When is the algorithm concept pertinent – and when not?

Thoughts about algorithms and paradigmatic examples, and about algorithmic and non-algorithmic mathematical cultures

Høyrup, Jens

Published in:
AIMS Mathematics

DOI:
10.3934/Math.2018.1.211

Publication date:
2018

Document Version
Publisher's PDF, also known as Version of record

Citation for published version (APA):
Høyrup, J. (2018). When is the algorithm concept pertinent – and when not? Thoughts about algorithms and paradigmatic examples, and about algorithmic and non-algorithmic mathematical cultures. *AIMS Mathematics*, *3*(1), 211-232. https://doi.org/10.3934/Math.2018.1.211

AIMS Mathematics, 3(1): 211–232.
DOI: 10.3934/Math.2018.1.211
Received: 19 February 2018
Accepted: 27 February 2018
Published: 29 March 2018

http://www.aimspress.com/journal/Math

Review

When is the algorithm concept pertinent – and when not? Thoughts about algorithms and paradigmatic examples, and about algorithmic and non-algorithmic mathematical cultures[1]

Jens Høyrup*

Section for philosophy and Science Studies, Roskilde University, Denmark

* **Correspondence:** Email: jensh@ruc.dk.

For Karine
hard-working scholar who always
finds time for friendship

Abstract: Until some decades ago, it was customary to discuss much pre-Modern mathematics as "algebra", without agreement between workers about what was to be understood by that word. Then this view came under heavy fire, rarely with more precision.

Now, instead, it has become customary to classify pre-Modern practical arithmetic as "algorithmic mathematics". In so far as any computation in several steps can be claimed to follow an underlying algorithm (just as it can be explained from an "underlying theorem", for instance from proportion theory, or from a supposedly underlying algebraic calculation), this can no doubt be justified. Traditionally, however, historians as well as the sources would speak of a *rule*.

The paper first goes through some of the formative appeals to the algebraic interpretation – Eisenlohr, Zeuthen, Neugebauer – as well as some of the better argued attacks on it (Rodet, Mahoney).

Next it asks for the reasons to introduce the algorithmic interpretation, and discusses the adequacy or inadequacy of some uses. Finally, it investigates in which sense various pre-modern *mathematical cultures* can be characterized globally as "algorithmic", concluding that this

[1] The following essay was originally presented as a contribution to the International Conference on History of Ancient Mathematics and Astronomy "Algorithms in the Mathematical Sciences in the Ancient World", held in Xi'an, 23–29 August 2015. Given its character as an invitation to reflection and discussion I have preferred to leave much of the style of an oral presentation.

characterization fits ancient Chinese and Sanskrit mathematics but neither early second-millennium Mediterranean practical arithmetic (including Fibonacci and the Italian abbacus tradition), nor the Old Babylonian corpus.

Keywords: historiography of mathematics; algebraic interpretation of early mathematics; algorithmic interpretation of early mathematics; algorithmic cultures; paradigm-based mathematics teaching; classical Chinese mathematics; classical Sanskrit mathematics; Babylonian mathematics; abbacus mathematics; rule of three; Fibonacci
Mathematics Subject Classification: 01A85

Contents

Algebra – a parallel and perhaps illuminative case

There is an old tradition for connecting algebra with geometry – beginning with al-Khwārizmī, author of the very first extant presentation of the discipline that at least etymologically developed into the algebra of Early Modern and ultimately modern times. Neither this beginning nor the later repetitions of the process at mathematically higher levels can be objected to – one of the ways mathematics grows is by producing bridges or mergers.[2]

A different matter is the application of algebra as a historiographic tool for interpreting other types of mathematics. This is a much younger occurrence – famous early instances are August Eisenlohr's and Moritz Cantor's approach to the problem solutions of the Rhind Mathematical Papyrus, and Hans Georg Zeuthen's notion of a Euclidean-Apollonian "geometric algebra".

Eisenlohr, as a matter of fact, does not speak about "algebra"; but he does refer [1877: 5 and passim] to some of the Rhind Papyrus problems as "equations".[3] He also speaks of the *'ḥ'* (meaning "heap" and referring to a generic quantity) as the "unknown magnitude in first-degree equations", and renders the corresponding problems as symbolic first-degree equations – for instance $\frac{x}{7}$ + x = 19 (pp. 60*f*). In the same vein he refers to a "common denominator" (p. 8 and passim). In short, Eisenlohr uses the mathematics he knows, and which he supposes his readers to know (rather simple school mathematics) as an explanatory tool, without pursuing the question whether this corresponds to the conceptualizations of the Middle Kingdom writer. Though slightly more cautious ("we shall write [these problems] as equations"), Moritz Cantor follows Eisenlohr on both accounts in volume I of his Vorlesungen [1880: 33].

[2] At most, those who distinguish "pure" not from "applied" but from "impure" in the realm of mathematics can object – as do Michael Fried and Sabetai Unguru [2001: 24].

[3] *Gleichungen*. Here, as generally if no translator is identified, the translation is mine.

What Eisenlohr had not claimed explicitly was however read as if it had been by Léon Rodet, who published an article on “The purported algebra problems in the manual of the Egyptian calculator” in [1881]. Basing himself on a large quantity of parallel examples in Arabic, Sanskrit, Hebrew and Persian as well as philological reading of the papyrus, he argued that the method applied by the Egyptian calculator was the simple false position[4] and the related trick to add fractions by taking them of an adequate *bloc extractif*.[5] Rodet reproaches “the two learned professors from Heidelberg” to let “a writer from the eighteenth century BCE [...] think and act too much as we think and act today”.[6]

Zeuthen’s appeal to the notion of algebra was quite different.[7] As a mathematician and a geometer centrally involved in advanced research he understood algebra not simply as the technique of equations. In his view, algebra was, on one hand, a way to deal with “general magnitudes, rational as well as irrational” (“Dedekind’s cut” had been introduced just 14 years earlier); on the other it made use of “means different from ordinary language in order to make its procedures manifest [*anschaulich*] and inculcate them in memory”. *In this sense*, he characterized *Elements* II. 1–10 as a “geometric algebra”, saying [Zeuthen 1886: 7, Zeuthen’s emphasis] that[8]

> in Euclid’s times it had developed so far that it could manage the same tasks *as our algebra, as long as this does not go beyond expressions of the second degree*, an area which it also, as it will turn out, filled out in its application to the conic sections. This application corresponds to the application of our algebra in analytic geometry.

Analogous function is also what Zeuthen speaks about in the preface (p. IX), where ancient geometry is claimed “not to have been developed for its own sake only but also to have served as a general theory of magnitudes, similarly to what algebra does today”.

---

[4] Rodet points out that Cantor had recognized on his p. 36 the use of this method in another problem of the papyrus.

[5] For example, in order to add $\frac{1}{3}$ and $\frac{1}{4}$, we take both fractions of some adequate number, say, of 12. $\frac{1}{3}$ of 12 is 4, and $\frac{1}{4}$ of 12 is 3, together 7. But 7 is $\frac{7}{12}$ of 12, whence the sum of the fractions must be $\frac{7}{12}$.

[6] He also reproaches Rodet and Cantor to see in the *ʿḥʿ*-problems “the origin of algebra” (p. 188), which is a misreading. Eisenlohr says nothing of the kind. Cantor on his part simply says that “they are according to their contents nothing but that which present-day algebra calls first-degree equations” (p. 32), and that it “was not uncommon to solve problems of an algebraic nature” in Egypt (p. 158).

[7] This, as well as the relation between what was said by Zeuthen and what was said by Paul Tannery, is treated in more depth in [Høyrup 2017: 130–134].

[8] It is often claimed that Zeuthen took over his “geometric algebra” from Tannery [1882] – [Szabó 1969: 457 n. 6] is but one example. This belief is the result of sloppy reading and failure to be interested in what Zeuthen says and aims at. Tannery has no explanation similar to that of Zeuthen, he only speaks about “geometric solution of second-degree problems”, which he then expresses in symbolic equations. There is nothing new in this. Apart from using Oughtred- and hence ultimately Viète-inspired and not Cartesian symbols, Isaac Barrow [1659: 39–47] had done as much when explaining *Elements* II; ultimately the conflation of algebraic expression and *Elements* II can be traced back to Abū Kāmil.

In [1887: vi], Tannery was to refer to Zeuthen’s work on the conics as having anticipated him, filling this lacuna in his general presentation of Greek geometry “better than he could dream of doing himself”. This does not endorse Zeuthen’s concepts explicitly, but almost.

