system *stops*. Instead of an equivalent of a decimal positional system (which Archimedes had right under his nose, in the abacus) Archimedes had developed a highly artificial system, which serves no obvious mathematical or scientific purpose (and indeed was never used by anyone in antiquity). What is the goal then? This is stated very clearly by Archimedes himself:

"Some [...] think that there exists no named number of such a size that exceeds its (i.e. the number of the sand's) multitude. [...] But I shall try to show to you that certain of the numbers named by us and published

 $^{^{12}}$ It is not without interest to point out that our own decimal positional system arises essentially as a written counterpart of the abacus: see [Herreman 2001] for discussion of the semiotics of the abacus.

in the work 'Against Zeuxippus' exceed not only the number of the sand having a magnitude equal to the filled-up earth, but also the $\langle \text{number} \rangle$ of the $\langle \text{sand} \rangle$ having a magnitude equal to the cosmos" [Heiberg 1913, 216.2–218.1].

Some people believe that (not-P), and Archimedes is going to show that (P). The work should—and does—revolve around this statement P which we may extract as follows:

"The number of sand13 is among named numbers."

In other words, the goal of Archimedes in the *Sand-Reckoner* is to name a fabulously large number. Once this goal is understood it is immediately obvious why Archimedes did not even consider anything equivalent to our decimal positional system in the *Sand-Reckoner*. The decimal positional system provides typographic symbols, but not names.

- (a) 'A Thousand Myriads of the Seventh Numbers', Archimedes' estimate of the number of sand in the *Sand-Reckoner*, is clearly a name, but
 - (b) 100000000... (with 63 zero signs)

is not: (a) is a series of natural Greek words, arranged in an easy to parse form, while (b) is an opaque and unpronouncable sequence of adhoc typographic symbols.

The Sand-Reckoner is a treatise addressed to King Gelon, that is, not to a professional scientist; Archimedes' position in this treatise is comparable, then, to that of Euclid in the introduction to Elements VII. The work addresses a non-mathematical audience and is attentive to non-mathematical goals. The very notion of the 'number of sand' arises from a well-known literary trope [Nisbet and Hubbard 1970, p. 321], and the treatise as a whole reads, in places, as a piece of literary prose rather than technical science. This explains, I believe, its goal, which cannot be defined in pure mathematical terms either: for the notion of a 'name' is not mathematical but semiotic. Once again, we see that when mathematics intersects with extra-mathematical interests and goals, the extra-mathematical seems to take precedence: and so, at the point where a Greek mathematician comes closest to inventing an original and more effective numerical system, the result is not equivalent to a decimal positional system and is in fact mathematically useless. To sum up: the

¹³ 'The number of sand' is the number of grains of sand it would take to fill up a universe whose size is based on a very generous estimate.

Greeks did have the concept of a decimal positional symbolism. They could develop it in writing; as far as the extant sources reveal, they did not. This may be because other goals had to them, at the crucial moments, higher precedence. I return to discuss this explanatory model in section 2 below.

1.6. The mathematics of calculation

There is very little calculation one needs to perform as one reads through Greek mathematics. Discussions are either in 'pure' arithmetic as in Euclid's *Elements* VII–IX, at most with some examples, calling typically for no calculation at all; or they are geometrical to the exclusion of any numerical values. The exceptions in, say, Hero, typically involve simple numerical values at the level of the elementary school. Some astronomical calculations, as well as some exceptions in geometry (e.g. Archimedes' Measurement of the Circle) or arithmetic (e.g. Diophantus' Arithmetic) can be mentioned. Even in those cases, calculation itself is hidden from the eye: the mathematician announces the terms and the operation and then announces the result. The reader might wish to check, for instance, that, in a right-angled triangle, if the hypotenuse stands to one side in the ratio 2:1 or 306:153 then the ratio between the sides is almost indistinguishable from 265:153. ¹⁴ But the checking is of limited importance in the argument itself where the most important thing for Archimedes appears to be the validity of the technique for deriving close boundaries on the ratio between the perimeter and the diameter of the circle. What we hardly see in extant Greek mathematics is an interest in calculation as such. The value of persuasive proof seems to be higher than that of precise calculation. 15 Since the two are in inherent tension (calculation is opaque and prone to mistakes), it is natural that calculation should give way to persuasion so that, on the whole, we predict that Greek mathematicians would devote most of their energies to the study of qualitative features of geometrical configuration (where proof is most compelling), rather than to numerically rich mathematical objects (where the need for complex calculation would

¹⁴ As Eutocius does in his commentary to Archimedes' Measurement of the Circle: [Heiberg 1915, p. 234 7–16].

