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Relations between algebra and analysis before Viète

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WHAT IS NEW AND WHAT IS OLD IN VIÈTE'S *ANALYSIS RESTITUTA* AND *ALGEBRA NOVA*, AND WHERE DO THEY COME FROM? SOME REFLECTIONS ON THE RELATIONS BETWEEN ALGEBRA AND ANALYSIS BEFORE VIÈTE

Marco Panza

Abstract. — François Viète considered most of his mathematical treatises to be part of a body of texts he entitled *Opus restitutæ mathematicæ analyseos seu algebra nova.* Despite this title and the fact that the term "algebra" has often been used to designate what is customarily regarded as Viète's main contribution to mathematics, such a term is not part of his vocabulary. How should we understand this term, in the context of the title of his *Opus*, where "new algebra" is identified with "restored analysis"? To answer this question, I suggest distinguishing between two kinds of problematic analysis: the former is that described by Pappus at the beginning of the 7th book of his *Mathematical Collection*, which I will call "intra-configurational"; the latter is the one Viète applied, which I will call "trans-configurational". In order to apply the latter kind of analysis, Viète relies on his new formalism. I argue, however, that the use of this formalism is not a necessary condition for applying it. I also argue that the same kind of analysis was largely applied before Viète for solving geometrical problems, by relying on geometrical inferences of a special sort which I call "non-positional", since they do not depend on a diagram. As an example of a similar systematic application of trans-configurational analysis, I consider al-Khayyām's *Treatise of Algebra and Al-muqābala.* Finally, I suggest that Viète, when speaking of algebra in the title of his *Opus*, refers to the system of techniques underlying trans-configurational analysis, that is, to the art of transforming the conditions of certain purely quantitative problems, using either an appropriate formalism relative to the operations of addition, subtraction, multiplication,

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division, root extraction and solving polynomial equations applied to indeterminate numbers, or appropriate geometrical, non-positional inferences.

RÉSUMÉ (Qu'est-ce qui est neuf et qu'est-ce qui est ancien dans l'*analysis restituta* et l'*algebra nova* de Viète et quelle en est leur provenance ? Quelques réflexions sur les relations entre l'algèbre et l'analyse avant Viète)

François Viète considérait la plupart de ses traités mathématiques comme des parties d'un corpus de textes auquel il donna le titre de *Opus restitutæ mathematicæ analyseos seu algebra nova*. Malgré ce titre et le fait que le terme « algèbre » ait été souvent employé pour designer ce qui est d'habitude considérée comme la principale contribution de Viète aux mathématiques, ce terme ne fait pas partie de son langage technique. Comment doit-on l'entendre, dans le contexte du titre de son *Opus*, où la « nouvelle algèbre » est identifiée à l'« analyse restaurée » ? Pour répondre à cette question, je propose de distinguer deux sortes d'analyse problématique : la première est celle décrite par Pappus au début du VII^{e} Livre de sa *Collection mathématique* et que je propose d'appeler « intra-configurationnelle » ; la seconde est celle qui est appliquée par Viète et que je propose d'appeler « trans-configurationnelle ». Pour appliquer cette seconde sorte d'analyse, Viète se réclame de son nouveau formalisme. Je maintiens, cependant, que l'usage de ce formalisme n'est pas nécessaire. Je maintiens aussi que cette sorte d'analyse était souvent appliquée avant Viète pour résoudre des problèmes géométriques, en se réclamant d'un type particulier d'inférénces géométriques que j'appelle « non-positionnelles », car elles ne dépendent d'aucun diagramme. Pour donner un exemple d'une telle application systématique de l'analyse trans-configurationnelle, je me penche sur le *Traité d'algèbre et al-muqābala* d'al-Khayyām. Je suggère enfin qu'en parlant d'algèbre, dans le titre de son *Opus*, Viète se référait au système de techniques sous-jacentes à l'analyse trans-configurationnelle, c'est-à-dire à l'art de transformer les conditions de certains problèmes purement quantitatifs, en utilisant soit un formalisme approprié, concernant les opérations d'addition, soustraction, multiplication, division, extraction de racines et solution d'équations entières appliquées à des nombres indéterminés, soit des inférences géométriques non-positionnelles.

When he published the first edition of his *In artem analyticem isagoge*, in 1591, François Viète presented it as the first of ten treatises that were to form a systematic body of mathematical texts entitled *Opus restitutæ mathematicæ analyseos seu algebra nova.*[1] Despite the identification of "restored

[1] Cf. [Viète 1591a, pp. 1r–1v]; Viète also provides a list of the treatises that were to form his *Opus*: cf. footnote 2. Some copies of the printed texts of the dedicatory letter to the Princess Catherine de Parthenay and of the *Isagoge* were bound together with a different title page mentionning the title of the *Opus* as a whole (*Francisci Vietæ Fontenæensis Opus restitutæ mathematicæ analyseos seu algebra nova*, and the same publisher and date as Viète [1591a]). A copy of such a volume is now kept at the Library of the Arsenal, in Paris, under the signature "FOL- S- 1091 (2), Pièce n° 2". Presumably Ritter was using this volume for his French translation of the dedicatory letter to the Princess Catherine de Parthenay and of the *Isagoge*: cf. [Ritter 1868], pp. 224 (footnote by B. Boncompagni) and 225.

analysis" and "new algebra" suggested by this title, Viète's vocabulary in the *Isagoge* and other treatises of his *Opus*[2] includes the term "analysis", but not the term "algebra".

Still, Viète's title had some consequences. In 1630, two different French translations of the *Isagoge* and the *Zeteticorum libri quinque* (the second treatise of Viète's *Opus*), appeared, prepared by Vaulézard and Vasset, respectively.[3] The former published his translations separately, and entitled the first of them *Introduction en l'art analytic, ou Nouvelle algèbre*; the latter united his translations in a unique volume, under the title *L'algèbre nouvelle de Mr Viète*. Also in 1630 Ghetaldi's posthumous treatise on Viète's art was published. Though, for Ghetaldi, this art pertained to analysis and synthesis (which, contrary to Viète, he called with their Latin names: "*resolutio*" and "*compositio*"), it applied a sort of "algebra": "not the vulgar sort [...], but that of which François Viète is the author".[4]

When van Schooten published his edition of Viète's *Opera mathematica*, sixteen years later, in 1646, he did not follow the practice of Vaulézard, Vasset and Ghetaldi: while he included in his collection all the treatises included in Viète's *Opus* (including Anderson's[5]), he did not mention this *Opus* as such, and attributed to Viète no sort of algebra. However, he transformed Viète's terminology too and called him "the first author of analysis by species [*analyseos speciosæ*]", whereas Viète himself had used the term "logistic by species [*logistice speciosa*]".

The comparison of the wording of Viète, Vaulézard, Vasset, Ghetaldi, and van Schooten shows that the technical vocabulary of Viète's school was unstable. Consequently, the mutual relations that, according to Viète and his followers, should have held between analysis, algebra and logistic, in particular when they were supposed to deal with species, are quite difficult to clarify by a literal examination of their works. As a matter of fact,

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2 Viète's *Opus* was to comprise ten treatises. Eight of them correspond eight of his later publications: [Viète 1631], [Viète 1591b], [Viète 1600], the first part of [Viète 1615a], [Viète 1591c], [Viète 1593a], [Viète 1593b]. The remaining two are denoted as *Ad logisticem speciosam notæ posteriores* and *Analytica angularum sectionum in tres partes tributa*. Ritter [1895, p. 396] has suggested that the former identifies with the second part of [Viète 1615a]. A. Anderson reconstructed the first part of the latter in [Viète 1615b], while there is no trace of the two other parts.

3 Cf. [Vaulézard 1630b], [Vaulézard 1630a], and [Vasset 1630].

4 Cf. [Ghetaldi 1630, pp. 1–2]: "*Duplex autem est resolutionis genus alterum quidem ad Theoremata pertinet* [...] *alterum vero ad Problemata* [...]. *sed omnia fere Theoremata, & Problemata, quæ sub Algebram cadunt facillime resolvuntur, ac per resolutionis vestigia componuntur: non quidem vulgaris Algebræ beneficio; quæ resolutionis vestigia omnino confundit; sed illius, cuius auctor est Franciscus Vieta* [...]."

5 Cf. footnote 2.

when we follow Ritter [1868, p. 223] and claim that Viète was the "inventor of modern algebra", or simply admit that he marked a turning point in the history of algebra, we are using the term "algebra" in a sense that we are attributing to Viète, rather than drawing from the examination of his works. And it is only based on this sense that we can describe Viète's (new) algebra.

Something similar happens, in general, when we speak of algebra with respect to early modern mathematics, or when we reconstruct a long-term evolution of algebra or compare different stages in such an evolution. We often have no room to claim that we are simply speaking of what the mathematicians we are considering were calling "algebra". This is not simply because, in such a way, we would hide the historical and mathematical reasons which justify the terminological choices of these mathematicians or those which make our account historically relevant. It is also, primarily, because a number of mathematicians we speak about, or we would like to speak about for tracing a coherent historical interpretation used the term "algebra" to denote something which they did not clearly feature, or did not even speak of algebra at all, or spoke of it by referring to things that are quite different from each other. When we deal with the history of algebra we are forced to interpret, to advance implicit or explicit historiographical hypotheses; we cannot limit ourselves to describe.

The aim of my paper is to advance just such an historiographical hypothesis. It concerns algebra in the early modern age, and especially Viète's algebra and its relation with analysis (and geometry). The early modern age and Viète, however, will be only the *termini ad quem* of my enquiry. My aim is to suggest a possible sense that we could attribute to the term "algebra" in the context of the title which Viète gave to his *Opus*. I shall rely for this on a distinction between two different sorts of analysis in pre-modern mathematics. I suggest that when Viète identified "new algebra" with "restored analysis", he was implicitly acknowledging that – within a mathematical tradition to which he felt himself to depend on – the second sort of analysis had gone together with what he was terming "algebra", and he was referring to a new way to perform this sort of analysis in order to solve a certain class of problems, namely a way based on the use of the formalism which the *Isagoge* was aiming to introduce.

The following quotation – drawn from the dedicatory letter to the Princess Catherine de Parthenay with which Viète prefaced the *Isagoge* –

can be read as a (partial) textual justification of my hypothesis; I will take it, in any case, as a guide for my enquiry: [6]

> "*Ecce ars quam profero nova est, aut demum ita vetusta, & a barbaris defædata & conspurcata, ut novam omnino formam ei inducere, & ablegatis omnibus suis pseudo-categorematis, ne quid suæ spurcitiei retineret, & veternum redoleret, excogitare necesse habuerim, & emittere nova vocabula, quibus cum parum hactenus sint adsue factæ aures, vix accidet, ut vel abipso limine non deterreantur multi & offendantur. At sub sua, quam prædicabant, & magnam artem vocabant, Algebra vel Almucabala, incomparandum latere aurum omnes adgnoscebant Mathematici, inveniebant vero minime. Vovebant Hecatombas, & sacra Musis parabant & Apollini, si quis unum vel alterum problema extulisset, ex talium ordine qualium decadas & eicadas ultro exhibemus, ut est ars nostra mathematum omnium inventrix certissima.* "

Provided that the art which Viète is speaking of at the beginning of this quotation is identified with his new algebra, these few lines suggest a number of things to us: (i) that Viète presents his new algebra as an art, that is a technique or a system of techniques, rather than as a discipline or a theory; (ii) that, according to Viète, it would have been already present, *in nuce*, in ancient mathematics, that is, presumably, that it would have been practiced, in its primitive form, by Greek mathematicians; (iii) that Viète takes his main contribution to consist in a reformulation of such an art, so that what is properly new, according to him, is not the art itself, but the form he has given to it; (iv) that, according to Viète again, his new algebra would also have been present, *in nuce*, in Arabic, and more generally medieval mathematics, and that his own reformulation would have revealed

6 Cf. [Viète 1591a, pp. 2v–3r]. Here is J.W. Smith's translation [Klein 1934-1936, pp. 318–319]: "Behold, the art which I present is new, but in truth so old, so spoiled and defiled by the barbarians, that I considered it necessary, in order to introduce an entirely new form into it, to think out and publish a new vocabulary, having gotten rid of all its pseudo-technical terms lest it should retain its filth and continue to stink in the old way, but since till now ears have been little accustomed to them, it will be hardly avoidable that many will be offended and frightened away at the very threshold. And yet underneath the Algebra or Almucabala which they lauded and called 'the great art', all Mathematicians recognized that incomparable gold lay hidden, though they used to find very little. There were those who vowed hecatombs and made sacrifice to the Muses and Apollo if any one would solve some one problem or other of the order of such problems as we solve freely by score, since our art is the surest finder of all things mathematical." Cf. [Ritter 1868, p. 227] for a French translation.

a general technique that is hidden in this algebra and only implicitly applied to some individual cases;[7] (v) that this technique applies to the solution of problems, so as to make it possible, once correctly formulated, to solve several problems at once. In his letter to the Princess Catherine de Parthenay, Viète does not speak of analysis, but the comparison of the previous quotation with the *Isagoge* (where the term "algebra" does not occur at all), and specially with its well-known first chapter (to which there will be no need to come back), immediately suggests that the techniques which Viète is speaking of – that is, his new algebra – is part of, or even should be identified with, a sort of problematic analysis, and that this sort of analysis, or at least a primitive version of it, was already practiced by Greek mathematicians and occurred in Arabic mathematics.

With this in mind, the plan of my enquiry becomes natural: I shall try to identify and describe this sort of analysis, together with the connected technique which Viète is referring to, by looking at both Greek and Arabic mathematical *corpus* (though I shall obviously limit myself to the consideration of few examples); this will be the subject matter of sections 2 and 3. Before doing that, I think useful to consider the previous point (i) in more detail. This will be the subject matter of section 1. Finally, section 4 will address some conclusions.

1. ON SOME DEFINITIONS OF "ALGEBRA", PARTICULARLY BOS' ONE

The term "algebra" and its cognates are and have been used by mathematicians and historians of mathematics with several and often very different meanings. It is not my aim, of course, to look for something such as the right meaning of this term or to disclose something such as the real nature of algebra. I would like only to fix one of these meanings.

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7 The Arabic origin of the term "algebra" and of the art it was denoting – that is the "Great Art" – was a quite accepted fact in early-modern age. Take as examples [Cardano 1545, p. 222], [Tartaglia 1556, p. IIv], [Bombelli 1572, "Agli Lettori", column 3] and [Clavius 1608, p. 4]. Nevertheless, very little is known about the effective relations of early-modern mathematicians with Arabic mathematical sources, and I have nothing to offer on this matter. In the great majority of cases, the Great Art was understood as part of arithmetic, that is as something like an arithmetic of indeterminate numbers. It seems however to me that this notion of algebra (to which I shall come back later at p. 96) is too restricted to account both for Arabic and Viète's conceptions and overall to supply an appropriate concept to be used to reconstruct the origins of early-modern mathematics and to identify some of its crucial features.

As I have said, it is the meaning which I suggest we should attribute to such a term in so far as it occurs in the title of Viète's *Opus*, and, consequently, when it is supposed to refer to the ingredient of Viète's mathematics to which he refers in this title. Still, Viète was not an isolated mathematician, and such an ingredient of his mathematics is also an essential ingredient of early-modern mathematics as a whole, at least before Descartes (perhaps things change with Descartes' *Geometry*, but I cannot discuss this question here). I suggest moreover to interpret the title of Viète's *Opus* as referring to an ingredient that was also present, though under a different formulation, in Greek and medieval mathematics. As a matter of fact, my aim is thus that of fixing a meaning that we should assign to the term "algebra" in many of its occurrences in medieval and early-modern mathematical texts, as well as in our historical reconstructions of the content and the mutual relations of Greek, medieval and early-modern mathematical texts. [8]

An important task of historians is certainly to account for differences in language and conceptions, but, as Henk Bos [2001, p. 119] has rightly emphasized in his enlightening book on "geometrical exactness" in early-modern mathematics, "in describing the conceptual developments that form the subject matter of [...] [an historical] study" that is not restricted to a very compact and small body of texts, "a reasonable constancy of the meanings" of the crucial terms is needed. This would in any case greatly improve our historical reconstructions in clarity, accuracy, and perspicuity.

For the term "algebra", when early-modern mathematics is concerned, this is all the more urgent in so far as its ambiguity has suggested a number of expressions which are often used to introduce historiographical concerns that do not answer, in my view, any genuine need of understanding. The most frequent of these concerns relies on the presumed passage that would have occurred in early-modern age from geometry to algebra. [9] I take it to be based on an important misunderstanding: by projecting

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8 This is of course only one of the meanings that the term "algebra" takes in medieval and early-modern mathematical texts and that we can appropriately assign to it when we use it in our historical reconstructions. I have elsewhere suggested other meanings for it, when early-modern mathematics is concerned, and I shall briefly come back to them on pp. 96–98. But to acknowledge a possible plurality of meanings for a certain term is not the same as to admit that these meanings had not to be accurately differentiated. My aim is to contribute to just such a differentiation.

9 Sometimes a similar concern is addressed by speaking of analysis instead of algebra. The continuation of my paper should make clear that I take such concerns to be equally misleading when referred to the early-modern age. On this matter, cf. [Panza 2005, pp. 1–44].

onto early-modern age the disciplinary organization of contemporary mathematics, one often takes algebra to be, in that age, a mathematical theory separated from and in fact opposed to geometry. I think that this is in any case wrong: early-modern mathematics does not include any theory that one could plausibly term "algebra" and that is separated from geometry and/or arithmetic.

