

## Explicit and Less Explicit Algorithmic Thinking, 1200-1500. Jordanus de Nemore, and the Contrast between Barthélemy de Romans et Chuquet

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**Explicit and less explicit algorithmic thinking, 1200–1500**  
**Jordanus de Nemore, and the contrast**  
**between Barthélemy de Romans et Chuquet**

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### *Abstract*

An introductory section discusses the utility of the algorithm concept in the historiographic analysis of non-recent mathematics, in particular the sense that can be given to claims that a particular mathematical culture was of algorithmic type. It concludes that the adequacy of this epithet when applied to a mathematical culture does not depend on whether texts used in teaching are built up around paradigmatic examples but on whether the *production* of rules or algorithms was regarded as a central activity for those whom we would count as “mathematicians” (that is, producers of mathematical knowledge).

Three medieval examples of attitudes to algorithms follow. First, Jordanus de Nemore's *De numeris datis* is shown to develop a method to combine algorithms and deductivity, in an alternative to algebra. Second, Barthélemy de Romans' graphic schemes for organizing the complex algorithms used to solve the sophisticated variants of the problem of the “unknown heritage” are discussed. Third is considered Nicholas Chuquet's dismissal of these schemes and algorithms, in favour of the algebraic tool.

## Introductory observations about concepts

An early version of what follows was presented at a workshop dealing with “deductive algorithmic practices in pre-algebraic mathematics”.<sup>1</sup> The only word here which does not ask for conceptual clarification is “in”; *provisionally*, we may pretend to know what “deductive”, “mathematics” and “practices” stand for.

We may claim, as a mathematician-friend of mine (Bernhelm Booß-Bavnbek, personal communication) once did polemically, that “there was no algebra before Emmy Noether”. Or, with Michael Mahoney [1971: 372], be more liberal and accept as algebra that which began in the epoch of Viète and Descartes, while the techniques of an al-Khwārizmī and a Cardano (etc.) still represent an “algebraic approach” only. Or, finally, we may accept that the technique which al-Khwārizmī and his successors until the mid-sixteenth century spoke of as “algebra” was algebra.

If we take one of the former two of these roads, “pre” in “pre-algebraic” may be taken without trouble in its usual, literal chronological sense, and “pre-algebraic” can be taken to mean simply “before 1900 CE” or “before 1600 CE”. If we choose the third road, the chronological interpretation of the term means “before 800 CE” – and if we accept Sanskrit “algebra” (not to speak of Old Babylonian “algebra”) as algebras, we get still earlier limits. This would at best prevent us from looking at anything from the Middle Ages or later, which would not make much sense.

Instead, “pre-algebraic” may be interpreted metaphorically, as “not affected by algebraic thinking”; this leaves us the possibility to work, and therefore I shall apply this reading of the term. With this choice it does not matter much whether al-Khwārizmī etc. made algebra or only had an algebraic approach. On the other hand, it is hard to imagine that anything mathematical which was created in Europe after, say, 1750 CE should not be somehow affected by algebra, given how pervasive use of algebraic symbolism had become by then – even *avoiding* algebraic reasoning had by then become a deliberate choice, no consequence of ignorance.

Regarding algorithms, we may start from the probably earliest paper which tried to apply this concept to non-recent historical material: Donald Knuth’s “Ancient Babylonian Algorithms” from [1972].<sup>2</sup> According to Knuth (p. 672),

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<sup>1</sup> “Pratiques algorithmiques dans les mathématiques pré-modernes”, Université de Lille 3, 12-14 octobre 2011.

<sup>2</sup> It is immaterial for the present purpose that Knuth’s argument was based on a reductive

## the Babylonians

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.

Two things are to be noted. As Knuth points out, an algorithm is a “step-by-step list of rules” (we shall return to Knuth’s complaints about the trivial character of the Babylonian “algorithms”). Secondly, such a list can be the equivalent of a *formula*. Indeed, a formula like  $\frac{ab+ac}{ab}$  is an algorithmic prescription, which we may sketch as follows:

1. calculate  $a \cdot b$ , save the outcome as  $p$ ;
2. multiply  $a \cdot c$ , save the outcome as  $q$ ;
3. calculate  $p+q$ , save the outcome as  $r$ ;
4. calculate  $a \cdot b$ , save the outcome as  $s$ ;<sup>3</sup>
5. determine  $r/s$ .

In algebraic symbolism, we may reduce the formula first as  $\frac{b+c}{b}$  and then, if we find that this is a simpler form, as  $1+\frac{c}{b}$ . We may do this “naively”, just removing a common factor and then dividing term by term as we know it can be done; or we may do it the “critical” way, arguing explicitly from arithmetical axioms and from theorems ultimately depending on axioms. In both cases, the more or less deductive process falls *outside* the algorithm expressed by the formula.

Elsewhere (p. 674), Knuth admits with regrets that the texts he has discussed offer

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

But alas, there is very little evidence of this in the Babylonian texts

– and all he is able to offer in this respect are choices made *outside* the calculation.<sup>4</sup> So, his “algorithms” turn out to be nothing but what had

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reading of the Babylonian texts that omitted all aspects of the meanings of the terminology that do not translate directly into the language of modern arithmetic.

<sup>3</sup> Evidently, step 5 is identical with step 1, and we might just remember that the outcome was  $p$ . However, the literal reading of the formula as it stands does not have this shortcut.

<sup>4</sup> [Proust 2012] discusses Babylonian texts applying an algorithm containing a loop (and

traditionally been known as “rules”. One is tempted to ask whether we are not confronted with a parallel to August Eisenlohr’s reading of the Rhind Mathematical Papyrus through the spectacles of the equation algebra of *his* time [1877: I, 5, 60–62, 65f, 69–72, 161, and *passim*] – to mention but one example, perhaps the first to call forth a thoroughly argued objection [Rodet 1881].