On page 12 Zeuthen states that *Elements* II, propositions 1–10 can be written as follows:

1. $a\,(b + c + d + ...) = ab + ac + ad + ...,$
2. $(a + b)^2 = (a + b)\,a + (a + b)\,b,$

…

but this is nothing but a symbolic shorthand, the discussion afterwards returns to geometry. All in all, it would be difficult to object to what a careful reading finds in the text, as long as one accepts Zeuthen's analytical aim – even though one might of course claim that he should rather have been interested in vegetarian cooking, in athletics (most ancient Greeks were) or in ancient ways of thought *without* trying to elucidate them by means of structural parallels.

Yet, as experience teaches, reading is not always careful, and many of Zeuthen's twentieth-century readers have got from his pages that Euclid was *really* making second-degree algebra.[9] One example, to judge from the almost identical formulas possibly borrowed directly from Zeuthen, is offered by Morris Kline [1972: 65]. According to Kline,

> The first ten propositions of Book II [of the *Elements*] deal geometrically with the following equivalent algebraic propositions. Stated in our notation some of these are:
>
> 1. $a\,(b + c + d + ...\,) = ab + ac + ad + ...,$
> 2. $(a + b)\,a + (a + b)\,b = (a + b)^2,$
>
> …

Nothing here goes directly against Zeuthen's caution, nor is it however repeated. Primarily, according to Kline, Euclid's text *deals with* algebraic propositions, the geometric explanation (which *is* given) comes afterwards, and is followed again by symbolic algebraic calculations.

Of particular consequence was Otto Neugebauer's adoption of Zeuthen's notion. His monumental three-volume *Mathematische Keilschrift-Texte* [MKT] from 1935–1937 uses algebraic symbolism abundantly, but does this in order to explain why the Babylonian procedures are correct, not as explanation nor *a fortiori* as purported interpretation of underlying patterns of thought. It also speaks a dozen times of certain methods as algebraic, or *arithmetisch-algebraisch* – apparently first of all to contrast them with geometric methods. Similarly to Cantor, and different than Zeuthen, Neugebauer seems not to feel it necessary to explain the word except negatively and by reference to the appearance of purely "formal" manipulations (if sides are added to areas, what we look at cannot be geometry and therefore must be algebraic – vol. II, p. 64).

But MKT was meant to be only a presentation of sources with the necessary explanations, not to draw general consequences (III, p. 79). We might look at Neugebauer's other writings from the epoch – in particular two articles containing "Studies in the History of Ancient Algebra" [1932; 1936]. In the former (p. 1) we read that he considers even problems that deal with geometric objects as "algebraic" if only they appear to be based on formal operations with magnitudes. In the latter (p. 246), we see that the criterion for this is the addition of inhomogeneous expressions (lengths and

[9] Though not as glaringly as many of his readers, Zeuthen himself becomes somewhat careless in his popularization *Geschichte der Mathematik im Altertum und Mittelalter*. Here [Zeuthen 1896: 44*f*] it seems that the Ancients were really *intent* to create a parallel to more recent algebra – for example, "Multiplication of two general magnitudes [represented by straight segments] makes no sense according to their immediate meaning. One therefore resorted to" representation by means of rectangles, already suggested by the figurate numbers.

areas, workers and days). Moreover, Neugebauer returns to an idea he had presented in [1934: 68–72]: that ideograms in the mathematical texts could function as mathematical symbols.[10]

As we see, Neugebauer's reasons to find algebra in the Babylonian texts differed from those of Eisenlohr and Cantor, and also from those of Zeuthen; though he is not explicit in this respect, he is probably inspired by recent developments of the field – which will not astonish, he was after all an offspring of the Göttingen environment. But he does not seem to notice that he has changed Zeuthen's understanding – in [1936: 249] he gives him credit for "the fundamental insight that in particular *Elements* II and VI deal with geometric presentations of properly algebraic problems".

Nor did those general historians of mathematics who a generation later drew on Neugebauer notice that they, on their part, changed the understanding back to that of Eisenlohr when reading his symbolic equations *as if* they had been paraphrases of the Babylonian texts themselves.[11]

Apart from Rodet, the first to assail the algebraic interpretations seriously was probably Michael Mahoney [1971], in an essay review of the reprint of [Neugebauer 1934].[12] Mahoney did not object to the idea which Neugebauer had formulated and van der Waerden later developed, namely, that Babylonian numerical "algebra" had inspired (or even somehow been translated into) Greek geometric algebra. Nor was he opposed to seeing the Babylonian approach as "algebraic", even though he would not consider it a genuine "algebra" – that term he would prefer to reserve for the mature type which was "a creation of the seventeenth century – AD" (p. 375); the algebraic approach, more or less developed, he saw (p. 372) as characterized by use of "a symbolism for the purpose of abstracting the structure of a mathematical problem from its non-essential content"; further, by search for "relationships (usually combinatory operations) that characterize or define that structure or

[10] However, the characteristics of symbols which enter his discussion are only these two – cf. [Høyrup 2006: 57–61]:

- they make it possible to combine magnitudes with each other that are not numerically denominated and to derive new combinations from them;
- the existence of conventional single symbols of ideographic type ensures the easy grasp of operations.

There is no request that one should be able to perform operations directly at the symbolic level, without recourse to understanding through normal language (as *we* do, for instance when reducing equations), nor explicit prohibition of ambiguity asking for context-dependent interpretation. On both accounts, the Babylonian "symbolism" would fail.

[11] Thus Morris Kline [1972: 9], as I discuss in [Høyrup, forthcoming].

[12] Formally, priority goes to Árpád Szabó, whose *Anfänge der griechischen Mathematik* appeared in [1969]. However, criticism based on badly understood secondary reports can hardly be considered serious. According to Szabó [1969: 34*f*]

> It was Zeuthen who noticed that there are some interesting geometric propositions in Books II and VI of the Elements which would normally be written out as algebraic formulae. From this he concluded that they dealt with "algebraic propositions in geometric clothing", as he put it – with geometric algebra.

Actually, what is "quoted" comes from [Neugebauer 1936: 249]. It has nothing to do with Zeuthen's own work.

One wonders how Szabó could claim (p. 30) that Zeuthen "could not really furnish satisfactory answers" to such questions as "whether the Greeks actually in early times had an algebra which they later geometrized" in a book he apparently never opened. Already on page 2, Zeuthen [1886] states unambiguously that the Greeks did not know algebra (... *Algebra, welche die Griechen nicht kannten*).

link it to other structures"; finally, by being "a mathematics of formal structures, [...] totally abstract and free of any ontological commitments".[13] He pointed out that according to Neugebauer's own description the ideographic "symbols" which Neugebauer had found in Babylonian texts "left off right at the point that most clearly characterizes a symbolic algebra" (p. 374).

Mahoney praised Neugebauer for pointing out "the continuing existence before, during and after the Greeks of two alternative approaches to mathematics: namely, the algebraic and the geometrical" (p. 372), and argued that Greek geometry could not be characterized as algebraic. Just as Neugebauer (and everybody else) he had forgotten Zeuthen's actual arguments and explications.

Since this is only a prolegomenon, there is no reason to continue the discussion of how algebra was used to characterize or analyze kinds of early mathematics that were not already unquestionably algebraic. It should already be clear that every worker tended to refer to the kind of algebra with which he was familiar – school manipulation of equations, analytic geometry, or adequately interpreted post-Dedekind or post-Noether algebra. It is no less obvious that even when one worker explained which aspects of the algebraic natural family[14] were taken into account, these explanations were not read by others, neither by those who believed to borrow nor by those who objected or rejected. Mahoney is a partial exception, but even he understood Zeuthen through Neugebauer.

Read through the spectacles of the individual worker, *his* use of the notion of algebra mostly seems acceptable, even though concentration on the algebraic aspect may have barred the attention to other facets of the matter – *vide* the Eisenlohr-Rodet dispute. None the less, since the reading becomes awkward at best when received through the spectacles of somebody else one can ask to which extent the tool is *useful*. Even if we leave out considerations of objectivity (whose meaning is even more unclear than usually when tools and not results are concerned), reasonable *intersubjectivity* should be a minimal request.

Algorithms

In recent decades, it has gone out of fashion to speak of algebra in connection with mathematical techniques and theories that are not indisputably connected to the algebraic family – since few have taken the pain to come to grips with the precise interpretations they criticize, "fashion" must be the word. Instead of Mahoney's "two alternative approaches to mathematics: namely, the algebraic and the geometrical", we have got the "demonstrative" and the "algorithmic" kinds. Can this be regarded as more fruitful?