Bos [2001, pp. 128–129] has proposed to "articulate the meaning" of the terms "arithmetic", "geometry" and "algebra" in the following way:

> "*Arithmetic* refers to the mathematical theory and practice that dealt with numbers.
>
> *Geometry* refers to the mathematical theory and practice that dealt with geometrical magnitudes [...].
>
> *Algebra* refers to those mathematical theories and practices that involved unknowns and/or indeterminates, employed the algebraic operations, involved equations, and dealt either with numbers or with geometrical magnitudes or with magnitudes in an abstract more general sense. In so far as it dealt with numbers, algebra was part of arithmetic. Algebra dealing with (geometrical or abstract) magnitudes presupposed (tacitly or explicitly) a redefinition of the algebraic operations so as to apply to such magnitudes. "

The two first definitions are plain, since Bos openly defines the relevant terms "number" and "geometrical magnitude as we would expect.[10] He also defines the term "abstract magnitude" [Bos 2001, p. 121]:

> "With the term *abstract magnitude* I refer to mathematical entities that, like geometrical magnitudes, could be joined, separated, and compared, but whose further nature was left unspecified. "

This is not the case for the term "algebraic operations", instead. Bos [2001, pp. 121–127] distinguishes "operations on numbers", "geometrical operations", "operations on abstract magnitudes" and "operations on ratios". He lists the first and the second ones, establishes a bijective relation of analogy which links each one of the former with each one of the latter, and claims that "the early modern period witnessed an interest in a 'universal mathematics' in which operations such as the arithmetical or geometrical ones were applied to magnitudes independently of their nature", and that "Viète was the first to elaborate such a theory" [Bos 2001, p. 125]. This makes him able to identify the operations on abstract magnitudes with those that Viète defines in the *Isagoge* [Bos 2001,

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10 Cf. [Bos 2001, p. 120]. Bos identifies geometrical magnitudes with "line segments, plane figures and solid figures as far as they were considered as to their size". I would add angles to the list: cf. [Descartes 1897-1910, vol. V, p. 395] (letter from Descartes to Carcavi, August 17, 1649).

p. 150]. Among the operations on numbers, he refers moreover to addition, subtraction, multiplication, division and extraction of square roots as "quadratic algebraic operations", but he abstains from specifying what algebraic operations (on numbers and magnitudes, both geometrical and abstract) would be, in general.

As Bos himself remarks, "root extraction is a kind of equation solving".[11] One can thus consider that all the operations on numbers that he lists, apart from that of "forming the ratio of two numbers" – that is, addition, subtraction, multiplication, division, root extraction and solving polynomial equations – are algebraic. By extension, one can then admit that this is also the case for the analogous operations on geometrical and abstract magnitudes, which we could for short call with the same names as the arithmetic ones. It would follow that "algebra" refers, for Bos, to the "mathematical theories and practices" which dealt with numbers, geometrical magnitudes or abstract magnitudes, "involved unknowns and/or indeterminates" and "employed" the operations of addition, subtraction, multiplication, division, root extraction and solving polynomial equations, as defined on numbers, geometrical magnitudes and abstract magnitudes, respectively. In other words, "algebra" would thus be a generic name used to refer to any "theory or practice" concerned with unknowns and/or indeterminate numbers, geometrical magnitudes or abstract magnitudes and understandable as a sort of generalization of arithmetic.

The most evident difficulty with this definition is that it is either all-embracing or depends on a hidden shift in the meaning of the term "geometrical operation": the geometrical operations are first introduced as constructive procedures,[12] then surreptitiously identified with formal rules of transformation, that is, applications, as we say today. Certainly this shift corresponds to a crucial historical fact. But an appropriate definition of "algebra" should either make the description (or reconstruction) of this fact possible, or issue from such a description (or reconstruction), rather than depending on its concealment.

I shall come back to this point shortly (cf. pp. 97–98, below). First, I would like to focus on a distinction which Bos does not seem to consider as a crucial one: that between theories and practices.

Bos' definition of "algebra" is essentially different from his definitions of "arithmetic" and "geometry": the latter ones depend on the identification

11 Cf. [Bos 2001, p. 122]: the calculation of $\sqrt[n]{a}$ can be reduced, indeed, to the solution of the equation $x^n - a = 0$.

12 Cf. [Bos 2001, pp. 123–125], specially table 6.2.

of a domain of objects, the former on the identification of a *modus operandi* on various and very different sorts of objects.

I take a mathematical theory to be a piece of mathematics characterized by the domain of objects which it is about, that is, I hold that a mathematical theory is identified if and only if a certain domain of objects is so, and – if this is the case – I say that this theory is about these objects.

Hence, I admit that in so far as they are identified with the pieces of mathematics that were about numbers and geometrical magnitudes, early-modern arithmetic and geometry have to be considered as genuine mathematical theories, and that they have to be taken as well-characterized mathematical theories, provided that these objects are taken, in turn, as well-identified objects. I shall not discuss here the difficult question as to whether this last condition should be considered as being satisfied or not (though I think that it should be, at least within reasonable limits of exactness). I emphasize that, in so far as Bos' definitions of "arithmetic" and "geometry" are adopted (apart from the term "practice" which occurs in them), arithmetic and geometry have to be considered as genuine mathematical theories, in my sense, and I suggest to adopt these definitions (but, cf. footnote 10) at least for the sake of my argument.

The situation is different for Bos' definition of "algebra", since (despite the occurrence of the term "theories" in it) it is not such as to warrant that algebra is a mathematical theory, in my sense. When algebra is characterized according to such a definition, it rather appears as a cluster of practices, or, to use Viète's language, as an art: a system of (mathematical) techniques. As far as such an art applies both to numbers and geometrical magnitudes – that is, the system of techniques that it consists of includes techniques to deal with numbers and geometrical magnitudes, respectively – algebra can be both "part of arithmetic" (as Bos explicitly admits) and part of geometry, that is: the term "algebra" can be used (in so far it is taken as a generic name) to refer either to an arithmetical technique or to a geometrical technique, or to a system of techniques including both arithmetical and geometrical techniques.

This is, I think, an appropriate view of the nature of early-modern algebra.[13] I term it for short "the inclusive view", and I take it to be suggested by Bos' definition (despite his use of the term "theory").

Still, this definition suggests more than that. It suggests that "algebra" can also be used to refer to a technique to deal with "abstract magnitudes". Bos' definition of "abstract magnitude" can be understood in different ways, however. I maintain that in early-modern mathematics there was room to join, separate and compare magnitudes, or more generally quantities (that is, magnitudes and numbers), "whose further nature was left unspecified", only in so far as it was admitted that this nature could be later specified, if needed. This is because intrinsically unspecifiable quantities were not part of the early-modern mathematical horizon; they only appeared some time later: first and implicitly in Newton's fluxional calculus[14] and then, explicitly, in Euler's theory of functions. Thus, I take Bos' definition of "abstract magnitudes" to be plausible only if the term "unspecified" is taken there as meaning "not yet specified", rather than "intrinsically not specifiable". And, once this is admitted, I take it to be appropriate and compatible with his definition of "algebra", provided that the term "magnitude" be replaced with (or understood as synonymous with) the term "quantity". But if it is so, "algebra" can refer to a technique to deal with abstract quantities only in so far as such a technique is understood as being a common technique subject to being used both in arithmetic and in geometry. And, if so, there is room to admit that such a technique could be applied to unspecified quantities – that is, to quantities which were identified neither with numbers nor with geometrical quantities – but not to conceive it as constituting or being part of a mathematical theory separated from arithmetic and geometry. Hence, according to Bos' definitions, as I suggest to understand them, the inclusive

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13 Though Barrow's view on the relations between arithmetic and geometry were quite peculiar in his age, the following passage from his *Lectiones mathematicæ* illustrates a similar view (cf. [Barrow 1683, p. 28]; notice that, as Viète, Barrow is identifying analysis and algebra, at least when mathematics is concerned): "I am wholly silent about that which is called *Algebra* or the *Analytic Art*. I answer, this was not done unadvisedly. Because indeed *Analysis*, understood as intimating something distinct from the Rules and Propositions of *Geometry* and *Arithmetic*, seems to belong no more to *Mathematics* than to *Physics*, *Ethics*, or any other Science. For this is only a Part or Species of *Logic*, or a certain Manner of using Reason in the Solution of Questions, and the Invention or Probation of Conclusions, which is often made use of in all other Sciences. Wherefore it is not a Part or Species of, but rather an Instrument subservient to the Mathematics [...]."

14 Cf. [Panza 2005], specially the Introduction, pp. 1–44.

view concerns algebra as a whole, and not only a part of it: algebra is a system of techniques which belong either to arithmetic or to geometry, or are common to both.

According to me, this is a quite appropriate characterization, and I suggest to adopt it. Still, this is also a quite general characterization and it can be specified in very different ways.

According to Bos, the different techniques which belong to algebra concern the operations of addition, subtraction, multiplication, division, root extraction and solving polynomial equations, and should involve unknowns and/or indeterminates. But this is vague enough, again. There are at least two ways to specify such a characterization so as to satisfy Bos' constraints and conclude that algebra is, after all, a (mathematical) theory, in my sense (though included,[15] of course, in arithmetic, geometry or in their union).

The first way consists in identifying algebra with the system of techniques aiming to transform and solve polynomial equations. Algebra would then be the theory of polynomial equations. In so far as these equations were concerned with numbers, it would be part of arithmetic; in so far as these equations were concerned with geometrical magnitudes, it would be part of geometry; in so far as these equations were taken as such, that is, independently of the nature of the involved quantities, it would be common to both arithmetic and geometry.

The second way consists in identifying algebra with the system of techniques aiming to transform the expressions formed by combining, according to appropriate rules of formation, different symbols referring to quantities and to the operations of addition, subtraction, multiplication, division and root extraction. Algebra would then be the theory of these expressions. In so far as the symbols involved in these equations were understood as symbols for numbers, it would be part of arithmetic; in so far as these terms were understood as symbols for geometrical magnitudes, it would be part of geometry; in so far as these terms were understood as symbols for unspecified quantities, it would be common to both arithmetic and geometry.

When it is understood according to both these characterizations, the notion of arithmetical algebra appears as perfectly clear and can be

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[15] The exact nature of the relation of inclusion between theories should be specified starting from a more precise characterization of the notion of (mathematical) theory. I cannot get into this question here, however. I simply remark that the set-theoretic relation of inclusion between the domains of objects of the involved theories is not appropriate.

fruitfully used in our historical reconstruction of early-modern and pre-modern mathematics without any other specification, I think.[16] Things look different for the notions of geometrical and common algebra. This depends on the fact that, according to both characterizations, these notions rely on a previous identification of the operation of addition, subtraction, multiplication, division and root extraction as defined on geometrical or abstract magnitudes.

Consider the case of geometrical magnitudes. As said, Bos introduces them as constructive procedures (cf. footnote 12) though remarking that they are "analogous" to appropriate operations on numbers. With respect to classical geometry, this is quite correct, of course. But it is essential to remark that what is concerned here is just an analogy, rather than an identity or even an equivalence.

To the well-known (and often repeated) formal differences between arithmetical operations and the analogous operations of classical geometry, I would like to add the following. Whereas any arithmetical operation was capable of being expressed through an equality of the form "$*(a, b) = c$", or "$*(a) = c$" (in the case of root extraction) – where the symbols "a", "b" and "c" denote appropriate numbers and the symbols "$*(a, b)$" and "$*(a)$" denote any operation on a and b or on a alone – this was not the case for any operation of classical geometry. A simple example will be enough to illustrate the point: given two segments a and b, it was certainly possible, in classical geometry, to construct a rectangle $R(a, b)$ having them as sides, but this operational relation was not capable of being expressed through an equality of the form "$*(a, b) = R(a, b)$": no appropriate object capable of being the referent of "$*(a, b)$" in a similar equality was available, indeed. This depends neither on a lack of notation, nor on the constraint of homogeneity. It rather depends on the notion of operation: in arithmetic, operations were understood as applications; in classical geometry, they were understood as constructions, and in many cases, the passage from a construction to the corresponding application was far from being immediate.

The five constructions analogous to the arithmetical operations of addition, subtraction, multiplication, division and root extraction played moreover in classical geometry a role that was quite different from that played

16 The identification of algebra with a part of arithmetic was in fact quite common in early-modern and pre-modern mathematics. As an example, take the complete title of Bombelli's *Algebra*: *L'Algebra* [;] *parte maggiore dell'Aritmetica* [Bombelli 1572] – or the very beginning of Ramus' *Algebra*: "*Algebra* est pars Arithmeticæ" [Ramus 1560, p. 1].

by these operations in arithmetic: the former did not constitute, as such, a base of elementary constructions to which other constructions could be reduced by decomposition.

This should be enough to show that, if characterized in the second of the two previous ways, geometrical algebra appears as a sort of (quite imprecise and undeterminable) duplicate of arithmetical algebra generated through a quite complex process that had its final stage in early-modern times. The situation is similar for common algebra, since it depends on the possibility of identifying the operations of addition, subtraction, multiplication, division and root extraction as such, that is, independently of the nature of quantities they are applied to. This possibility appeared only when Viète defined them, in the *Isagoge*, by means of a sort of axiomatic system, and Descartes defined, in the first book of the *Geometry*, the operations of multiplication, division and root extraction on magnitudes by means of proportions involving an unitary magnitude, in such a way that their formal properties were exactly coincident with the formal properties of the analogous operations on numbers.

Though plausible, the second of the two previous characterizations of common algebra can thus be used – unless algebra were not reduced to arithmetical algebra – only to speak of a scant and already established part of early modern mathematics.

The first of these two characterizations also depends on the possibility of identifying the operations of addition, subtraction, multiplication, division and root extraction, as such. Hence, the same conclusion holds for this characterization of common algebra. The situation could, instead, appear to be different for the first characterization of geometrical algebra, since a geometrical polynomial equation of first, second or third degree should be understood as a statement of a problem. The homogenous equality involving a sum of segments, rectangles or parallelepipeds, which express, respectively, an equation of first, second or third degree, is, indeed, a condition that a segment has to satisfy. The problem consists in looking for the segment or the segments which satisfy such a condition (cf. section 3). And, in so far as such a condition can be easily stated within the limits (and through the language) of classical geometry, such a problem is in turn a problem of classical geometry. Still, a similar problem seems to me, as such, far from being understandable as being an object. Only the equality which expresses its condition, and which is written within a formalism similar to the one involved in the second characterization of algebra, can be so understood. Thus, the considerations concerning the second characterization of geometrical algebra also apply to the first one.

It follows that if the notion of algebra has to be used to reconstruct the origins of early modern mathematics and its genealogical links with an old tradition of mathematical endeavors – as Viète's use of the term "algebra" in the title of his *Opus* invites us to do –, the two previous characterizations (certainly appropriate for other respects [17]) should be replaced by a more comprehensive one. As far as it is suggested by Bos' definitions, the inclusive view also invites us to do that. By connecting algebra with the consideration of "unknowns and/or indeterminates", Bos is drawing, indeed, our attention to the habit, very common in medieval mathematics, to speak of algebra when the consideration of some unknown or the calculation with, or the approximation of, some radicals (cf. footnote 11) were concerned. [18] My aim is to look for such a more comprehensive characterization of algebra and to explain, at the same time, the strict relation between algebra and analysis that is openly evoked in the title of Viète's *Opus*.

2. TWO KINDS OF PROBLEMATIC ANALYSIS

The term "analysis" is also greatly ambiguous with respect to its uses both in science – especially in mathematics – and in philosophy. [19] However, among its very different meanings, it is fairly easy to pick up one which was reasonably constant from ancient geometry and philosophy to early modern mathematics. [20] According to it, analysis is an argumentative pattern, or more generally, a form of reasoning.

Though Aristotle (as well as any other Greek philosopher, so far as I know) never defined it explicitly, he referred to it on several occasions. A comparison of the relevant passages [21] suggests that he understood it

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[17] I have myself adopted in [Panza 2005, p. 21] quite similar characterizations of algebra (which I have of course distinguished among themselves and from other possible ones).

[18] An example of that is the *Distinctio octava* (especially the fourth treatise) of Pacioli's *Summa* [Pacioli 1494].

[19] Cf. [Otte & Panza 1997], Introduction, pp. IX–XI.

[20] [Panza 1997b] is a study of the evolution of the notion of analysis – this term being taken with this meaning – from Aristotle to Descartes. Though my current views are in some regards different than those I argued for in this study, my following considerations are based on the material I discussed there. Among other numerous studies I also base myself on [Mahoney 1968-1969], [Hintikka & Remes 1974], [Knorr 1986], [Timmermans 1995], [Gardies 2001], and [Berggren & Brummelen 2000].

[21] Cf. [Panza 1997b, pp. 370–383]. The passages that I considered there are: *Prior Analytics*, 51*a* 18–19; *Posterior Analytics*, 78*a* 6–8, 84*a* 8, 88*b* 15–20; *Metaphysics*, 1005*b*, 4; *Nicomachean Ethics*, 1112*b* 20–24.

as the typical pattern of arguments which: (i) start from the (hypothetical) assumption that something that is not actually given or established – and that one aims to obtain or establish – is instead given or established; (ii) lead to obtaining or establishing, as a result of an argumentative procedure, something that is independently given or established; and (iii) because of that, either suggest a way of effectively obtaining or establishing what was aimed to be obtained or established, or directly prove that there is no way to obtain or establish it.