However, dismissal of a concept through polemical questions does not promise much insight. So, let us look instead at the actual and possible historiographic uses of the algorithm concept.

Firstly, of course, it may be the historian’s tool to analyze the procedures of the sources (just as algebra may serve legitimately for this purpose). Even though formulas are in principle to be read as algorithms, they are not always an adequate means to render procedures in detail – for instance, to quote the above example, to make clear whether  $ab$  in the denominator is recalculated, or the result of the preceding determination of the same number which has been (implicitly or explicitly) saved and is now simply retrieved; worse, modern readers have a tendency not to read a formula literally, as the description of a particular calculation but, so to speak, as an accidental representative of the whole equivalence class of formulas into which it can be algebraically transformed – thus, for instance, seeing no difference between  $\frac{ab+ac}{ab}$ ,  $\frac{b+c}{b}$  and  $1+\frac{c}{b}$ . Therefore, an explicitly algorithmic interpretation may be useful, sometimes even needed. Such use of the algorithm concept as a tool for modern analysis is what Annette Imhausen [2003: 1] offers in her *Ägyptische Algorithmen*, explicitly intended to give “eine der Struktur der Texte gerechtwerdende Beschreibung der Aufgaben”.

This must be sharply distinguished from any ascription of “algorithmic thinking” to the authors of the sources; one of course does not exclude the other, but nor does one entail the other. So this deserves a separate discussion.

Evidently, any text which tells how to find a particular numerical result or how to construct a particular geometric or other mathematical object can be described as “an algorithm”, simply because it cannot avoid being “step-by-step”.<sup>5</sup> *Elements* I.1, the construction of an equilateral triangle on a given base, is such an algorithm; Euclid also offers a proof, it is true, but this can be understood as a “comment field”.

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thus implicitly a decision to stop) for the determination of the reciprocals of numbers. But such texts were unknown to Knuth.

<sup>5</sup> In principle, a numerical result or number is also a “mathematical object”. Since most of those who find such a number – from the accountant to the engineer – do not think of their results in such terms, it may be useful to uphold the distinction.

Roughly speaking, a mathematical practice can aim at producing theorems; at calculating something; and/or at constructing mathematical objects according to given specifications. Saying that it is algorithmic in this broad sense is equivalent to saying that it is *not* aimed exclusively at the production of theorems.

That, however, is not common usage. Mostly, the term is reserved (in the historiography of non-recent mathematics) for texts that teach how to produce numerical results. In many cases one may get the impression that the term seems to serve as a more positive formulation of the traditional characterization of non-scholarly mathematical traditions, in particular those not derived from Greek theory, as based on (supposedly empirically derived) *rules* and not on insight; since the algorithms in question are precisely of the type Knuth characterizes as “‘straight-line’ calculations, without any branching or decision-making involved”, the difference belongs solely at the level of evaluative connotations.<sup>6</sup> However, whether we speak of “algorithms” or – in better agreement with the words of the texts themselves when they speak of *regula*, μέθοδος, etc.) – of “rules”, the importance of such prescriptions is undeniable in certain mathematical cultures. This is not only true when rules in abstract formulation precede application to one or more examples (for instance, in many Chinese and Indian works) but also at least suggested when (as in the late medieval abacus tradition) paradigmatic examples are followed by a phrase “do similarly in

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<sup>6</sup> It may be objected that the algorithms for calculating on an abacus board or within a place value system involves branching etc. However, who looks at early (and not so early) books teaching these will discover that the explanations do not make this algorithmic branching explicit but relies instead of some level of intuitive understanding of what goes on. Look for instance at addition within a decimal place value system:

$$\begin{array}{r} d \ c \ b \ a \\ h \ g \ f \ e \\ \underline{1 \ k \ j \ i} \end{array}$$

An algorithm which is not restricted to three addends and not to four places will need for each place to add all corresponding digits and the number carried; then to make a loop where 10 is subtracted from this sum as long as possible, while a number 1 is added to the carried number for the next place each time 10 is subtracted; and finally to write the remainder at the corresponding place of the sum and go to the next level. If formulated as a computer algorithm, all carried numbers will initially have to be set to 0, and a procedure for deciding when to stop (not trivial if the number of digits in the addends is not limited a priori). This may be a good exercise in an introductory course in programming, but nobody will use it when teaching children how to add numbers by hand.

corresponding cases”.

However, the suggestion should not automatically and not always be taken to be a proof. At times, what has to be learned from the paradigmatic example is not the exact rule but a principle that can be varied according to circumstances (like the principles used in the addition within a place value system, cf. note 5).<sup>7</sup> Even the use of a term like “rule” within the texts may be misleading – for instance, when Fibonacci speaks of applying the *regula recta* [ed. Boncompagni 1857: 191 and *passim*], he refers to the expression of the givens of a problem within a rhetorical first-degree equation with unknown *res*. In order to decide whether a text that does not explicitly formulate rules abstractly is really meant to train algorithms and not flexible use of more general principles we must look at the text as a whole, investigating first of all whether it presents a plurality of strictly parallel problems where only the “dress” and the numerical parameters change.<sup>8</sup>

Whether texts that *really* appear to aim at the training of algorithms/rules are meant to train blind obedience or understanding may be difficult to decide unless the texts offer adequate explanations; we rarely know about possible oral explanations that were supposed to accompany the teaching; nor do we know to which extent commentaries, when they are known to exist or have existed, were meant to be studied by students, by their teachers in general, or by select “mathematicians”. Since no adequate general answer can be given, I shall leave the matter aside.

Let us then approach the question from a different angle. If the notion of “algorithms” is to tell us something of interest about a mathematical *culture*, we

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<sup>7</sup> In order to bar the proposal of a notion of a “flexible algorithm” for this case I shall emphasize that this is a contradiction in terms which implies that the core of the algorithm-concept disappears and thus removes its cognitive potentials; it has nothing to do with the fruitful extension of concepts discussed by Imre Lakatos [1976]. But we may of course see such texts as training a flexible ability to *modify* algorithms, that is, to create new algorithms on the basis of others already known.