At least, the word "algorithm" is much less ambiguous. Admittedly, from the early thirteenth until the mid-nineteenth century, "algorism" (with the Renaissance hypercorrection "algorithm") meant computation with Hindu-Arabic numerals. This it obviously the etymology, but also another exemplification of the principle that "etymology is the lore of what words no longer mean". The earliest instance if not of the modern usage then of something preparing it which I have been able to trace is [Littré 1873: I, 106] (obviously not the absolutely earliest instance, dictionaries record what is already around):

[13] So, the description is, so to speak, a post-Noether (or post-Bourbaki) view of Cartesian and later algebra.

[14] Some critics seem to conceive of algebra as a Platonic idea. None the less, I prefer to understand it as a Wittgensteinian "natural family".

> ALGORITHME [...] 1° En terme d'algèbre, procédé de calcul. 2° Genre particulier de notations. Algorithme différentiel.

The second sense is not too far from Tannery's characterization of *Elements* II in 1882 as an "algorithme géométrique" (reprint [Tannery 1912: 257]). Neither, of course, have anything to do with the sense in which the word is used by modern mathematicians, informatics professionals and historians of mathematics. But the first, "calculational procedure", clearly has – and it antedates the appearance of electronic computers (programmable or not) by many decades.[15] It corresponds to the definition given in a recent textbook [Cormen 2009: 5]:

> Informally, an *algorithm* is any well-defined computational procedure that takes some value, or set of values, as *input* and produces some value, or set of values, as *output.* An algorithm is thus a sequence of computational steps that transform the input into the output.
>
> We can also view an algorithm as a tool for solving a well-specified *computational problem.* The statement of the problem specifies in general terms the desired input/output relationship. The algorithm describes a specific computational procedure for achieving that input/output relationship.

The first to apply the concept to historical material was probably Donald Knuth in [1972]. Using Neugebauer's translations he looked at some Babylonian computations and concluded (p. 672), not least because the texts multiply with a factor even when this factor happens to be 1, that they were not just *ad hoc* calculations ("merely the solutions to specific problems") but meant as "general procedures for solving a whole class of problems", where "the numbers shown are merely included as an aid to exposition, in order to clarify the general method". Nobody familiar with the material had probably thought the calculations (which were known to be school problems) to be isolated solutions of specific problems, but earlier writers had kept categories clear and not conflated the *procedure understood abstractly* and *illustrative examples meant to function paradigmatically*, with as much space for variation as called for in single case. In Knuth's view, on the other hand, the calculations *were* (thus did not exemplify, represent or instantiate) "general procedures". He concluded that "the Babylonian procedures are genuine algorithms".

On the other hand he found (p. 674)

> only "straight-line" calculations, without any branching or decision-making involved. In order to construct algorithms that are really nontrivial from a computer scientist's point of view, we need to have some operations that affect the flow of control

– and in order to find such non-trivial algorithms he had to interpret texts containing repeated interest calculations *as if* they had contained WHILE ... DO ... instructions. They do not, they are just as

[15] In contrast, the only pre-1950 example offered by the *Oxford English Dictionary* (Second Edition on CD-ROM, 2009) is a reference to "Euclid's algorithm" from 1938. This concept also appears, as "Kettenbruchalgorithmus", in [Tropfke 1902: I 65], whereas p. 123 of the same work refers, in agreement with Littré's second interpretation, to "the algorithm of differential and integral calculus", and p. 143 has "algorithm of determinants". "Kettenbruchalgorithmus" is already found in [Cantor 1880: 229, 273]; on p. 612, Cantor explains the modern reference of the word to be "any recurrent calculational procedure that has become a rule".

linear as the other examples. This, indeed, is the reason that Knuth can conflate the single example with the algorithm – *one unbranched path through a branching flow chart can never be the same as the flow chart itself.*

For Knuth, the algorithmic reading was no alternative to the interpretation through algebra, which for him was a given fact. Indeed, his aim was not to provide historians with a new interpretive tool but to "make computer science respectable" by showing "that it is deeply rooted in history, not just a short-lived phenomenon", for which it seemed adequate "to turn to the earliest surviving documents which deal with computation". As Knuth [1972: 1] saw it, the

> Babylonian mathematicians were [...] adept at solving many types of algebraic equations. But they did not have an algebraic notation that is quite as transparent as ours; they represented each formula by a step-by-step list of rules for its evaluation, i.e. by an algorithm for computing that formula. In effect, they worked with a "machine language" representation of formulas instead of a symbolic language.

Since then, however, Knuth's article, when referred to by historians as a source for the algorithmic reading, is stripped of algebra.

The reason for the broad adoption of the algorithmic reading is likely to have little to do with Knuth's work, which has just served as global justification – it may be cited by historians of mathematics but (as once Zeuthen) is never used substantially nor apparently read for the details of the argument in publications I have looked at.[16] We should rather look to an attitude (more precisely, opposition to that attitude) reflected in this quotation from [Kline 1972: 14]:

> [The Babylonians] did solve by correct systematic procedures rather complicated equations involving unknowns. However, they gave verbal instructions only on the steps to be made and offered no justification of the steps. Almost surely, the arithmetic and algebraic processes and the geometrical rules were the end result of physical evidence, trial and error, and insight. That the methods worked was sufficient justification to the Babylonians for their continued use. The concept of proof, the notion of a logical structure based on principles warranting acceptance on one ground or another, and the consideration of such questions as under what conditions solutions to problems can exist, are not found in Babylonian mathematics.

Behind this we find dichotomy once more: no longer between geometric and algebraic approach, but between mathematics built on demonstration[17] and mathematics consisting of "rules" based on "physical evidence, trial and error, and insight" and accepted because they work.

This should not in itself be a reason to speak of algorithms instead of rules. As Neugebauer had

---

[16] Computer scientists, on the other hand, often use Knuth's article with its originally stated intention, as a way "to help make computer science respectable". They obviously take Knuth's words uncritically at face value to the extent they are able to read them; [Maresca 2003] exemplifies that they are not always.

[17] "Logical structure" is of course a mere buzzword, if any mathematics produced before Russell and Whitehead is to count – Euclid does *not* build on Aristotelian logic, even though Aristotle's *Second Analytic* is inspired by geometers' striving for axiomatization. Whether Kline wants to imply axiomatics is unclear, "principles warranting acceptance on one ground or the other" may well be meant more loosely. Also unclear is what he means by "insight" – I would guess at some kind of intuitive direct understanding.

already pointed out long before Kline wrote [MKT I, 438], views like his are

> an obvious nonsense, which nobody would set forth who had taken the trouble to work through the full material of Babylonian mathematics. Indeed, this not only shows us that the examples of the single tablet from our series texts are often ordered with the most rigorous technical (*sachlich*) order [...], which is only made possible by real insight in the mathematical relationships; we also possess a wealth of other texts which prove through explicit calculation that, for instance, the particular biquadratic equations – which are also plentiful in the series texts – were solved absolutely correctly.

Whoever is familiar with early mathematical texts of the types that teach by means of paradigmatic examples will also know that many of them regularly explain the reason that something works – not from some axiomatic system but with reference to things the user can be supposed to understand.[18]

In any case, a main reason for adopting the description in terms of algorithms can be seen in this passage from [Acerbi & Vitrac 2014: 31]:

> In general, the algorithmic approach to mathematics, found in all ancient civilizations (Mesopotamia, Egypt, India, China, Greece) was misunderstood for long, assimilated to an empirical and scarcely rational approach, while instead it belongs to the prehistory of algebra.

This looks more like a spell than as an argument. We are not told why "algorithms" should be less empirical or more rational than "rules" (or "rules" more empirical and less rational than "algorithms") – and nowhere in the book is it explained why algorithms should belong "to the prehistory of algebra", which appears to be meant as explanation (*mal comprise ... alors que ...*). Since practically all the "algorithms" that can be extracted from the problem solutions produced by the mathematical cultures in question (not all the *calculation* prescriptions, on which below) are "straight-line", as Knuth complained, the gain by speaking of them as "algorithms" instead of "rules" as the sources do themselves (*nēpešum*, *mēthodos*, *regula*, etc.) seems to be that it sounds modern, and therefore presumably rational (because *we* believe to be all-round rational, so help us God!). In short, instead of *argument* we get *political correctness.* That is, instead of following Neugebauer's footsteps and arguing that (for example) the Babylonian problem solutions show their authors to understand why their procedures worked, we replace the despised "rule" by a euphemism.[19]

To this, it is true, comes another classical condition for scholarship – that is, *patronage*

---

[18] And, reversely: how many of those who use their computer or smartphone to extract a square root understand the algorithm that is used down to the level of machine code? Probably nobody, because of the division of labour between those who create operating systems and those who use them to develop applications (if not the intention then at least the effect of the "Microsoft .NET Framework" packages is to cut the bridge between the two levels). And how many understand the algorithm at least at the level of mathematics? Some, of course, but still very few. If anybody, modern users of algorithms are satisfied if they work. *Use* of present-day algorithms is no more reasoned than complying blindly with rules.