The second case mentioned in point (iii) is that of a proof by *reductio ad absurdum.* In the first case, analysis is not enough to obtain or establish what is aimed to be obtained or established. It has thus to be followed by another argument. There is no evidence that Aristotle understood such another argument as complying with a general pattern (that is, there is no evidence that Aristotle possessed a general notion of synthesis, as opposed to his general notion of analysis).

2.1. *Pappusian analysis*

As far as geometrical analysis is concerned, this is the case of Pappus, instead, at least if the well-known opening passage of the 7$^{\text{th}}$ book of his *Mathematical Collection* is taken in the form established by Hultsch's edition (cf. [Pappus 1876-1878, II, pp. 634, 1–636, 14]). In this form, Pappus' passage presents a general characterization of the geometrical method of analysis and synthesis, where the former is depicted in a way that is consistent with Aristotle's conception and is opposed to synthesis, understood as another pattern of geometrical argumentation.

In an equally well-known paper which appeared at the end of the 1960s, M.S. Mahoney [1968-1969, pp. 321–326] argued that this passage is corrupted, and that in its original form it contained no general characterization of synthesis. Mahoney's argument is that in Hultsch's text, analysis is characterized in two "incompatible" ways: first by identifying it with a "path from what one is seeking, as if it were established, by way of its consequences, to something that is established by synthesis"; and secondly by asserting that "in analysis we assume what is sought as it has been achieved, and look for the thing from which it follows, and again what comes before that, until by regressing in this way we come upon some one of the things that are already known, or that occupy the rank of a first principle". [22] According to Mahoney, the second characterization was added by "same later editor" that, "seeing the single occurrence of the word 'synthesis' [...] may have felt the need to add a definition of it, along with a definition of analysis as he understood it" [Mahoney 1968-1969, p. 325]. Thus, also the supposed characterization of synthesis from Pappus would result from an interpolation. Here it is [Pappus 1986, p. 82]:

> "in synthesis, by reversal, we assume what was obtained last in the analysis to have been achieved already, and, setting now in natural order, as precedents, what before were following, and fitting them to each other, we attain the end of the construction of what was sought".

As a matter of fact, whereas the first characterization of analysis seems to depict it as a deduction, the second seems to depict it as the inverse of a deduction, or even – in Mahoney's interpretation – as a conjectural inverse of a deduction. [23] From a logical point of view, this is certainly a relevant difference, so relevant that Mahoney thinks that Pappus could not have presented these two characterizations together. [24]

Nevertheless, Aristotle's notion of analysis – at least as I have reconstructed it above – is compatible with both characterizations. Moreover, the second one seems to be conceived so as to respond to an intrinsic difficulty which appears some lines later in Pappus' text, in a part of this

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22 Here and later, I quote from Jones' translation appeared in 1986: cf. [Pappus 1986, p. 82]. Mahoney refers instead to a personal translation based on Heath's and Thomas' ones: cf. [Heath 1921, II, pp. 400–401] and [Thomas 1957, pp. 597–599].

23 Mahoney [1968-1969, p. 323] expresses such a characterization with the schema

$$P \overset{?}{\leftarrow} P_1 \overset{?}{\leftarrow} P_2 \overset{?}{\leftarrow} \cdots \overset{?}{\leftarrow} P_n \overset{?}{\leftarrow} K,$$

where "K" represents what is "already known" and "P" what is "sought".

24 The second characterization fits quite well with Cornford's [1932, p. 43] definition of analysis as an "upward movement of intuition" looking for "preconditions". Recently, Fournarakis and Christianidis [2006, pp. 45–47] have proposed a "new interpretation of geometrical analysis" according to which it would be composed by two successive parts: an "hypothetical" one, consisting in a "searching from preconditions", and a "confirmatory" one consisting of a "deduction" and aiming to warrant that the hypothetical argument, "if reversed, constitute[s] a deduction". A similar pattern seems however to be satisfied by quite a few arguments among those that are habitually understood as examples of analysis. One of them (the solution of the proposition 105 of the 7th book of Pappus' *Collection*) is reconstructed and discussed in Fournarakis and Christianidis' paper. For a more general model of analysis and synthesis including four stages – two for analysis, transformation and resolution, and two for synthesis, construction and demonstration – (and based on Hankel's [1874] classical model), cf. [Berggren & Brummelen 2000, pp. 11–13].

text that there is no reason to consider as being interpolated, where Pappus distinguishes between the case in which analysis is applied to prove theorems and the case in which it is applied to solve problems. Here is what Pappus writes [Pappus 1986, pp. 82–84]:

> "In the case of the theorematic kind [of analysis], we assume what is sought as a fact and true, then, advancing through its consequences, as if they are true facts according to the hypothesis, to something established, if this thing that has been established is a truth, then that which was sought will also be true, and its proof the reverse of the analysis; but if we should meet with something established to be false, then the thing that was sought too will be false. In the case of the problematic kind, we assume the proposition as something we know, then, proceeding through its consequences, as if true, to something established, if the established thing is possible and obtainable, which is what mathematicians call "given", the required will also be possible, and again the proof will be the reverse of the analysis; but should we meet with something established to be impossible, then the problem too will be impossible."

As Mahoney remarks, Pappus uses the term "proof" twice where a reader of Hultsch's text would have expected that he used the term "synthesis". [25] This, however, provides no difficulty. The difficulty lies rather in the fact that Pappus says in both cases that the proof, or synthesis, is the "reverse of the analysis", which could be the case only if analysis were a deduction composed of nothing but (logically) reversible implications. The second of the two previous characterizations of analysis seems just have been conceived to answer this difficulty: analysis would be reversible, in so far as it were not a deduction but rather the inverse of a deduction, or an abduction as Peirce would have said.

According to Mahoney [1968-1969, pp. 326–327], the difficulty is solved, instead, by observing that in mathematics "the usual state of affairs" is reversibility of deduction, since "mathematics operates primarily with biconditional connectors" and this is the situation of Greek mathematical analysis. But in this way, a logical difficulty is solved by appealing to a contingent feature of mathematical arguments: a usual, but not necessary one.

Rather than to appeal to the usual features of mathematical arguments in order to discard a logical possibility (after having discarded a characterization of analysis based on logical considerations), is it not better to admit that Pappus understood his double characterization of analysis and synthesis as vague enough to fit with different logical possibilities which different

[25] Cf. [Mahoney 1968-1969, p. 325]. This is another argument in favor of Mahoney's thesis.

arguments could comply with? Moreover, if the problem is that of reconstructing a general notion of mathematical analysis that was operating in early-modern mathematics and could be brought back to Pappus, is it not more appropriate to accept the apparent incoherence of Pappus' text, in Hultsch's form – which is also the form under which this text appeared in Commandinus' edition of the *Collection* [26] – , and try to extract from it a characterization of the geometrical method of analysis and synthesis that fits with early-modern applications of it? [27]

It seems to me that there is room for doing that, provided that the question of the direction of the inferences occurring in analysis and synthesis, consisting in wondering whether they should be deductive or abductive inferences, is distinguished from the question of the structural direction of analysis and synthesis, consisting in wondering which should be the logical nature of their initial and final points. The problem of reversibility of analysis is concerned only with the former, but possibly this is not the crucial question which Pappus' characterization of analysis and synthesis is concerned with. [28] Possibly, the crucial question is rather the latter. This is also suggested by Aristotle's characterization of analysis, since it depends on the structural direction of the relevant argument rather than on the logical nature of the inferences which occur in it.

Let us consider this question in more detail, in the light of Pappus' distinction between problematic and theorematic analysis. In classical geometry, what is properly sought in the solution of a problem is an object satisfying a certain condition, and this is considered as being found when an appropriate construction of it is tracked down. Thus, problematic analysis should start with (the supposition that) such an object (is given) and

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26 Cf. [Pappus 1588, p. 157r]: "*Resolutio igitur est via a quæsito tanquam concesso per ea, quæ deinceps consequuntur ad aliquod concessum in compositione: in resolutione enim id quod quęritur tamquam factū ponentes, quid ex hoc contingat, consideramus: & rursum illius antecedens, quousque ita progredientes incidamus in aliquod iam cognitum, vel quod sit è numero principiorum.*"

27 This seems also to be Bos' proposal in chapter 5 of his [Bos 2001].

28 This is also the opinion of Hintikka and Remes for whom "this aspect of the concepts of analysis and synthesis is one of the more superficial ones" [1974, p. 11]. Despite that, they have proposed [*ibid*, chap. II, pp. 7–21] a simple way to reconcile the two characterizations of analysis occurring in Pappus' text under Hultsch's form. It simply consists in translating the term "ἀκόλουθον" which occurs in the first characterization, as well as in the successive description of theorematic and problematic analysis, with "concomitants", rather than with "consequences" as it is usually done. In this way, the first characterization of analysis becomes quite vague with respect to the logical nature of the inferences occurring in it, to the effect that the second takes a prominent role.

proceed to (the statement that) other objects, that are actually given or established to be impossible (are given). What is instead sought in the proof of a theorem is the truth or falsehood of a certain statement, and this is considered as being established when either this statement or its negation are deduced from other statements the truth of which is already established. Thus, theorematic analysis should start with (the supposition of the truth of) the first statement and proceed to (the statement of the truth of) other statements the truth-value of which is known. If so, in both the problematic and the theorematic case, analysis and synthesis have inverse structures: in both cases, the starting point of analysis is (the supposition that) something that is not actually given or established (is instead given or established), whereas its final point is (the statement that) something that is actually so (is so); also in both cases, the starting point of synthesis is the final point of analysis, whereas its final point is the starting point of analysis.

I guess that when Pappus argues that synthesis, or better "proof", is the "reverse of the analysis", he refers to their structures, rather than to the logical nature of the inferences that occur in them, which – he seems to admit – might vary from case to case. [29] This is the same as suggesting that Pappus' characterization of the method of analysis and synthesis consists in a structural description of the two subsequent stages of this method. Let us call, for short, this characterization, so understood, "Pappus' structural description".

The question arises whether Pappus' structural description is appropriate. To answer this question, I shall consider three examples, one for the theorematic case, and two for the problematic case. All these three examples concern arguments where the method of analysis and synthesis is openly used. For the theorematic case, there are very few cases in the pre-modern and early-modern mathematical *corpus* where this is so [30];

29 This pattern is consistent with Berggren and Van Brummelen's following remarks: "One must, on the evidence, accept the fact that both reduction (a search for preconditions) and deduction (a search for consequences) were regarded by ancient writers as being activities included under the rubric of 'analysis' " [Berggren & Brummelen 2000, p. 9].

30 Cf. [Pappus 1986, p. 67]. According to Knorr, the notion of theorematic analysis is "gratuitous", and "to the extent that [...] [Pappus] is viewing the expository form of the method, rather than its heuristic role, he seems not to sense that [...] [theorematic] analyses are entirely redundant" [*ibid.*, p. 358] and note 33. In so far as my paper is essentially concerned with problematic analysis, I shall not discuss this question here. I remark however that the *Elements* present several instances of analysis used to prove theorems but complying with the problematic pattern. They concern the (constructive) proofs of non-constructibility (or non-existence) of certain mathematical

I have chosen that which is most frequently discussed.[31] For the problematic case, the situation is similar for the Greek mathematical *corpus*, since, as it is well known, Greek mathematicians used to hide their analysis. The example I have chosen is drawn from one of the few treatises in this *corpus* – the *Cutting-off of a Ratio*, by Apollonius[32] – where the method of analysis and synthesis is openly used. The third example concerns early-modern mathematics. I have chosen it because it is one of those which Bos presents in his book, where a general characterization of Pappusian analysis and synthesis – which I shall discuss later – is also presented.

2.1.1. *An example of theorematic analysis*

The first example concerns the proof of proposition XIII.1 of the *Elements* which is contained in an interpolation to Euclid's text, possibly due to Heron, and probably composed sometime between the first and the third century of the Christian era, or perhaps even after Pappus composed his *Mathematical Collection*.[33] Such an interpolation contains alternative proofs for the first five propositions of book XIII, all of them applying the method of analysis and synthesis.[34] The proposition XIII.1 states that if a

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objects which are conducted by *reductio ad absurdum*. Two examples are propositions I.7 and I.27: an object (namely a point) is supposed to be given (that is, a problem is supposed to have been solved), and the negation of an already proven theorem is deduced. In these cases, analysis is, so to say, mixed: partly problematic and partly theorematic.

31 For example by [Mahoney 1968-1969, pp. 326–327].

32 Apollonius' text has been transmitted in an Arabic translation (the Arabic text has never been printed; for a recent English translation, see [Apollonius 1988]) and was translated into Latin by Halley [Apollonius 1706].

33 This interpolation is edited in [Euclid 1969-1977, IV, pp. 198–204]. On its attribution to Heron, cf. [Heiberg 1903, p. 58], and [Euclid 1926, III, p. 442]. On Heron's dates and the date of composition of such an interpolation, cf. [Neugebauer 1938], [Heath 1921, II, pp. 298–306], and [Knorr 1986, p. 355], which, without adopting a definite position regarding the attribution of the interpolation, simply remarks that "an actual dependence of the scholiast on Pappus is entirely possible".

34 These proofs are preceded by a general characterization of the method of analysis and synthesis. Here is Heath's translation [Euclid 1926, III, p. 442]: "Analysis is the assumption of that which is sought as if it were admitted ⟨and the arrival⟩ by means of its consequences at something admitted to be true. Synthesis is an assumption of that which is admitted ⟨and the arrival⟩ by means of its consequences at the end or attainment of what is sought." In another version one finds, instead of the words "the end or attainment of what is sought", the same words, "something admitted to be true", occurring in the characterization of analysis, but this seems to be due to a corruption of the text. According to Mahoney [1968-1969, p. 321], this is a "probably pre-Euclidean" characterization that the scholiast would have simply reported. The attribution to Heron of such a characterization and of the following alternative proofs for the first five propositions of book XIII (cf. footnote 33) depends on their alleged

segment a is cut in such a way that its parts α and β satisfy the proportion $a : \alpha = \alpha : \beta$ and b is the half of it, then the square constructed on the conjunction γ of b and α is five times the square constructed on b. Suppose that for any pair of segments α and β, $Q(\alpha)$ and $R(\alpha, \beta)$ are respectively the square constructed on α and the rectangle constructed on α and β.[35] Thus the analysis goes as follows:

Let's suppose that:	$Q(\gamma) = 5Q(b)$.	$A.1$
For $\gamma = b + \alpha$ and *El.*, II.4:	$Q(\alpha) + 2R(b, \alpha) = Q(\gamma) - Q(b)$.	$A.2$
For $A.1$–$A.2$:	$Q(\alpha) + 2R(b, \alpha) = 4Q(b)$.	$A.3$
For $a = 2b$ and *El.*, II.1:	$2R(b, \alpha) = R(a, \alpha)$.	$A.4$
For $a : \alpha = \alpha : \beta$ and *El.*, VI.17:	$Q(\alpha) = R(a, \beta)$.	$A.5$
For $A.3$–$A.5$:	$R(a, \beta) + R(a, \alpha) = 4Q(b)$.	$A.6$
For $\alpha + \beta = a$ and *El.*, II.2:	$R(a, \beta) + R(a, \alpha) = Q(a)$.	$A.7$
For $A.6$–$A.7$:	$Q(a) = 4Q(b)$.	$A.8$

Equality $A.8$ ends the analysis, since it follows from condition $a = 2b$ alone, according to *El.*II.4 or *El.*II.2 and *El.*II.3, and is thus already established.

similarity with Heron's characterization of analysis and synthesis and his considerations and alternative proofs of the propositions 2-10 contained in his commentary to the propositions 1–14 of the book II of Euclid's *Elements*, as reported by Nayrīzī. Here is Gherard of Cremona's Latin translation of Heron's characterization of analysis and synthesis: "Dissolutio *autem est, cum qualibet questione proposita dicimus: ponamus illud in ordine rei quesite que est inventa, deinde reducemus ad rem cuius probatio iam precessit. Cum ergo manifestum est, dicimus quod iam inventa est res quesita secundum dissolutionem.* Compositio *vero est ut incipiamus a re nota, deinde componamus donec res quesita inveniatur; ergo tunc res quesita iam erit manifesta secundum compositionem*" ([Anaritius 1994, p. 74], and also [Euclid 1883-1889, Suppl., p. 89]). In fact, this is a characterization quite different from the scholiast's one. Moreover Heron's alternative proofs of propositions II.2–II.10 are far from being similar to the scholiast's alternative proofs of the propositions of book XIII.1–XIII.5: in both cases, Euclid's diagrams are replaced by more simple ones consisting in nothing but a segment on which an appropriate number of points is taken, but whereas in the latter ones no reference to rectangles is actually needed and no other diagrams have to be imagined (cf. p. 120), in the former ones, it is asked to consider other appropriate segments together with the "surfaces which are contained by" them ([Anaritius 1994, p. 74], and also [Euclid 1883-1889, Suppl., p. 90]). Thus, when Heron remarks that proposition 11 (which, in contrast to the prior ones, is a problem) "cannot be proven without diagram" ([Anaritius 1994, p. 86], and also [Euclid 1883-1889, Suppl., p. 106]), he seems to mean that the relevant diagrams have to be drawn rather than simply supposed.