<sup>8</sup> Or, alternatively, whether in the presentation of solutions it is made explicit what has to be done “in general”, that is, independently of the actual parameters. This is particularly common in the pseudo-Heronian corpus [ed. Heiberg 1912; 1914], which sometimes uses καθολικῶς/“in general”, sometimes παντός or ἀεί/“always”, and sometimes πάντοτε/“at all times” – see [Høyrup 1997: 92f]; but it is also done often enough to be significant in Abū Bakr’s *Liber mensurationum* [ed. Busard 1968: 95, 98 and *passim*] (the word being *semper* in the Latin translation).



should rather ask about its *production* of mathematical knowledge than about the written traces of its teaching, even though these traces are often our only way to know not only about the teaching but also about the production. Indeed, if a particular mathematical culture is to be characterized distinctively as “algorithmic”, it is not because those who are trained in *using* its knowledge do so following rules or algorithms in any sense; everybody who uses mathematics does so somehow. What is decisive is whether or not the *production* of rules or algorithms is regarded as a central activity for those whom we would count as “mathematicians” (that is, producers of mathematical knowledge). This production can never be blind nor by mere trial-and-error except in the simplest cases (which, *eo ipso*, we would hardly characterize as “mathematics”).<sup>9</sup> In *this* respect it is probably justified to speak of much of Chinese and Sanskrit mathematics as “algorithmic” – cf. also [Duan & Nikolantonakis 2010: 171].

### **First example: Jordanus’s *De numeris datis***

Neither medieval Latin university mathematics nor the abacus tradition and what was derived from it were “algorithmic” in this sense. None the less, they offer examples that elucidate some of the general deliberations above. Let us first look at Jordanus de Nemore’s *De numeris datis*.

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<sup>9</sup> A. P. Juschkewitsch [1964: 5], after having discussed some of the advanced techniques of Chinese mathematics, has this to say:

Certain works dealing with the history of science are of the opinion that ancient Chinese mathematics was purely empirical. They argue that the old Chinese books contain no proofs, that they are in the main collections of recipes explained through examples. [...]. However, one should distinguish between the way matters are presented, which is mainly determined by the purpose of the book, and the method of investigation. The dogmatism of the presentation, the mechanical rote learning of various rules as the multiplication splitting-up of the latter and division followed because the medieval textbooks were written primarily for practical people, such as merchants, surveyors, officials, masons, etc. Such people needed mechanical and, as far as possible, succinct rules for the solution of precisely and narrowly defined problem types.

It should also be remembered that far from all old Chinese books about mathematics were deprived of arguments and explanations. Both are, for instance, contained in various works whose aim was the explanation and further development of the *Nine Chapters on Arithmetic*. Many scientific results could not at all be reached empirically but must build on logical deduction.

(My translation, as subsequent translations with no identified translator.)

According to its title as well as its format, this work was intended as an arithmetical counterpart of Euclid's *Data*, and it is related to Jordanus's *De elementis arithmetice artis* in the same way as Euclid's *Data* are related to the geometrical books of his *Elements*. It is likely to have been written in the 1220s,<sup>10</sup> but the precise date is unimportant for the present discussion.

The propositions of the *De numeris datis* state that if certain arithmetical combinations of certain numbers are given, then these numbers will also be given – for instance (I.17), [ed. Hughes 1981: 63],<sup>11</sup>

When a given number is divided into two parts, if the product of one by the other is divided by their difference, and the outcome is given, then each part will also be given.

Such propositions evidently correspond to algebraic equations, but Jordanus says nothing about algebra; on the other hand, the propositions are followed by numerical examples, and these often coincide with problems known from Arabic algebra or from the algebra section of Fibonacci's *Liber abbaci*.<sup>12</sup> Some are also obvious repetitions of matters currently dealt with in familiar algebra treatises, like IV.9 [ed. Hughes 1981: 29], indicating the existence of a double solution to what we would express  $x^2 + b = ax$ , “a square which with the addition of a given number makes a number that is produced by its root multiplied by a given number, can be obtained in two ways”. Since the propositions are provided with deductive proofs, there is no doubt that Jordanus's intention was to derive the results known from Arabic algebra in a way which agreed better than there with the norms of Euclidean mathematics.

Let us look at a simple example, prop. I.3 [ed. Hughes 1981: 58]:

If a given number is divided into two and if the product of one with the other is given, each of them will also be given by necessity.

Let the given number  $abc$  be divided into  $ab$  and  $c$ , and let the product of  $ab$  with  $c$  be given as  $d$ , and let similarly the product of  $abc$  with itself be  $e$ . Then

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<sup>10</sup> It must be written after the *De elementis*, since it refers explicitly to this treatise, which for its part must postdate the second version of the algorism treatises. Here, indeed, the letter symbolism is first developed which was then used more fully in the *De elementis* and the *De numeris datis*. Finally, one of the algorism treatises is copied (apparently by Grosseteste) in 1215/16 [Hunt 1955: 133f].

<sup>11</sup> Translated from Barnabas Hughes' edition of the Latin text, his English translation being too free for our present purpose.

<sup>12</sup> Details in [Høyrup 1988:310].

the quadruple of  $d$  is taken, which is  $f$ . When this is withdrawn from  $e$ ,  $g$  remains, and this will be the square on the difference between  $ab$  and  $c$ .<sup>13</sup> Therefore the root of  $g$  is extracted, and it will be  $b$ , the difference between  $ab$  and  $c$ . And since  $b$  will be given,  $c$  and  $ab$  will also be given.<sup>14</sup>

As we see, Jordanus uses letters to represent numbers (juxtaposition of two numbers indicates their sum); as we also see, each operation produces a *new* letter.