[19] Euphemisms and their kind, however, have great power; anybody trying to use "gay" as a synonym for "merry" or "queer" in the sense of "strange" will learn that from the reactions of the audience. In consequence, I shall surrender and mostly speak of "algorithms" and not of "rules" in what follows – though only when genuine *rules* and not mere calculations or examples are concerned.

(nowadays called "funding"). It is certainly easier to find pecuniary support for a project about "algorithms" than for one about "rules". But this condition for our work we may leave aside discreetly.

"Correctness" as well as patronage, so to speak, concern the "context of discovery", even though this context for the "discovery" of algorithms in early texts may not quite be of the kind Hans Reichenbach [1938: 6*f*] was thinking of when he introduced that concept – it certainly cannot be assimilated to that inductive experience which takes up a large part of his deliberations. Irrespective of the reasons for choosing the tool it may still be useful. As I shall argue in what follows, it sometimes is.

Firstly, of course, there *are* genuine algorithms, those which via their appearance in the *algorismus* genre gave original fame to the word, and their analogues elsewhere. Karine Chemla has written much about Chinese division and root extraction algorithms, and shown how analysis of their algorithmic structure elucidates the underlying thought, thus demonstrating that those who created them understood the underlying principles – I shall restrict myself to references to her [1987] and [1991]. But even though Sacrobosco does not write in a particular "quasi-language designed for the description and use of procedures" (cf. [Chemla 1987: 301]), we also find indubitable non-trivial algorithms in his early thirteenth-century Latin *Algorismus vulgaris*. Already in the initial teaching of the addition of two numbers [ed. Curtze 1897: 3], for instance, we find a branching depending on whether the sum of digits is a *digitus* (< 10); an *articulus* (= 10); or a *numerus compositus* (> 10).

However, even in such cases one may be led astray by thoughtless application of the algorithm concept. In a recent analysis of al-Kāšī's and Stevin's extraction of square roots [Aydin & Nuh 2015], the authors find that Stevin's procedure is flawed. What they actually *see* is that Stevin's *paradigmatic example* when understood as a straight-line algorithm does not work on al-Kāšī's example $\sqrt{331781}$. The reason is simple, and familiar to everybody who has worked on the algorithm in school: In the first step, $\sqrt{33}$ is approximated from below as 5, with remainder 8. In the next step, if the paradigm is followed to the letter, $3317 - 2500 = 817$ at first has to be divided by 5×20, with result 8. However, the real divisor should then be 108, and $8 \times 108 = 864$, larger than 817. Therefore, 8 has to be replaced by 7 (and 8×108 by $7 \times 107 = 749$, smaller than 817). Stevin would probably have given the same or a very similar explanation if one of his examples had presented him with this problem. Al-Kāšī instead asks for experimentation to find a number B such that $(20 \times 5 + B) \times B$ approaches 817 as closely as possible from below. Unspecified experimentation is hardly to be considered algorithmic in the strict sense, and on the other hand there is no reason to believe that Stevin would not be aware that his procedure had to be varied adequately to fit cases where mechanical application turned out to be problematic. So, instead of concluding "that Stevin's technique contains some flaws", as the authors do in their abstract, we might claim that the flaws belong with the modern algorithmic reading.

In an essay review of the second edition of Jean-Luc Chabert et al (eds), *Histoire d'algorithmes. Du caillou à la puce*, Maarten Bullynck [2016: 336] touches at the possibility that the algorithmic reading of early mathematics might give rise to "the same anachronistic misreadings as reading it with algebraic glasses" but surmises that most researchers have used a procedural reading "as an analytical, nearly linguistic tool that helps to foreground particular aspects of old texts, not as a goal per se". Here, "procedural" and "algorithmic" appear to be used synonymously, even though early procedures (like that of Stevin) were probably meant to be more flexible than allowed for by the modern strict meaning of "algorithm" (which is exactly what makes the algorithm concept relevant for electronic computers, and which corresponds to what Bullynck quotes Wu Wenjun for[20] with approval in note 2, namely "mechanized" mathematics).[21] In any case, I find Bullynck is too optimistic – the term has become pervasive even in such historiography as does not analyze procedures.

But Bullynck has some good examples, and not only such as Chemla's investigations of indubitable algorithms.

One is Jim Ritter's development of a scheme for showing the structure of numerical procedures unambiguously – extracting, so to speak, an algorithm of which the paradigmatic example could be an instantiation; see, for example, [Ritter 2004].[22] For simple calculations, this can be achieved by expression in algebraic letter symbolism;[23] but when matters get complex algebraic formulas easily become ambiguous witnesses of procedural structure. For comparison of calculations, Ritter's schemes are therefore clearly to be preferred. Applying this formalism, he can thus show that the Old Babylonian procedure used for solving (what expressed as algebra becomes) a normalized mixed second-degree problem functions as a sub-routine in the solution of a more complicated problem. Actually, this is not quite true, Ritter's intention to approach the model of present-day algorithms makes him gloss over the circumstance that the Babylonian calculator knows that a doubling outside the embedded procedure and a halving within it will cancel each other. For this reason the calculator omits both steps, while Ritter takes care to use only part of the simple calculation for his sub-routine. This does not affect the utility of the scheme, which indeed highlights what goes on if it is read carefully.

But Ritter's article is also of interest in the present context for two other reasons. Firstly, in the discussion of precisely this couple of texts he observes in note 13 (p. 198) that the

> actual meaning of the Akkadian technical term *waṣītum* is a subject of debate into which I do not enter here since it has no bearing on its functional use, the only one pertinent in this particular context.

This is true; the meaning of the word is essential for understanding the underlying mode of thought and representation – that the texts deal with cut-and-paste manipulations of squares and rectangles

[20] The reference is indirect, through [Hudecek 2012] – all Wu Wenjun's pertinent publications seem to be in Chinese, and hence inacessible to Bullynck as well as to the present author. Unfortunately for the present purpose, Hudecek is mainly interested in political context and its impact – also in his book-length study from [2014] – and does not go in depth with Wu Wenjun's analysis of ancient Chinese mathematics. In particular, Hudecek does not understand that Wu Wenjun has a much better case than Knuth, and therefore compares him unfavourably to the latter [Hudecek 2014: 118].

[21] On p. 5, indeed, Bullynck says about training through sequences of problems that it is

> of course part of an educational context in which a procedure is learned through repeated exercise, but where a procedure also becomes more flexible in the hands of a pupil who proceeds through more difficult, intricate or compounded problems.

[22] Ritter's text is actually considerably older; I first read the already finished manuscript in August 1997. At the conference in Xi'an, Ritter discussed the topic once again, making more explicit some of the points I extract below.

[23] For example, the formula $a\cdot(b + \sqrt{c})$ clearly tells that at first $\sqrt{c}$ has to be calculated, then the sum $b + \sqrt{c}$ must be found, and finally this sum is to be multiplied with a. $ab + \sqrt{(a^2c)}$ clearly describes a different procedure, though with the same numerical outcome.

and not with purely numerical operations as once believed by Neugebauer and Thureau-Dangin (and repeated by Knuth). So, Ritter's schemes are *explicitly not meant to reveal anything about how the Babylonians understood their mathematics*, neither "from the inside" (which was the nature of the operations it performed, and what were they assumed to do, apart from producing numbers?) nor "from the outside" (metamathematically, so to speak[24]). In this respect, Ritter's formalism is a parallel to the equations used by Eisenlohr (quite useful for anybody wanting to get a first understanding of what goes on in the Rhind Papyrus), and objections like those of Rodet to Eisenlohr could be repeated (once again missing the intention of that which is criticized).

Secondly, Ritter has some observations about what *he* wants to express by the word "algorithm". In note 8 (p. 197, but to p. 180) he states that

> By algorithmic I do not mean "merely empirical" or "recipe-style" collections of isolated solutions, a characterization that was once the standard interpretation before the work of Neugebauer, and remains so in a large number of popularizations

and on p. 186 he refers to

> yet another level of the algorithm, more general than that of the calculational techniques or that of the arithmetical operations, the level of method of solution, the choice of strategy of resolution.

That is clearly a non-standard understanding of the word, and goes beyond what can be expressed in Ritter's (or any) schemes or flowcharts; it appears to conflate what is expressed through the schemes and the higher-order analysis which the modern worker may read out of the schemes. In this way it can certainly be maintained that the whole Babylonian and Egyptian mathematical undertaking is "algorithmic" – but only on the condition that the non-standard definition is accepted. The parallel offered by Zeuthen's careless readers demonstrates the dangers inherent in reliance on such particular concepts.