35 In fact, in the original text the segments are indicated by two letters referring to a pair of points on a diagram and no particular symbol is used for denoting rectangles. As both analysis and synthesis are independent of this diagram, and as their features on which that I want to focus do not bear on what notation is used, I do not see any detriment in adopting a more expeditious notation. I shall come back to the role of the diagram in this argument in section 2.2.3.

The synthesis, that is the proof of equality $A.1$, under the conditions $\alpha+\beta = a = 2b$, $a : \alpha = \alpha : \beta$, and $\gamma = b + \alpha$, is simply the inversion of it:

For $a = 2b$ and *El.*, II.4:	$Q(a) = 4Q(b)$.	$S.1$
For $\alpha + \beta = a$ and *El.*, II.2:	$R(a, \beta) + R(a, \alpha) = Q(a)$.	$S.2$
For $S.1$–$S.2$:	$R(a, \beta) + R(a, \alpha) = 4Q(b)$.	$S.3$
For $a : \alpha = \alpha : \beta$ and *El.*, VI.17:	$R(a, \beta) = Q(\alpha)$.	$S.4$
For $a = 2b$ and *El.*, II.1:	$R(a, \alpha) = 2R(b, \alpha)$.	$S.5$
For $S.3$–$S.5$:	$Q(\alpha) + 2R(b, \alpha) = 4Q(b)$.	$S.6$
For $\gamma = b + \alpha$ and *El.*, II.4:	$Q(\alpha) + 2R(b, \alpha) = Q(\gamma) - Q(b)$.	$S.7$
For $S.6$–$S.7$:	$Q(\gamma) = 5Q(b)$.	$S.8$

I shall come back to this argument in section 2.2.3. For now, I limit myself to remark that it perfectly complies with Pappus' structural description and is such that analysis and synthesis are the perfect inverse of each other.

2.1.2. *Two examples of problematic analysis*

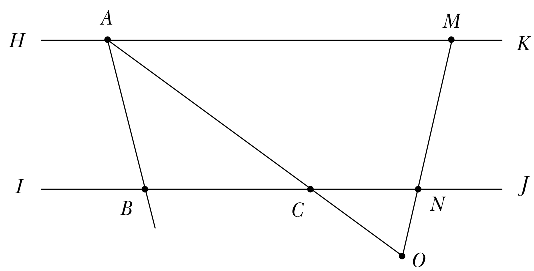

FIGURE 1.

This is not the case in a number of other arguments that one may want to consider as genuine examples of the method described by Pappus, in the problematic case, and that, as a matter of fact, have been so understood or even so openly presented. Namely, it is not the case in the two following arguments.

The first is that which Apollonius relies on to solve the first case of the first "*locus*" in the first book of the *Cutting-off of a Ratio* [Apollonius 1706, pp. 1–3]. The problem consists in looking for a straight line through a given point O (Fig. 1), cutting two other parallel straight lines HK and IJ given in position in two points M and N such that the segments AM and BN, taken on these lines from two given points A and B, are to each other in a given ratio, under the condition that the point O is inside the angle formed by the straight lines AB and BN and the straight line OM is supposed to cut the given straight lines respectively between A and K and between B and J.

Apollonius supposes the problem solved, traces the straight line AO cutting BN in C, and remarks that $AM : CN = AO : CO$, so that the ratio between AM and CN is given. But, since the ratio between AM and BN is given as a condition of the problem, this means that the ratio between BN and CN is also given by composition, and, from here, the ratio between BC and CN is given by subtraction, so that the point N is given. This ends the analysis. In the synthesis, Apollonius then constructs the point N by constructing the fourth proportional CN between the given segment BC and two other segments constructed according to what the analysis suggests.

As a matter of fact, the analysis ends by stating that the point N is given, though this point is actually the one sought. This seems to contrast with Pappus' structural description. Still, Apollonius' argument starts by stating a proportion involving two segments – AM and CN – which are given if and only if the point N is given, and consists in comparing this proportion with other ones – also involving some segments which are given if and only if the point N is given – so as to get a proportion involving only a segment such as these, together with three segments which are actually given. Hence, it is quite easy to reformulate it in such a way that it satisfies Pappus' structural description. Analysis would then go as follows:

Let's suppose that the point N is given.	A.1
Then, as the points B and C are given, the ratio between BC and CN is given.	A.2
And thus also the ratio between BN and CN is given by addition.	A.3
But the ratio between AM and BN is given, thus the ratio between AM and CN is given by composition.	A.4
Thus the ratio between AO and CO is given.	A.5

Step A.5 would end the analysis, since the segments AO and CO, and thus their ratio, are actually given, that is, they are given independently of the supposition that the point N is given. Thus, analysis would start with (the supposition that) the point N (is given) and would proceed to the (statement that the) ratio between the segments AO and CO (is actually given), according to Pappus' structural description. Hence, the synthesis would start with the (statement that the) ratio between the segments AO and CO (is given), then proceed to (the determination of) the ratio between the segments BN and CN, and continue up to (the determination of) the point N. Let the proportion $AM : BN = a : b$ (where a and b are two given segments) be a condition of the problem. Synthesis would then go as follows:

Construct c such that $AO : CO = a : c$ (BN and CN should thus be such that $BN : CN = b : c$).	S.1
Construct the difference $b - c$.	S.2
Construct N such that $BC : CN = b - c : c$.	S.3

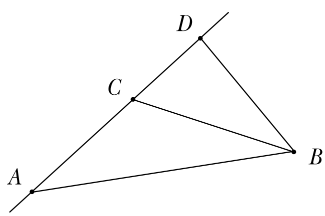

FIGURE 2.

Similar considerations apply to the example Bos advanced to support the thesis that in "the actual practice of analysis as we find it in classical and early modern sources [...] [the] direction of argument was not so definite", although such an analysis always "started with the assumption of the required [Bos 2001, p. 102]. This concerns Ghetaldi's solution of the following problem ([Ghetaldi 1607, pp. 41–42] and [Bos 2001, pp. 98–100]): two segments a and b and an angle φ being given, construct on a a triangle of which the other sides, x and y, are such that $y = b + x$ and the angle opposite to y is equal to φ. Bos' reconstruction of Ghetaldi's analysis ends by stating that the sought triangle is given and that the direction of the analysis is "exactly the same" [Bos 2001, p. 102] as that of the synthesis. Once again it is quite easy to reformulate Ghetaldi's arguments in such a way that it complies with Pappus' structural description. Analysis would go as follows:

Let's suppose that the triangle ABC (Fig. 2) constructed on the segment $BC = a$, and such that $AB = y$, $AC = x$, and $\widehat{ACB} = \varphi$, is given.	$A.1$
Then the straight line AC is given, and so a point D on it, such that $AD = AB = y$.	$A.2$
It follows that the triangle BDC is given.	$A.3$
Thus, the sides BC and CD and the angle $\widehat{BCD}$ of this triangle are given.	$A.4$

Step $A.4$ would end the analysis, since the sides $BC = a$ and $CD = b$ and the angle $\widehat{BCD} = \pi - \varphi$ are actually given. As the triangle BDC is given in its turn if its sides BC and CD and its angle $\widehat{BCD}$ are given, it would then be easy to start the synthesis with the (statement that the) segments BC and CD and the angle $\widehat{BCD}$ (are given), then proceed to (the construction of) the triangle BDC, and then continue up to (the determination of) the triangle ABC. It would thus go as follows:

Construct the triangle BDC on the segments a and b taken so as to form an angle complementary to the angle φ.	$S.1$
Construct at B, on the straight line DB, an angle equal to the angle $\widehat{CDB}$.	$S.2$
Produce the side of this angle other than DB, so as to cut the straight line CD in A.	$S.3$

Though this synthesis is not the inversion of the analysis step by step, it is clearly suggested by the analysis, and has not the same direction of the latter, as it is, instead, the case in Bos' formulation of Ghetaldi's argument.

2.1.3. *Intra-configurational (problematic) analysis*

Apollonius' and Ghetaldi's arguments should doubtlessy be understood as genuine examples of the method of problematic analysis and synthesis. In so far as they do not comply, as such, with Pappus' structural description, they seem to show that this description is not appropriate for characterizing this method. Still, the fact that these arguments can be reformulated in such a way that they comply with this description is not irrelevant. It suggests that there is room for detecting a common pattern, which both these arguments and their previous reformulations are complying with, and which is just that of Pappusian problematic analysis and synthesis. One could then argue that Pappus' structural description should not be taken by the letter, but rather understood as a fairly vague way to evoke such a pattern. This is precisely what I suggest.

This is obviously a twofold pattern. First comes a stage – the analysis – which consists in reasoning on a configuration of given and ungiven [36] geometrical objects and data so as those that are ungiven were given, and leads to isolate in it a sub-configuration that only includes the objects and data that are given though determining the whole configuration. This first stage aims to suggest the way to perform the second – the synthesis – which consists in constructing the objects which are sought based on the sub-configuration of given objects and data isolated in the analysis. Such a general pattern can be better detailed. Once a certain geometrical problem is supposed to be solved, its hypothetical solution displays an hypothetical configuration of geometrical objects and data, some of which are given according to the conditions of the problem, while others are not actually given, being rather required in order for the problem to be solved. This configuration is represented by a diagram together with appropriate information concerned with its elements (like the value of a ratio, the "area" of a polygon, or a perimeter, that is, what I term "data" [37]). Analysis is based on such a diagram and this information. Eventually, it includes an auxiliary construction that extends the diagram conservatively. This is a construction permitted under the supposition that all the objects entering such a configuration and the relative information are given. [38] The aim is to isolate in such a configuration, or in the extended one, a sub-configuration which only consists of given objects (and is thus actual rather than hypothetical) and which, together with the information that is available, determines (by construction) the entire configuration

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[36] The adjective "ungiven" means of course here "not yet given" or "actually not given".

[37] The question concerning the nature of these data is one of the more difficult questions about the interpretation of classical geometrical texts: is the value of a ratio nothing but a pair of segments (or numbers) and the value of the "area" of a polygon or a perimeter nothing but a square or a segment, respectively, or are they something essentially different (and, so to say, more abstract)? I cannot but leave this question open, here. I simply remark that if the former answer is espoused, than data are nothing but geometrical objects, in turn.

[38] Hintikka and Remes [1974, especially chap. V, pp. 41–48] have strongly emphasized the possibility (and in certain cases the necessity) of such a construction. Two examples of it are the drawing of the straight line *AO* (Fig. 1) and of the segment *DB* (Fig. 2) in Apollonius' and Ghetaldi's arguments, respectively.

which would supply the solution of the problem. Synthesis operates on the sub-diagram isolated in analysis. Starting from such a sub-diagram, it constructs the objects that are sought [39].

According to such a pattern, analysis is thoroughly internal to the configuration – represented by an appropriate diagram – which corresponds to the hypothetical solution of the problem, or at least to a (conservative) extension of it. This is why I suggest to term the analysis which complies with this pattern "intra-configurational analysis" (taking of course for granted that it is problematic).

My characterization of intra-configurational analysis fits well enough with the "two characteristic features of a classical analysis of problems" identified by Bos: the fact that "it proceeded by means of a concept 'given', and [that] it was performed with respect to a figure in which the required elements were supposed to be drawn already." [40] I add that the final aim of intra-configurational analysis is to isolate, in the configuration represented by this figure (but I prefer to use the term "diagram"), a sub-configuration that only consists of given objects and data and which determines (by construction) the entire configuration which would supply the solution of

39 This pattern fits well enough with the model suggested by Behboud, which was intended to account for Pappus' own use of the method of analysis and synthesis in the 7th book of his *Collection* (occurring in 13 problems and about ten theorems of this book [Behboud 1994, p. 57]). Here is as Behboud [*ibid.*, p. 70] describes this model (only for the case of problematic analysis): "Suppose that it is required to [[...]] solve $\forall \bar{x}[\varphi(\bar{x}) \Rightarrow \exists \bar{y}\chi(\bar{x},\bar{y})]$ (that is, in his notation: "given φ's, find (or determine) χ's", where "the barred variables abbreviates tuples of variables ranging over geometrical objects [...] and φ [...] and χ stands for (complex) relations which may be obtained between these objects" [*ibid.*, p. 58]). Then [...] one starts with an appropriate collection of geometrical objects $\bar{a}$ instantiating $\varphi(\bar{x})$ and one assumes [...] that objects $\bar{b}$ satisfying $\chi(\bar{a},\bar{b})$ are already given. Moreover, the accompanying diagram may be supplemented by suitable *auxiliary* objects constructed from $\bar{a}$ or $\bar{a}$ together with $\bar{b}$. In the *transformation* various properties of the figure are derived using only a restricted set of reversible rules [notice that this constraint is not included in my pattern] and observing the uniqueness condition for the *zêtoumenon* [that which is sought], until one comes 'upon some one of the things [...]' [...] that are given. [...] The *resolution* proceeds to show that the thing known holds independently of the *zêtoumenon*, or that all the auxiliary objects are given on the basis of $\bar{a}$ alone and that they together with $\bar{a}$ determine $\bar{b}$. The synthesis [...] starts by turning the information obtained in the resolution into an 'explicit' *construction*. The *demonstration* part [...] essentially reverses the A-tree [that is, the system of inferences composing the analysis] with respect to the *zêtoumenon*, which may be supplemented by additional steps that show how to derive the root of the A-tree independently from the *zêtoumenon*."

40 Cf. [Bos 2001, p. 100]. I understand that Bos is speaking here of a particular version (namely the Euclidean one) of the concept "(to be) given", rather than of a given concept, since what are properly given in classical geometrical arguments are objects rather than concepts.

the problem. I also emphasize that intra-configurational analysis extends, if need be, the hypothetical configuration which is displayed once the problem is supposed to be solved, but leaves it otherwise unaltered. [41]

2.2. *Another kind of problematic analysis*

The previous pattern is respected by a large number of arguments that have been understood as particular instances of the method of problematic analysis and synthesis and that form a very relevant *corpus* within both classical and early-modern mathematics. Still, early-modern mathematics presents another relevant *corpus* of arguments that have also been understood as examples of the method of problematic analysis and synthesis, and whose first part complies, in fact, with conditions (i) and (iii) included in Aristotle's characterisation of analysis (cf. p. 100). This invites us to contrast intra-configurational analysis with another kind of problematic analysis.

There is a simple way to do it: one could remark that, in the *Isagoge*, Viète promoted a substantial reform of the method of problematic analysis and synthesis, and argue that such another kind of problematic analysis is nothing but the one that Viète described in the *Isagoge*, then applied, for example, in his *Zeteticorum Libri*. This would lead to identifying non-Pappusian problematic analysis [42] with problematic analysis *à la* Viète. This strategy presents two difficulties, however. The first depends on the fact that Viète's pattern of problematic analysis is quite particular and does not fit, at least in its details, with many arguments produced by other early-modern mathematicians and that it would be plausible to understand as examples of a non-Pappusian kind of analysis (it should be enough to think of Descartes' geometry in order to be convinced of that). The second is more directly concerned with the subject matter of my paper. Viète's reform not only might be conceived as the starting point of a mathematical

41 This is also not far from what Hintikka and Remes say: "The most important aspects [of analysis (in classical or Pappusian sense)] seem to be (i) the idea of studying the interrelation of geometrical objects in a *given* configuration and (ii) the general heuristic idea of bringing the maximal information to bear on this configuration [...]" [Hintikka & Remes 1974, p. 38].

42 Notice that with the term "Pappusian problematic analysis", I do not mean to refer to the common form of all examples of problematic analysis that should have been contained in the mathematical *corpus* selected by Pappus, the so-called Treasury (or Domain) of Analysis (cf. [Euclid 1926, I, p. 138], [Hintikka & Remes 1974, p. 8], [Pappus 1986, p. 82]), but rather to the form of problematic analysis that I have described in section 2.1.

program, but it should also be understood in the light of an older mathematical tradition from which it results. This is what Viète himself invites us to do (cf. pp. 86–90). By identifying non-Pappusian problematic analysis with problematic analysis *à la* Viète, we would *ipso facto* prevent ourselves from the possibility of suggesting that the kernel of this tradition is a non-Pappusian form of analysis. This, however, is what I would like to suggest. Hence, I cannot be satisfied with such an identification.

