View metadata, citation and similar papers at core.ac.uk
brought to you by CORE
provided by Archive Ouverte a LUniversite Lyon 2

HAL
archives-ouvertes.fr

Isaac Barrow and the Bounds of Geometry

Marco Panza

To cite this version:

Marco Panza. Isaac Barrow and the Bounds of Geometry. Patricia Radelet De-Grave. Liber Amicorum Jean Dhombres, Brepols, pp.365-411, 2008. <hal-00456290>

HAL Id: hal-00456290
https://hal.archives-ouvertes.fr/hal-00456290

Submitted on 13 Feb 2010

HAL is a multi-disciplinary open access archive for the deposit and dissemination of scientific research documents, whether they are published or not. The documents may come from teaching and research institutions in France or abroad, or from public or private research centers.

L'archive ouverte pluridisciplinaire **HAL**, est destinée au dépôt et à la diffusion de documents scientifiques de niveau recherche, publiés ou non, émanant des établissements d'enseignement et de recherche français ou étrangers, des laboratoires publics ou privés.

Isaac Barrow and the Bounds of Geometry*

Marco Panza
CNRS (REHSEIS, UMR 7596, CNRS and Univ. of Paris 7)

December 16, 2007

During the 17th century, mathematics changed deeply. As it has often been remarked, this went together with a new methodological or, rather, a new philosophical attitude. Nevertheless, in the 17th century, philosophy of mathematics was not just a marginal effect or an implicit and, of course, partial cause of a broad transformation of mathematics itself. It was also a domain of research and the subject of controversies and intellectual disputes; in short, an autonomous discipline[1]. Of course, the internal evolution of such a discipline could not remain completely unaffected by the contemporary evolution of mathematics. Still, many of the topics that constituted its specific content were intrinsically dependent on traditional quarrels and quite old questions. One of these topics was concerned with the mutual relations and the opposite virtues of arithmetic and geometry. Though inherited from the traditional distinction of quantities in the opposite genera of numbers (or discrete quantities) and magnitudes (or continuous quantities), this question appeared in a new light because of the impressive disclosure of new algebraic techniques to be applied both to numbers and magnitudes.

Isaac Barrow and John Wallis, the most prominent and innovative British mathematicians of their time, devoted much of their intellectual energy to such a topic, arguing for two diametrically opposite views. Barrow's *Lectiones Mathematicæ*[2] and Wallis's *Mathesis uni-*

*The material discussed in the present paper was first presented at the HOPOS conference held in Paris, on June 2006, during a joint talk with Antoni Malet. I thank Antoni Malet himself and David Rabouin for having drawn my attention to this material and for the many instructive discussions of it and related subjects. I also thank Michel Blay, Sara Confalonieri, Massimo Galuzzi, Daniel Garber, Niccolò, Guicciardini, Doug Jesseph, Vincent Jullien, Susan Klaiber, Paolo Mancosu, Sébastien Maronne, and Theodora Seal, for valuable comments on my talk and earlier versions of my paper. Many thanks also to Luigi Maierù for several very stimulating discussions about Barrow and Wallis, among other things.

[1]This view is, at least implicitly, endorsed in [30], [23], [31], and [17].

[2]Cf. [9] and [6]. Though published only in 1683, six years after Barrow's death, this treatise includes the lectures given by him at the University of Cambridge, as Lucasian professor of mathematics in 1664-1666 (the crucial years in the mathematical schooling of Newton: cf. [27], vol. I, and [28]). In 1734, an English translation of it, due to J. Kirkby, was published: cf. [7]. I quote from this translation. A new edition of the original Latin text is available in [8], pp. 1-378. Concerning the passages which are relevant for my purpose, Kirkby's translation is accurate enough. Thus, I'll quote it, though pointing out some typographical errors, and suggesting how to correct it in the few cases where it seems to me to differ considerably from the original.

versalis and *Algebra*[3] were quite elaborate treatises aiming to defend (according to different methodological strategies) two opposite theses: the foundational superiority of geometry over arithmetic and of arithmetic over geometry, respectively.

My aim is to discuss two examples that Barrow relies on in his lecture III. His conception would of course deserve more attention and a much more detailed analysis[4]. Here I shall limit myself to a very specific point, which I chose because it is very close to a topic discussed by Jean in one of his papers which I particularly favor for both scientific and personal reasons[5].

1 Barrow's thesis about arithmetic and geometry

The following quotation, taken from the very beginning of lecture III, should suffice to point out the thesis defended by Barrow in this lecture[6]:

> [...] I affirmed the Whole of Mathematics to be in some sort contained and circumscribed within the Bounds of Geometry. And indeed concerning the other Sciences then mentioned, I imagine no Body will much deny, but they are conveniently enough reduced to Geometry. But now I have brought a harder Task upon myself, to shew allowable and fit Causes for expunging *Arithmetic* out of the List of Mathematical Sciences, and as it were degrading the most noble Science from the Degree which it has been so long possessed of. [...] Be it far from me to take away or seclude a Science so excellent and profitable that of Numbers from the Mathematics. I will rather restore it into its lawful Place, as being removed out of its proper Seat, and ingraff and unite it again into its native Geometry, the Stock from whence it has been plucked. [...] For I am convinced that Number[7] really differs nothing from what is called Continued Quantity, but is only formed to express and declare it; and consequently that Arithmetic and Geometry are not conversant about different Matters, but do both equally demonstrate Properties

[3]Cf. [41], [42] and [43].

[4]For a general description and an assessment of the whole treatise, cf. [18], pp. 181-202. The matter has also been treated by: C. Sasaki—in the context of a study of the acceptance of the theory of proportion in the 16th and 17th centuries [cf. [34]]—, H. Pycior—in the context of a reconstruction of the historical setting for Berkeley's views about mathematics [cf. [32], pp. 274-277]—, L. Maierù—in the context of a general reconstruction of the mathematical works of Barrow and Wallis [cf. [19]]—, K. Hill—in the context of a study of Wallis' and Barrow's conceptions about the composition of continua [cf. [15]]—, A. Malet—in the context of a comparison between Barrow's and Wallis attitudes toward the method of indivisibles [cf. [21]] and in that of a reconstruction of Barrow's program of mathematization of natural philosophy [cf. [22]]—, and D. Jesseph—in the context of the controversy between Hobbes and Wallis' on the quadrature of circle.

[5]Cf. [11].

[6]Cf. [7], pp. 29-31.

[7]Kirkby's translation omits here a relevant parenthesis: "(illum saltem quem Mathematicus contemplatur)". One could include it in the main statement thus: "For I am convinced that Number (at least in so far as it is considered by a mathematician) really differs nothing..."

common to one and the same Subject; and very many, and very great Improvement will appear to be derived from hence upon the Republic of Mathematics.

I. For first of all it appears that there is neither any general Axiom nor particular Conclusion agreeing with Geometry (which respects Magnitudes not taken absolutely, but in Comparison with one another according to certain Proportions of Equality or Inequality, *i. e.* as they are capable of being designed and compared together by Measures) but what by the same Reason also agrees with Arithmetic: And on the other hand that nothing can be affirmed, concluded, or demonstrated, concerning Numbers which may not in like manner be accommodated to Magnitudes; whence accrues a remarkable Light and vast Improvement to both Sciences; the Cause being removed of superfluously repeating and demonstrating Theorems by Nature altogether the same, also of often solving the same Problems.[...]

II. This Coalition of Numbers and Magnitudes being admitted, a plentiful Accession accrues to each Discipline. For it will be a very easy thing to discover and demonstrate very many Theorems concerning Numbers by the Assistance of Geometry, which, by keeping within the common Limits of Arithmetic would scarcely, if at all, be capable either of Investigation or Demonstration: Also very many Things may be more briefly and clearly found out and demonstrated from hence. And reciprocally, the Ratio's or Reasons of Numbers being well understood will communicate not a little to the more evident Explication and strong Confirmation of many Geometrical Theorems.

To sum it up in Mahoney's words[8], Barrow's thesis is that "the domain of arithmetic [...][is] subsumed under that of geometry", or, as Jesseph says[9], that "numbers, far from being self-subsistent objects, are mere symbols whose content derives from their application to continuous geometric magnitude".

If we considered it in the light of the internal evolution of mathematics, this proposal—and specially Barrow's promise of a "great Improvement [...] upon the Republic of Mathematics" that should have been derived from the envisioned reduction of arithmetic to geometry—would inevitably appear as completely out of place. It was made in spring 1664[10], more than seventy years after Viète's axiomatic definition of an algebraic formalism for any sort of quantity[11], almost thirty years after Descartes interpretation of this same formalism on any sort of magnitude[12], and on the immediate eve of Newton's results that brought him to the theory of fluxions[13]. These results did certainly not provide any argument to deny that

[8] Cf. [18], p. 188.
[9] Cf. [17], p. 33.
[10] Cf. [13], p. 68.
[11] Cf. [37], ch. 2.
[12] Cf. [10], pp. 297-298.
[13] Cf. [27], vol. I, parts I and II, and [28]. It is quite probable that Barrow's lectures were published from a manuscript that he had given to Collins in March 1668 [cf. [13], pp. 68-79; specially, p. 69.]. At that date,

number "is only formed to express and declare" geometric magnitudes, that it merely results from a way to speak of them. But, in the light of them, the essential question for the evolution of mathematics could no more be that of the intrinsic nature of numbers and their relations with magnitudes. Questions connected with the development of an appropriate formalism to be used for treating both numbers and magnitudes became much more relevant, indeed.