The usefulness of the schemes themselves was demonstrated by Annette Imhausen. She used them to provide a "thick" description (in Clifford Geertz's sense) integrating the form, the substance and the techniques of Egyptian mathematical problem texts [Imhausen 2003: 11*f*, 191]. She pointed out explicitly that this is only one aspect of the history of Egyptian mathematics, whose embedding in socio-cultural, in particular in administrative context was still waiting by then – the gap has now been filled by [Imhausen 2016]; she also abstains from general claims about Egyptian mathematics being "algorithmic".

Algorithmic cultures

Above the level determined by the analysis of simple calculations, the notion of algorithms might also help us get a more fine-grained picture of mathematical culture types, instead of conflating all those mathematical cultures that teach how to obtain a numerical result by means of examples or abstract rules (Mesopotamia, Egypt, India, China, [the pseudo-Heronic corpus from] Greece, according to Acerbi and Vitrac).[25]

[24] But metamathematical in a very wide sense; Ritter's complex problem deals with the measurement of the dimensions of a supposedly real field.

[25] And, by the way: Why should only these types be algorithmic? Apart from producing a figure and not a number,

I already referred to the ancient Chinese genuine algorithms for numerical calculation.[26] But a look at the *Nine Chapters* as a whole [ed. trans. Chemla & Guo 2004] shows an overarching structure where an abstract rule is set out first, as a counting rod algorithm; afterwards follow concrete examples, which may vary the numerical parameters and the concrete topic but possess the same mathematical structure.[27] For example, chapter III (starting p. 282) is stated to deal with distribution weighted according to rank, which is indeed the topic of the first example; but then the topic is varied – the second problem deals with payment according to consumption, the third with customs paid according to possession. Since these are supposed to follow the same computational scheme, it seems justified to speak of a genuinely algorithmic organization (obviously following a straight-line algorithm, which we might as well call a "rule" or "procedure", perhaps corresponding better to Chinese *shu*). The fourth problem, however, expands the mathematics beyond the initial algorithm. A woman is told to weave each day twice as much as the day before, and to have woven in 5 days for 5 *chi*. Here, the weights are immediately given as 1, 2, 4, 8 and 16, respectively; that is, only that part of the calculation that fits inside the algorithm is specified, what falls outside the algorithmic part is just stated. We may conclude, at least tentatively, that the *stylistic ideal* is algorithmic or centred on fixed procedures, even when the actual task goes beyond the algorithmic framework. The commentaries (most famously that of Liu Hui) also explain why algorithms work, but by being commentaries to the algorithms they confirm the central position of these.

The background to the emergence of the Sanskrit style which we know from Āryabhaṭa onward is different.[28] In the *Āryabaṭīya* from c. 500 we find versified rules or statements of facts ("theorems", though not supported by demonstrations) that often do not have the character of algorithms even in a wide sense – for instance [trans. Keller 2006: 33–42]:

> Half of the even circumference multiplied by the semi-diameter, only, is the area of a circle. That multiplied by its own root is the volume of the circular solid without remainder.
>
> The two sides, multiplied by the height ⟨and⟩ divided by their sum are the "two lines on their own fallings". When the height is multiplied by half the sum of both widths, one will know the area.
>
> For all fields, when one has acquired the two sides, the area is their product.

Such compact statements called for explanatory and expanding commentaries, and these would

proposition 1 of *Elements* I, the construction of an equilateral triangle, is a perfect algorithm. The prescription is followed by a proof, it is true. We may regard that as a "comment" field – comments in computer algorithms often serve to explain why steps are pertinent; alternatively, we might see it as belonging at Ritter's more general level.

[26] My treatment of the Chinese and Sanskrit material is utterly succinct and built exclusively on translations and secondary literature. This is all I need for establishing the contrast. A precise characterization of these two clearly distinct types of algorithmic culture would ask for a very different approach and would be beyond my competence, linguistic (obviously) and otherwise.

[27] This characterizes not only the *Nine Chapters* but most of the texts on which Imperial examinations were based – see [Siu & Volkov 1999: 93].

[28] It is close at hand to point to the importance of memorization in Indian culture; but since we know about the existence of vernacular mathematics for the larger part of a millennium before Āryabhaṭa's time – cf. [Høyrup 2012b] – but know nothing precisely about it, it might be cautious to allow for the possibility of co-determining factors.

often include numerical examples. The style in the mathematical chapter 12 of Brahmagupta's sixth-century *Brāhmasphuṭasiddhānta* [ed. trans. Colebrooke 1817: 277–324] is similar, and it also invited commentaries including numerical examples; his algebraic chapter 18 [ed. trans. Colebrooke 1817: 325–376], on the other hand, contains rules as well as examples.

Over time, this became a general style. Rules (or statements of facts that easily translate into rules) are followed by illustrative numerical examples; this is what we find in Mahāvīra's ninth-century *Gaṇita-Sāra-Sangraha* [ed. trans. Raṇgācārya 1912], in Bhāskara II's *Līlāvatī* from the twelfth century [ed. trans. Colebrooke 1817], and in the Bakhshālī manuscript [ed. trans. Hayashi 1995], the date of which is disputed.

If we count lines, numerical examples and commentaries explaining why rules work outweigh the statement of the rules themselves by far. None the less it seems legitimate to argue that also this mature Sanskrit type of mathematics is algorithmic, having rules as the centre around which examples and commentaries are organized, and which provides their *raison d'être*.[29]

One might object that the categorization as "algorithmic" refers to the *ideology* of mathematics in the cultures concerned and does not describe the materiality of their mathematics – ideology being understood as an inextricable mixture of the descriptive and the prescriptive, of "is" and "ought to". However, to the extent the prescriptive aspect is obeyed or reacted to (and to some extent it always is), ideology also contributes to the shaping of practice, and hence to the historical process.

Returning for a moment to first-millennium China under this perspective we may observe that the commentaries often speak about constructing new procedures, not only about using procedures flexibly in non-standard situations [Siu & Volkov 1999: 94]. This is hence their conceptualization of mathematical innovation, which by necessity influenced which kind of creativity was pursued. As Siu and Volkov argue, this could also have been a task during examinations – alternatively, candidates may have been asked to apply the rules to new situations.[30] So, the examples are really *illustrative examples,* subordinated to the rules/procedures/algorithms; they are *not* free-standing paradigmatic calculations meant to be emulated with as much creativity as might be required by the situation (normally, of course, none).

Paradigm-centred mathematical cultures

Comparison will show that not all types of mathematics aiming at "finding the right number" are algorithmic in this sense.

Let us first look at a type where at least one abstractly formulated rule played a central role: Late medieval Italian mercantile mathematics, its broader Mediterranean inspiration and its various European Renaissance offsprings. Merchants were certainly just as keen as Chinese officials to find the right

[29] "Sanskrit mathematics" should not be confounded with "Indian mathematics". We have much indubitable indirect evidence for the existence of mathematical activity outside this "great tradition" but no direct evidence – see the recapitulation in [Høyrup 2012b: 146–148], with further discussion of evidence presented in [Keller 2006] and [Sarma 2010].

[30] Not as easy as those with good mathematical training tend to believe: I remember how difficult it was for many of my engineering students 45 years ago to perform integrations when the independent variable was time (t) and not x – and how astonished I was as a young teacher. They learned, and I learned too. They, to transfer – I, not to consider transfer obvious.

number, and we do find in the "abbacus books"[31] reflecting the mathematical training of their sons one rule formulated abstractly: the "rule of three things".

This is how the rule is stated in Jacopo da Firenze's *Tractatus algorismi*, written in 1307 [ed., trans. Høyrup 2007: 236*f*, error corrected]:[32]

> If some computation should be given to us in which three things were proposed then we should always multiply the thing that we want to know against the one which is not similar, and divide in the third thing, that is, in the other that remains.

Very similar formulations can be found in other abbacus books; as it turns out, the reference to the similar and the non-similar points back, via the language of Islamic commercial calculators, to Indian vernacular practices [Høyrup 2012b].

As do other abbacus books, Jacopo gives a number of concrete examples after the abstract rule. Other topics he treats through paradigmatic examples alone – but there, time and again, after the statement of a problem and before the ensuing numerical calculations the phrase "and this is its rule" is inserted. The rule of three as just quoted, on the other hand, is not even spoken of as a rule (*regola*).