Historians tend to see this as an algebraic symbolism on which operations can be performed, and complain that it is not adequate as such. Thus Florian Cajori [1928: II, 3]:

Letters are used instead of special particular numbers. But Jordanus Nemorarius was not able to profit by this generality on account of the fact that he had no signs of operation – no sign of equality, no symbols for subtraction, multiplication, or division. He marked addition by juxtaposition. He represented the results of an operation upon two letters by a new letter. This procedure was adopted to such an extent that the letters became as much an impediment to rapid progress on a train of reasoning as the legs of a centipede are in a marathon race.

Similarly, but much more recently [Alten et al 2008: 211]:

während noch lange nach [Jordanus] jede Operation mit allgemeinen Zahlen an Strecken oder Rechtecken ausgeführt wurde, tritt bei ihm das Buchstabensymbol als rein arithmetisches Zeichen für eine beliebige Zahl auf. Allerdings führt er für die Ergebnisse der Zwischenschritte stets weitere neue Buchstabenbezeichnungen ein; dies erschwert dem modernen Leser das Verständnis.

In such cases, it is always wise to reflect on Georg Christoph Lichtenberg's aphorism, "If a book and a head collide and the ring is hollow, is the book necessarily at fault?" Actually, if we take a closer look at Jordanus's text, we discover that its initial part translates easily into an algorithm:

$$\begin{aligned} ab \cdot c &\longrightarrow d \\ abc \cdot abc &\longrightarrow e \\ 4d &\longrightarrow f \end{aligned}$$

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<sup>13</sup> This follows from *De elementis* I.17 [ed. Busard 1991: 69], which in symbolic translation states that  $(a+b)^2 = 4a \cdot b + (a-b)^2$ .

<sup>14</sup> This follows from *De numeris datis* I.1, "If a given number is divided into two parts whose difference is given, each of them will be given".

$$\begin{array}{l} e-f \longrightarrow g \\ \sqrt{g} \longrightarrow b \end{array}$$

We also observe that the deductive argument is external to the algorithm – not posterior, as the proof of *Elements* I.1, but inherent in the formulations, which make us recognize the theorems that are drawn upon. None the less, we are clearly confronted with a case of deductive algorithmic thought – not pre-algebraic, however. On the contrary, indeed, since the text is a deductive reformulation of existing algebra, *post-algebraic*.

The final part of the formulation does not translate directly; this has to do with the eventual reduction to the situation of I.1, which has induced Jordanus to designate the two parts  $ab$  and  $c$ , where  $b$  is the difference and therefore  $a = c$ . In the numerical example, where no reduction occurs,  $\sqrt{g}$  is subtracted from the total, which is halved, thereby giving the minor part. In modern algorithmic language, this might result in the embedding of a sub-routine; but Jordanus’s algorithms are, in Knuth’s words, and in spite of their theoretical sophistication in other respects, “‘straight-line’ calculations, without any branching or decision-making involved”.

The echo of Jordanus’s treatise was extremely faint. It is nothing but a fable that it served as the standard textbook for algebra teaching in the scholastic university. Firstly, there is not the slightest evidence that there was any regular teaching of the topic; nor, secondly, is the existence of such teaching to be expected, algebra having no place, neither within the quadrivial tradition<sup>15</sup> nor within the framework of that “medico-astrological naturalism” which had been the driving force behind the twelfth-century translations from the Arabic and for the university teaching of natural philosophy and Greek-style mathematics. Secondly, we have very few references to the treatise before the mid-fifteenth century. Campanus refers explicitly to the *De elementis* once in his version of the *Elements*, and uses its propositions elsewhere [ed. Busard 2005: I, 174, cf. 33], but he never mentions or uses the *De numeris datis*. Richard de Fournival took care to have most of Jordanus’s works copied, but his own interests as revealed by the library he collected [Birkenmajer 1970/1922] were directed toward geography, astronomy, astrology and magic. Roger Bacon refers to the *De elementis* repeatedly in his *Communia mathematica* [ed. Steele 1940: 47 and *passim*]

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<sup>15</sup> No doubt, we may claim that this was exactly what Jordanus attempted to give it by basing it on theoretical arithmetic – but even his *De elementis* with its Euclidean aspirations was too different from what was customary in the quadrivial tradition to be generally accepted before Lefèvre d’Étaples made an edition in [1496].

but finds it much less useful than Boethius's arithmetic because of its being burdened by proofs; evidently the *De numeris datis* would be beyond his horizon. And around 1300 the Dominican chronicler Nicholas Trivet confuses Jordanus of Nemore and the Dominican General Jordanus of Saxony, ascribing to the latter a work on weights and another one *De lineis datis*, which seems to mix up Jordanus's *Liber de triangulis* and his *De numeris datis* ([Curtze 1887: iv], cf. [Høyrup 1988:341 n. 76]). This confusion would hardly have come about if the latter work had been in use.

After the mid-fourteenth century, Oresme cites the *De elementis* and the *De numeris datis* in three works<sup>16</sup>. Already in 1343, Jean de Murs had included algebra derived from al-Khwārizmī, Fibonacci and contemporary abacus writers in his *Quadripartitum numerorum*, but he seems to have known nothing about the *De numeris datis*. Actually, we have to wait for George Peurbach and Regiomontanus before we find anybody who gives evidence of having understood Jordanus's undertaking: in a poem, Peurbach [ed. Größing 1983: 210] refers to "the extraordinary ways of the Arabs, the force of the entirety of numbers so beautiful to know, what algebra computes, what Jordanus demonstrates"; Regiomontanus speaks in his Padua lecture on the mathematical sciences from 1464 [ed. Schmeidler 1972: 46] about the "three most beautiful books about given numbers" which Jordanus

had published on the basis of his *Elements of arithmetic* in ten books. Until now, however, nobody has translated from the Greek into Latin the thirteen most subtle books of Diophantos, in which the flower of the whole of arithmetic is hidden, namely the art of the thing and the *census*, which today is called algebra by an Arabic name.