Still, the right perspective in which Barrow's proposals should be read is not that of the evolution of mathematics. Barrow was rather looking for a way to found the new mathematical results on the secure roots of the classical tradition. This is a twofold philosophical requirement: it is both a requirement of foundation and a requirement of tradition. Many historical reasons could explain it. I merely suggest that it was such a double philosophical requirement that commanded Barrow's *Lectiones Mathematicæ*, and that it was for this reason that this work is so profoundly involved with topics, arguments and rhetorical figures derived from the Aristotelian philosophical repertory. This suggestion has a historiographical implication: the program exposed in this work should not be understood as it were intended to promote some sort of mathematical progress, and should not be judged on the basis of the natural criteria used to judge a mathematical program. I guess that, *mutatis mutandis*, this is also the case of the opposite program exposed in Wallis' *Mathesis universalis* and *Algebra*.

The previous long passage that I have quoted from the third of Barrow's *Mathematical Lectures* is followed by two examples that Barrow introduced without any foreword: "We will illustrate the Matter with an Example or two", he simply wrote[14]. Barrow provided, in each case, what he considered to be a geometric incontestable proof of an arithmetical theorem, and pretended that no equally exact or clear proof of these statements could be obtained by means of purely arithmetic arguments[15].

Barrow was certainly already aware of the results obtained by Newton between 1664 and 1666. And, though, at that time, Newton had not yet coined the term "fluxion" and did not have such a general concept as that which he designated with this term in the *De methodis* (composed in 1671), these results should have shown, to any attentive interpreter, that the mathematical properties of quantities depended much less on their specific nature than on their possible operational relations.

[14]Cf. [7], p. 31.

[15]The term "arithmetic" certainly did not have, in the middle of the 17th century, a fixed and univocal meaning. It was used both in a strict sense, to refer to that part of mathematics that dealt with numbers through the basic operations of addition, subtraction, multiplication, division and extractions of roots defined on them, and in a broader sense, to refer to what was also termed, more precisely, "universal arithmetic". The term "algebra" was even more ambiguous. Very often, historians use it today—when speaking of early modern mathematics—to refer to the same matter that the term "universal arithmetic" denoted in early modern age, and this was also a common use at that time [about the notion of algebra in early modern age, cf. [29]]. It follows that there is room, when early modern mathematics is concerned, to understand the terms "arithmetic" and "algebra" as synonymous. It seems to me that this understanding does not apply to the case under consideration, however. I rather suggest that in his third lecture, Barrow was using the former term in the first of the two previous senses. At the end of his second lecture, he was, moreover, quite explicit in arguing that algebra should be understood as "a Part or Species of Logic", that is, as a "certain Manner of using Reason", and that, so understood, it could apply both to geometry and arithmetic [cf. [7], p. 28]. It seems thus that for

The first example concerns a theorem that is actually very hard to prove in purely arithmetical terms. Nevertheless, Barrow's geometric proof depends on the supposition of a sort of solidarity between arithmetic and geometry that—despite being clearly justified—could have been understood, and actually had been understood by Wallis, as a symptom of a possible reduction of geometric results to arithmetical ones, rather than *viceversa*. The theorem involved in the second example admits, instead, a number of simple purely arithmetical proofs, some of which were certainly known or could have been known (and understood) by Barrow. Hence, were it judged on a purely mathematical basis, Barrow's claim would appear in both cases as highly questionable. If we are not ready to admit that Barrow's examples have—and also had, at Barrow's time—no argumentative force, and simply reduce to rhetorical devices, we have, thus, to offer a different interpretation of them.

I suggest that what Barrow was arguing for, through these examples, was not the greater deductive power of geometry over arithmetic, but rather the foundational priority of a form of infinity, quite naturally involved in geometric arguments, over another form of infinity, usually involved, instead, in arithmetical arguments. In other words, I suggest that what Barrow was implicitly maintaining, when he asserted that the theorems under consideration did not admit a satisfactory purely arithmetical proof, was that such a proof would in any case be grounded on some infinitary arguments that should have not been admitted on a purely arithmetic basis, but could have been justified only if infinity were understood geometrically. Under this interpretation, his arguments depend on an hidden premise (and, in a sense, finally reduce to an implicit assertion of this same premise): infinity can only be conceived, correctly, as the potential infinity of the possible divisions of a given finite magnitude; an infinite sum of terms is correctly conceivable only if these terms represent the infinitely many parts in which an already given finite magnitude can be divided.

Though Barrow's general thesis about the subsumption of the domain of arithmetic under that of geometry, constituted, as Mahoney himself remarks[16], a "radical departure from tradition", the more local thesis that he was arguing for, by relying on his two examples, seems thus to be nothing but a consequence of a well-known Aristotelian view, that Barrow explicitly endorsed in his ninth lecture[17]:

> Indeed we can approach nothing with Sense, but as it is terminated [...]. Neither we can conceive any Magnitude in Thought, at least distinctly, but only as it is contained and comprehended within some Limits. We may indeed have a confused Imagination of some Magnitude in Power, suppose a Right Line, as *Aristotle* said,

Barrow, the problem of the relations between arithmetic and geometry was merely that of the relations between arguments or methods concerned with numbers (independently from the way they were understood) and the basic operations on them, and arguments or methods concerned with geometric magnitudes. According to him, the fact that these arguments or methods could or could not take an "algebraic" form—that is, use symbols for species (or *logisice speciosa*)—was apparently not relevant for this question.

[16]Cf. [18], p. 186. Cf. also [15], p. 173.

[17]Cf. [7], pp. 142-143.

infinite; *i. e.* produced or encreased at pleasure, or to a greater Distance than any Distance assigned, which consequently was denominate infinite. Nor is the Philosopher's Definition of Infinite improper, though very subtle; *That is infinite, from which if one take any Quantity, something still may be assumed beyond*[18]. But this nothing else but to consider many Lines successively, according to a certain arbitrary and indeterminate Power, and not one real Indeterminate Line distinctly; which according to the Philosopher again we cannot comprehend in the Mind. [...]. The same is of an *infinite Subduction* or *Subdivision*, by which nothing else is understood, but that a Subtraction or Division may be continued at pleasure, without being brought to an Impossibility of proceeding farther. Again, *Aristotle* speaking of Mathematicians says, *Mathematicians do neither want nor use infinite Magnitude, but take as much as they please, when they are minded to terminate it*[19].

2 Parabola and square roots: Barrow's inversion of an argument of Wallis

Here is the first of Barrow's examples[20]:

> It is an Arithmetical Theorem, that *the Sum of an infinite (* or *indefinite) Series of Numbers increasing from Nothing to a certain Term, which is the greatest according to the Ratio of the square Roots of Numbers continually exceeding one another by Unity, (i.e. as* 0, 1, $\sqrt{2}$, $\sqrt{3}$, *&c.* ad infinitum*) is subsesquialter the Sum of as many equal to the said greatest Term*; Which Theorem I am of Opinion can never be exactly demonstrated by any Method in Arithmetic itself: but it is plainly deduced from Geometry.

In modern terms, Barrow's theorem could be stated as follows:

$$\lim_{n\to\infty} \frac{\sum_{i=0}^{n} \sqrt{i}}{(n+1)\sqrt{n}} = \frac{2}{3}. \tag{1}$$

[18]Barrow refers to *Physics* III. 9. The reference to Bekker's text is rather 207a, 7-8. Here is Hardie's and Gaye's translation, revised by J. Barnes [cf. [4], vol. I, p. 352]: "Thus something is infinite if, taking it quantity by quantity, we can always take something outside."

[19]Barrow refers to *Physics* III. 11. The reference to Bekker's text is rather 207b, 27-31. Here is Hardie's and Gaye's translation revised by J. Barnes [cf. [4], vol. I, p. 354]: "Our account does not rob the mathematicians of their science, by disproving the actual existence of the infinite in the direction of increase, in the sense of the untraversable. In point of fact they do not need the infinite and do not use it. They postulate only that a finite straight line may be produced as far as they wish."

[20]Cf. [7], p. 31.

Hence, his last claim is, strictly speaking, false: this theorem can be proved by purely arithmetical means. Still, this is quite hard to do, and Barrow can certainly not be blamed for not having grasped this possibility. A way to get a proof is to rely on a beautiful result obtained by Srinivasa Ramanujan[21] that provides a general expression for the sum $\sum_{i=0}^{n} \sqrt{i}$, for any value of n. I expose this proof in the annex 1.

If no general expression for the sum $\sum_{i=0}^{n} \sqrt{i}$ is known, there is no way to prove Barrow's theorem in arithmetical terms. The more natural and easy non arithmetical way to prove it is through an argument based on an appropriate integration. In the annex 2, I show how this argument works.

The geometric proof suggested by Barrow is, in a sense, an informal version of such an argument. Here is his how he presents it[22]:

> For if the Diameter of any Parabola be conceived to be divided indefinitely into many equal Parts, then the Right Lines which are ordinately applied to the Diameter, through the Points of the Divisions[23], will proceed in the same Ratio, as is shewn in Geometry: But the Parabola which is constituted of these, whether Right Lines or Parallelograms, is there also demonstrated to be Subsesquialter to the Parallelogram, upon the same Base and the same Height, or which is the same thing, to the Sum composed of as many Right Lines or Parallelograms equal to the greatest: From whence, the Agreement of Arithmetic with Geometry being supposed which we desire to advance, it plainly follows, that a Series of Numbers of this sort is Subsesquialter the Sum of as many equal to the greatest.

Supposing that ω is an infinite (integer) number, the theorem we have stated before in the form (1) could be stated as follows

$$\frac{\sum_{i=0}^{\omega} \sqrt{i}}{(\omega + 1)\sqrt{\omega}} = \frac{2}{3}, \tag{2}$$

[21]Cf. [33]. Ramanujan's short note contains a more general result than the one that this proof appeals to. This last result is stated in the first part of section 1: less that ten lines of text.

[22]Cf. [7], p. 31.