This is no idiosyncrasy: even though *regula* or *regola* turn up often in the abbacus books, its meaning is anything except, precisely, the abstract rule. It may, as in Jacopo, refer to a paradigmatic exemplification from which a rule *could* be extracted; or it may refer to something much broader. In Jacopo's *Tractatus* [ed. Høyrup 2007: 236], for instance, the exposition of the arithmetic of fractions (addition, subtraction, multiplication, comparison, taking a fraction of another fraction) is followed by the statement that "we have said enough about fractions, because the similar computations with fractions all are done in one and the same way and by one and the same rule" (then follows the rule of three, presented however as "some computations").

The word is at times meant just as broadly in Fibonacci's *Liber abbaci*. In chapter 12, a *regula recta*, "direct rule", is made appeal to and applied repeatedly; it is told to be used by the Arabs and to be very useful [ed. Boncompagni 1857: 191]. This "rule" consists in naming some unknown and searched-for magnitude *res*, "a thing"; to write down the statement in terms of this thing; and then reduce the expression – that is, simply in applying (first-degree) rhetorical algebra.

But Fibonacci is no more consistent in his usage than Jacopo. A rule may also refer to a paradigmatic example; for instance, a problem on p. 177 is said to be similar to "the rules of the tree", because the method of a single false position is introduced by a number of examples involving a tree; on p. 179, the "rule of the eggs" stands for a specific variant of the single false position which was introduced in two examples about buying and selling eggs.

A third representative of the broad Mediterranean tradition is the *Liber augmenti et diminutionis* composed by some Abraham[33] and translated into Latin in the twelfth century [ed. Libri 1838: I,

---

[31] *Abbaco*/*abbacho*, in the Italian dialects of the later Middle ages, meant something like "practical computation" – it has nothing to do with the abacus but was probably the main channel for the spread of computation with "Hindu-Arabic numerals".

[32] Jacopo's *Tractatus* is known from three fifteenth-century copies. I translate from Vatican, Vat. lat 4826, which has the most elaborate version of the rule and is likely to represent Jacopo's original (but whether it does so is immaterial for our present merely illustrative use).

[33] Certainly not Abraham Savasorda, nor Abraham ibn Ezra – they write differently (both have been proposed). Actually, the author's name may as well have been Ibrahim in his own language. That the text is a translation is

304–376].[34] It solves a sequence of problems by means of three methods: the single false position; the double false position; and first-degree rhetorical algebra with unknown *res*. The third of these is identical with Fibonacci's *regula recta*, but here it is simply called *regula*. The other two carry no name at all.[35]

The only mathematical domain where abstract rules are used systematically (and sometimes spoken of as such[36]) is *algebra*. These are the standard procedures for solving the six "cases" (equation types) of the first and second degree, for instance "possession and roots equal to number", with the rule

> You halve the [number of] roots, multiply it by itself, add it to the number, take the [square] root, and subtract the half of the roots which you multiplied by itself.

In the abbacus books from Jacopo onward, similar rules (not always correct) are also stated for higher-degree cases.

In the abbacus tradition, algebra only served to display mastery of algebra.[37] It is thus not strange that the presentation of abstract rules was taken over as tradition – there was no living practice to challenge inherited habits and transform them in imperceptible steps. The tradition had been borrowed, visibly from al-Khwārizmī, but behind him from some earlier practice. Al-Khwārizmī tells so himself: he informs the reader that he is presenting what was noble and subtle in an existing technique and about what he "has found" [ed. trans. Rashed 2007: 94–96]). We do not know where this earlier technique was practised and where al-Khwārizmī found it, but a good yet vague guess is somewhere in Central Asia (al-Khwārizmī's own Khwarezm?) with some kind of connections to India – cf. [Høyrup 2001: 124; Høyrup 2007: 103]. In any case, algebra is no witness of the character of Mediterranean medieval

---

obvious from the way the author is referred to (p. 305), *Hic post laudem Dei inquit*, "after the praise of God he says".

[34] Libri's edition, based on a single manuscript, is transformed into a critical edition when combined with [Hughes 2001].

[35] Elsewhere, the method of double false position does carry a name – Fibonacci speaks of "the rule of *elchataicum* in Arabic, two false positions in Latin" [ed. Boncompagni 1857: 318]; similarly (one or often both) in a variety of abbacus books. But no abstract formulation is offered, only paradigmatic examples.

Arabic *al-khaṭa'in* means "the two errors".

[36] The term *regula* is used in Gherardo of Cremona's translation of al-Khwārizmī about the rules for the three composite cases [ed. Hughes 1986: 234–36] – but Gherardo also uses the word about specific calculations – for instance, "the rule for this computation", p. 248; Robert of Chester mostly speaks of *regula* but once of *modus*, however referring backwards to this as a *regula* [ed. Hughes 1989: 32–35]. Al-Khwārizmī himself [ed. Rashed 2007: 101] has *fa bābah* ("and then the chapter/kind", if I understand correctly; Rashed translates *procédé*).

Fibonacci refers to the "six rules" for the basic cases, and also by number to the rules for single cases, "the first rule" etc. [ed. Boncompagni 1857: 409 and *passim*]. With one or two exceptions, this is the only use of the term in the algebra section of the work.

Abbacus books rarely if ever use the term *regola* about the rules they give for the algebraic cases.

[37] In the later fourteenth century, Antonio de' Mazzinghi constructed the earliest tables of composite interest, which can at a pinch be considered an expansion of his work on algebra. But apart from Antonio, we need to wait until Regiomontanus' *De triangulis* before anybody used algebra for anything but display of algebraic competence – and if again we disregard Regiomontanus's use of very simple abbacus techniques, we need to wait until the mid-sixteenth century.

mercantile mathematics. Its use of abstract rules was a discipline-specific characteristic with no set-off in the wider world of mathematics; it does not tell us that this wider world was "algorithmic" or centred on abstract rules.

We cannot use the same argument about the rule of three; admittedly, it was also an import, and indubitably (though indirectly) from Indian vernacular mathematics. But the rule of three was something a merchant calculator had to use again and again and again; it was certainly part of living mathematical practice.

Living practice, however, is subject to wear, tear and unplanned gradual change; it does not conserve something for aeons just because it is inherited tradition. This consideration, on the other hand, explains why precisely the rule of three was transmitted and taught *as a rule* in a context where everything else was taught through paradigmatic examples: the rule of three was used so often that it was worth the while to learn it abstractly, *even though* it is counter-intuitive (the intermediate product has no concrete interpretation). Operations you perform sufficiently often are remembered in spite of failing understanding, and the computational advantages of the rule of three (fewer difficult small fractions, or no rounding errors that are multiplied) outweigh the pedagogical disadvantages.[38] As soon as teaching went beyond the rule of three, however, it was invariably based on paradigmatic examples – often spoken of as "rules", but almost as often of a character that would not allow direct extraction of an algorithm.

We may draw the conclusion that the Mediterranean mathematical culture of the earlier second millennium was *not* algorithmic but centred on paradigmatic examples. That, of course, can be correlated with the teaching situation: Italian merchants' sons were taught at age 11–12 for at most two years in the abbacus school; ibn Sīnā (not Mediterranean, but belonging to Islamic culture), as he tells, learned his "Hindu arithmetic" (probably more than the mere numerals and numerical operations) with a local vegetable seller [ed. Gohlman 1974: 21] – a situation quite different from the seven to nine years spent by bright and hard-working students of the Tang College of Mathematics to learn by heart the words of the Mathematical Classics [Siu & Volkov 1999: 90*f*]. In the abbacus school, and its analogues, the best that could be achieved was to make pupils understand as much as each of them could grasp, and use that as creatively as each one was able to and needed afterwards (as far as some were concerned, also in further vocational training – see [Sapori 1955: I. 67–78]).

Some observations may be added about Babylonian – more precisely, Old Babylonian – mathematics. The second half of the Old Babylonian period (that is, 1800 to 1600 BCE according to the "middle chronology") is the period from which we know a large number of texts containing mathematical problems with prescriptions for solving them (for instance, the two problems on which Ritter builds his argument, and also most of Knuth's examples). In general, these are paradigmatic examples submitted to variation, sometimes only of numerical parameters, sometimes also of the general conditions. There are very few examples of abstract rules – but there is one, and a hint to another one, and they are interesting.