University teaching of mathematics, based on lectures and disputations, with texts, commentaries and *questiones* as the appurtenant literary genres, tended to perpetuate existing theory and bolster it up with metatheoretical reflection. It had little space for anything reminding of algorithms even in the most diluted sense. However, since Carolingian times one (minor) strain of Latin mathematics had consisted of (mostly) recreational problems with the solutions. If there had

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<sup>16</sup> *De elementis* in *Algorismus proportionum* [ed. Curtze 1868: 14], in *De proportionibus proportionum* [ed. Grant 1966: 140, 148, 180] and *Tractatus de commensurabilitate vel incommensurabilitate motuum celi* [ed. Grant 1971: 294] (a complaint that Jordanus's subtle work is inapplicable to the presumably irrational ratios of celestial speeds); *De numeris datis* in *De proportionibus proportionum* [ed. Grant 1966: 164, 266] (but references to propositions about elementary proportion theory).

been interest for it, Jordanus's technique for making deductive algorithms would have offered an opportunity to submit this kind of mathematics to theoretical scrutiny and justification. Since this seems never to have been done, we may conclude that there was no such interest.<sup>17</sup>

### **Barthélemy de Romans and the schematization of algorithms**

Problems solved (by necessity) via step-by-step procedures of course abound in late medieval *abbacus* culture (and its Provençal and Iberian cognates, as well as that early reflection of the same culture which is represented by the *Liber abaci*); it would certainly be possible to dissect the way these procedures are handled so as to distinguish more from less algorithmic aims of the texts. This, however, I shall leave aside, and concentrate instead in the present section on a peculiar graphic representation of algorithms found in Barthélemy de Romans' *Compendy de la pratique des nombres* [ed. Spiesser 2003], an outgrowth of the Provençal branch of the tradition.<sup>18</sup>

These algorithms concern a strange problem type.<sup>19</sup> In the simple version it may run as follows:

Somebody toward the end of his life tells his oldest son thus. Divide my moveable property between you in this way: you take one bezant, and a seventh of what remains. An to the second son he says, you take 2 bezants, and the seventh of what remains. An to the third, that he should take 3 bezants, and

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<sup>17</sup> Interest in *procedures* for solving simple problems was not totally absent. Six late thirteenth- to fourteenth-century copies survive of a small treatise *De regulis generalibus Algorismi ad solvendum omnes questiones propositas* [ed. Hughes 1980]. It starts by giving the rules for finding the smallest common multiple of numbers (used for adding fractions) and then shows how to find a number from the sum of specified fractions of it or vice versa; the total of the sum of the parts from the residue; the fourth proportional; and the initial possession of somebody offering God 4 pence for doubling his possession, doing so four times, after which he is broke (with variants; misunderstood by the modern editor). The latter problem is widespread in Arabic practical arithmetic and is first known from the seventh-century Armenian priest Ananias of Shirak, the others though formulated in the abstract are provided with examples known from the latin "sub-universitarian" tradition and from *abbacus* as well as Arabic mathematics. There are no proofs, only explanation of the rules.

<sup>18</sup> Probably first written in 1467, but known from a revised redaction from 1476 due to Mathieu Préhoude.

<sup>19</sup> The problem type, its variations and its occurrences are dealt with in [Høystrup 2008].

take control of  $\frac{1}{7}$  of what remained. And in this way he called all his sons in order, giving to each of them one more than to the other, and afterwards always  $\frac{1}{7}$  of what was left. The last, however, got the remainder. It happened, however, that each of them got from the property of their father the same, on the given condition. It is asked, how many were the sons, and how much was his possession.

This version is taken from the *Liber abbaci* [ed. Boncompagni 1857: 279]. Next comes a variation where each *first* receives  $\frac{1}{7}$  of the available amount, and only afterwards 1, 2, 3, ... bezants – and then further variations where the fraction is no aliquot part, and where the absolute contributions, still in arithmetical progression, do not have the difference equal to the first member.

Fibonacci is the earliest extant source for the problem type but not its inventor. This follows already from the fact that his algebraic solution for one of the cases does not fit the rule he gives for the same case – see imminently. For convenience we may introduce these notations for the various types:

- $(\alpha, \varepsilon | \phi)$  designates the type where absolutely defined contributions  $\alpha + \varepsilon i$  ( $i = 0, 1, \dots$ ) are taken first, and a fraction  $\phi$  of the remainder afterwards;
- $(\phi | \alpha, \varepsilon)$  designates the type where a fraction  $\phi$  of what is at disposal is taken first and absolutely defined contributions  $\alpha + \varepsilon i$  ( $i = 0, 1, \dots$ ) afterwards.

In this notation, Fibonacci's examples are the following:

$(1, 1   \frac{1}{7})$	$(1, 1   \frac{2}{11})$	$(2, 3   \frac{6}{31})$	$(3, 2   \frac{5}{19})$
$(\frac{1}{7}   1, 1)$	$(4, 4   \frac{2}{11})$	$(\frac{6}{31}   2, 3)$	$(\frac{5}{19}   3, 2)$
$(3, 3   \frac{1}{7})$	$(\frac{2}{11}   1, 1)$		
$(\frac{1}{7}   3, 3)$	$(\frac{2}{11}   4, 4)$		

If  $N$  designates the number of sons,  $\Delta$  the share of each, and  $T$  the total ( $T = N \cdot \Delta$ ), the rules for the case  $(1, 1 | \frac{1}{7})$  are given as  $N = \Delta = 7 - 1$ , and those for  $(\frac{1}{7} | 1, 1)$  as  $N = 7 - 1$ ,  $\Delta = 7$ . If  $\frac{2}{11}$  it understood as  $\frac{1}{5\frac{1}{2}}$ , the rules for all cases in the two leftmost columns are easily derived (for non-integer values of  $d = \frac{1}{\phi}$ , the last son gets a fractional part – for  $\frac{1}{\phi} = 5\frac{1}{2}$ , the last son (being counted as  $\frac{1}{2}$  of a son) only gets half of what the others receive). All these rules are stated without argument.