2.2.1. *Bos' pattern of algebraic method of problematic analysis and synthesis*

A fortiori, I cannot be satisfied with the identification of non-Pappusian analysis with algebraic analysis, if the latter is identified, in turn, with the typical kind of analysis promoted by Viète. This is Bos' suggestion:

> "Algebra entered geometry through its use in the analysis of problems and from c. 1590 the development of this analytical use of algebra can be identified as the principal dynamics [...] within the early modern tradition of geometrical problem solving. From 1591 onward Viète consciously and explicitly advocated the use of algebra as an alternative method of analysis, applicable in geometry as well as in arithmetic. [[...]] This use of algebra in geometry had been pioneered by some Renaissance mathematicians before 1590, but it was Viète's conscious identification of this method with analysis that brought it into the centre of attention." [Bos 2001, pp. 97–98]

Bos is clearly referring here to problematic analysis. According to his definition of "algebra", to say that the analysis which enters the solution of a problem is algebraic is the same as saying that it employes the operations of addition, subtraction, multiplication, division, root extraction and solving polynomial equations as defined on the relevant magnitudes. This depends on the reduction of the problem to a system of equations. Once this reduction is fulfilled, the formalism connected with these operations – that is, in Viète's case, that which is introduced in the *Isagoge* – works by itself, or better: independently of the specific nature of the problem these equations come from. This is of course the crucial point. Bos emphasizes this point by refining his characterization of "algebraic analysis" and including it in a more general pattern that we could term for short "Bos' pattern of algebraic method of problematic analysis and synthesis":

> "The whole procedure consisted of three or four distinct parts:
>
> A) The derivation of the algebraic equation from the problem.
>
> A′) If possible, the algebraic solution of the equations, that is, finding an explicit algebraic expression for the unknown. Otherwise, if necessary a rewriting of the equation into some standard form.

> B) The construction of the problem based on the expression found in A′, or, if such an expression was lacking or uninformative, on the basis of the equation found in A (if necessary reduced to some standard form).
>
> C) The proof that that construction was correct.
>
> Items A) and A′) constituted the analysis. [...] Part B), the construction, started from the given elements and operated on them in the manner indicated by the explicit expression in A′) or by the structure of the coefficients in the equation found in A) [...]. Thus the direction of the synthesis was opposite to that of the analysis, but it did not retrace the steps of the analysis. The construction used the equation or its solution, but not the way it was derived in the analysis." [Bos 2001, p. 105]

This is a faithful and accurate description of the way in which Viète's formalism (or an equivalent one) operated in problematic analysis and synthesis as they occur in a large *corpus* of early modern mathematical texts. I wonder however: is the structure of an argument that is supposed to comply with Bos' pattern the effect or the precondition of the introduction of this formalism? Bos' answer clearly points to the first option. Instead, I propose the second answer.

2.2.2. *A generalization of Bos' pattern.*

More particularly, I propose to appeal to an independent characterization of such a structure – that is, to describe a pattern of a method of problematic analysis and synthesis different from Pappus' one – and to define algebra afterwards as a mathematical technique – or better, a system of mathematical techniques, *i.e.* an art – used to solve a certain class of problems through the application of such a method, and particularly of a non-Pappusian (that is non-intra-configurational) kind of analysis. In other words, I propose to reverse the direction of Bos' characterization, not from algebra in his sense to a non-classical kind of analysis, but from the latter to algebra in my sense.

Bos' pattern of algebraic method of problematic analysis and synthesis matches a number of arguments that differ each from one another according to their particular nature.

To describe what is common to these arguments, let me introduce a convenient terminology. I suggest to term "purely quantitative" those problems and conditions that depend only on the relative size of some homogeneous quantities. This is the case of every arithmetical problem as well as every problem concerned with abstract quantities, since numbers and abstract quantities are characterized, with respect to each other, only on their positions in the numerical order and on their mutual operational relations, respectively. But this is not the case of every geometrical problem, since geometrical magnitudes are often characterized their respective positions in space.

This being established, let us wonder under which conditions a problem could be solved through an argument that complies with Bos' pattern. A necessary condition is that this problem could be reduced to a system of "algebraic equations". I suppose that by speaking of algebraic equations, Bos is referring to polynomial equations. Thus the question is: which problems could be reduced to a system of polynomial equations?

The question is relevant only for geometrical problems, since, for arithmetical problems or problems concerned with abstract quantities, the answer is merely that this is the case for any problem concerned with addition, subtraction, multiplication, division and root extraction, that is, for any problem that one could enunciate relying on Viète's formalism or, in the case of arithmetical problems, on any analogous system of standard codified procedures to be applied to determinate and indeterminate numbers. [43]

In conformity with the formalism of the *Isagoge*, a necessary condition for a geometrical problem to be subject to being reduced to a system of polynomial equations is that its conditions could be expressed either in the form of proportions involving four segments, two segments and two rectangles, or two homogeneous magnitudes of any sort and two numbers, or in the form of equalities between two sums of magnitudes (which, in order to be added to each other, have to be mutually homogeneous). [44] It is easy to understand that conditions that are so expressed are purely quantitative. It follows that among geometrical problems only purely quantitative ones could be solved through an argument that complies with Bos' pattern, using Viète's formalisms.

From a very general point of view, these problems can be distributed in two classes: problems which are directly formulated so as to depend on nothing but purely quantitative conditions; problems which are firstly formulated so as to depend on some non purely quantitative, or positional, conditions.

Among the former, there are classical problems like the problem of constructing two mean proportionals between two given segments, problems

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43 An obvious example is the system of procedures at work in Diophantus' arithmetic, the explicit model of Viète's *zetetica*. Cf. [Diophantus 1575, I], [Diophantus 1893-1895], [Freguglia 1999, pp. 116–133], [Freguglia 2005], and [Freguglia forthcoming].

44 These possibilities are of course not exclusive, since the formalism of the *Isagoge* allows the transformation of proportions into equalities.

of application of areas, problems of partition of a given segment into two parts satisfying some conditions relative to the quantitative relations of two appropriate rectangles, or, more generally, problems asking for the side of appropriate rectangles or parallelepipeds satisfying some mutual quantitative conditions. Based on *El.* VI.16, and admitting that the construction of a rectangle on two segments corresponds to the multiplication of these segments, these problems are immediately identified with the problems of solving appropriate equations of first, second or third degree. And once this identification is admitted and a formalism such as Viète's one is introduced, these problems can be solved by relying on this formalism. But the mere admission that the rectangle constructed on two segments has some formal properties in common with the product of two numbers (which is made clear by a number of propositions of the *Elements*, especially from books II and VI) is not the same as the introduction of such a formalism, though it certainly suggests a multiplicative language to be used to enunciate these problems (cf. section 3, below). Hence, these problems can be enunciated in a language similar to that used for writing polynomial equations, without being understood as algebraic problems in Bos' sense. Moreover, the uses of a similar language for enunciating these problems is in no way a necessary condition for their solution. What is typical in these problems, however they are enunciated, is rather that what is sought is a geometrical magnitude that is characterized and identified without relying on any diagram.

The second class of purely quantitative geometrical problems includes problems, depending on certain positional relations, represented by appropriate diagrams that enter into their formulation. These problems are purely quantitative insofar as it is possible, on account of these diagrams, to reduce any relevant positional condition they are concerned with to a quantitative one. This amounts to relying on diagrams for eliminating these same diagrams, or at least for making them essentially useless, and thus transforming a problem of the second class into a problem of the first one. All the examples Bos gives in his book of what he calls "algebraic analysis" are of this sort, and stage A in his pattern of algebraic method of problematic analysis and synthesis seems, indeed, to be concerned with this process of elimination of diagrams, directly understood as a "derivation" of an appropriate equation.

Viète's main concern in the *Zeteticorum libri* was not however this process of elimination of diagrams, but rather geometrical purely quantitative problems of the first class, or, more generally, problems concerned with abstract quantities capable of expressing the common forms of arithmetical and geometrical purely quantitative problems.

Now, according both to Viète's method and to Bos' pattern, the solution of these problems needs, at least in most cases, a second reduction occurring after the fulfillment of stage A) of this pattern. This is the fulfillment of stage A′). Once this stage is fulfilled, stage B) leads to the solution of the problem, by means of a classical geometrical construction or a numerical calculation, suggested by the result obtained in stages A) or A′). The procedure that Bos describes as consisting of stages A)-A′)-B) in his pattern can thus, more generally, be described as a sequence of two reductions (a reduction of the problem to a system of polynomial equations and a reduction of this system of equations to other equations or to appropriate equalities or proportions [45]) and a geometrical construction or a numerical calculation suggested by the result of the second reduction. Both in the case of geometrical purely quantitative problems of the first class and that of problems concerned with numbers or abstract quantities, the first reduction merely consists of the transcription of the conditions of the problem in a new appropriate language. In these cases, what is essential is thus the application of a twofold procedure consisting of a reduction followed by a construction suggested by it: this is just an analysis followed by a synthesis. Of course, in so far as it consists either of a geometrical construction or a numerical calculation, the synthesis cannot be performed unless the nature of the quantities it is concerned with has been determined. Thus, the procedure applies to problems concerned with abstract quantities only if the nature of these quantities is unveiled at last. Suppose that the problem is geometrical, or has been finally interpreted as a geometrical one. The synthesis can then be understood as the construction of an element of a certain geometrical configuration that can be represented by a certain diagram (since in classical geometry, there is no construction without diagrams). This diagram is thus a sort of geometrical and positional model of a purely quantitative problem.

One should thus conclude that a geometrical purely quantitative problem, or a problem concerned with abstract quantities finally interpreted as a geometrical problem, is solved, through such a procedure, when a geometrical and positional model is found and an element of it is constructed

45 The centrality, in Viète's method, of the reduction of the equations occurring in stage A) into an appropriate system of proportions has been largely emphasized by Giusti and Freguglia. Cf. for example [Freguglia 1988, p. 73], [Freguglia 1999, pp. 116–133], and [Giusti 1992].

starting from other elements which are supposed to be given. This is similar to what is done in classical geometry in order to solve any purely quantitative problem: one finds out a particular positional model for it and constructs the ungiven elements of this model which correspond to the sought quantities of the problem. The crucial question consists in choosing an appropriate model, possibly the more appropriate one with respect to certain relevant criteria. When Bos' pattern of algebraic method of problematic analysis and synthesis is thus generalized and referred to geometrical problems, analysis enters it as an argument suggesting an appropriate choice of this model, namely a reduction of the given problem to another problem: the problem of constructing the unknown elements of this model.

Once this is admitted, my point reduces to the following claim: this reduction can be performed, and in fact has been performed in pre-modern mathematics,[46] without appealing to Viète's formalism or to any other analogous formalism relative to the operations of addition, subtraction,

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46 A recent book by R. Netz, somewhat anticipated by a previous paper of his (cf. respectively [Netz 2004] and [Netz 1999b], especially pp. 43–45) is devoted to the "historical development of a single mathematical proposition" [*ibid.*, p. 8] going from its first occurrence, under the form of a "problem", in book II of Archimedes' treatise *On the Sphere and the Cylinder*, to its reformulation, under the form of an "equation", in al-Khayyām's *Treatise of Algebra* (cf. footnote 55). Put in Archimedes' terms, the question consists of cutting a given segment AB in a point O, in such a way that, supposing that another segment $A\Gamma$ and an "area" Δ are given, the following proportion holds:

$$AO : A\Gamma = \Delta : Q(OB).$$

This is the question to which Archimedes reduces the proposition 4 of book II of his treatise: to cut a sphere so that its segments are in a given ratio to each other. Netz shows how Archimedes' solution is largely dependent on this specific context, or, in his own words, how it is "embedded within a geometric world, studying particular geometric configurations", but he also notices that the complexity of the question "demands a simplification that [...] holds in it the germs of the abstract or indeed the algebraic" [*ibid.*, pp. 15–16]. The transition Netz is concerned with consists of the gradual flowering of these germs. This is what he describes as a passage from a problem to an equation. Behind the obvious differences between Netz's approach, terminology, and historical concern, there is, I think, a fundamental convergence between a number of his views and the considerations I shall develop in the following part of my paper. A symptom of this convergence is Netz's use of the terms "quantitative", or even "purely quantitative", in a sense that is not far from mine. A symptom of our differences is the fact that Netz never refers these terms to problems, speaking rather of (purely) quantitative thoughts, ways, relations, geometrical objects, or approaches [*ibid.*, pp. 35–38, 52–54, 64, 90, 97–98, 102–103, 107, 123]. A geometrical problem is in fact for him concerned as such with objects "participating in local configurations, and [...] manipulated to obtain relations within such a local configuration" (notice that the term "configuration" refers here to a system of geometrical objects satisfying certain positional relations, as it is represented by a particular diagram), whereas an equation is concerned with objects that "belong to more general structures, and are related to each other in more general ways, independently of the local configuration multiplication, division, root extraction and solving polynomial equations defined on geometrical or abstract quantities. To justify this claim, I just have to provide a more precise characterization of what I take as the non-Pappusian kind of problematic analysis which is underlying Bos' pattern, and present some examples of it.

2.2.3. *Trans-configurational (problematic) analysis*

A geometrical purely quantitative condition is, in the great majority of cases, concerned with segments which are taken either as such or as sides of rectangles or parallelepipeds. Though the respective sides of rectangles and parallelepipeds have to be placed in a mutually appropriate position, this position is fixed beforehand and rectangles and parallelepipeds are geometrical quantities the respective size of which depends only on the respective size of these segments. Hence, there is no difficulty in admitting that a purely quantitative condition can be concerned with the respective sides of rectangles or parallelepipeds. The proposition XIII.1 of Euclid's *Elements*, considered in section 2.1.1, provides a clear example of a similar condition.

To prove it, Euclid refers to a diagram where the relevant rectangles take certain mutual positions. The proof of the scholiast shows that this is not necessary.[47] Though it is actually accompanied by a diagram , the function of such a diagram – which reduces to a straight line DB on which two points, A and C, are taken, so that DA, AB, AC, CB, DC, and the whole DB identify with b, a, α, β, γ, and $a+b$, respectively – is simply that of identifying the relevant segments, so as to fix the reference of the corresponding individual constants.[48] The proof entirely depends on appropriate substitutions and already proven theorems, which work in fact as rules of inference. Analysis goes in the same way. For short, in my reconstruction I have expressed the conditions of the theorem by writing "$a = 2b$", "$\alpha + \beta = a$" and "$a : \alpha = \alpha : \beta$", and used the symbols "$R(-,-)$"

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they happen to be in" [*ibid.*, p. 29], that is, as he says some pages later, with quantitative objects. It seems, instead, to me that a problem like that of finding a point O which satisfies the previous proportion is, as such, purely quantitative, though it can be interpreted on a specific geometrical model characterized by particular positional relations. On the other hand, for me, a purely quantitative problem is far from being identified, as such, with an equation. To be the case an appropriate formalism is needed.

47 The scholiast is even explicit on this matter: in cases of propositions XIII.1 and XIII.2, he claims that his "analysis and synthesis" are "without diagram ἄνευ καταγραφῆς" [Euclid 1969-1977, IV, pp. 199.1–2 and 200.11–12]. I thank F. Acerbi for having pointed out this point to me.

48 On this function of diagrams in classical geometry, cf. [Netz 1999a, pp. 19–26].

and "$Q(-)$". It is however easy to understand that if I had simply said, as Euclid and the scholiast did (cf. footnote 35), that DA is the half of AB and the point C cuts AB in extreme and mean ratio, then openly spoken of the rectangles and squares constructed on the different parts of DB, the analysis and the synthesis would have proceeded in the same way, so as to prove that the square constructed on DC is five times that constructed on DA. What is essential in the argument of the scholiast is not the way in which the relevant segments and rectangles are denoted and the relevant conditions expressed, but rather the fact that these segments and rectangles are denoted and these conditions expressed in such a way that it is possible to apply appropriate theorems and to operate with appropriate substitutions.

In *El.*XIII.1, all segments are understood as being given (the segments α and β are clearly supposed to have been actually constructed). It is clear, however, that the inferences entering the scholiast's arguments do not depend on that. Suppose that a purely quantitative problem is addressed. Instead of representing its solution by a diagram, one could act as the scholiast did in this case: simply denoting the given or ungiven quantities in such a way that it is possible to refer to them a number of appropriate theorems and to operate with appropriate substitutions. These theorems and substitutions might then be used to perform appropriate inferences, concerned with geometrical objects but completely independent of their mutual positions – which I shall term, for short, "geometrical non-positional inferences" – and transform the configuration of given and ungiven quantities into another configuration expressing a new problem the solution of which appears to be simpler [49]. The argument thus produced would certainly be an analysis, since the ungiven quantities would be taken as if they were given. But this analysis would not be intra-configurational. As long as

49 According to Mahoney [1968-1969, p. 331], the analysis entering the scholiast's argument relative to *El.*XIII.1 "illustrates the use of simplified algebra of line segments", whose possible "application [...] to problematic analysis should be clear". I do not know if, in Mahoney's intentions, "the use of simplified algebra of line segments" was close to what I understand as the performing of geometrical non positional inferences, but it can certainly be understood like this. In another, but similar vein, Netz [2000, p. 155] has remarked that in the Greek corpus, theorematic analysis "makes a departure from tradition, explicitly: it offers a solution with less reliance upon the diagram than the original Euclidean proposition (claiming, indeed, not to rely upon the diagram at all)." He continued by observing that "these proofs no longer rely upon geometrical cut-and-paste techniques, but upon a more abstract manipulation of objects according to rules referring, essentially, to the formulaic language."

it aims to transform the given configuration into a new one, it could rather be termed "trans-configurational". [50]

Consider the classical problem of the construction of two mean proportionals between two given segments. To solve it, one could look for a positional model for this problem, that is a particular geometrical situation involving four segments mutually related so as to satisfy the two mean proportionals condition. Once such a configuration has been found, the problem of the construction of two mean proportionals has been reduced to another problem: that of constructing two of these four segments, starting from the two remaining ones. To attain such a reduction one could suppose that the problem has already been solved and that x and y are the sought segments, then remark that the continuous proportion

$$a : x = x : y = y : b \tag{1}$$

among these segments and the two given segments a and b, may be split into two distinct proportions

$$a : x = x : y \qquad \text{and} \qquad x : y = y : b, \tag{2}$$

each of which provides the symptom of a parabola. It is possible that Menaechmus had proceeded in a similar way to discover his second construction of two mean proportionals by intersection of conics, [51] and it is actually in a similar way that Eutocius argues when he exposes such a construction, observing that, because of condition (1), the rectangle constructed on a and y and the rectangle constructed on b and x are respectively equal to the squares constructed on x and on y, so that the point of orthogonal co-ordinates x and y belongs to two parabolas. [52]

This argument – which leads from the problem of the construction of two mean proportionals to the other problem of the construction of two parabolas – is certainly an analysis, since it is founded on the supposition that the sought segments are given, but it is not an intra-configurational or Pappusian analysis. It does not rest on a diagram representing – together with appropriate information concerning its elements – the hypothetical configuration of geometrical objects and data corresponding to the solution of the problem, and does not lead to isolate in such a configuration,

50 Notice that I use here the term "configuration" to refer to a system of equalities or proportions, rather than to a system of geometrical objects as it is represented by a diagram, like I do, instead, when I speak of intra-configurational analysis.