¹⁵ This stress on proof would follow from standard pictures of Greek intellectual life, especially following Lloyd's work on the cultural and political setting of Greek science: see especially [Lloyd 1979, 1990].

tend to dilute the compelling power of proof). This is indeed the picture as it emerges from the extant sources.

As a consequence, we also expect that certain mathematical sciences would be left completely on the sideways of Greek science. Combinatorics, in particular, has calculation as its soul: the point of an argument in combinatorics is to show how a number is found, the fun is in actually finding it. The absence of combinatorics from the extant sources on Greek mathematics was thus taken to be meaningful, giving rise to verdicts as following (I use the quotation from a path-breaking study by Fabio Acerbi, to which I shall immediately turn): 'The Greeks took no interest in these matters [= combinatorics]', [Biggs 1979, p. 114]. In other words, the view used to be that they couldn't.

In [Acerbi 2003], renewed attention is paid to an obscure passage in Plutarch, De Stoicorum Repugnantiis [1047C-E]: "But now he [Chrysippus says himself that the number of conjunctions produced by means of ten assertibles exceeds a million, though he had [not] investigated the matter carefully? Chrysippus is refuted by all the arithmeticians, among them Hipparchus himself who proves that his error in calculation is enormous as in fact affirmation gives 103049 conjoined assertibles and negation 310954." 16 It was very recently observed by mathematicians that the numbers in question can be interpreted in technical combinatorical terms, [Stanley 1997], [Habsieger et al. 1998], and Acerbi has shown how Hipparchus could have obtained them as correct answers to the problem extractable from Chrysippus' statement. Since the numbers are the correct answer to the mathematical question, it is effectively proved that Hipparchus had worked out the relevant piece of combinatorics: as shown by Acerbi, this is a remarkable feat of ingenuity, which however is quite feasible given quite elementary mathematical tools.

Once again, then, we see a claim of impossibility—followed by a counterexample, and a very striking one at that. Of course it is now clear that Biggs was wrong and it turns out that Hipparchus, at least, was interested in combinatorics. But views such as Biggs' are convincing, to start with, only as long as we believe that different cultures are characterized by conceptual blocks that define what they can and cannot do. Absent that

 $^{^{16}}$ I emend the text as I believe we now have to: the manuscripts have 310952 but the mathematical analysis by Acerbi makes 310954 an almost certain reading.

belief, we should not for a moment follow such categorical claims as 'the Greeks did not have X'.

But what should we do about a counter-example such as Acerbi's? Shall we simply give up on the very notion of differences between cultures—admit, finally, that mathematics is always the same? In fact, Acerbi's counter-example reminds us of a central methodological concern regarding the past: namely, the selective nature of the survival of evidence. We happen to know about Hipparchus' treatise through a rather irrelevant claim in a philosophical treatise; the mathematical text itself left no trace in the surviving sources. How many other such works were lost and how much of our picture of the past—the sense that different cultures are different—is not simply a product of this optical illusion of aleatory survival?

Of course we should always be humble in the realization of our ignorance. Yet it is also important to note that survival, after all, is not aleatory. What made works survive? The fact that there were readers interested in them: the fact that they were valued. The pattern of survival of evidence for past cultures is the result of the overlay of patterns of past values: to survive, a work had to be valued enough, often enough. Such, apparently, was the case with Greek geometry—it was valued enough, often enough. Such was not the case with Greek combinatorics. So that, finally, we are back at the starting-point, substituting Biggs' absolute claim that the Greeks (all Greeks!) did not value combinatorics with a qualified, relative claim: many Greeks valued combinatorics less than they did geometry. History is greatly affected, I believe, by such hierarchies of value: I now move on to discuss such methodological claims in greater detail.