For the case  $(2, 3 | \frac{6}{31})$ , Fibonacci derives the solution by means of first-degree algebra (*regula recta*, cf. above), identifying  $T$  with the *res* and assuming the equality of the first two shares. This leads to  $T = 56\frac{1}{4}$ ,  $N = 4\frac{1}{2}$ ,  $\Delta = 12\frac{1}{2}$ . Fibonacci does not mention that this has not been shown to be an actual solution to the heavily overdetermined problem, but he may have been aware of the logical

difficulty; in any case he makes a complete calculation of all shares. Afterwards he claims to “extract” this rule from the calculation ( $\Phi = \frac{p}{q}$ ) – evidently expressed rhetorically:

$$(1^a) \quad T = \frac{[(\varepsilon - \alpha)q + (q - p)\alpha] \cdot (q - p)}{p^2},$$

$$(1^b) \quad N = \frac{(\varepsilon - \alpha)q + (q - p)\alpha}{\varepsilon p},$$

$$(1^c) \quad \Delta = \frac{\varepsilon(q - p)}{p}.$$

If we look closer at the matter, the rule is seen *not* to be extracted. Following the algebraic calculation step by step, we get

$$(2^a) \quad T = \frac{q^2(\alpha + \varepsilon) - (q - p)q\alpha - (q - p)p\alpha - (\alpha + \varepsilon)pq}{p^2}$$

which (by means at Fibonacci’s disposal) transforms into

$$(2^{a*}) \quad T = \frac{[q(\alpha + \varepsilon) - (p + q)\alpha] \cdot (q - p)}{p^2}$$

but not in any obvious way into (1<sup>a</sup>) – if anything, further manipulation would rather lead to

$$(3^a) \quad T = \frac{[\varepsilon q - \alpha p] \cdot (q - p)}{p^2}.$$

We may conclude that Fibonacci adopted a rule whose basis he did not know, and then pretended that it was a consequence of his own (correct but partial) calculation.

This is confirmed by his treatment of the case (3,2 |  $\frac{5}{19}$ ). Here,  $\varepsilon - \alpha$  becomes negative, for which reason Fibonacci (who knew well how to make such operations) replaces (1) by

$$(4^a) \quad T = \frac{[(q - p)\alpha - (\alpha - \varepsilon)q] \cdot (q - p)}{p^2},$$

$$(4^b) \quad N = \frac{(q - p)\alpha - (\alpha - \varepsilon)q}{\varepsilon p},$$

$$(4^c) \quad \Delta = \frac{\varepsilon(q - p)}{p}.$$

If Fibonacci had derived (1<sup>a</sup>) from the outcome (2<sup>a</sup>) of his algebraic calculation, why would he have chosen to reduce it to a form that is neither fully reduced



nor valid independently of the sign of  $\varepsilon - \alpha$ , as are (2<sup>a</sup>), (2<sup>a\*</sup>) and (3<sup>a</sup>)?

For the case ( $\frac{6}{31} \mid 2,3$ ), Fibonacci states and applies the rules

$$(5^a) \quad T = \frac{[(\varepsilon - \alpha) q + (q - p) \alpha] \cdot q}{p^2},$$

$$(5^b) \quad N = \frac{(\varepsilon - \alpha) q + (q - p) \alpha}{\varepsilon p},$$

$$(5^c) \quad \Delta = \frac{\varepsilon q}{p},$$

without algebra, and for ( $\frac{5}{19} \mid 3,2$ )

$$(6^a) \quad T = \frac{[(q - p) \alpha - (\alpha - \varepsilon) q] \cdot q}{p^2},$$

$$(6^b) \quad N = \frac{(q - p) \alpha - (\alpha - \varepsilon) q}{\varepsilon p},$$

$$(6^c) \quad \Delta = \frac{\varepsilon q}{p}.$$

If (1<sup>a</sup>) had really resulted from the algebraic solution, why should (5) and (6) be set forth without being derived from the corresponding but different algebraic operations?

In the late thirteenth century, the problem ( $1,1 \mid \frac{1}{7}$ ) is dealt with by Maximus Planudes [ed. Allard 1981: 191–194], who bases the solution on a number-theoretical statement (probably based on *psephoi* arranged in a square pattern). Subsequently, the problem is dealt with (without argument, and regularly with  $\frac{1}{10}$  replacing  $\frac{1}{7}$ ) in many Italian, Provençal and Byzantine abacus treatises; some of them also give easily reducible variants ( $n,1 \mid \frac{1}{q}$ ) (whose solution takes away  $n-1$  sons from the solution to ( $1,1 \mid \frac{1}{q}$ )). Occasionally, solutions by means of algebra or double false position (based in both cases on the equality of the first two shares) are offered.

Sophisticated variant like those in the two right-hand Fibonacci columns only turn up again in Barthélemy de Romans' *Compendy de la pratique des nombres* [ed. Spiesser 2003: 391–423]. In this work, the problem type receives the most extensive treatment ever under the heading “composite progressions” (*progressions composees*) – as Barthélemy has noticed, the principle of the problem combines those of the arithmetical and the geometrical progressions. At the same occasion, the inheritance dress is left behind, Barthélemy deals with *numbers in composite*

*progression*. Because of partial coincidence of  $\Phi$ -values, Maryvonne Spiesser [2003: 156] supposes Barthélemy to have borrowed from the *Liber abbaci*. Closer statistical analysis undermines this conclusion [Høyrup 2008: 635 n. 31]; since there is evidence that Cardano knew about solutions to the sophisticated versions that do not come from the *Liber abbaci* [Høyrup 2008: 641], it seems plausible that even Barthélemy drew upon knowledge that circulated during the thirteenth through fifteenth centuries but has left no traces in sources preceding Barthélemy which we know of.