[23]Kirkby's translation omits here a parenthesis: (vel parallelogramma que altra illis insistentia)". Barrow seems not to be willing, here, to take a definite decision about the dimension of the indivisibles he is using. This is, in fact, an irrelevant question, for the purpose of the argument he is exposing (hence, I shall not insist on this point either, in my commentaries). Later, namely in lecture IX, he comes back on the question by discussing both the notion of indivisible and the grounds of the method of indivisibles, and arguing that the former is rightful insofar as indivisibles are taken not to be homogenous with the magnitudes they are the indivisibles of and these magnitudes are not supposed to be composed by them, but the latter is correct insofar as indivisibles are replaced by infinitesimals, which are, instead, homogenous parts of the relevant magnitudes. Cf. on this matter [21], pp. 75-81.

which seems to me to be closer to Barrow's conceptions.

To prove that it is so, Barrow suggested inverting the type of arguments that Wallis had relied on, in his *Arithmetica infinitorum*[24], in order to justify his method of quadrature. In the case of the parabola[25], Wallis' argument depended on the preliminary admission of the arithmetic equality

$$\frac{\sum_{i=0}^{h} i^2}{(h+1)\,h^2} = \frac{1}{3} + \frac{1}{6h}. \tag{3}$$

By supposing that the (finite) sums entering the ratio figuring in the left-hand side of this equality are replaced by "infinite series", he got, then,

$$\frac{\sum_{i=0}^{\omega} i^2}{(\omega+1)\,\omega^2} = \frac{1}{3}, \tag{4}$$

where ω is an infinite number[26]. This was, for him, a sufficient basis for concluding that[27] the ratio between the curvilinear triangle OMA and the parallelogram OPMA is that of 1 to 3, supposing that OM is an arc of parabola of vertex O, OP is a portion of the axis of this same parabola, and OA is tangent to OM at O.

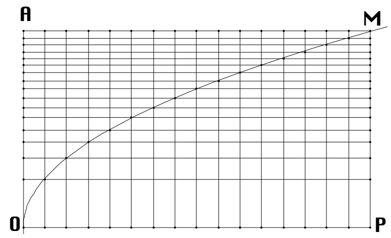
The reason he advanced for justifying this conclusion was that the segments parallel to OP, drawn from OA up to OM (and thus their projections over OP), are to the segments parallel to OA, drawn from OP up to OM "in ratione duplicata" (and the segments parallel to OA, drawn from OP up to OM—and thus their projections over OA—are to the segments parallel to OP, drawn from OA up to OM "in subduplicata ratione"), and that the curvilinear triangle OMA "consists of" the former, whereas the parallelogram OPMA "consists of all the segments like OM, equal to the greatest of them."

[24]Cf. [40] and [28], ch. 1. Similar methods were also imagined and employed by Fermat, Mengoli, Pascal, and Roberval: cf.: [44], p. 319; [5], pp. 150-156; [20], p. 36; and [24], pp. 259 and 265-268.

[25]Cf. [40], propp. XIX-XXIII, pp. 15-18.

[26]As a matter of fact, Wallis did not write any equality like (4). He simply claimed [cf. [40], prop. XXI, p. 17] that: "Si pro ponatur series infinita Quantitatum in dupplicata ratione arithmetice-proportionaliū [...] continue crescentium, a puncto seu 0 inchoatarum; erit illa ad seriem totidem maximaæ æqualium, ut 1 ad 3." Considering the simpler case of the ratio $\frac{\sum_{i=0}^{h} i}{(h+1)h}$ (which is easily proved to be equal to $\frac{1}{2}$, for any value of h), Wallis was a little bit more explicit about the nature of his sums [cf. [40], prop. II, p. 2] : "Si sumatur series Quantitatum Arithmeticè proportionalium [...] continuè crescentium, a puncto vel 0 inchoatarum, & numero quidem vel finitarum vel infinitarum (nulla enim discriminis causa erit,) erit illa ad seriem totidem maximaæ æqualium, ut 1 ad 2." It seems thus that Wallis was willing to avoid any explicit discussion about the difference between the finitary and the infinitary cases of his equalities, by presenting the latter as quite natural (that is, unproblematic) extensions of the former. My mere replacement of "h" with "ω" is just intended to reflect this attitude.

[27]Cf. [40], propp. XXIII, pp. 17-18.



Wallis' method had thus three main components. In the case of the parabola, these are the following: the establishment of the equality (3) and the infinitary extension of this result enabling one to get the equality (4); the identification of the curvilinear triangle OPM and the parallelogram OPMA—at least when their ratio was concerned—with appropriate infinite aggregates of parallel segments; the assimilation of the ratio of these aggregates of segments with the ratio of appropriate series of numbers[28].

Barrow's argument shares the last two components of Wallis' method, but, by inverting the direction of deduction, replaces the first one with the admission of a previous quadrature of the parabola. Though he did not mention his sources for this last result, he would have certainly mentioned the proposition 24 of Archimedes' *Quadrature of the parabola*, if the question had been asked of him. Two years after the publication of Barrow's *Mathematical Lectures*, in 1685, the English (and first) edition of Wallis's *Algebra* was published, and there, the latter came back to his method of quadrature[29], by ascribing to the same Archimedes the crucial result (3), that he identified with the content of the proposition 10 of the treatise *On Spiral*. Moreover, he appropriately accommodated Archimedes' proof of such a proposition so to get a perfectly rigorous proof, by *reducio ad absurdum*, of this arithmetical result, and maintained that analogous proofs could also be given for the corresponding results entering the other applications of his method of quadrature contained in the *Arithmetica infinitorum*.

[28]As it is clearly shown by the quotations included in the previous footnote (26), Wallis claimed that the equality (4), as well as the corresponding equality $\frac{\sum_{i=0}^{\omega} i}{(\omega+1)\omega} = \frac{1}{2}$, hold for any series of quantities in double or arithmetical proportion, respectively. This is quite obvious, since, for any exponent n and any quantity α, one has:

$$\frac{\sum_{i=0}^{h} (i\alpha)^n}{(h+1)\,(h\alpha)^n} = \frac{\alpha^n \sum_{i=0}^{h} i^n}{\alpha^n\,(h+1)\,h^n} = \frac{\sum_{i=0}^{h} i^n}{(h+1)\,h^n},$$

and the problem of determining the first of these ratios reduces to that of calculating the last one. The crucial point in Wallis' argument is not concerned, however, with the simple and quite obvious equality of a ratio of series of segments (that is, magnitudes) and a ratio of series of numbers, but rather with the assimilation of these ratios with a ratio of infinite aggregates of segments, each of which constitutes a bidimensional figure. As a matter of fact, this is a sort of analogy between an arithmetical and a geometrical relation.

[29]Cf. [42], pp. 298-304.

Though this last claim is highly questionable (at least if possibility is understood as an effective one, with respect to the actual capacities of early-modern mathematicians), Wallis's appeal to the authority of Archimedes would have been, in itself, sufficient for denying that Barrow's argument could provide a geometric justification for the equality (3) that were somehow more appropriate than any arithmetical justification.

But, as a matter of fact, this is not what Barrow claimed. His point was subtler. He rather claimed that the interpretation of a ratio of arithmetical sums as a ratio of geometric figures is appropriate to show that the equality (4) is equivalent to the equality (2). In other words, Barrow's point seems to be the following: once this interpretation is admitted and the quadrature of the parabola is obtained, the passage to arithmetic can be realized in two trivially equivalent ways: either by remarking that the arithmetical ratio which corresponds to the ratio of the curvilinear triangle OMA and the parallelogram OPMA—according to a partition of these figures in their components parallel to the axis OP—is $\frac{1}{3}$, or by remarking that the arithmetical ratio corresponding to the ratio of the curvilinear triangle OPM and the parallelogram OPMA —according to a partition of these figures in their components parallel to the tangent OA—is $\frac{2}{3}$. The triviality of this equivalence is the gain obtained by the involvement of geometry that Barrow seems to underline.

This is an effective gain, indeed: so effective that Wallis himself had surreptitiously taken advantage of it, by claiming, in the *Arithmetica infinitorum*[30], that, provided that the "complement of the semi-parabola" (that is, OMA) is to the parallelogram OPMA as 1 to 3, "consequently, the semi-parabola itself is to the same parallelogram as 2 to 3."

But is this gain enough to support the thesis of a subsumption of the domain of arithmetic under that of geometry? Three points should be made here.

Firstly, one should observe that, without the help of some version of the *calculus*—which Barrow did not know in 1664 and that could hardly be understood as a natural extension of classical geometry—a similar gain could not be but a local one, since it is quite hard to generalize Archimedes' quadrature of the parabola with the mere tools of classical geometry. This simple remark seems to me strong enough to show that Barrow's argument was, from the point of view of the internal evolution of mathematics, completely out of place.

Secondly, it is easy to remark that, in getting the equality (2) by relying on the quadrature of the parabola, rather than on an infinitary extension of an appropriate finite equality analogous to the equality (3)—that is, of an equality equivalent to the equality (27), providing Ramanujan's expression for the sum $\sum_{i=0}^{n}\sqrt{i}$, for any value of n—Barrow's argument does not provide any clue as to such a finite equality. Certainly, as already said, no one could blame Barrow for not having got Ramanujan's expression. Still, his very argument shows that geometry—at least if it is used the way he suggested—is not powerful enough to lead to any arithmetical result that one could be willing to obtain.

[30]Cf. [40], propp. XXIII, pp. 17.