2. DISCUSSION: THE CHOICES MATHEMATICIANS MAKE

As the reader realizes by now, I believe we should be critical of the very notion of 'concepts'. Let me begin with a few general words on what, I believe, may be the historiographical background.

The historiography of conceptual structures is, perhaps, no more than the benign version of the history of mentalities. Not that long ago, the view used to be that, for instance, Greek science is explicable as expressing a certain Greek mentality that aspired at the abstract, etc. Lloyd [1990]

is now the locus classicus for the attack on such views which, at the end of the day, are nothing but the product of colonialist, racist prejudice. Enlightened history of ideas in the twentieth century has always abhorred this reduction to 'mentalities', but what would be the alternative to that? Just to say that 'everyone is and always has been the same' is just to give up on the job of the historian. In the history of mathematics, this would be to say that mathematics is always the same—that the differences are merely those of notation. Yet there is a strong intuition that different mathematical cultures are different: and so, as 'mentalities' are cast away, 'concepts' are brought in. Concepts are, then, the politically correct version of mentalities. It seems to me that, in authors such as Klein, concepts do precisely the same job mentalities were supposed to do: they are taken to constitute the 'mode of thought' of a group of individuals, without any historical account of why these mental possibilities should be indeed limited in that particular way. The only-laudable-difference between 'concepts' and 'mentalities', is then political: 'concepts' do not carry with them any suggestion of *inherent* inferiority on the part of the conceptually challenged. Otherwise 'concepts', in the sense of the 'mode of thought' of a group, are just another way of extending the arbitrary historiography of mentalities.

Hoyrup has set out in his article 'Conceptual Divergence-Canons and Taboos-and Critique' to criticize this very historiography of 'modes of thought'. He does not do this by dismissing the notion of the 'concept' as such, but by relativising and qualifying it. There is indeed another, more historically realistic way to consider concepts: as tools. It is clear that as people produce artifacts-such as treatises-they have recourse to several tools that are culturally available: clay tablets or papyrus; or certain methods, terms and theoretical assumptions. So it is clear that the notion of the 'tool' can be extended from material objects, to cognitive and indeed theoretical objects. There is just so much you can do with the differential and integral calculus, which you cannot do without it: in this sense it makes absolute sense to speak of the possibilities opened up by 'conceptual tools'. Hoyrup qualifies this in two ways. First, there is no clear-cut way to define the possibilities opened up by a tool, whether material or cognitive. There is always a grey area of what a tool can do, depending on which task you put it to: grey area which is not fully

determined by the tool itself—so that a dialectic of tools and tasks ensues. So it's not that they couldn't, quite: rather, it is that they gradually could. This (Marxist) historiography has been pursued by Hoyrup himself as well as by other authors (especially Peter Damerow¹⁷) always with great insight and interest.

Yet the entire historiography of tools is ultimately, I find, unsatisfactory—unless, that is, we subscribe to the notion that intellectual progress must follow a certain pre-established order. ¹⁸ For otherwise, how do we explain the basic claim, that 'they diverged from us in that they had tool X and not tool Y'—how do we account for the fact that working with tool X, they did not invent tool Y? Is it because that they *couldn't*? Indeed this is plausible if we assume a certain route where concept A is necessary before you can contemplate concept B which is necessary before you contemplate concept C. But without this assumption of a *natural order of conceptual tools*—an assumption which, in cool reflection, seems much too strong—the historiography of conceptual tools collapses back into the historiography of concepts. It is once again saying that they couldn't.

Hoyrup's second qualification of the historiography of conceptual tools—and the main argument of his article—is as follows. Many divergences in mathematical practice do not arise, in fact, from any conceptual divergence, but they rather represent the taboos of representation, or the result of philosophical critique (it is in this context that Hoyrup discusses Euclid's definition of number). People, in different cultures, may share exactly the same concepts; only that they would prefer not to express themselves in a certain way, avoiding for instance mention of '1' as a number, for a reason which is essentially that of superficial, stylistic preference.

The evidence Hoyrup marshals for the origin of the divergence between kinds of mathematics in such taboos of representation is that, in fact, we can find exceptions (sometimes systematic) to the rules. (Hoyrup analyses this with evidence from the Ancient Near East in particular.) It appears that to Hoyrup, any strong tendencies that are not the product of conceptual impossibilities are to be ascribed to explicit codification—

 $^{^{17}}$ See [Damerow, Freudenthal, McLaughlin and Renn 1992], [Damerow 1996].