The hint is in the text Db_2-146, in which the sides of a rectangle are found from the diagonal and the area. In a concluding verification, the diagonal is found from the resulting sides by means of the "Pythagorean rule" [ed. trans. Høyrup 2002a: 258*f*]. The calculation starts by asking for the square on the length, *without indicating the value of the latter*. This is unique in the corpus, and suggests that the

[38] Yet already when we come to the inverse rule of three, things easily go astray. The Vatican manuscript of Jacopo's *Tractatus* does not point out when it is the inverse version that is used (though it calculates correctly); and the two other manuscripts get caught in the confusion and apply the direct rule when the inverse rule should have been employed [Høyrup 2007: 80, 88].

writer quotes an abstract formulation of the rule. Now, this text can be dated to c. 1775 BCE, a period where (so it appears) a number of riddles were adopted into the scribal school from a profession of lay (that is, non-scribal) surveyors trained orally in an apprenticeship-system. In an oral system, the master may set out a rule abstractly while pointing to a drawing to make clear what he means; in an educational system carried by writing, it is safer to identify by other means, and the authors of the Old Babylonian mathematical texts used the numerical values for that purpose (sometimes even betraying the value of a magnitude that is asked for, but taking care not to use this knowledge in the procedure).

The full-fledged example is problem #1 in the text AO 6770 [ed. trans. Høyrup 2002a: 179*f*]. This text is probably some 25 years younger, but it is written in a different area, in which the adoption of lay problems and procedures took place precisely at this later moment.[39] The text is so opaque that historians have struggled with its meaning since it was first published in the 1930s (and, not least, struggled with each other). It appears that the Old Babylonian scholars or students had to struggle too; in any case, teachers must have concluded that rules in abstract formulation were inefficient, and decided to concentrate their efforts on the production of paradigmatic problems. So, the Old Babylonian mathematics teachers, at least at the sophisticated level, refused to develop an algorithmic culture.

What about simple numerical arithmetic, the kind which is indubitably algorithmic in the *Algorismus vulgaris?* Let us look at a simple multiplication, 10 50 × 10 50 (the number system was a floating-point place value system with base 60, so 10 50 may mean $10 + {}^{50}\!/_{60}$, 10 60 + 50, etc.). We know that students would write one number below the other on a tablet, and then the result afterwards. The intermediate computations must have been performed on a different support – a reckoning board, some kind of abacus, where students inserted the partial products and performed the additions. Some of the partial products could be taken from tables that had been learned by heart. Every student would know that 10 × 10 = 1 40. Some might also know that 50 × 50 = 41 40. Multiplication tables containing this value exist indeed – but they are rare, and most students would probably have had to find that product as 25 × (1 40).[40]

If multiplication should be trained as a true algorithm, all users would have to be able to perform the same steps. If they were not – as it seems – then the prescription would have to be much more open, leaving the detailed choices to the reckoner. Even simple arithmetic is thus likely to have been trained on something like paradigmatic examples, and not as genuine algorithms.

Even at this level the Old Babylonian mathematical culture thus appears to have been built around paradigmatic teaching, not algorithms.[41]

The situation in the environment of Babylonian *late first-millennium BCE mathematical astronomy* may have been different. Astronomical procedure texts were formulated as abstract branching

[39] These chronologies are argued in [Høyrup 2012a].

[40] Indeed, the use of some kind of abacus follows precisely from an error in a mathematical text where the intermediate product 25 × 1 has been inserted twice – see [Høyrup 2002b].

[41] A number of interesting analyses have been made of a way to find inverses of numbers that do not appear in the standard table of reciprocals – first of which [Sachs 1947], recently [Proust 2012]. As a simple example we may pretend that A = 44.26.40 does not appear (it actually does), and try to find ${}^{1}\!/_{A}$. We observe that the final part of the number is 6.40, which is the reciprocal of 9. We therefore write A as a sum, A = 44.20.0 + 0.6.40, and find that $9 \times A$ = 6.39.0 + 0.1.0 = 6.40. Now, 6.40 is still the reciprocal of 9, whence $9 \times 9 \times A = 1$. Therefore, ${}^{1}\!/_{A} = 9 \times 9 = 81$. But we might also just notice that 40 (= ${}^{2}\!/_{3}$) is the reciprocal of 1 30 (= ${}^{3}\!/_{2}$); the procedure would be longer, but it would lead to the same end result. Even this, with the built-in necessity to make some adequate choice, is not a genuine algorithm, no proper instance of "mechanized mathematics".

algorithms – see an example in [Brack-Bernsen & Hunger 2008: 7]. The rare mathematical problem texts produced in the same environment, which indeed do not represent a direct continuation of the Old Babylonian school texts, sometimes formulate abstract rules, sometimes offer paradigmatic examples, and sometimes combine – see the text editions in [MKT I, 96–102], [MKT III, 14–19], [Friberg, Hunger & al-Rawi 1990] and [Friberg 1997].

I shall not go into details with Pharaonic and ancient Greek practical mathematics; but I will suggest that even they are instances of paradigm-based rather than algorithmic mathematical culture.

From a global point of view, there were certainly fundamental differences between medieval Mediterranean mercantile mathematics, Old respectively Late Babylonian mathematics, Pharaonic mathematics and ancient Greek practical mathematics, however much all four or five centred teaching around paradigmatic examples – just as there were fundamental global differences between classical Chinese and Sanskrit mathematics, even though both were centred around algorithms. To which extent these global differences can be characterized also through their different ways to deal with paradigmatic examples respectively algorithms is a question I shall not take up at the present occasion.

Conflict of interest

The author declare no conflicts of interest in this paper.

References

1. Acerbi, Fabio, & Bernard Vitrac (ed., trans.), 2014. Héron d'Alexandrie, *Metrica*. Pisa & Roma: Fabrizio Serra.
2. Aydin, Nuh, & Lakhdar Hammoudi, 2015. Root Extraction by Al-Kashi and Stevin. *Arch Hist Exact Sci* **69**, 291–310.
3. Barrow, Isaac, 1659. *Euclidis Elementorum libri XV*. Canterbury: William Nealand.
4. Boncompagni, Baldassare (ed.), 1857. *Scritti* di Leonardo Pisano matematico del secolo decimoterzo. I. Il *Liber abbaci* di Leonardo Pisano. Roma: Tipografia delle Scienze Matematiche e Fisiche.
5. Brack-Bernsen, Lis, & Hermann Hunger, 2008. BM 42484+42294 and the Goal-Year method. *SCIAMUS* **9**, 3–23.
6. Bullynck, Maarten, 2016. [Essay Review of Jean-Luc Chabert et al (eds), *Histories of algorithms: Past, present and future*. *Histoire d'algorithmes. Du caillou à la puce*. Second edition. Paris: Belin, 2010]. *Hist Math* **43**, 332–341.
7. Cantor, Moritz, 1880. *Vorlesungen über Geschichte der Mathematik*. Erster Band, *von den ältesten Zeiten bis zum Jahre 1200 n. Chr.* Leipzig: Teubner.
8. Chemla, Karine, 1987. Should They Read Fortran As If It Were English? *Bulletin of Chinese Studies* **1**, 301–316.
9. Chemla, Karine, 1991. Theoretical Aspects of the Chinese Algorithmic Tradition (First to Third Centuries). *Historia Scientiarum* **42**, 75–98.
10. Chemla, Karine, & Guo Shuchun (eds), 2004. *Les neuf chapitres. Le Classique mathématique de la Chine ancienne et ses commentaires*. Paris: Dunod.
11. Colebrooke, H. T. (ed., trans.), 1817. *Algebra, with Arithmetic and Mensuration from the Sanscrit of Brahmagupta and Bhascara*. London: John Murray.