Thirdly, and mainly, I think: one cannot forget that Barrow's argument shares the second and third main components of Wallis' method. So the question is obvious: how could one pretend to have offered an argument for the subsumption of the domain of arithmetic under that of geometry, by relying on an argument based on the assimilation of the ratio of some infinite aggregates of segments with the ratio of some series of numbers? This assimilation could, indeed, be read both to show that geometry provides a model for the mutual relations between the terms of an arithmetical sum, and to show that the relevant features of this model are also the features of an arithmetical ratio. This is a general problem concerning Barrow's thesis: the claim that arithmetic is nothing but an appropriate language to speak about (geometric) magnitudes seems to be nothing but the reverse of the claim that arithmetic is the pure and more abstract expression of the relations that (geometric) magnitudes comply with. Thus, many reasons advanced to support the former claim can, quite easily, be reversed so as to produce reasons to support the latter. This is, in any case, what happens with Barrow's argument that we are discussing. Barrow himself implicitly acknowledges this, when he remarks that the equality (4) follows from the quadrature of the parabola, provided that "the Agreement of Arithmetic with Geometry [...][is] supposed." His argument is, essentially, the same as that of Wallis: whereas Barrow relies on it for supporting his thesis of a subsumption of the domain of arithmetic under that of geometry, Wallis relies on it for founding an arithmetical method of quadrature. But for this argument to hold, the agreement of arithmetic with geometry has to be supposed, and there is no way to decide, on the basis of this argument, whether such an agreement has to be understood as a reduction of the former to the latter, or of the latter to the former, or also as nothing but a local fruitful cooperation.

Thus, all that Barrow could have pretended to show with his example, in the perspective of the internal evolution of mathematics, is that, in the case under examination, the geometric understanding of the relevant relations—which is a particular form of functional understanding—is able to make manifest the equivalence of the equalities (4) and (2). This is because this understanding allows us to grasp, so to speak, the hidden linearity that is responsible for this equivalence.

Still, Barrow's point is open to another interpretation: not a mathematical one, but rather an outright philosophical one. When Wallis' deduction of the quadrature of the parabola based on the equality (4) is compared with Barrow's inverse deduction of the equality (2) based on this same quadrature, in the light of the conceptual tools of the Aristotelian philosophy of mathematics—rather than of the conceptual tools of mathematical practice—an asymmetry appears. The sort of infinity involved in the equality (2) is actual infinity by addition; the sort of infinity involved in the quadrature of the parabola is potential infinity by division. In the first case, infinity is an attribute of an object—the infinite sum—that appears as being constituted by an infinity of parts. In the second case, infinity is an attribute of a sequence of possible acts which take as their substratum a continuous object given beforehand. Whereas, in the first case, the arithmetical theorem to be proved on a purely arithmetical basis is appropriately expressed by the very equality (2), in the second case, this same theorem is more appropriately expressed by an equality like

$$\frac{\sum_{i=0}^{n} \sqrt{i\varepsilon}}{(n+1)} = \frac{2}{3}, \tag{5}$$

where ε is an infinitely small portion of OP which is supposed to be equal to $\frac{1}{n}$ of it (so that n becomes infinite).

I suggest that the only point that Barrow's example can support is that his own deduction of the equality (2), based of the quadrature of the parabola is preferable over Wallis inverse deduction since the latter form of infinity is a legitimate one, whereas the former is not, or, better, since the former form becomes legitimate only if it is understood as an expression of the latter: if a model for it is provided, where the infinity appears to be a potential infinity by division. This is a classical thesis, an Aristotelian thesis, in fact. Moreover, it is a thesis that—sometimes asserted, sometimes denied—appears at different historical junctures. But this is a philosophical thesis: a thesis that has often gone together with an assessment of some mathematical theories, or that has even suggested or promoted the establishment of some of them, but that has never provided the relevant conceptual tools to help mathematics to progress[31].

3 Motion and geometric series

I come now to the second example[32]. Here it is:

> *The Sum of a Series of Numbers infinitely decreasing in a triple Ratio [better: "The Series or Sum of Numbers infinitely decreasing continually from Unity (included) in a triple ratio"] (i.e. as* 1, $\frac{1}{3}$, $\frac{1}{9}$, $\frac{1}{27}$, *&c.) till the last or least Term be nothing is to Unity as* 3 *to* 2*; or the same Sum excluding Unity is equal to* $\frac{1}{2}$ [better: "the half of the Unity"]. This Theorem (which for Easiness and Perspicuity sake, I have proposed in a determinate, *viz.* a triple Ratio) although it may be demonstrated Universally, yet I think, by making Use of continued Quantity, it will be shewn much more clearly and expeditiously, at least more handsomely and elegantly. [...] This Theorem universally is as follows. *The Sum of any Series of Numbers [better: "The Series of Numbers"] continually decreasing from Unity infinitely, or to Nothing, in any Proportion, will be to Unity, as the Antecedent, or greater Term of the Proportion, is to the Excess of the Antecedent above the Consequent. Or the*

[31] In the particular case under examination, this is confirmed by the fact that, in the absence of a functional notion of limit and/or of any version of the calculus, the difference between the equalities (2) and (5) is perfectly ineffective.

[32] Cf. [7], pp. 31-33. The brackets include some suggestions for slight corrections of Kirkby's translation.

> *same Sum, excluding Unity, will be to Unity, as the Consequent, or lesser Term of the Proportion, is to the Excess of the Antecedent above the Consequent.*[...] So easily are Arithmetical conclusions, (otherwise sufficiently intricate and difficult to be investigated) drawn from the Consideration of Geometry. Thus doth the one Science require the help of the other, and after a friendly sort conspire together.

Barrow's claim about the virtue of a geometric proof (or, as he said, of a proof "making Use of continued Quantity", that is, magnitudes) is, in this case, much weaker than in the case of the first example: far from maintaining that the arithmetical theorem under consideration cannot be proved in purely arithmetical terms, he argued that geometry makes it possible to prove such a theorem "much more clearly and expeditiously, at least more handsomely and elegantly", and that any other different proof (that is, any arithmetical one) is "sufficiently intricate and difficult". Such a cautious attitude was imposed by the fact that Barrow was probably aware that, at the time of his lectures, the theorem had been already proved arithmetically at least once, namely by Viète, in a treatise first published in 1593, and reprinted in 1646 within Van Schooten's edition of Viète's *Opera Mathematica*[33]. But, judged from a strictly mathematical point of view, Barrow's attitude is not cautious enough, however, since Viète's proof—or proofs, since, as we shall see later, one could recognize two distinct proofs of the same theorem in Viète's text—are far from being "intricate and difficult", and could be hardy considered as less clear, compact and elegant than Barrow's one. Moreover, the same theorem can also be proved—and was actually proved, after 1664, by many mathematicians—in purely arithmetical, or at least in non-geometric terms, according to quite simple arguments, different from Viète's, that Barrow probably became aware of, or, in any case, would have certainly easily understood. Thus, also in this case, if we want to assign to Barrow's example some argumentative force, we need to consider it from a different point of view.

3.1 Barrow's proof

Let us begin with the geometric proof he suggested. This is, in fact, a double one, since it concerns successively the particular case where the base and the ratio of the geometric series are 1 and $\frac{1}{3}$, respectively, and the general case.

In the particular case, the theorem can be stated as follows, in modern notation:

$$1+\frac{1}{3}+\frac{1}{9}+\frac{1}{27}+\ldots=\sum_{i=0}^{\infty}\left(\frac{1}{3}\right)^i=\frac{3}{2}, \quad \text{that is,} \quad \frac{1}{3}+\frac{1}{9}+\frac{1}{27}+\ldots=\sum_{i=1}^{\infty}\left(\frac{1}{3}\right)^i=\frac{1}{2}. \qquad (6)$$

[33]Cf. [38], ch. XVII, and [39], pp. 397-398. The conjecture that Barrow knew of Viète's proof is supported by the fact that Van Schooten's edition was part of Barrow's library at the time of his death, in 1677, and was probably there also in 1664: cf. [14] , pp. 368 and 336, respectively.

In the general case, this becomes, of course:

$$1 + x + x^2 + x^3 + \ldots = \sum_{i=0}^{\infty} x^i = \frac{1}{1-x}\ , \quad \text{that is,} \quad x + x^2 + x^3 + \ldots = \sum_{i=0}^{\infty} x^i = \frac{x}{1-x}, \quad (7)$$

where one should implicitly suppose that $x < 1$. This can also be written in an even more general form, at least apparently. It is enough to replace the unity with any number α, as the base or first term of the series, and to suppose that the second term is any other number β, so that the ratio—or "proportion", as Barrow said—is $\frac{\beta}{\alpha}$. One gets, then:

$$\alpha + \beta + \frac{\beta^2}{\alpha} + \frac{\beta^3}{\alpha^2} + \ldots = \sum_{i=0}^{\infty} \frac{\beta^i}{\alpha^{i-1}} = \frac{\alpha^2}{\alpha - \beta}, \quad (8)$$

where one should implicitly suppose that $\beta < \alpha$.

Barrow's proof relies on the consideration of a geometrical model associated with the Achilles paradox, that I shall call, for short, "Achilles paradox model". Here is this proof[34]:

> Suppose the movable point A to be carried through the Right Line AZ, with an uniform Motion: Also imagine the point E to be moved uniformly through the same, but with a Velocity subtriple the Velocity wherewith the Point A is carried; then, in whatsoever time the Point A runs through the Right Line AE in the same time will the Point E run through a third part of AE, suppose EF. Also in whatsoever time the former Point runs through the Right Line EF, in the same time will the latter run through the third Part of EF, suppose FG, and so on, *ad infinitum*, till the Point A overtake E in Æ. Now if we suppose the Line AE to be the Unity, then will EF be $\frac{1}{3}$; FG $\frac{1}{9}$, and GH $\frac{1}{27}$, &c. *ad infinitum*, according to the Hypothesis; but it appears that the line A[[Æ]] run through by A is triple the Line EÆ which is run through by E in the same time, because A is supposed to move with three times the Velocity of E. Therefore EÆ is to AE[[, hoc est series decrescens, exclusâ unitate, ad unitatem, ut 1 ad 2: Et AÆ se habet ad AE]] (*i. e.* the decreasing

[34]Cf. [7], pp. 32-33. The simple brackets include some suggestions for slight corrections of Kirkby's translation. The double brackets include some other corrections relative to three typographical errors in the English translation of Kirkby (only one of which also appears in the original Latin text [cf. [9], pp. 36-37): in the first case the English text has "E", instead of "Æ" that correctly occurs in the original text; in the second case the part in double brackets, that occurs in the original text, is omitted; in the third case, both the English and the original texts have "AE : EÆ :: $R - S : S$", instead of "AÆ : EÆ :: $R : S$", as would be correct (this typographical error is corrected in [8] , p. 49).