Barthélemy gives rules for all cases, similar to those of Fibonacci but not identical. He has no derivation of these from the givens of the problem,<sup>20</sup> but he performs some kind of theoretical work *on* the rules and the problem type. He introduces a name for the quantity  $d = \frac{1}{\Phi}$ , which already Fibonacci had used, namely “the true denominator”, and distinguishes two “modes”. The first mode is the one where the absolutely defined contributions (*les nombres de la progression*) are taken first and the fraction of what remains (*la partie ou les parties que l'on veut du demourant*) afterwards; the second is the one where “part or the parts” are taken first, and afterwards “the numbers that make the progression” from what remains.

The introduction of the true denominator  $d$  allows Barthélemy to formulate a “general rule” for the first mode:

$$\begin{aligned} (7^c) \quad & \Delta = (d-1) \cdot \varepsilon , \\ (7^a) \quad & T = ([d-1]\varepsilon - \alpha) \cdot d + \alpha , \\ (7^b) \quad & N = T/\Delta . \end{aligned}$$

If  $\alpha = \varepsilon$  he points out that it “can be done by another practice, for which this is the appurtenant rule”:

$$\begin{aligned} (8^b) \quad & N = d-1 , \\ (8^c) \quad & \Delta = (d-1) \cdot \varepsilon , \\ (8^a) \quad & T = N^2 \cdot \varepsilon , \end{aligned}$$

which seems not to be derived from his general rule but rather to be a formulation *as a rule* of the current practice of *abbacus* books.

For the second mode, the rule for the case  $\alpha = \varepsilon$  is given first,

$$(9^b) \quad N = d-1 ,$$

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<sup>20</sup>To a superficial inspection, he may seem to make a derivation by means of a double false position. What he actually does is to find  $\Delta$  from the corresponding formula and *then* to make two guesses for  $T$  and derive the corresponding values for the first share. From their deviations from the true common share  $\Delta$  the true value of  $T$  can then be determined.

$$(9^c) \quad \Delta = d \cdot \varepsilon ,$$

$$(9^a) \quad T = (d-1) \cdot d \cdot \varepsilon .$$

Then separate rules are given for the cases  $\alpha < \varepsilon$  and  $\alpha > \varepsilon$  (similarly to what is done in the *Liber abaci*), respectively

$$(10^a) \quad T = \frac{[(\varepsilon - \alpha) q + (q - p) \alpha] \cdot q}{p^2} ,$$

$$(10^b) \quad N = \frac{(\varepsilon - \alpha) q + (q - p) \alpha}{\varepsilon p} ,$$

$$(10^c) \quad \Delta = \frac{\varepsilon q}{p} ,$$

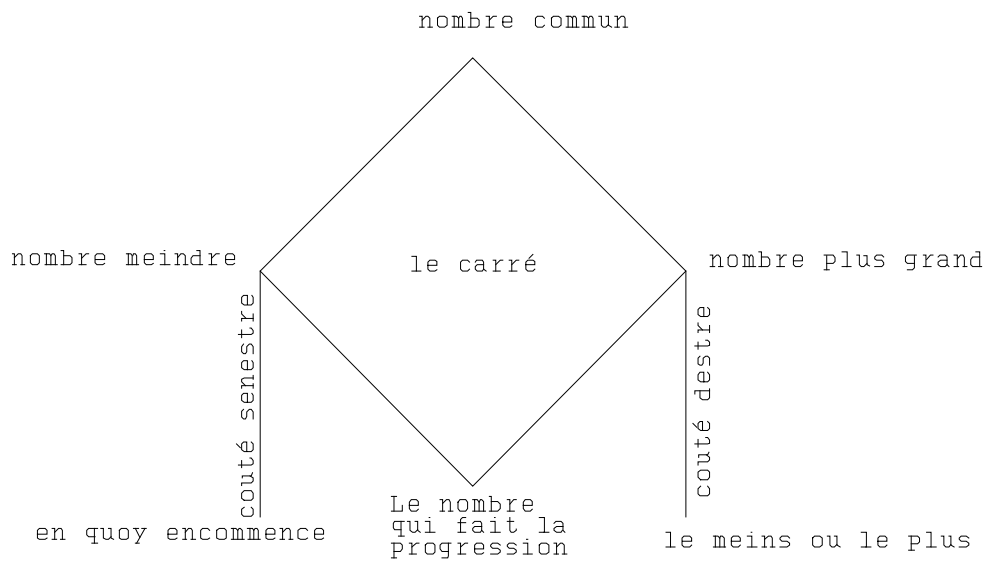
and

$$(11^a) \quad T = \frac{[(q - p) \alpha - (\alpha - \varepsilon) q] \cdot q}{p^2} ,$$

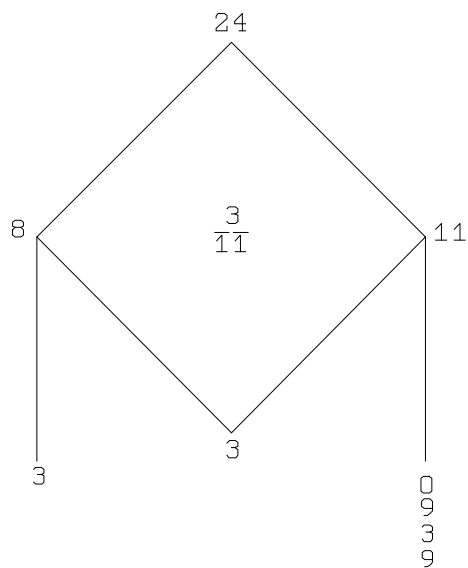
$$(11^b) \quad N = \frac{(q - p) \alpha - (\alpha - \varepsilon) q}{\varepsilon p} ,$$