 $^{^{18}}$ This is precisely the assumption made by Damerow or Freudenthal, based on Marxian and/or Piagetian principles.

to an explicit manual of style telling the authors how they should write. I doubt this (and I shall immediately sketch below an alternative account) but I wish to note the more fundamental point of agreement I share with Hoyrup. Namely: these divergences, to which counter-examples can so often be found, are to be excluded as any evidence for 'modes of thought'.

The implication, to Hoyrup, seems to be that they are therefore of less importance for the history of mathematics itself. His view, if I understand it correctly, is that the taboos on allowed forms are somehow external to the mathematical thinking itself: this is where mathematicians pay lip-service to rules that are more or less imposed from the outside in a mechanical way.

Here is where I differ from Hoyrup. My argument in this paper is that such taboos of representation are in fact of great historical consequences and, finally, can do a great deal of the task the history of conceptual structures was taken to fulfill. In fact, they can account for mathematics' not always being the same. To make the argument, I shall sum up the evidence covered in section 1 above.

First-contrary to claims made in the literature-Christianidis had shown that Greeks were capable of dealing with fractions in operations such as 'the *n*th of *m* multiplied by the *q*th of *p* gives *nq*th of *mp*'. This could be taken in two ways. First-the argument Christianidis makes—this shows that there was no conceptual block preventing Greeks from articulating operations with fractions. But in the absence of conceptual blocks, we should not expect total absence but merely relative scarcity. This is what we in fact see: Christianidis' example from Diophantus is indeed remarkable for its isolation. The formulation I prefer, then, is that the Greeks did *not* articulate operations with fractions—although there was no conceptual block preventing them from doing so.

Second, I have shown the example from Archimedes' Sphere and Cylinder, Book II, where Archimedes uses, effectively, the expression 'the area multiplied by the line'. Once again: what we see is that the Greeks did not articulate geometrical relations in terms of arithmetical operations, although there was no conceptual block preventing them from doing so. The discussion for Archimedes suggested why this might be the case: in Archimedes' context, the arithmetical presentation of geometrical

relations would have been marked and therefore would be avoided in the unmarked setting. This, magnified to the level of entire treatises or even genres, can be re-told for Hero's geometrical work: it appears, following Tybjerg's study, that Hero systematically sets numerically geometrical objects, so as to set his work apart from geometry in the Euclidean-Archimedean tradition: numerical values are marked and thus carry a meaning.

My fourth example, based on Hoyrup, is different in character. Hoyrup had shown that the Euclidean (and philosophical) definition of number to exclude unity is ignored in mathematical practice. This however is not representative of any deeply held conviction shared by Greeks as such: the exclusion of unity is the standard position to take for philosophical purposes, while unity is typically included for mathematical purposes. Cultures, then, display an array of practices, depending on the goals of the practice; significantly, the philosophical goal takes precedence in the 'mixed' practice of mathematical definition (that is simultaneously 'philosophical' and 'mathematical'), creating a gap between the definition and the proofs.

A similar hierarchy explains, I believe, my fifth example: the Greek failure to articulate a written decimal positional system (although the concept was easily available in the abacus practice). Greek writing was dominated by verbal, indeed literary values: when Archimedes sat down to introduce a new numerical system, it was important not that it would be an efficient tool for calculation, but that it would be a able to name, in a natural language sense, fabulously large numbers.

Finally, the very marginality of calculation-heavy science in the ancient Greek world is explicable on such grounds. As Acerbi has shown, fairly sophisticated combinatorics was developed in the ancient world. However, I would argue that it was always secondary in importance relative to the main form of Greek mathematics, in geometrical proof—the genre where the art of persuasion is brought to perfection.

Examples 1–3 point at the following principle of markedness:

Cultural activities show a variety of more and less common practices: the less common practices, just by their scarcity, become marked and therefore carry a meaning; they are thus used only so as to carry that meaning, a choice that further perpetuates their scarcity, markedness and

meaning.