Series including Unity is to Unity) as 3 to 2. Q. E. D. [...] For the foregoing Premisses being supposed, let the Ratio be given between R and S, and let AE represent [better: "be"] Unity, as above; then because $\mathsf{A\AE}$ (*i. e.* $\mathsf{AE}+\mathsf{E\AE}$) : $\mathsf{E\AE}$ :: R : S. therefore (by dividing the Proportion) AE : $\mathsf{E\AE}$ :: $R-S$: S. *i. e.* Unity is to the Sum of the proposed Series [better: "to the proposed Series"], as the Excess of the Terms of the Proportion is to the lesser Term: And again, because [[$\mathsf{A\AE}$: $\mathsf{E\AE}$ :: R : S]]. therefore $\mathsf{A\AE}$: AE :: R : $R-S$, by Conversion, *i. e.* the proposed Sum [better: "the proposed Series"] including Unity, is to Unity, as the greater Term of the Proportion is to its Excess above the lesser.

Let us suppose two "movable points" to be moved with uniform rectilinear motion on a straight line AZ, starting from two fixed points A and E. Let us also suppose that the velocity of the first of these points is the velocity of the second as 3 to 1, in the particular case, or as R to S ($S < R$), in the general case. Let us finally suppose that the first point reaches the second at $\mathsf{\AE}$. It follows that

$$\mathsf{A\AE} : \mathsf{E\AE} = 3 : 1 \quad \text{or} \quad \mathsf{A\AE} : \mathsf{E\AE} = R : S, \tag{9}$$

and, as $\mathsf{AE} = \mathsf{A\AE} - \mathsf{E\AE}$,

$$\mathsf{A\AE} : \mathsf{AE} = 3 : 2 \qquad \text{and} \qquad \mathsf{E\AE} : \mathsf{AE} = 1 : 2,$$

or

$$\mathsf{A\AE} : \mathsf{AE} = R : R - S \qquad \text{and} \qquad \mathsf{E\AE} : \mathsf{AE} = S : R - S.$$

But, to come to $\mathsf{\AE}$, the two points pass through the points E, F, G, H,... and F, G, H,..., respectively, where they are at the same time. Thus

$$\begin{gathered} \mathsf{AE} : \mathsf{EF} = \mathsf{EF} : \mathsf{FG} = \mathsf{FG} : \mathsf{GH} = \ldots = 3 : 1 \\ \text{or} \\ \mathsf{AE} : \mathsf{EF} = \mathsf{EF} : \mathsf{FG} = \mathsf{FG} : \mathsf{GH} = \ldots = R : S, \end{gathered} \tag{10}$$

and hence:

$$\begin{aligned} \mathsf{A\AE} \quad &= \mathsf{AE} + \mathsf{EF} + \mathsf{FG} + \mathsf{GH} + \ldots &&= \mathsf{AE} + \frac{1}{3}\mathsf{AE} + \frac{1}{3}\mathsf{EF} + \frac{1}{3}\mathsf{FG} + \ldots \\ & &&= \mathsf{AE} + \frac{1}{3}\mathsf{AE} + \frac{1}{3}\frac{1}{3}\mathsf{AE} + \frac{1}{3}\frac{1}{3}\frac{1}{3}\mathsf{AE} + \ldots \end{aligned} \tag{11}$$

or

$$\begin{aligned} \mathsf{A\AE} \quad &= \mathsf{AE} + \mathsf{EF} + \mathsf{FG} + \mathsf{GH} + \ldots &&= \mathsf{AE} + \frac{S}{R}\mathsf{AE} + \frac{S}{R}\mathsf{EF} + \frac{S}{R}\mathsf{FG} + \ldots \\ & &&= \mathsf{AE} + \frac{S}{R}\mathsf{AE} + \frac{S}{R}\frac{S}{R}\mathsf{AE} + \frac{S}{R}\frac{S}{R}\frac{S}{R}\mathsf{AE} + \ldots \end{aligned}. \tag{12}$$

It is thus enough to suppose that AE is, or represents unity, to conclude that

$$1 + \frac{1}{3} + \frac{1}{9} + \frac{1}{27} + \ldots = \frac{3}{2} \qquad \text{and} \qquad \frac{1}{3} + \frac{1}{9} + \frac{1}{27} = \frac{1}{2}, \tag{13}$$

or

$$1 + \frac{S}{R} + \left(\frac{S}{R}\right)^2 + \left(\frac{S}{R}\right)^3 + \ldots = \frac{R}{R-S} \qquad \text{and} \qquad \frac{S}{R} + \left(\frac{S}{R}\right)^2 + \left(\frac{S}{R}\right)^3 + \ldots = \frac{S}{R-S}, \tag{14}$$

according to (6) and (7), respectively.

If one supposes, instead, that $\mathsf{A\AE}$, AE, and EF are or represent any triple of quantities Z, α, and β (such that $\beta < \alpha < Z$), one gets:

$$Z : Z - \alpha = \alpha : \beta,$$

instead of (9), and hence, because of (10):

$$Z = \frac{\alpha^2}{\alpha - \beta} = \alpha + \beta + \frac{\beta^2}{\alpha} + \frac{\beta^3}{\alpha^2} + \ldots, \tag{15}$$

according to (8). This is just what Barrow remarks in a short, purely formalistic, footnote inserted after the claim that the theorem "may be demonstrated Universally[35]". The footnote specifies how it might be done. Here is this footnote:

> Thus:
>
> $$\begin{aligned} &Z - o : Z - \alpha :: \alpha : \beta. \\ &Z\beta = Z\alpha - \alpha\alpha. \\ &\alpha\alpha = Z\alpha - Z\beta. \\ &\frac{\alpha\alpha}{\alpha - \beta} = Z. \end{aligned}$$

3.2 Is Barrow's proof essentially geometric?

Barrow's proof is quite easy and, of course, it is right. But, as already said, an equally easy arithmetical proof for the same theorem was quite certainly known by Barrow himself. Before considering the latter in detail, one should answer a preliminary question concerning the former: is this a genuine geometric proof? Of course, what is relevant is not whether it is geometric or kinematic (since we can, for our purpose, admit Barrow's view, according to which kinematics—or at least the small fragment of kinematics that is used in this proof—and geometry do not essentially differ). The point is rather whether its geometric features

[35] Cf. [7], pp. 32.

are essential for the success of the argument, and whether these depend or not on more fundamental arithmetic conditions.

The Achilles paradox model plays a double role in Barrow's proof. Firstly, it provides a model for the geometric series. This is not only a geometric model. It is also, so to speak, a phenomenal one: it includes both an infinite partition of a finite segment, and an interpretation of this segment and its partition as geometric representations of a physical phenomenon. The model is thus strong enough to ensure that, in so far as the ratio of a geometric series is smaller than 1 (or, more generally of the ratio that any quantity has with itself), this series has a (finite) sum, and to represent this sum under the form of an already given segment: the segment AÆ. The proof of the theorem is thus reduced to the solution of a problem: to determine the quantitative relations that this segment has with its parts under the relevant partition. Secondly, the Achilles paradox model suggests the way to solve this problem, since it goes together with the establishment of the proportions (9). These proportions being given, and the interpretation of the geometric series provided by this model being admitted, the solution of the problem is indeed a trivial exercise.

Now, let us suppose that the existence of a (finite) sum for a geometric series is admitted on different and independent bases. Of course, this is not the same as supposing that this sum is known. But this sum is not known under the interpretation provided by the Achilles paradox model either. It is represented by a segment, but this segment is, in fact, unknown. To trace such a segment and take it as if it were known, as Barrow suggests, is thus part of an analytic practice. A similar practice can also be applied to the (finite) sum of a geometric series that, according to our supposition, has been admitted to exist: let U be this sum. We shall have:

$$1+\frac{1}{3}+\frac{1}{9}+\frac{1}{27}+\ldots=U \qquad \text{or} \qquad 1+\frac{S}{R}+\left(\frac{S}{R}\right)^2+\left(\frac{S}{R}\right)^3+\ldots=U. \tag{16}$$

As a matter of fact, this is a sufficient premise to prove the theorem in a purely arithmetic way, since from it, it follows that:

$$\begin{aligned} U-1 &= \frac{1}{3}+\frac{1}{9}+\frac{1}{27}+\ldots \\ &= \frac{1}{3}\left(1+\frac{1}{3}+\frac{1}{9}+\ldots\right) \\ &= \frac{1}{3}U \end{aligned} \qquad \text{or} \qquad \begin{aligned} U-1 &= \frac{S}{R}+\left(\frac{S}{R}\right)^2+\left(\frac{S}{R}\right)^3+\ldots \\ &= \frac{S}{R}\left(1+\left(\frac{S}{R}\right)+\left(\frac{S}{R}\right)^2+\ldots\right) \\ &= \frac{S}{R}U \end{aligned}$$

and thus

$$\frac{2}{3}U=1 \qquad \text{or} \qquad \frac{R-S}{R}U=1,$$

according to (13) and (14), respectively.

The equalities $U-1=\frac{1}{3}U$ and $U-1=\frac{S}{R}U$ are equivalent, of course, to the proportions

$$U:U-1=3:1 \qquad \text{and} \qquad U=U-1=R:S,$$

which are, in turn, equivalent to the proportions (9). On the other hand, the equalities (11) and (12) add no information which supplements that in the equalities (16): they simply correspond to these last equalities under the interpretation provided by the Achilles paradox model. It follows that, once the existence of a (finite) sum for the geometric series is admitted without relying on the Achilles paradox model—that is, when other and independent reasons for supporting this admission are provided—the theorem can be proved, in purely arithmetical terms, according to a line of argumentation that parallels Barrow's geometric argument. The second role played in this proof by the Achilles paradox model is thus far from essential: the proof can be repeated, *mutatis mutandis*, without relying on such a setting relative to this role, provided that the existence of a (finite) sum for the geometric series is admitted on independent bases.