51 An analogous point has been made by Mahoney [1968-1969, p. 333].

52 Cf. [Archimedes 1972-1975, III, pp. 82–85] and [Heath 1921, I, pp. 251–255].

or in an extended one, a sub-configuration which only consists of given objects and determines this entire configuration. Far from leaving this configuration unaltered, it includes, as an essential part of it, the transformation of such a configuration, the continuous proportion (1), into another configuration, the pair of proportions (2). Its starting point is a certain relation between what is actually given and what is only supposed to be given, whereas its final point is a different relation between these same elements. It is not intra-configurational, it is rather trans-configurational. [53]

This example is very simple, since analysis merely consists of a nearly imperceptible transformation, as the splitting of a continuous proportion into two distinct proportions, and in the understanding of these proportions as the symptoms of two parabolas. The same pattern can however

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53 In [Panza 1997b, pp. 401–410], I advanced a distinction similar to the present one, between intra-configurational and trans-configurational analysis, but presented it in a quite different context and with a different language, and argued for quite different theses. The distinction I advanced there had been suggested to me by P. Mäempää's reformulation of Hintikka and Remes' distinction between "directional sense" of analysis ([Hintikka & Remes 1974, 11] and [Hintikka & Remes 1976, 269]) and "analysis of configurations" (or "figures") ([Hintikka & Remes 1974, XIII, XV, chap. IV, pp. 31–40 and chap. VII, pp. 70–83] and [Hintikka & Remes 1976, p. 269]). Mäempää's reformulation goes as follows: "Pappus described analysis as the reduction of a proposition to be solved or proved successively backward to its antecedents until arriving at a proposition whose solution is known [...]. Descartes' methodological description of his algebraic method of analysis introduced an important novelty with respect to Pappus's description. Descartes said that analysis serves to determine how the unknown quantities of a problem depend on the given ones. Instead of seeking a deductive connection between the proposition to be solved or proved and propositions the solution or proof of which were known, Descartes sought to determine the dependencies of unknown quantities on the given ones. This is the 'configurational interpretation' of analysis" [Mäempää 1997, pp. 201 and 202]. More recently, M. Beaney has distinguished among "three main modes of analysis [[...]] [which] may be realized and combined in a variety of ways, in constituting specific conceptions or practices of analysis": the "regressive", the "resolutive or decompositional", and the "interpretative or transformative" [Beaney 2002, p. 55]. According to him, the first mode "was seen as central" in "ancient Greek geometry" [*ibid.*], while both the second and the third ones clearly appear in Descartes' geometry, where "problems can indeed [...] be broken down into simpler problems" [*ibid.*, p. 60], and "the geometric problem is first 'translated' into the language of algebra and arithmetic in order to solve it more easily" [*ibid.*, p. 67]. It seems to me that the resolutive mode is typical of a conception of analysis that, despite becoming very common in modern age, has little to do with the Aristotelian conception, rather depending on the Aristotelian notion of διαίρεσις (*Physics*, 184a, 16–184b, 14), a notion that is only implicitly and marginally evoked in Pappus' definition [Panza 1997b, pp. 396–398]. The transformative mode is instead typical of trans-configurational analysis, though the transformation that is involved in such an analysis is more a transformation of the given problem than a transformation of "the framework of interpretation" of analysis itself, as Beaney [2002, p. 67] argues, instead.

involve more complicated and less evident transformations relying, rather than on an obvious property of proportions, on a quite larger toolbox including appropriate geometrical theorems concerned with segments, rectangles and parallelepipeds, used as rules of inferences perfectly independent of the consideration of any diagram, and applied together with appropriate substitutions. Books II and VI of the *Elements* provide an essential part of this toolbox but, of course, they do not exhaust it. Moreover, this pattern also complies with analytical arguments referred to numerical problems and aiming to transform the conditions of these problems into other conditions, by using an appropriate formalism relative to the operations of addition, subtraction, multiplication, division, root extraction and solving polynomial equations.

Of course, both this formalism and geometrical non-positional inferences can also occur – and actually occurred – in the context of arguments that cannot be understood as examples of trans-configurational analysis: either, locally, within intra-configurational analyses or other sorts of arguments – side by side with inferences of a quite different nature –, or within series of other similar inferences providing non analytical arguments. [54] But as my aim is that of suggesting a sense that we could attribute to the term "algebra" in the context of the title of Viète's *Opus*, where "new algebra" is identified with "restored analysis", I am overall interested in examples of systematic use of such a formalism and of geometrical non-positional inferences in the context of trans-configurational analysis,

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54 A great part of the examples presented by Mahoney, in the second part of his [Mahoney 1968-1969], as well as Archimedes', Dionysodorus' and Diocles' solutions of the problem mentioned in footnote 46, and Eutocius' additions to them – discussed by Netz [2004] – include geometrical non positional inferences. Moreover, according to Mahoney [1968-1969, p. 337], "as it developed [in Greek mathematics], [...] analysis came to mean much more than a simple process of reverse reasoning; it involved a steadily growing body of generally applicable mathematical techniques by which a mathematician facing a new situation could reduce it to a recognizable form of a known problem or problems." Though insisting on this understanding of analysis as a body of techniques for reducing problems to other problems, Mahoney does not distinguish, however, between reductions obtained within an unaltered configuration, possibly extended and represented by an appropriate diagram (due to the identification of an appropriate sub-configuration, starting from which the sought objects can be constructed relying on known procedures), from reductions consisting in a change of configuration. In other terms, he does not distinguish between intra-configurational and trans-configurational analysis. Still, his examples suggest that Pappus' Treasury (or Domain) of Analysis had contained instances of problematic trans-configurational analysis, at least locally (cf. footnote 42). This is also the case of the 7th book of Pappus' *Collection* itself: an example is given by proposition VI.85, which is solved through a problematic analysis including some geometric non-positional inferences.

applied, in turn, in a systematic way so as to solve purely quantitative problems. Al-Khayyām's *Treatise of Algebra and Al-muqābala*[55] provides such an example.

3. AL-KHAYYĀM'S ART OF ALGEBRA AND *AL-MUQĀBALA*

According to al-Khayyām, "the art of algebra and *al-muqābala*" is an art aiming to solve a certain class of problems, namely: to "determine unknown [quantities] both numerical and geometrical"; these are particular sorts of purely quantitative problems which would be expressed today by means of polynomial equations of first, second or third degree.[56] The language used by Al-Khayyām is codified enough to express these problems as different variants of a common form. But this form is not obviously the one of polynomial equations written in Cartesian notation which we use today. Here are three examples, corresponding respectively to problems that we would express by means of an equation of first, second and third degree[Rashed & Vahabzadeh 1999, pp. 128–129, 140–141 and 184–185]:

> "A root is equal to a number.
> A square plus a number are equal to some roots.
> A cube plus some squares, plus some sides, are equal to a number."

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55 I refer here to Rashed's edition and French translation of the treatise (originally written, probably, in the first part of the 12th century), offered in [Rashed & Vahabzadeh 1999, pp. 116–237]. They are accompanied by an introduction, a rich and detailed commentary and a reconstruction of the history of the text. All of these have been very useful to me. An older edition, also accompanied by a French translation, had been given by Woepcke [Khayyām 1851]: Al-Khayyām's treatise enters a very large, rich and diversified tradition which has been reconstructed, in its different aspects and forms, in [Rashed 1984, specially chap. I]. Here, I shall limit myself to consider this treatise as such, by emphasizing the crucial role that trans-configurational analysis takes in it.

56 Cf. [Rashed & Vahabzadeh 1999, pp. 116–120]: "One of the notions we need in the part of knowledge which is known under the name of mathematics is the art of algebra and *al-muqābala*, intended to determine unknown [quantities] both numerical and geometrical. [...] the art of algebra and *al-muqābala* is a scientific art whose object is absolute number and measurable magnitudes in as far as they are unknown but related to a known thing through which they can be determined [...]." Al-Khayyām seems thus to admit that the art of algebra and *al-muqābala* is a sort of auxiliary art for arithmetic and geometry. On the meaning of "algebra and *al-muqābala*" in the technical language of Arabic mathematics, cf. [Diophante 1984, III, pp. 102–104]. These terms were first used, in their technical sense, by al-Khwārizmī's [1831] to denote two procedures (or operations) used to simplify an appropriate equality (by adding or subtracting equal terms to its two members), or more generally the systems of operations to be applied in order to reduce the condition of an equation-like problem to its standard form.

What is sought is of course (the determination of) the "root" or the "side" which satisfies these conditions. The correspondence between multiplication and construction of a rectangle on two given segments being admitted, there would be no difficulty in transcribing the enunciation of these problems under the form of a polynomial equation written in Viète's or Descartes' symbolic language, at least if the terms "squares" and "cubes" were understood in their usual geometrical sense. But this would certainly not be enough for warranting the possibility of solving these problems through a method which complies with Bos' pattern of algebraic method of problematic analysis and synthesis. The applicability of such a method depends, indeed, not only on the possibility of rephrasing the conditions of the problem in a similar language, but also, and above all, on the availability of a formalism connected with this language to be applied, together with appropriate constructive clauses, in order to achieve the stages A′) and B). Hence, a similar method applies to the solution of al-Khayyām's problems only if they are immersed in a mathematical context characterized by the availability of Viète's formalism, or of an equivalent one. This context was not al-Khayyām's. Moreover, to state a condition like "a square plus a number are equal to some roots" was not for him the same as giving a mathematical object, as it is the case when a polynomial equation is presented in the context of Viète's formalism.[57] Thus, in order to avoid any sort of possible misunderstanding, I shall refer to al-Khayyām's problems by the quite cumbersome expression "equation-like problems", rather than by the term "equation" alone, which is used in contrast in Rashed's translation.[58]

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57 The aim that Viète [1615a] pursued seems to have been just that of warranting the possibility of understanding an equation as a mathematical object, rather than, merely, as the condition of a problem. I thank C. Alvarez for having attracted my attention on this matter.

58 The term "equation" is also used by Netz [2004], where it is explicitly opposed to the term "problem" (cf. footnote 46). This is only an example of the differences between my reading of al-Khayyām's treatise and that of Netz. These differences certainly rely, for a great part, on the emphasis we bestow upon different aspects of such a treatise, but also concern the general interpretation of it. Netz seems to consider that it relies on equations (rather than problems) as long as it "downplays geometry" [*ibid.*, p. 182] and that this goes together with its use of equalities (rather than proportions), its systematic nature and its generality [*ibid.*, pp. 182–186]. For him, al-Khayyām "opens up the possibility of considering his objects symbolically, as elements manipulated by the rules of calculation, yet essentially conceives of them as components in a geometric configuration" [*ibid*, pp. 163–164]. For me, al-Khayyām's objects are, without any ambiguity, numbers or geometrical magnitudes, though he adopts a general language to speak of the common form of certain problems (the equation-like problems) that pertain to them. Al-Khayyām's generality concerns thus for me more

These problems are presented by stating appropriate conditions under the form of equalities involving: "a number"; "a side", "some sides", "a root", or "some roots" (in a single occasion al-Khayyām says "some things" [59]) "a square" or "some squares"; and "a cube". [60] Despite the term al-Khayyām uses to denote it, a number is a given quantity, that is, either a given number in the proper arithmetical sense of this term, [61] or a given geometrical quantity, namely a segment, a rectangle or a parallelepiped. A side or root is an unknown quantity to be determined, that is either an unknown number or an unknown segment. A square is either the second power of the root or side (if this is a number) or a geometrical square constructed on this root or side (if this is a segment). Analogously, a cube is either the third power of the root or side (if this is a number), or a geometrical cube constructed on this root or side (if this is a segment). When the root or side is a number, then for "some roots", "some sides", or "some squares", al-Khayyām understands other numbers obtained by taking this number or its second power a certain number of times. When the root or side is a segment, the meaning of these expressions varies. If the problem involves a cube, then it never involves a square or a root or a side, but always "some squares" and/or "some roots" or "some squares". The term "some squares" is then used to denote a parallelepiped the base of which is the square constructed on the root or side and the height of which is obtained by taking a certain number of times a given segment supposed to be unitary, while the terms "some roots" or "some sides" are used to denote a parallelepiped the height of which is the root or side and the base is obtained by taking a certain number of times a square constructed on a given segment supposed to be unitary. If the problem

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his language than his objects. Moreover, though I do not deny that his approach is systematic, I do not think that this situates him beyond geometry. When Al-Khayyām's equation-like problems are interpreted on segments, they are genuine geometrical problems, and the systematic use of geometrical non positional inferences does not go together with the admission that these inferences concern "more abstract objects" [*ibid.*, p. 164], as Netz claims, instead.

59 Cf. [Rashed & Vahabzadeh 1999, pp. 134–135]: "Some things are equal to a cube."

60 Al-Khayyām speaks of one or more roots when the problem does not involve a cube or can be reduced to a problem that does not involve a cube (it is an equation-like problem of first or second degree), while he speaks of one or more sides when the problem does involve a cube and cannot be reduced to a problem that does not involve a cube (it is an equation-like problem of third degree). The reason is quite clear: he is able to solve the problems of this latter class only when they are interpreted geometrically.

61 From now on, I shall use the term "number" only in its proper arithmetical sense, unless it appears in a quotation from al-Khayyām.

does not involve a cube, but a square, then it never involves a root or a side, but always "some roots" or "some sides". The terms "some roots" or "some sides" are then used to denote a rectangle the base of which is the root or side and the height of which is obtained by taking a certain number of times a given segment supposed to be unitary. Finally, in the only case where the problem does involve neither a cube, nor a square, it involves a root.

This language is in fact quite a sophisticated tool used to express the common forms of arithmetical and geometrical equation-like problems. Al-Khayyām's treatise is about all the possible forms of numerical and geometrical equation-like problems of the first three degrees. It aims to classify these different forms and to show how these problems can be systematically solved.

A classificatory criterion concerns the numbers of addenda entering the equalities which express the conditions of the problems. This comes to distinguish two-addenda, three-addenda and four-addenda problems. After the general conditions characterizing al-Khayyām's equation-like problems, a two-addenda problem can take six different forms, a three-addenda problem can take twelve different forms, and finally a four-addenda problem can take seven different forms. Hence, one has twenty-five possible different forms. These forms can belong, in turn, to three classes: that of problems which involve neither a square nor a cube, or that can be reduced to problems like these (the equation-like problems of the first degree); that of problems which do not involve a cube, involving instead a square, or that can be reduced to problems like these (the equation-like problems of the second degree); and finally that of problems which do involve a cube (the equation-like problems of the third degree).

This produces a threefold classification which is only concerned with the form of the equality expressing the condition of the problem. If we use our notations to express this form, we can illustrate it by means of the scheme of table 1,[62] where x is the root or side, c is a given quantity, p and q are numbers, and the numbers in brackets indicate the species (in the order in which al-Khayyām considers them).

These notations are however essentially foreign to al-Khayyām. The language I have described above is precisely used, instead of it, to express the common forms respected by any problem of any species, independently of the fact that it is an arithmetical or a geometrical problem. It is thus a

62 My scheme slightly differs from the one given by [Rashed & Vahabzadeh 1999, p. 11], but it is suggested by it.

	2 *addenda*	3 *addenda*	4 *addenda*
1^{st} class	[1] $x = c$ [4] $x^2 = px$ [6] $x^3 = qx^2$		
2^{nd} class	[2] $x^2 = c$ [5] $x^3 = px$	[7] $x^2 + px = c$ [8] $x^2 + c = px$ [9] $x^2 = px + c$ [10] $x^3 + qx^2 = px$ [11] $x^3 + px = qx^2$ [12] $x^3 = qx^2 + px$	
3^{nd} class	[3] $x^3 = c$	[13] $x^3 + px = c$ [14] $x^3 + c = px$ [15] $x^3 = px + c$ [16] $x^3 + px^2 = c$ [17] $x^3 + c = qx^2$ [18] $x^3 = qx^2 + c$	[19] $x^3 + qx^2 + px = c$ [20] $x^3 + qx^2 + c = px$ [21] $x^3 + px + c = qx^2$ [22] $x^3 = qx^2 + px + c$ [23] $x^3 + qx^2 = px + c$ [24] $x^3 + px = qx^2 + c$ [25] $x^3 + c = px + qx^2$

Table 1

common language for arithmetic and geometry. Take the species [19]. The formula used by al-Khayyām to characterize it is: "a cube plus some squares, plus some sides are equal to a number". It refers both: to an arithmetical equality, where the third power of an unknown number x, plus the second powers of x taken a certain number q of times, plus x taken a certain numbers p of times are supposed to be equal to a given number c; and to a geometrical equality where the cube constructed on an unknown segment x, let us say $C(x)$, plus the parallelepiped constructed on the square constructed on x and on a given segment, supposed to be unitary, taken a certain number q of times, let us say $P(x, x, qu) = q[P(x, x, u)]$, plus the parallelepiped constructed on x and on a rectangle constructed on the unitary segment and on this same unitary segment taken a certain number p of times, let us say $P(x, u, pu) = p[P(x, u, u)]$, are supposed to be equal to a parallelepiped constructed on a given segment c and on the square constructed on the unitary segment, let us say $P(u, u, c)$.