$$(11^c) \quad \Delta = \frac{\varepsilon q}{p} .$$

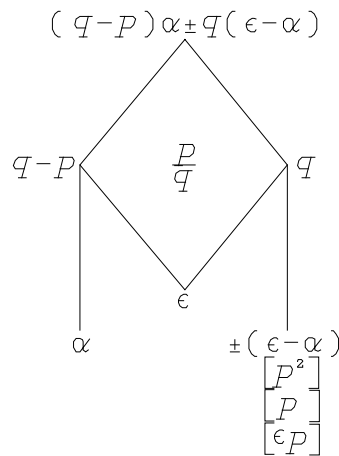
This is already complicated enough when everything is stated in algebraic symbolism. In words, it is evidently worse, even when all rules are illustrated by examples. If we accept that the subject is important (and for Barthélemy it is the high point of his treatise), Barthélemy therefore has very good reasons to introduce a graphic representation of the algorithms, which is almost certainly his own idea. “For the practice of this rule and in order to see rapidly how one should make the necessary multiplications for the three numbers that should be divided by the three dividers to get the three hidden numbers”, he shows “how the necessary numbers can be put into a diagram”, here following [Spiesser 2003: 405]:



A number of examples show the use of the diagram; for the problem  $(3,3 | \frac{3}{11})$  it becomes



In our general symbolic terms, the diagram can be seen from the examples to stand for



Evidently, this diagram does not represent an argument leading to the formulae. Nor is it the analogue of a flow chart representing the algorithm – which anyhow would make no sense when no branchings are present. What it does is to lay out all numbers that enter the algorithm, after which the calculator has to remember how to use them. However, by freezing the oral formulae graphically, Barthélemy makes it more clear (to us) that a fixed algorithm is really thought of. The diagram helps as much as the Indian graphic representations of the algorithms by which algebraic equations are solved, and it has the same limitations (*pace* Nesselmann [1842: 302f], who saw no difference between these schemes and symbolic algebra): it is unable to represent more than *one* linear algorithm, and has no space for embedding (of subroutines, if we speak the algorithmic language; of the replacement of a single number by an algebraic composite if we choose that language).

### Nicholas Chuquet and algebraic rejection of algorithmic schemes

Chuquet probably understood the potentials of algebra better than anybody else in Europe during his century – probably better than anybody between Antonio de’ Mazzinghi and Cardano, perhaps even Bombelli; he certainly understood them better than Estienne de la Roche, whose borrowings from Chuquet’s *Triparty* for his *Larismetique* from 1520 made public part of Chuquet’s mathematics but excluded everything too radically new [Moss 1988: 120f].

Apart from Barthélemy, nobody has dedicated as much space to the “unknown heritage” as Chuquet. He does so in the problem collection attached to his *Triparty* from 1484. The problems, listed in [Marre 1881: 448–451], are of the following types:

$(1,1   \frac{1}{10})$	$(2,3   \frac{2}{13})$	$(\frac{1}{7}   3,5)$	$(\frac{1}{6}   5,3)$
$(2,1   \frac{1}{7})$	$(3,2   \frac{1}{13})$	$(\frac{2}{8}   3,5)$	$(\frac{2}{11}   5,2)$
$(2,3   \frac{1}{8})$	$(\frac{1}{7}   2,2)$	$(\frac{8}{31}   2,3)$	$(\frac{11}{19}   5,3)$
	$(\frac{2}{11}   3,3)$		

Those in the left-hand column are independent of Barthélemy. All the others are found in Barthélemy's *Compendy* in the same order, and only one is missing from the sequence of problems which Barthélemy brings before going into "theoretical" deliberations (after these deliberations, Barthélemy has more problems, probably of his own making, whereas those preceding his "theory" probably come from his sources). Since Chuquet knew Barthélemy's treatise (he refers to it elsewhere [ed. Marre 1881: 442]), it is a fair guess that Barthélemy is Chuquet's source for these problems; alternatively, if they use a common source, he is at least likely to have known what Barthélemy did to that source.

In any case, Chuquet treats the material in a different way than Barthélemy. He returns to the inheritance dress, speaking of "the number of children" even when  $N$  is not integer. He gives no diagrams and only one rule (after the problem  $(2,1 | \frac{1}{7})$  [ed. Marre 1881: 449]),

Multiply the number which is 1 less than the denominator of the common part by the number which makes the progression. Which multiplication [i.e., product] you put aside, because it is the number of deniers which each one shall receive. Then subtract from this multiplication the number which the first one takes when he goes to the box, that is the number by which the progression begins. And multiply the remainder by the denominator of the common part, to which multiplication join the number by which the progression begins, because the addition [i.e., sum] is the number of deniers in the box. Which number divide by the multiplication which was put aside, that is, by the share which each one gets, and you have the number of children.

In symbols once more:

$$\begin{aligned}
 (11^c) \quad & \Delta = (d-1) \cdot \varepsilon , \\
 (11^a) \quad & T = ([d-1] \cdot \varepsilon - \alpha) \cdot d + \alpha , \\
 (11^b) \quad & N = T / \Delta ,
 \end{aligned}$$

that is, Barthélemy's "general rule" (7) for his "first mode". But Chuquet speaks of  $d$  simply as the denominator, not as a "true denominator" – at this point in his text only integer values for  $d$  have in fact occurred. Apart from that (including in the problems that follow the "second mode"), no explanations or calculations

but only results are given.<sup>21</sup> But after the last problem of the group [ed. Marre 1981: 451] there is the observation (in italics in the edition, thus probably in red in the manuscript) that *Toutes telles raisons facilement se peuvent faire par la règle des premiers*, “all such calculations can easily be done by the rule of algebra”.

In Chuquet’s view, it appears, rules or algorithms embodied in diagrams were *pre-algebraic* and not worth conserving once the algebraic tool was understood.

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<sup>21</sup> Marre’s transcription is incomplete in as far as calculations are concerned, but after inspecting the manuscript Stéphane Lamassé confirms to me (personal communication) that there are no further rules or calculations.

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