On the other hand, one could wonder whether the admission of the existence of a sum for a geometric series and its expression by a term like "U" are essential for conducting the previous proof. It is easy to understand that they are not. It is rather enough that a geometric series itself is handled as a (finite) quantity. If this is admitted, the proof can be rephrased as follows:

$$\left[\begin{array}{ll} 1+\dfrac{1}{3}+\dfrac{1}{9}+\dfrac{1}{27}+\ldots & =1+\left[\dfrac{1}{3}+\dfrac{1}{9}+\dfrac{1}{27}+\ldots\right] \\ & =1+\dfrac{1}{3}\left[1+\dfrac{1}{3}+\dfrac{1}{9}+\ldots\right] \end{array}\right] \Rightarrow \frac{2}{3}\left[1+\frac{1}{3}+\frac{1}{9}+\frac{1}{27}+\ldots\right]=1$$

or

$$\left[\begin{array}{ll} 1+\dfrac{S}{R}+\left(\dfrac{S}{R}\right)^2+\left(\dfrac{S}{R}\right)^3+\ldots & =1+\left[\dfrac{S}{R}+\left(\dfrac{S}{R}\right)^2+\left(\dfrac{S}{R}\right)^3+\ldots\right] \\ & =1+\dfrac{S}{R}\left[1+\dfrac{S}{R}+\left(\dfrac{S}{R}\right)^2+\ldots\right] \end{array}\right] \Rightarrow$$
$$\Rightarrow \frac{R-S}{R}\left[1+\frac{S}{R}+\left(\frac{S}{R}\right)^2+\left(\frac{S}{R}\right)^3+\ldots\right]=1.$$

Barrow's proof is thus, as a matter of fact, a geometric version of a purely arithmetical proof based on the supposition that a geometric series has a (finite) sum or can, at least, be handled as a (finite) quantity: the result of an interpretation of such a proof according to a model as vivid as the Achilles paradox one. This interpretation is certainly elegant and heuristically evocative, but it has no relevant mathematical function in the proof of the theorem other than that of warranting the existence of a (finite) sum of a geometric series or, at least, the possibility of handling this series as a (finite) quantity.

Still, it openly suggests a generalization of this theorem to any sort of quantity, namely its extension to magnitudes. Under this extension, this theorem ceases to be an arithmetical theorem and cannot, of course, have a purely arithmetical proof. The equivalence between Barrow's proof, relying on the Achilles paradox model, and its underlying arithmetical proof has, thus, an important mathematical function: that of showing which operational properties a domain of magnitudes has to satisfy for the sum of a geometric series to be definable and provably determinable on such a domain. Both the notation that Barrow uses in the general version of his proof, and the short, purely formalistic text of his footnote that provides a proof of the equality (15) suggest that Barrow was willing to implicitly point out such a possible extension. The symbols "R", "S", "Z", "α", and "β" in the context of these proofs, have, indeed, a quite natural interpretation as symbols designating any sort of quantity, namely numbers or continuous magnitudes: note that, whereas Barrow writes the fraction "$\frac{1}{3}$", giving the ratio of the geometric series in the particular case, he does not write the corresponding fractions "$\frac{S}{R}$", and "$\frac{\alpha}{\beta}$ ", and, when he writes the fraction "$\frac{\alpha\alpha}{\alpha-\beta}$", he is, quite probably, as we shall see, quoting Viète implicitly.

But, if the Achilles paradox model is not understood as a geometric model of a configuration of arithmetical objects, that is, numbers, but rather as a kinematic interpretation of the geometric configuration attached to a geometric series defined on the domain of segments—the domain where any relation between magnitudes was usually represented in the modern age—, then it ceases to be a basis for a geometric reformulation of an arithmetical proof of an arithmetical theorem. It rather becomes a basis for interpreting a general theorem concerning any sort of quantity, and specially magnitudes, on the domain of segments. In 1664, two possible contexts for stating a similar theorem, and proving it, were available[36]: Viète's common algebra, where multiplication and division were axiomatically defined on quantities of any sort; and Descartes' geometric algebra, where multiplication and division were explicitly defined on magnitudes by relying on appropriate proportions. In both cases, far from being reduced to geometry, arithmetic provided an explicit model for a formalism to be used to make geometry in a new way. Thus, although it suggests the possibility of extending the theorem that Barrow explicitly refers only to numbers also to magnitudes, Barrow's geometric proof of this theorem is quite far from supporting the thesis of a subsumption of the domain of arithmetic under that of geometry.

From these considerations it follows that such a proof can be understood as essentially geometric only in so far as the appeal to a geometric model—like the Achilles paradox one—is conceived as the only way to warrant the existence of a (finite) sum for a geometric series, or, at least, to license the manipulation of such a series as a (finite) quantity. Were one

[36]Cf. [28], Introduction, and p. 3 above.

ready to admit the existence of such a sum or the possibility of a similar manipulation of a geometric series, this proof should not be understood as essentially geometric. Moreover, its reformulation as a purely arithmetical proof is enough to show that it is not clearer, more compact and elegant than any arithmetical proof.

3.3 Viète's proof

This last claim is also confirmed by the consideration of Viète's proofs[37].

Chapter XVII of Viète's *Variorum de rebus mathematicis responsorum, liber VIII*[38] has a quite explicit title: "Progressio Geometrica". It is a very short chapter, since, as Viète said, the whole theory of geometric progressions can be summed up in only one theorem, together with four simple corollaries that, by alluding to Euclid's *Data*[39], Viète calls "*δεδομένα*"[40]:

> Progressionis Geometricæ doctrina uno ferè absoluitor Theoremate, diductis videlicet ex eo quatuor *δεδομένοις*.

Here is this theorem, as it appears in Van Schooten's edition of Viète's *Opera Mathematica*[41], where Barrow probably read it[42]:

> Theorema. Si fuerint magnitudines continue proportionales: erit ut terminus rationis major ad terminum rationis minorem, ut differentia compositæ ex omnibus & minimæ ad differentiam compositæ ex omnibus & maximæ.
>
> Sint magnitudines continue proportionales, quarum maxima sit D, minima X, & composita ex omnibus F, & sit ratio majoris ad minorem sicut D ad B. Dico esse ut D ad B, ita F minus X ad F minus D.

In Viète's original treatise, the first part of the theorem is different[43]:

> Si fuerint magnitudines continuè proportionales, Erit ut terminus rationis maior ad terminum rationis minorem, ita composita ex omnibus ad differentiam compositæ ex omnibus & maximæ.

The difference is all the more surprising as it does not repeat itself in the second part of the theorem, that, in the original text of Viète, is thus inconsistent with the first part.

[37] My attention was first drawn to this proof by Jean Dhombres who—in his [11], pp. 149-154—has suggested it provides an example of a "suspended analysis".

[38] Cf. [38], pp. 28*v*-29*r*, and [39], pp. 397-398.

[39] Cf. [11], p. 151.

[40] Cf. [38], p. 28*v*, and [39], p. 397.

[41] Cf. [39], p. 397.

[42] Cf. [39], p. 397. Cf. also the previous footnote (33).

[43] Cf. [38], p. 28*v*.

In Viète's notation, D and X are, respectively, the first and the last term of the progression, which are also the greater and the smaller, since the progression is taken to be decreasing; F is its sum, and its ratio is that of D to B, so that B is the second term. Hence, in Van Schooten's edition, the first and second part of the theorem agree in stating that

$$D : B = F - X : F - D. \tag{17}$$

It seems thus, under this version of Viète's statement, that the progression is finite, and is, thus, not yet a series, in our sense. The existence of its sum F would then be unproblematic, and Viète's theorem would amount to an obvious consequence of the conjunction of the propositions V.12 and VII.12 of the *Elements*, since it seems obvious that Viète included (positive integer) numbers among what he called "magnitudes", in the statement of the theorem.

Here are Euclid's propositions, in Heath's translation[44]:

> [V.12] If any number of magnitudes be proportional, as one of the antecedents is to one of the consequents, so will all the antecedents be to all the consequents.
>
> [VII.12] If there be as many numbers as we please in proportion, then, as one of the antecedents is to one of the consequents, so are all the antecedents to all the consequents.