I have offered this principle in [Netz 1999a, chapter 2], using it to explain the phenomenon of self-perpetuating conventions. I have described in that book the strong tendency of Greek mathematicians to follow a certain canonical form of writing, which however is sufficiently often violated to show that there was no active constraint forcing anyone to follow the form; instead, the convention was self-perpetuating in the obvious way described above. In my discussion in section 1.2 above I referred to the seemingly 'tautological' nature of this principle, which comes down to saying that people 'do not usually do what they do not usually do'. I am not afraid of the tautology: tautologies, after all, are true. This particular tautology is also informative, because it should be taken in a dynamic sense: people keep on doing what they usually do, just because this is what they usually do—so that deviating from the norm becomes marked.

The principle of markedness explains, however, the self-perpetuation of conventions rather than their origin. To understand this, we need to see why cultural practices in a given culture tend, to start with, to have a certain pattern of distribution: this being done more often than that. Examples 4–6, I believe, go some way towards explaining that. The moral arising from the examples may be summed up as the principle of *hierarchy of values*:

In a cultural activity, the choice of preferred practices is determined by a hierarchy of values which is typical to that particular activity.

There are many values brought to bear on any particular activity, and depending on the different value, different practices might be expected. For the sake of efficient calculation, typographic representations of numbers are preferable; for the sake of proximity to natural language, verbal representations are preferable. Without the principle of hierarchy of values we might expect a balanced distribution of the practices but, in fact, this is not what we see. Practices are determined not by the totality of values brought to bear, but by the most important of such values: the value of efficient calculation is important to Archimedes—but, in the context of a literary treatise, it is trumped by an even more important value, that of proximity to natural language. Since the values rule out each other, one must give way to the other and so, naturally, the one higher up in the hierarchy is the only one visible in the practice. Thus practices do not do

sometimes this, sometimes that: they tend to do the very same thing over again—because they express not just any value felt by the practitioners, but rather they express a much narrower set of dominant values.¹⁹

We can therefore put forward the following explanation of the nonarithmetical nature of Greek mathematics. Greek literary production is marked by a hierarchy of values always related to a certain 'literary' or 'verbal' preference: literature is ranked above science, inside science philosophy is ranked above mathematics; persuasion (to the Greeks, the central verbal art) is ranked above precision and natural language above other symbolic domains. Hence it is easy to understand Euclid's deference to philosophy in his definition of number. More significant, inside Greek mathematical writings, the qualitative statements of geometrical demonstration-that allow the persuasive texts we are familiar with (though short on numerical information)—become the norm against which arithmetical representations of the same object come to be seen as marked. Hence Archimedes' multiplication of areas by lines, as well as Hero's geometry, are deliberately marked. On the whole, the articulation of the representation of the numerical system inside mathematical writing is blocked by the higher ranking of verbal representations (hence there are few variations on the standard form of representation, and those from the late author Diophantus, already active in a rather different milieu; while we can see why Archimedes himself had never articulated a decimal positional system). Finally, any number-rich science was marginalized simply because it did not serve the main value of Greek mathematical writing.

The main outcome of the principle of hierarchy of values is that one can predict considerable cultural variety, on the basis of strong universals. There is no need to assume that different practices, in different cultures, are based on different *values*, let alone concepts: everyone values both efficiency in calculation and proximity to natural language, everyone values both precision and persuasion. In practice, however, one must rank

¹⁹ In historiographical practice, of course, we need in principle to give a complete sociological account why, for practitioners in a certain setting, this hierarchy of values obtained and not another; otherwise, if we merely state that, say, 'the Greek hierarchy of values was?' we are open to all the methodological criticism against any history of mentalities. In this article I always mean 'the Greek hierarchy of values was?' as provisional shorthand for 'in the setting of high-culture texts of the Hellenistic period, the hierarchy of values was? and this is explicable in principle given the social setting of high-culture writing at the period'.

one's values somehow and it is only natural that, in different social and cultural contexts, different rankings would ensue. This subtle difference in ranking can then give rise to sharp differences in practice: some things become more common than others, more natural than others. This immediately gives rise to the principle of markedness so that the pattern of the practice is immediately endowed with meaning and is thus further sharpened; also, to a large extent, it is self-perpetuating. In this way we come up with an account where mathematics is not always the same, while people are: which forms, I believe, the historian's intuition.

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