Al-Khayyām's common language for arithmetic and geometry does not go together with a common method or a number of common methods to solve equation-like problems independently of their particular nature, however. Thus, when he passes from the classification of his problems to their solution, he is forced to distinguish between the two possible interpretations of his language. In the case of the geometrical interpretation, Al-Khayyām is able to associate to any species of his problems a method of solution that applies to any problem of this species. It is not the same, of course, for the arithmetical interpretation, since al-Khayyām did not know how to solve the arithmetical equation-like problems of the third class.

At the beginning of the present section, I have said what al-Khayyām claims "the art of algebra and *al-muqābala*" to be, for him. Still, this is a quite vague characterization. The previous general presentation of al-Khayyām's treatise makes us able to advance a preliminary characterization of what this treatise is about, which should count as a more precise – though still preliminary – characterization of what the art of algebra and *al-muqābala* was for its author. This was for him a mathematical art aiming at: (i) expressing the common form of equation-like problems both numerical and geometrical; (ii) classifying these problems; (iii) showing how these problems can be systematically solved. It would thus be an art serving both arithmetic and geometry.[63] Still, this art should in no way be understood as a unitary theory of quantities. It was a system of techniques, rather than a theory. And it was common rather than unitary (cf. section 1): although it was employing a common language to speak of numbers and geometrical quantities, it was not a general context within which inferences concerning numbers and geometrical quantities could be warranted and things about them asserted. This common language was used to express the common forms of certain arithmetical and geometrical problems and to classify them, but when these problems had to be solved such a language was interpreted either arithmetically or geometrically.[64]

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63 Cf. footnote 56. It seems thus that, according to al-Khayyām, the art of algebra and *al-muqābala* was not opposed to geometry, being rather an art common to arithmetic and geometry. Other Arabic mathematicians, like Thābit Ibn Qurra, seemed, instead, to speak of algebra as an arithmetic art and compare "the method of [the] solution [of al-Khwārizmī's problems] through geometry" to "the method of their solution through algebra", though admitting that these methods agree with each other [Luckey 1941, p. 108]. Thābit's attitude seems to me analogous to that of many early-modern mathematicians, like Cardan or Bombelli: cf. footnotes 7 and 16.

64 The following passage – where al-Khayyām betokens not to be able to solve numerical equation-like problems of third degree – is particularly clear in this respect: "But when the object of the problem is an absolute number, neither I nor any other man of this art has succeeded in the solution of these species but for the first three degrees, that are the number, the thing, and the square; perhaps someone else who will follow us will be able to do it. And I shall often point out the numerical proofs of what is possible to prove starting from Euclid's work. Mind that the geometrical proof of these methods does not dispense you from the numerical proof if the object is a number and not a measurable magnitude" [Rashed & Vahabzadeh 1999, p. 124].

In order to detail this preliminary characterization of al-Khayyām's art of algebra and *al-muqābala,* we should consider his solutions of equation-like problems. I shall limit myself to consider three examples, respectively concerned with problems of species 7, 16, and 22. By considering the second of these examples, I shall come *en passant* to the problems of species 3.

3.1. *First example: "A square plus ten of its roots are equal to thirty-nine in number"*

This example [Rashed & Vahabzadeh 1999, pp. 136–141] is one of the few cases where al-Khayyām considers a particular example of equation-like problems of the species he is concerned with. This is given by the choice of the number ten as the numerical coefficient for roots and of the number thirty nine for the given quantity. This is just al-Khwārizmī's example [Khwārizmi 1831, p. 8] for the case where "roots and squares are equal to numbers" and just one square is considered. If al-Khayyām comes back to it, it is merely to stay close to the tradition. His solution is, indeed, completely independent of the choice of the numbers ten and thirty-nine and holds for any problem of species 7.

Al-Khayyām begins by presenting al-Khwārizmī's arithmetical solution, which is, for him, the general solution of such a species of problems when they are interpreted as numerical ones. When these problems are interpreted as geometrical ones, this solution does not apply and a different, properly geometrical, solution has to be provided. It has to display an appropriate construction of a segment x such that

$$Q(x) + R(a, x) = R(b, u), \quad \big([a = pu], [b = nu]\big), \tag{3}$$

where p and n are any numbers, a, b and u are three given segments, the last of which is supposed to be unitary, and for any segments α and β, $Q(\alpha)$ and $R(\alpha, \beta)$ denote the square constructed on α and the rectangle constructed on α and β, respectively.

Al-Khayyām presents two distinct arguments. The second is the same as al-Khwārizmī's geometrical argument for this species of problems [Khwārizmi 1831, pp. 13–16]. The first is quite similar to Thābit ibn Qurra's one [Luckey 1941, pp. 105–106] and can be reconstructed as follows: [65]

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[65] Here is how al-Khayyām expresses himself [Rashed & Vahabzadeh 1999, 136–137]: "Let us suppose that the square AC [Fig. 3] plus ten of its roots is equal to thirty-nine in number. Let us suppose, on the other hand, that ten of its roots are equal to the rectangle CE; the straight line DE is thus ten. Cut it in half at G. Since we have cut the straight line DE in half at G, and we have added AD on its prolongation, the

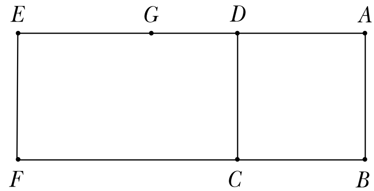

FIGURE 3.

Suppose that x were given.
Then $Q(x)$, $R(a,x)$ and $Q(x)+R(a,x)$ would be given, too. A.1

a, b and u are given. A.2

Thus, also $\frac{1}{2}a$, $Q\left(\frac{1}{2}a\right)$ and $R(b,u)$ are given. A.3

If $z = x + \frac{1}{2}a$, then,
$Q(z) = Q(x) + R(a,x) + Q\left(\frac{1}{2}a\right)$. A.4

Hence, for (4), $Q(z) = R(b,u) + Q\left(\frac{1}{2}a\right)$. A.5

And $Q(z)$ is then given. A.6

As a matter of fact, al-Khayyām refers to a diagram (Fig. 3), but he does not rely on it in his argument. Like that accompanying the scholiast's proof of *El.*XIII.1 (cf. section 2.1.1 and p. 120), such a diagram is merely of use for identifying the relevant segments and fixing the reference of the corresponding individual constants:[66] the segments AD, DE and DG identify with x, a and $\frac{1}{2}a$ respectively, so that the square $ADCB$ and the rectangles $DEFC$ and $AEFB$ identify with $Q(x)$, $R(a,x)$, and $Q(x)+R(a,x)$, respectively (step A.1 in my reconstruction corresponds to such an identification). No square which would identify with $Q(z)$ is drawn. It is thus clear that the equality which occurs in step A.4 and constitutes the essential step of the argument, is not deduced from the diagram, but rather through

product of EA and AD, that is equal to the rectangle BE, plus the square of DG, are equal to the square of GA; now the square of DG, that is the half of the number of roots, is known, and the rectangle BE, that is the given number, is known; the square of GA is thus known, and the straight line GA is known. If one takes away GD, it remains AD, known." Al-Khayyām then presents another solution, corresponding to al-Khwārizmī's well-known geometrical proof [Khwārizmi 1831, pp. 13–16].

66 Of course, al-Khayyām does not use the letters "a", "b", "u", "x" and "z", and the symbols "$Q(-)$" and "$R(-,-)$" to denote the segments, squares and rectangles he is concerned with. He rather refers to them as those represented by his diagram.

*El.*II.4 used as a rule of inference, so that this step consists in fact in a geometrical non-positional inference.

Al-Khayyām's argument is clearly an analysis, namely a trans-configurational analysis. It relies on such an inference to transform the configuration corresponding to the conditions of the problem into a new configuration corresponding to a new and essentially simpler problem: that of constructing a segment z such that

$$Q(z) = R(b,u) + Q\left(\tfrac{1}{2}a\right). \tag{4}$$

The synthesis is thus easy. One should first construct a rectangle equal to $R(b,u) + Q(\frac{1}{2}a)$, that is, according to *El.*VI.16 and *El.*II.3, the rectangle $R(\frac{1}{2}a, \frac{1}{2}a + t)$ constructed on $\frac{1}{2}a$ and on the segment t satisfying the condition

$$\frac{a}{2} : b = u : t \tag{5}$$

(which is but a fourth proportional). Then, according to *El.*VI.17, one should seek the segment z satisfying the condition

$$\frac{a}{2} : z = z : \frac{a}{2} + t. \tag{6}$$

(which is but a mean proportional, in turn). Because of the equality $z = x + \frac{1}{2}a$, the sought root of the original problem will then be

$$x = z - \frac{a}{2}. \tag{7}$$

Like the scholiast's proof of *El.*XIII.1, this last argument relies on a number of Euclid's theorems that make useless the consideration of any diagram. But, being a construction, rather than a deduction, it does not use these theorems as rules of inference. Rather, it uses them, so to say, as rules of constructive calculation. Notice however that al-Khayyām does not make the synthesis explicit. He concludes his solution by observing that since $Q(\frac{1}{2}a)$ and $R(b,u)$ are both known, $Q(z)$ is known, and then z is known, so that x is also known. Hence, his solution is not properly a synthesis, but rather a reduction obtained thanks to a trans-configurational analysis. Once this reduction is obtained, the construction of the sought segment – that is, the synthesis – is so easy that it is left to the reader.

3.2. *Second example: "A cube plus some square are equal to a number"*

These are problems of the third class, and thus al-Khayyām [Rashed & Vahabzadeh 1999, pp. 170–175] is not able to solve them under an arithmetical interpretation. Hence, he directly supposes that they are geometrically interpreted, so that the solution has to display an appropriate construction of a segment x such that

$$(8) \qquad C(x) + P(a, x, x) = P(c, u, u), \quad \big([a = qu], [c = nu]\big),$$

where q and n are any numbers, a, c and u are three given segments, the last of which is supposed to be unitary, and for any segments α, β, and γ, $C(\alpha)$ and $P(\alpha, \beta, \gamma)$ denote the cube constructed on α and the parallelepiped constructed on α, β, and γ, respectively.

To obtain this solution, al-Khayyām relies on the first of three lemmas that he had proved before. This is lemma 1 [Rashed & Vahabzadeh 1999, pp. 152–157], and states the second of Menaechmus' constructions of two mean proportionals, which depends on the intersection of two parabolas (cf. note 52). This is actually equivalent to the solution of geometrical equation-like problems of species 3: "A cube is equal to a number".[67] To obtain it, al-Khayyām remarks that the equality

$$(9) \qquad C(x) = P(c, u, u), \quad [c = nu],$$

(providing the condition for these problems) is equivalent to the proportion

$$(10) \qquad Q(u) : Q(x) = x : c$$

(since, according to *El.*XI.34, cubes the bases of which are inversely proportional to their heights are equal), which follows, in turn, from the continuous proportion

$$(11) \qquad u : x = x : y = y : c.$$

(since: if $u : x = x : y$, then $Q(u) : Q(x) = u : y$, for *El.*VI.17 and *El.*VI.1; and, if $u : x = y : c$, then $u : y = x : c$, for *El.*V.16).

Hence, lemma 1 establishes how to construct a cube equal to a given parallelepiped. This is just the first step of al-Khayyām's solution of problems corresponding to condition (8). This solution can be reconstructed as follows:

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[67] Cf. [Rashed & Vahabzadeh 1999, pp. 160–161]. As a matter of fact, Al-Khayyām's solutions of equation-like problems of the third class are ingenious generalizations of such a solution.

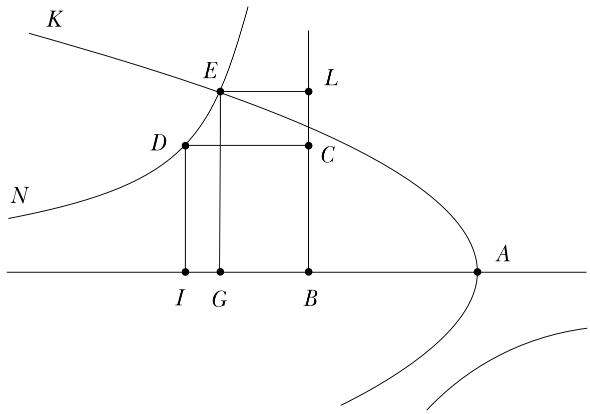

FIGURE 4.

The segment a is given; take AB equal to it (Fig. 4).	*Sol.*1
By lemma 1, construct a segment h, such that $C(h) = P(c, u, u)$.	*Sol.*2
Thus h is given. On the straight line AB, take BI equal to h, and construct on it the square $BCDI$.	*Sol.*3
Construct the hyperbola NDE passing through D with asymptotes BC and BI.	*Sol.*4
Construct the parabola AEK with *latus rectus* BC, axis AB and vertex A.	*Sol.*5
From the point of intersection E of these conics trace the perpendicular EG to the straight line AB.	*Sol.*6
The segment BG is the sought side.	*Sol.*7

Having exposed this solution, Al-Khayyām proves that it is right: [68]

The point E belongs to the parabola AEK, thus: $AG : EG = EG : BC$.	P.1
The point E belongs to the hyperbola NDE, thus: $EG : BC = BC : BG$.	P.2
One then gets the continuous proportion: $AG : EG = EG : BC = BC : BG$.	P.3

68 Al-Khayyām also proves by *reductio ad absurdum* that the two conics actually intersect for any choice of a and h: cf. footnote 69.

And, from here, the equality:
$C(BC) = P(AG, BG, BG) = C(BG) + P(AB, BG, BG).$ *P*.4

By posing $BG = x$, this equality just reduces to
$C(h) = C(x) + P(a, x, x),$ *P*.5
which, for *Sol*.2, is equivalent to condition (8).

If one would admit that the argument *Sol*.1–*Sol*.7 were a synthesis, this solution and this proof would perfectly correspond to stages B) and C) of Bos' pattern of algebraic method of problematic analysis and synthesis. Al-Khayyām says nothing, however, neither about the construction of the two conics entering this argument nor about the construction of the two parabolas mentioned in lemma 1. I shall come back later to the reason for this choice (cf. section 3.4). For the time being, let me remark that insofar as al-Khayyām does not make explicit how to construct his conics, his solution is not properly a synthesis. From this point of view, his argument is quite similar to that concerning geometrical equation-like problems of species 7 which I have considered in the previous section: in both cases, al-Khayyām hints at how to construct the sought segment, but as a matter of fact he does not actually construct it; he actually reduces the given problem to another one, which is left to the reader to solve. But, whereas in the case of geometrical equation-like problems of species 7 such a reduction is obtained by means of an explicit trans-configurational analysis, in the present case this stage is lacking. All al-Khayyām does is to describe how it would be possible to obtain a certain segment, supposing that one were able to construct four particular conics, and to prove that this segment is just the one being sought. Nevertheless, one could ask: how is it possible to pass from the given problem to such a description, which is actually the description of the solution of another problem (which is proved to be equivalent to the given one only afterwards)? The answer seems to me quite obvious: this is done by means of a trans-configurational analysis that, according to a classical habit, al-Khayyām does not make explicit. It is still easy to reconstruct this analysis by turning our attention to the proof. It goes as follows:

Consider condition (8), and according to lemma 1, replace in it the parallelepiped $P(c, u, u)$ with a cube $C(h)$, so as to get the new equality $C(x) + P(a, x, x) = C(h)$, where the segments a and h are both given. *A*.1

Suppose that x were given. The cube $C(x)$ and the parallelepiped $P(a, x, x)$, could then be added so as to get the new equality $P(a+x, x, x) = C(h)$, the two terms of which would be given.	$A.2$
This equality follows from the proportions $x : h = h : y$ and $h : y = y : a + x$.	$A.3$
These are respectively the symptoms of an hyperbola subtending the square $Q(h)$, and a parabola of *latus rectus h*, the axis of which coincides with an asymptote of the hyperbola and the vertex of which is at a distance a from the centre of the hyperbola	$A.4$

Like the scholiast's proof of *El.*XIII.1, this argument includes nothing but geometrical non-positional inferences. It is a twofold trans-configurational analysis: its first part is composed of steps $A.1$–$A.2$ and reduces the problem of seeking a segment x satisfying condition (8) to the problem of seeking a segment x satisfying the condition $P(a+x, x, x) = C(h)$; its second part is composed of steps $A.3$–$A.5$ and reduces this latter problem to the problem of constructing the point of intersection of two conics. The first part relies on the replacement, in a purely quantitative condition, of a parallelepiped with a cube (licensed by lemma 1) and of a sum of a cube and a parallelepiped with another parallelepiped. The second part relies on *El.*XI.36 and *El.*XI.37, which license the transformation of an equality between a cube and a parallelepiped into a pair of proportions which are then understood as the symptoms of two conics. What al-Khayyām explicitly presents is nothing but a description of these two conics followed by a proof of the equivalence of the given problem and the problem of constructing their point of intersection.