Using indexes, and supposing that α_i and β_i $(i = 1, 2, \dots, n)$ are $2n$ quantities such that

$$\alpha_1 : \beta_1 = \alpha_2 : \beta_2 = \alpha_3 : \beta_3 = \ldots = \alpha_{n-1} : \beta_{n-1} = \alpha_n : \beta_n, \tag{18}$$

these propositions assert that

$$(\alpha_1 + \alpha_2 + \alpha_3 + \ldots + \alpha_{n-1} + \alpha_n) : (\beta_1 + \beta_2 + \beta_3 + \ldots + \beta_{n-1} + \beta_n) = \alpha_1 : \beta_1, \tag{19}$$

provided that $\alpha_1 : \beta_1 = \alpha_\lambda : \beta_\lambda$, for any λ such that $1 \leq \lambda \leq n$. Euclid's proofs rely on his definitions of proportion between magnitudes and (positive integer) numbers—the very well known definitions V.5 and VII.20—and to appropriate linearity properties of magnitudes and (positive integer) numbers, established in propositions V.1, VII.5 and VII.6, that is:

$$\text{if } a = ha', \; b = hb', \; \ldots z = hz', \text{ then } a + b + \ldots + z = h\left(a' + b' + \ldots + z'\right)$$

and

$$\text{if } h\alpha_1 = k\beta_1 \text{ and } h\alpha_2 = k\beta_2, \text{ then } h\left(\alpha_1 + \alpha_2\right) = k\left(\beta_1 + \beta_2\right),$$

where a, b,..., z, and a', b',..., z' are whatever magnitudes and h and k, are whatever (positive integer) numbers. In both cases the proof is immediate. Supposing that the fundamental

[44]Cf. [12], vol. 2, pp. 159 and 312.

properties of proportions are admitted, the same propositions can, on the other hand, be proved by reiterating the following argument as many times as necessary[45]:

$$\begin{array}{llll} \text{if} & \alpha_1:\beta_1=\alpha_2:\beta_2 & \text{then} & \alpha_1:\alpha_2=\beta_1:\beta_2, \\ & & \text{and hence} & (\alpha_1+\alpha_2):\alpha_1=(\beta_1+\beta_2):\beta_2, \\ & & \text{that is,} & (\alpha_1+\alpha_2):(\beta_1+\beta_2)=\alpha_1:\beta_2. \end{array}$$

Whatever proof one adopts, the implication 'if the multiple proportion (18) holds, then the (simple) proportion (19) holds' is quite unproblematic in the tradition of Euclid's mathematics. To draw, from it, the proportion (17), is then enough to suppose that $\alpha_{i+1}=\beta_i$ $(i=1,2,\ldots,n-1)$, then replace α_1 with D, $\beta_1=\alpha_2$ with B, and β_n with X. Under these replacements, from the continuous proportion

$$D:B=B:\beta_2=\beta_2:\beta_3=\ldots=\beta_{n-2}:\beta_{n-1}=\beta_{n-1}:X,$$

one gets, indeed, according to this implication:

$$(D+B+\beta_2+\ldots+\beta_{n-2}+\beta_{n-1}):(B+\beta_2+\ldots+\beta_{n-2}+\beta_{n-1}+\beta_n)=D:B,$$

that is just the proportion (17), provided that

$$D+B+\beta_2+\ldots+\beta_{n-2}+\beta_{n-1}+\beta_n=F.$$

The second part of Viète's theorem—or this whole theorem, in Van Schooten's version—is thus a triviality in the tradition of Euclid's mathematics. Viète's new algebraic formalism could have allowed to add very little to such a triviality: just an explicit way to denote the terms of the progression included between D and X, so as to write the geometric progression under the form

$$D+B+\frac{B^2}{D}+\frac{B^3}{D^2}+\ldots+\frac{B^n}{D^{n-1}},$$

where $\frac{B^n}{D^{n-1}}=X$, and thus to express, openly, the fact that these terms are in a continuous proportion to each other[46].

It is plausible to guess that Van Schooten's version of the theorem depends on nothing but a correction of a banal material mistake made by Viète himself because of his eagerness

[45] But remark that this argument appeals to the proposition V.16, which asserts that if $\alpha_1:\beta_1=\alpha_2:\beta_2$, then $\alpha_1:\alpha_2=\beta_1:\beta_2$, and is proved, in turn, by Euclid, by appealing to V.12.

[46] But remark that, once the progression is so written, there is room to prove Viète's theorem in an alternative way, by observing that

$$D+B+\frac{B^2}{D}+\frac{B^3}{D^2}+\ldots+\frac{B^n}{D^{n-1}}=\frac{D^n+D^{n-1}B+D^{n-2}B^2+\ldots B^n}{D^{n-1}},$$

and

$$D^{n+1}-B^{n+1}=(D-B)\left(D^n+D^{n-1}B+\ldots+DB^{n-1}+B^n\right),$$

to state his own relevant addition to the previous Euclidean result. If it were so, there would be no reason to be surprised that Viète offered no proof of his theorem, and that he used his algebraic formalism only to draw from the proportion (17) the announced corollaries, that, according to such a formalism, immediately follow from this theorem, whatever the nature of the quantities D, B, and X. Here are these corollaries[47]:

$$\begin{aligned} &i) \quad F = \frac{D^2 - BX}{D - B}; \\ &ii) \quad X = \frac{BF + D^2 - DF}{B}; \\ &iii) \quad B = \frac{DF - D^2}{F - X}; \\ &iv) \quad DF - D^2 = BF - BX. \end{aligned} \tag{20}$$

Of course, it is the first corollary that mainly interests us. If one adopts Viète's formalism to express the terms of the geometric proportion, and writes "$\frac{B^n}{D^{n-1}}$" in place of "X", one gets from it the following equality:

$$D + B + \frac{B^2}{D} + \frac{B^3}{D^2} + \ldots + \frac{B^n}{D^{n-1}} = \frac{D^2 - \frac{B^{n+1}}{D^{n-1}}}{D - B}.$$

Though there is no doubt that Viète understood his progression as decreasing (supposing that $B < D$), this equality holds for any geometric progression whose ratio differs from unity (that is, also if $D < B$). Still, whereas in the second case, as the term $X = \frac{B^{n+1}}{D^{n-1}}$ progressively increases, the sum $F = \frac{D^2 - \frac{B^{n+1}}{D^{n-1}}}{D-B}$ becomes infinite with n, in the first case, since the term $X = \frac{B^{n+1}}{D^{n-1}}$ progressively decreases, when n becomes infinite such a term vanishes and the sum $F = \frac{D^2 - \frac{B^{n+1}}{D^{n-1}}}{D-B}$ is then transformed in the finite ratio $\frac{D^2}{D-B}$. This is just what Viète remarked in the second part of his chapter, and possibly had in his mind when he wrote, unadvisedly, the first part of his theorem as it appears in the original edition of his treatise.

---

so that:

$$D + B + \frac{B^2}{D} + \frac{B^3}{D^2} + \ldots + \frac{B^n}{D^{n-1}} = \frac{D^{n+1} - B^{n+1}}{(D - B)\, D^{n-1}} = \frac{D^2 - B\frac{B^n}{D^{n-1}}}{D - B}.$$

It is perhaps of some interest to remark that the second of the previous equalities is the fundamental equality of Sluse's method of tangents, a method that Sluse was probably elaborating at the beginning of the sixties and that he exposed, some years later in two letters that were published in the *Philosophical Transactions*: cf. [35] and [36].

[47] Cf.[38], p. 29*r*, and [39], p. 397. Of course, Viète writes "$\sim$ *quadratum*" or "$\sim$ *quadrato*" (abridged in "$\sim$ *quad.*" in [39]) where we write "$\sim^2$" and "$\sim$ *in* $\sim$" where we write "$\sim\sim$". This apart, his notation is the same than ours. In [39] there is a typographical error in the first corollary, since "*B quad.*" appears in place of "*D quad.*"

Here is what he wrote[48]:

> An verò cum magnitudines sunt continuè proportionales in infinitum, abibit X in nihilum. Et evanescere asserent Mechanici, cum minima quantitas subsit tantum intellectu.
>
> *Itaque erit secundum eos. Ut differentia terminorum rationis ad terminum rationis majorem, ita maxima ad compositam ex omibus.*

There is no doubt that Viète was arguing that, when the geometric progression proceeds to infinity, the proportion (17) reduces to

$$D - B : D = D : F_\infty, \tag{21}$$

where F_∞ is just the sum of the geometric progression continued to infinity, that is, the geometric series[49], and there is also no doubt that he was admitting that a possible argument for this claim is that, when the progression proceeds to infinity, X vanishes. If one puts, indeed, $X = 0$ and $F = F_\infty$, in the proportion (17) and in the equality (20.i), one gets, respectively:

$$D : B = F_\infty : F_\infty - D \quad \text{and} \quad F_\infty = \frac{D^2}{D-B},$$

which are both equivalent to the proportion (21). It is sufficient to replace F_∞, D, and B with Z, α, and β, to understand that Viète has thus provided a proof of the equality (15), and proved Barrow's theorem, in the general case where the terms of the geometric series are any sort of quantity.

But it is also clear that, for Viète, this argument did not have the same nature as an "analytic" one. Rather it was a "mechanical" argument, that is—if I understand well—an argument based, merely, on successive approximate estimates. Thus, Viète considered it necessary to offer another argument for the same conclusion.

Here is what he wrote, firstly[50]:

> *Cum alioquin esset, Ut differentia terminorum rationis ad terminum rationis minorem, ita minima ad crementum.*
>
> *Et ut differentia terminorum rationis ad terminorum rationis maiorem, ita maxima ad compositam ex omnibus plus cremento.*

According to Dhombres[51], the first of these two proportions provides a definition for the "increment" relative to the last term of the progression, that is, X. The second is, instead, a

[48]Cf. [38], p. 29r, and [39], p. 398.
[49]The symbol "F_∞" is of course not used by Viète. I introduce it for simplicity.
[50]Cf. [38], p. 29r, and [39], p. 398.
[51]Cf. [11], p. 152.

consequence of the equality (20.i), together with this last definition. If the increment Δ_X (or Δ_n, if one prefers, provided that $X = \frac{B^{n+1}}{D^{n-1}}$) is so defined that

$$D - B : B = X : \Delta_X, \tag{22}$$

from the equality (20.i) it follows that

$$F = \frac{D^2}{D-B} - \frac{BX}{D-B} = \frac{D^2}{D-B} - \Delta_X \tag{23}$$

that is:

$$D - B : D = D : F + \Delta_X,$$

according to the second proportion. The proportion (21) follows from this last proportion, provided that Δ_X vanishes with X. This last condition is certainly necessary for the proportion (22) to hold, in this case. But to rely on it in order to prove the proportion (21) is the same as relying on the supposition that the proportion (22) continues to hold when X vanishes, and thus, implicitly, to the supposition that X vanishes when the progression proceeds to infinity. Hence, if this were Viète's argument, it would be as little "analytic" as the previous one.