12. Cormen, Thomas, et al, 2009. *Introduction to Algorithms*. Third Edition. Cambridge, Mass., & London: MIT Press.
13. Curtze, Maximilian (ed.), 1897. *Petri Philomeni de Dacia in Algorismum vulgarem Johannis de Sacrobosco Commentarius, una cum algorismo ipso*. København: Høst og Søn.
14. Eisenlohr, A., 1877. *Ein mathematisches Handbuch der alten Ägypter (Papyrus Rhind des British Museum) übersetzt und erklärt*. I. *Kommentar*. II. *Tafeln*. Leipzig: J. C. Hinrichs.
15. Friberg, Jöran, 1997. "Seed and Reeds Continued". Another Metro-Mathematical Topic Text from Late Babylonian Uruk. *Baghdader Mitteilungen* **28**, 251–365, pl. 45–46.
16. Friberg, Jöran, Hermann Hunger & Farouk N. H. al-Rawi, 1990. "Seed and Reeds": A Metro-Mathematical Topic Text from Late Babylonian Uruk. *Baghdader Mitteilungen* **21**, 483–557, Tafel 46–48.
17. Fried, Michael N., & Sabetai Unguru, 2001. *Apollonius of Perga's Conica. Text, Context, Subtext*. Leiden etc.: Brill.
18. Gohlman, William E. (ed., trans.), 1974. *The Life of ibn Sina*. Albany: State University of New York Press.
19. Hayashi, Takao, 1995. *The Bakhshālī Manuscript: An Ancient Indian Mathematical Treatise*. Groningen: Egbert Forsten.
20. Høyrup, Jens, 2001. On a Collection of Geometrical Riddles and Their Role in the Shaping of Four to Six "Algebras". *Sci Context* **14**, 85–131.
21. Høyrup, Jens, 2002a. *Lengths, Widths, Surfaces: A Portrait of Old Babylonian Algebra and Its Kin*. New York: Springer.
22. Høyrup, Jens, 2002b. A Note on Old Babylonian Computational Techniques. *Hist Math* **29**, 193–198.
23. Høyrup, Jens, 2006. Artificial Language in Ancient Mesopotamia – a Dubious and a Less Dubious Case. *Journal of Indian Philosophy* **34**, 57–88.
24. Høyrup, Jens, 2007. *Jacopo da Firenze's Tractatus Algorismi and Early Italian Abbacus Culture*. Basel etc.: Birkhäuser.
25. Høyrup, Jens, 2012a. A Hypothetical History of Old Babylonian Mathematics: Places, Passages, Stages, Development. *Gaṇita Bhāratī* **34**, 1–23.
26. Høyrup, Jens, 2012b. Sanskrit-Prakrit Interaction in Elementary Mathematics As Reflected in Arabic and Italian Formulations of the Rule of Three – and Something More on the Rule Elsewhere. *Gaṇita Bhāratī* **34**, 144–172.
27. Høyrup, Jens, 2017. What Is "Geometric Algebra", and What Has It Been in Historiography? *AIMS Mathematics* **2**, 128–160.
28. Høyrup, Jens, forthcoming. In Which Way Can We Speak of Algebra when Describing Babylonian Sources? To be published in a volume edited by Karine Chemla and Tian Miao.
29. Hudecek, Jiri, 2012. Ancient Chinese Mathematics in Action: Wu Wen-Tsun's Nationalist Historicism after the Cultural Revolution. *East Asian Science, Technology and Society* **6**, 41–64.
30. Hudecek, Jiri, 2014. *Reviving Ancient Chinese Mathematics: Mathematics, History and Politics in the Work of Wu Wen-Tsun*. London & New York: Routledge.
31. Hughes, Barnabas B. (ed.), 1986. Gerard of Cremona's Translation of al-Khwārizmī's *Al-Jabr*: A Critical Edition. *Mediaeval Studies* **48**, 211–263.
32. Hughes, Barnabas B. (ed.), 1989. Robert of Chester's Latin translation of al-Khwārizmī's *Al-jabr*. A New Critical Edition. Wiesbaden: Franz Steiner.

33. Hughes, Barnabas B. (ed.), 2001. A Treatise on Problem Solving from Early Medieval Latin Europe. *Mediaeval Studies* **63**, 107–141.
34. Imhausen, Annette, 2003. *Ägyptische Algorithmen. Eine Untersuchung zu den mittelägyptischen mathematischen Aufgabentexten.* Wiesbaden: Harrassowitz.
35. Imhausen, Annette, 2016. *Mathematics in Ancient Egypt: A Contextual History*. Princeton and Oxford: Princeton University Press.
36. Keller, Agathe, 2006. *Expounding the Mathematical Seed*. A Translation of Bhāskara I on the Mathematical Chapter of the *Āryabhaṭīya*. 2 vols. Basel etc.: Birkhäuser.
37. Kline, Morris, 1972. *Mathematical Thought from Ancient to Modern Times*. New York: Oxford University Press.
38. Knuth, Donald E., 1972. Ancient Babylonian Algorithms. *Communications of the Association of Computing Machinery* **15**, 671–677, with correction of an *erratum* in **19** (1976), 108.
39. Libri, Guillaume, 1838. *Histoire des mathématiques en Italie*. 4 vols. Paris: Jules Renouard, 1838–1841.
40. Littré, Émile, 1873. *Dictionnaire de la langue française*. 4 vols. + suppl. Paris: Hachette, 1873–1877.
41. Mahoney, Michael S., 1971. Babylonian Algebra: Form *vs.* Content. [Essay Review of the 1969 reprint edition of O. Neugebauer 1934]. *Stud Hist Philos Sci* **1**, 369–380.
42. Maresca, Paolo, 2003. Introduction to the Fundamentals of Algorithms, pp. 1–16 *in* Shi-Kuo Chang, *Data Structures and Algorithms*. New Jersey etc.: World Scientific.
43. MKT: O. Neugebauer, *Mathematische Keilschrift-Texte*. 3 vols. Berlin: Julius Springer, 1935, 1935, 1937.
44. Neugebauer, Otto, 1932. Studien zur Geschichte der antiken Algebra I. *Quellen und Studien zur Geschichte der Mathematik, Astronomie und Physik.* Abteilung B: *Studien* **2** (1932–33), 1–27.
45. Neugebauer, Otto, 1934. *Vorlesungen über Geschichte der antiken mathematischen Wissenschaften.* I: *Vorgriechische Mathematik*. Berlin: Julius Springer.
46. Neugebauer, O., 1936. Zur geometrischen Algebra (Studien zur Geschichte der antiken Algebra III). *Quellen und Studien zur Geschichte der Mathematik, Astronomie und Physik.* Abteilung B: *Studien* **3** (1934–36), 245–259.
47. Proust, Christine. 2012. Interpretation of Reverse Algorithms in Several Mesopotamian Texts, pp. 384–412 *in* Karine Chemla (ed.), *History of Mathematical Proof in Ancient Traditions*. Cambridge: Cambridge University Press.
48. Raṇgācārya, M. (ed., trans.), 1912. The *Gaṇita-sāra-sangraha* of Mahāvīrācārya with English Translation and Notes. Madras: Government Press.
49. Rashed, Roshdi (ed., trans.), 2007. Al-Khwārizmī, *Le Commencement de l'algèbre*. Paris: Blanchard.
50. Reichenbach, Hans, 1939. *Experience and Prediction: An Analysis of the Foundations and the Structure of Knowledge*. Chicago: University of Chicago Press.
51. Ritter, Jim, 2004. Reading Strasbourg 368: A Thrice-Told Tale, pp. 177–200 *in* Karine Chemla (ed.), *History of Science, History of Text*. Dordrecht: Kluwer.
52. Rodet, Léon, 1881. Les prétendus problèmes d'algèbre du manuel du calculateur égyptien (Papyrus Rhind). *Journal asiatique*, septième série **18**, 184–232, 390–559.
53. Sachs, Abraham J., 1947. Babylonian Mathematical Texts. I: Reciprocals of Regular Sexagesimal Numbers. *Journal of Cuneiform Studies* **1**, 219–240.

54. Sapori, Armando, 1955. *Studi di storia economica: (secoli XIII-XIV-XV)*. 3 vols. Firenze: Sansoni, 1955–67.
55. Sarma, Sreeramula Rajeswara, 2010. Mathematical Literature in the Regional Languages of India, pp. 201–211 *in* B. S. Yadav (ed.), *Ancient Indian Leaps in the Advent of mathematics*. Basel: Birkhäuser.
56. Siu, Man-Keung, & Alexeï Volkov, 1999. Official Curriculum in Traditional Chinese Mathematics: How Did Candidates Pass the Examinations? *Historia Scientiarum* **9**, 85–99.
57. Szabó, Árpád, 1969. *Anfänge der griechischen Mathematik*. München & Wien: R. Oldenbourg/ Budapest: Akadémiai Kiadó.
58. Tannery, Paul, 1882. De la solution géométrique des problemes du second degré avant Euclide. *Mémoires de la Société des sciences physiques et naturelles de Bordeaux*, 2e Série, 4, 395–416. Consulted via the reprint in Tannery 1912.
59. Tannery, Paul, 1887. *La géométrie grecque. Comment son histoire nous est parvenue et ce que nous en savons. Essai critique*. Première partie, *Histoire générale de la géométrie élémentaire*. Paris: Gauthiers-Villars.
60. Tannery, Paul, 1912. *Mémoires scientifiques*. I. *Sciences exactes dans l'Antiquité, 1876–1884*. Toulouse: Édouard Privat / Paris: Gauthier-Villars.
61. Tropfke, Johannes, 1902. *Geschichte der Elementar-Mathematik in systematischer Darstellung*. 2 vols. Leipzig: von Veit, 1902–1903.
62. Zeuthen, Hieronimus Georg, 1886. *Die Lehre von den Kegelschnitten im Altertum*. København: Höst & Sohn.
63. Zeuthen, Hans Georg, 1896. *Geschichte der Mathematik im Altertum und im Mittelalter. Vorlesungen*. København: Höst & Sön.