Mutatis mutandis, this is the general scheme of al-Khayyām's arguments concerning all equation-like problems of the third class: these problems are directly interpreted as geometrical problems; a hidden trans-configurational analysis reduces each of them to the problem of constructing the point of intersection of two conics; these conics are described – without making explicit how they could be effectively constructed – and the equivalence of the former and the latter problem is proven.[69] By passing from three-addenda to four-addenda problems, the

69 In some cases, al-Khayyām explicitly proves that his conics actually intersect for any choice of the given segment entering the condition of the given problem or indicates under which conditions they intersect; in some other cases, he does not face this

hidden trans-configurational analysis and the proof of equivalence of the two problems becomes of course a little more complicated. The next example should make clear how the former works.

3.3. *Third example: "A number plus some sides plus some squares are equal to a cube"*

This is also a species of problems of the third class [Rashed & Vahabzadeh 1999, pp. 198–201]. Once they are interpreted as geometrical problems, their solution has to display an appropriate construction of a segment x such that

$$(12) \qquad P(c,u,u) + P(b,u,x) + P(a,x,x) = C(x), \\ \big([a = qu], [b = pu], [c = nu]\big),$$

where p, q and n are any numbers and a, b, c and u are three given segments, the last of which is supposed to be unitary.

In this case al-Khayyām does not distinguish explicitly the description of the relevant conics from the proof of the equivalence of the given problem and the problem of constructing this point. Still, his argument is clearly twofold. For short, I limit myself to exposing al-Khayyām's hidden trans-configurational analysis, which is easy to reconstruct from the second part of this argument.

Both this analysis and the argument which al-Khayyām explicitly exposes make use of the second of the three lemmas which he proves before passing from problems of the second class to problems of the third one. This is lemma 2 [Rashed & Vahabzadeh 1999, pp. 156–159] and consists of describing how to construct a parallelepiped the base of which is a given square and which is supposed to be equal to a given parallelepiped the base of which is also a square. This reduces to the successive construction of two fourth proportionals.[70] Once such a lemma is admitted, analysis goes as follows:

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question. A complete treatment of it will be done some time later by Sharaf al-Dīn al Tūsī: cf. [Rashed & Vahabzadeh 1999, 26] and [Tūsī 1986, t. I].

70 This is how al-Khayyām reasons. Supposing that both a parallelepiped $P(\alpha,\alpha,\beta)$ and a square $Q(\gamma)$ are given, one can construct a segment μ such that $\alpha : \gamma = \gamma : \mu$ and then a segment ν such that $\mu : \alpha = \beta : \nu$. If this is done, ν and β are inversely proportional to $Q(\gamma)$ and $Q(\alpha)$, and so: $P(\gamma,\gamma,\nu) = P(\alpha,\alpha,\beta)$.

Consider condition (12), and replace in it
the parallelepiped $P(b, u, x)$ with another
parallelepiped $P(k, k, x)$ (*i. e.* the rectangle
$R(b, u)$ with the square $Q(k)$), and, according
to the lemma 2, the parallelepiped $P(c, u, u)$ $A.1$
with another parallelepiped $P(k, k, h)$,
so as to get the new equality
$P(k, k, h) + P(k, k, x) + P(a, x, x) = C(x)$,
where the segments a, k, and h are given.

Suppose that x were given. It would be such that
$a \leq x$, since $P(a, x, x) \leq C(x)$. Suppose that
$x = a + (x - a)$, where $x - a$ is a segment that
is supposed to be given, and split the cube $C(x)$ $A.2$
into the two parallelepipeds $P(a, x, x)$
and $P(x, x, x - a)$, in order to get the equality
$P(k, k, h) + P(k, k, x) + P(a, x, x) = P(x, x, a) + P(x, x, x - a)$,
whose terms would all be given.

This equality reduces to the other one
$P(k, k, h) + P(k, k, x) = P(x, x, x - a)$,
where the parallelepipeds entering the left-side $A.3$
member can be easily added so as to get the
new equality $P(k, k, h + x) = P(x, x, x - a)$,
the two terms of which would be given.

This equality is equivalent to the proportion $A.4$
$Q(k) : Q(x) = x - a : h + x$.

As the segments a and h are given and the segment x
has been supposed to be given, this would also $A.5$
be the case of the segment y such that
$Q(y) : Q(h + x) = x - a : h + x$.

The comparison of the two proportions occurring in
$A.4$ and $A.5$ provides the new proportion
$Q(k) : Q(x) = Q(y) : Q(h + x)$, $A.6$
which easily reduces first to $k : x = y : h + x$
and then to $k : y - k = x : h$.

The proportions occurring in $A.5$ and $A.6$ are
respectively the symptoms of two equilateral
hyperbolas, the first having both its $A.7$
latus rectus and *latus traversus* equal
to $h + a$, and the second subtending the rectangle $R(h, k)$.

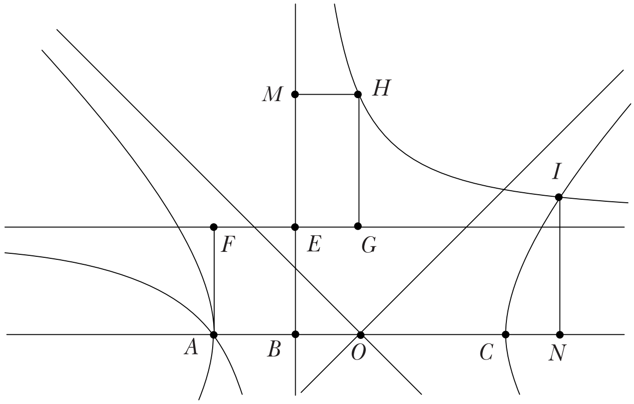

FIGURE 5.

In modern formalism, these hyperbolas are respectively defined by the equations

$$(13) \qquad y^2 - x^2 = x(h-a) - ha \quad \text{and} \quad x(y-k) = kh,$$

which easily derive from the proportions

$$(14) \qquad y^2 : (h+x)^2 = x-a : h+x \quad \text{and} \quad k : y-k = x : h.$$

To solve the problem it is enough to construct these hyperbolas in an appropriate relative position and to project their point of intersection on a common axis. Thus al-Khayyām first constructs the rectangle $ABEF$ (Fig. 5) on the segments $AB = h$ and $BE = k$, and takes the segment $BC = a$ on the straight line AB; then he constructs, on the straight lines FE and BE, a rectangle $EGHM$, equal to $ABEF$ and opposite to it in position; and finally, he states that the first hyperbola is the equilateral one (of centre O, such that $AO = OC$) the vertices of which are the points A and C, and the second is the (equilateral) one which subtends the rectangle $EGHM$ (and the centre of which is E). These hyperbolas intersect in the point A and in another point I, which is of course the only one that al-Khayyām considers, and the projection N of which on the axis AC gives the side $x = BN$ which was sought.

As well as in the previous case, al-Khayyām's solution is thus a reduction justified by a hidden trans-configurational analysis which, without referring to any diagram and relying on nothing but appropriate geometrical non-positional inferences, transforms the condition of the given problem into another equivalent condition which is easy to satisfy by constructing two conics, the actual construction of which is left to the reader. Such a trans-configurational analysis is, once again, composed of two parts. Steps $A.1$–$A.3$ reduce the problem of seeking a segment x satisfying condition (12) to the problem of seeking a segment x satisfying the condition $P(k, k, h + x) = P(x, x, x - a)$; steps $A.4$–$A.7$ reduce this latter problem to the problem of constructing the point of intersection of two hyperbolas.

3.4. *The aims of the art*

The previous three examples should be enough to illustrate the nature and role of trans-configurational analysis in al-Khayyām's treatise. There is however an essential difference between the first example and the other two. It concerns the nature of the construction that al-Khayyām leaves to the reader, that is properly, the synthesis. While in the first example this construction can be easily performed by ruler and compass, this is not the case for the second and the third example. Thus a question arises: is al-Khayyām right in avoiding any consideration concerned with the actual construction of the effective solution of his problems?

During his treatment of equation-like problems of species 21, al-Khayyām mentions *en passant* a mathematician who had lived in the final part of the 10th century: Abū al-Sahl al-Qūhī. He is the author of a *Treatise on the Perfect Compass,* probably the first one of a number of treatises composed some time before al-Khayyām's and devoted to the description and study of a mechanical tool to be used to trace continuously any sort of conic.[71] This is a three-dimensional compass with a sliding drawing point which it is possible to regulate in different ways. In his treatise, al-Qūhī shows how to do so, in order to trace any conic. By following his instructions, one can easily trace all the conics entering al-Khayyām's solutions of his equation-like problems of the third class. Hence, their construction is, from al-Khayyām's point of view, not only easy, but also thoroughly standard.

There is thus a very simple reason one could evoke in order to justify al-Khayyām's choice to leave to the reader the constructions providing the effective solution of his problems: once these problems have been reduced

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71 Cf. [Woepcke 1874], and [Abgrall 2004, pp. 158–178]. On the perfect compass and the question of the continuous tracing of conics in the 10th century, cf. also [Rashed 1993, pp. LXXXI–LXXXII], [Rashed 2003], and [Rashed 2004, pp. 46–118 and 229–291].

to other ones, there would have been no interest in detailing such a construction. Still, it seems to me that this reason might go together with another one: al-Khayyām was not understanding this construction as being included in the subject-matter of his treatise, that is, he was supposing that it falls outside the domain of the art of algebra and *al-muqābala*, in so far as he was understanding this art as being concerned more with the reduction of equation-like problems to other problems than to the solution of these problems. The third clause entering my previous preliminary characterization of what the art of algebra and *al-muqābala* was for al-Khayyām should thus be made more precise: to show how equation-like problems can be systematically solved is not the same as solving them. It seems thus that for al-Khayyām, the art of algebra and *al-muqābala* was a mathematical art aiming at: (i) expressing the common form of equation-like problems both numerical and geometrical; (ii) classifying these problems; (iii) reducing them to other problems that one knew how to solve. Trans-configurational analysis was the means used to attain the third of these aims.

4. ALGEBRA AND TRANS-CONFIGURATIONAL ANALYSIS

Of course, I do not want to argue that al-Khayyām's treatise directly influenced Viète or some other early-modern mathematicians. A copy of such a treatise was brought to Europe by Golius around 1630 [Rashed & Vahabzadeh 1999, pp. 17 and 109–110], but it is very difficult to know if it was read, and if so by whom; and it is even more difficult to establish if European mathematicians at the end of the 16th and at the very beginning of the 17th century were acquainted with it. Still, I suggest that what Viète meant with "algebra" in the title of his *Opus* has much to do with al-Khayyām's art of algebra and *al-muqābala*.

Namely, I suggest that Viète was referring to the art of transforming purely quantitative conditions using either an appropriate formalism relative to the operations of addition, subtraction, multiplication, division, root extraction and solving polynomial equations applied to indeterminate numbers, or appropriate geometrical non-positional inferences. This was a system of techniques underlying trans-configurational analysis, though it was more than that (since both this formalism and these inferences were also occurring in the context of other sort of arguments [72]).

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72 A vivid illustration of the common use of geometrical non positional inferences among Arabic mathematicians starting from the 9th century is offered by Rashed and Houzel's recent edition of a collection of geometrical propositions by Na'īm ibn

The analogies between such a formalism and the properties of rectangles and parallelepipeds that warranted such inferences were well known and largely exploited before Viète. But they had never led to the definition of a new formalism relative to abstract quantities to be used to conduct trans-configurational analyses concerned with purely quantitative problems, independently of their interpretation as numerical or geometrical problems. In the *Isagoge*, Viète defined such a formalism and proposed to use it for reforming the method of problematic analysis and synthesis. It is thus quite natural to imagine that by speaking of algebra as an old art, in the title of his *Opus*, he was referring to a similar system of techniques that he was aiming to unify and develop within a new methodological context.

An example taken from Viète's *Zeteticorum libri* can be used to show how Viète's arguments are structurally similar to al-Khayyām's ones. It concerns the *zeteticum* II.17: "*Data differentia laterum, & differentia cuborum: invenire latera*".

Viète denotes with B the difference of sides and with D the difference of cubes. He does not denote the sides by any letter (calling them respectively the "greater side" and the "smaller side"), but rather denotes by "E" their sum. To use more familiar symbols, let us write z, instead, and denote the sides with x and y. In our notation, the problem consists in solving the system of equations

$$x - y = B, \quad x^3 - y^3 = D, \tag{15}$$

where B and D are given. From the position $x + y = z$, it follows

$$z + B = 2x, \quad z - B = 2y, \tag{16}$$

and

$$(z + B)^3 - (z - B)^3 = 6z^2B + 2B^3 = 8D, \tag{17}$$

that is

$$z^2 = \frac{4D - B^3}{3B}. \tag{18}$$

As the side is given when the square is given, the problem is thus reduced to *zeteticum* I.1: "*Data differentia duorum laterum, & aggregato eorundem: invenire latera*". To do this, Viète denotes respectively with B and D the difference and the sum of sides, and with A the smaller side. Let us

Mūsā, a quite modest mathematician whose work seems to reflect the common mathematical culture of the period in Bagdad: cf. [Rashed & Houzel 2004] and [Panza forthcoming].

write y instead of A, and, to avoid any confusion, C instead of D. It follows that

(19) $$B + 2y = C,$$

and so [73]

(20) $$y = \frac{C - B}{2}.$$

To solve *zeteticum* II.17, one should then substitute $\sqrt{(4D - B^3)/3B}$ in place of C to obtain

(21) $$y = \frac{\sqrt{(4D - B^3)/3B} - B}{2}.$$

As a matter of fact, Viète does not follow this procedure. He stops his solution of *zeteticum* II.17 by remarking that, because of equality (18), if B and D were respectively 6 and 504, the square of the sum of sides would be 100, and he also stops his solution of *zeteticum* I.1 by noticing that, because of equality (20), if B and C were respectively 40 and 100, the sides would be 70 and 30.

Supposing that Viète's *zetetica* had been interpreted as arithmetical problems, there would be of course nothing else to do in order to solve the second one, while to solve the first, one should simply have to make explicit the content of equality (21), and eventually continue with the numerical example chosen by Viète, observing that in this case x and y would respectively be 8 and 2. But assuming that Viète's *zetetica* had been interpreted as geometrical problems, a complete solution would have consisted of the geometrical constructions that equalities (21) and (20) only hint at. Though the first of these constructions is a little elaborate, both of them are standard constructions to be performed by ruler and compass. Thus Viète seems to have with respect to his *zetetica* – taken as geometrical problems – the same attitude that al-Khayyām had with respect to his geometrical equation-like problems: he limits himself to reducing them to other problems that one was already able to solve, and, as a matter of fact, he does that by means of a trans-configurational analysis.

There is a crucial difference however, between al-Khayyām's and Viète's arguments. The former had at his disposal a language to be used for expressing the common forms of both arithmetic and geometrical equation-like problems, and in certain cases, he could imagine geometrical models

73 Viète also presents an alternative argument. By denoting the greater side with x one gets $2x - B = C$ and so $x = \frac{1}{2}(C + B)$.

for arithmetical problems. But he could not operate directly on the common forms of his problems to reduce them to other problems. In order to perform his analysis, he had to interpret his problems either as arithmetical or as geometrical problems, and rely either on arithmetic or on geometrical non-positional inferences. The latter could, instead, rely on a repertoire of rules of inference to be applied, together with appropriate substitutions, to transform the same equalities expressing the common forms of purely quantitative problems both arithmetical and geometrical into other equalities or proportions expressing new problems. He could perform his analysis without being forced to make a preliminary choice of interpretation. Hence, a trans-configurational analysis is for him neither an arithmetical nor a geometrical procedure, but an abstract one, as it is concerned with abstract quantities.

It is difficult to overestimate such a novelty. But it seems to me that we would gain in historical clarity, accuracy, and perspicuity if we were able to see the deep relations connecting this crucial novelty with an old art which, I suggest, Viète was considering as an ancient form of algebra and which was used, long before him, to conduct trans-configurational analyses concerned with geometrical problems. [74]

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74 It seems to me that one of the advantages of my suggestion could be that of clarifying the sense in which one should be able to speak of Euclid's "geometrical algebra" (and so to contribute to the solution of a classical historical question that Netz [2004] has recently addressed in a new and quite stimulating form). There is no doubt that the second book of the *Elements* contains nothing similar to Viète's or Descartes' formalism. Still a large number of its propositions – as well as many other propositions of the *Elements,* – are apt to be used – and, as a matter of fact, were used both in Greek and in Arabic geometry – as rules of inferences occurring in trans-configurational analysis.

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