But this is not Viète's argument. Here is what he wrote after the passage quoted before[52]:

> *Sint magnitudines proportionales in continuâ ratione subquadruplâ εἰς ἄπειρον, & sit maxima omnium* 3. *Composita ex omnibus fiet* 4. *Neque enim magnitudinibus illis in continuâ ratione subquadruplâ existentibus quarum maxima est* 3 *tantula potest addi, quin composita sit major* 4. *Eoque pertinet quadratio Paraboles Archimedæa.*

This could appear to be an argument by authority. Still, it is not so. Certainly Viète was referring here to a particular result obtained by Archimedes in the *Quadrature of the Parabola*, namely to the propositions 23 and 24 of this treatise[53]. But he was also, and overall, implicitly suggesting a possible application—or better, a generalization—of Archimedes' proof by exhaustion, to the general case of any (decreasing) geometric series.

In the proposition 23 of his treatise, Archimedes had proved a particular case of the equality (20.i), namely that

$$A + B + C + \ldots + Y + Z + \frac{1}{3}Z = \frac{4}{3}A, \tag{24}$$

where A, B, ..., Z are any number of magnitudes that are to each other as 4 to 1, that is, $B = \frac{1}{4}A$, $C = \frac{1}{4}B$, ..., $Z = \frac{1}{4}Y$. Archimedes' proof is quite different from the previous

[52]Cf. [38], p. 29r, and [39], p. 398.

[53]For short, I adopt here Heath's transcription of Archimedes' results and arguments: cf. [1], pp. 249-252. For the Greek text and its translation in Latin and French, cf. [3], vol. II, pp. 310-315 and [2], vol. II, pp. 192-195.

one based on the propositions V.12 and VII.12 of the *Elements*. Though finitary, of course, it is rather similar, in spirit, to the arithmetical version of Barrow's proof, which I have reconstructed above. Suppose that $b = \frac{1}{3}B$, $c = \frac{1}{3}C$, ..., $z = \frac{1}{3}Z$, so that

$$B + b = \frac{1}{3}A, \quad C + c = \frac{1}{3}B, \quad \ldots \quad Z + z = \frac{1}{3}Y;$$

it follows that

$$B + C + \ldots + Y + Z + b + c + \ldots + y + z = \frac{1}{3}(A + B + \ldots + Y)$$

and

$$b + c + \ldots + y = \frac{1}{3}(B + C + \ldots + Y),$$

and thus, by subtraction:

$$B + C + \ldots + Y + Z + z = \frac{1}{3}A,$$

which is equivalent to the equality (24).

This last equality provides a lemma for Archimedes' proof of the proposition 24. There, Archimedes considers a sequence of triangles, A, B, ..., Y, Z so constructed inside a segment of a parabola (the figure delimited by the parabola itself and any one of its chords, parallel to the tangent at its vertex) that $B = \frac{1}{4}A$, $C = \frac{1}{4}B$, ..., $Z = \frac{1}{4}Y$, and such that their sum continuously approaches such a segment from below. He proves, then, by exhaustion, that the segment of parabola is to the first of these triangles as 4 to 3, that is, it is equal to $\frac{4}{3}A$. The proof goes as follows. Suppose that the segment of parabola, let us say Π[54], is greater than $\frac{4}{3}A$. It would then be possible to take so many triangles A, B, ..., Z so that the difference $\Pi - \frac{4}{3}A + \frac{1}{3}Z$ between the segment of parabola and their sum becomes smaller than the difference between Π and $\frac{4}{3}A$, which is impossible since, whatever Z may be, the inequality

$$\Pi - \frac{4}{3}A + \frac{1}{3}Z > \Pi - \frac{4}{3}A.$$

holds true. Suppose, then, that Π is smaller than $\frac{4}{3}A$. It would then be possible to take so many triangles A, B, ..., Z so that Z—and thus, *a fortiori*, $\frac{1}{3}Z$—becomes smaller than the difference between $\frac{4}{3}A$ and Π, which is impossible since from $\frac{1}{3}Z < \frac{4}{3}A - \Pi$, it follows that

$$A + B + \ldots + Z = \frac{4}{3}A - \frac{1}{3}Z > \Pi,$$

against the supposition that the sum $A + B + \ldots + Z$ approaches Π from below.

[54] The symbol "Π" does not appear in Archimedes' text. I introduce it for simplicity.

Supposing that in the equality (23), one replaces D with A, B with $\frac{1}{4}A$ and X with Z, one gets:

$$F = \frac{A^2}{A - \frac{1}{4}A} - \frac{\frac{1}{4}AZ}{A - \frac{1}{4}A} = \frac{4}{3}A - \frac{1}{3}Z = \frac{4}{3}A - \Delta_Z.$$

Archimedes' equality (24) is thus consistent with Viète's equalities ($20.i$) and (23), and the term $\frac{1}{3}Z$ which appears in the first of these equalities is nothing but the particular value of Δ_X for $\frac{D}{B} = \frac{1}{4}$ and $X = Z$.

It is thus quite natural to understand Viète's mention of Archimedes' result as a sketch of a possible generalization of Archimedes' proof by exhaustion. This would simply work as follows. Suppose that, when the progression proceeds to infinity, its sum F_∞ is greater than $\frac{D^2}{D-B}$. As $\frac{B}{D-B}$ is constant and $\Delta_X = \frac{B}{D-B}X$, it would then be possible to take so many terms $D, B, \ldots, X$ so that the difference $F_\infty - \frac{D^2}{D-B} + \Delta_X$ between the total sum F_∞ and the partial sum $F = \frac{D^2}{D-B} - \Delta_X$ is smaller than the difference between F_∞ and $\frac{D^2}{D-B}$, which is impossible since, whatever Δ_X may be, the inequality

$$F_\infty - \frac{D^2}{D-B} + \Delta_X > F_\infty - \frac{D^2}{D-B}.$$

holds true. Suppose, then, that F_∞ is smaller than $\frac{D^2}{D-B}$. It would then be possible—again, since $\frac{B}{D-B}$ is constant and $\Delta_X = \frac{B}{D-B}X$—to take so many terms $D, B, \ldots, X$ so that Δ_X is smaller than the difference between $\frac{D^2}{D-B}$ and F_∞, which is impossible since from $\Delta_X < \frac{D^2}{D-B} - F_\infty$, it follows that

$$D + B + \ldots + X = F = \frac{D^2}{D-B} - \Delta_X > F_\infty.$$

against the supposition that the series $A + B + \ldots + Z$ is decreasing.

Though Archimedes' proof by exhaustion is certainly geometric, the generalization of it that Viète seems to suggest is not, since it simply relies on the relations between the terms of any geometric series and is based on the equality (23) that is proved, both by Viète and Archimedes, without relying on any geometric argument.

Now, the relevant question is: is this second proof advanced by Viète essentially different from the first—mechanical—one? From our point of view it seems to be so, since it seems to be a rudimentary version of a ε-δ argument. But our point of view could not have been that of Viète: whereas we see in this proof the announcement of something that will come later, Viète could only see a tentative generalization of a particular proof by exhaustion, concerning a particular geometric construction. So it is far from sure that Viète could have seen what, to us, makes the crucial difference between his first and his second argument. And the conclusive phrase of his chapter confirms that he had some doubts about the fact that, unlike the former, the latter was "analytic"[55]:

> Sed vix adsentientur Platonici, cum ipsa omnis Geometria sit intellectu.

Still, this is not a reason to conclude that Barrow could have had some reason to claim that, once restricted to the case where the terms of the geometric series are numbers, Viète's proofs are not purely arithmetic. If one had argued that it is so for this proof is infinitary, one should also have maintained that, for the very same reason, Barrow's theorem itself is not arithmetic. But it is hard to think that Barrow would have advanced a similar argument. The quite schematic proof that he offers for the equality (15), in the footnote we have quoted before is, indeed, nothing but a sketch of the first of Viète's proofs, since the proportion

$$Z - o : Z - \alpha : \; : \alpha : \beta$$

is, manifestly, a rewriting of Viète's proportion (17) with o in place of X[56]. On the other hand, it would have been quite hard to argue that, from a mathematical point of view, Viète's proofs are less simple, clear, compact and elegant than Barrow's one. If I have taken so much time to reconstruct them, it is, merely, because I have tried to make manifest their classical—that is, Euclidean and Archimedean—sources. And certainly Barrow would have not considered the classical argument I have mentioned as less virtuous than his own[57].

Then, where could Barrow have seen the advantages of his own proof over Viète's? The only possible answer that I see is that he could have maintained that the Achilles paradox model supplied Viète's implicit generalization of Archimedes' particular proof of the proposition 24 of the *Quadrature of the Parabola* with, so to speak, a concrete framework, similar to that which the segment of parabola and the sequence of triangles constructed inside it supplies Archimedes' proof with. But the need for a similar framework only arises if the sort of infinity involved both in Viète's first proof and in his implicit generalization of Archimedes' proof is seen with suspicion. And one can take this need to be satisfied by the Achilles paradox model only if one considers that the sort of infinity involved in this model is essentially different than the first one.

Thus, we come back to a similar conclusion as that we had reached in section 3.2: the only reason that Barrow could have had to prefer his own proof over Viète's is the same as the only one he could have relied on in order to maintain that his proof is essentially geometric. This

[55]Cf. [38], p. 29r, and [39], p. 398. Remember that Viète was agreeing with a traditional belief, quite widespread in his times, according to which the origins of analysis, as an argumentative form, went back to Plato: cf. [37], p. 4.

[56]Notice the symbol "o", probably used so as to allude to the fact that the quantity so denoted is vanishing (unfortunately, in [8], p. 48, this symbol is replaced with "0").

[57]A proof of that, if it were necessary, is that Archimedes' quadrature of the parabola is just the crucial result on which Barrow's proof of his first theorem is founded.

reason would have just been the following: only the Achilles paradox model, or some similar geometric models, could license the sort of infinitary argument involved both in the arithmetic proofs expounded in section 3.2 and in Viète's.

Mutatis mutandis, this is the same philosophical point that Barrow's first example seems to be concerned with: where potential infinity by division is legitimate, actual infinity by addition is not, or better, the latter becomes legitimate only if a model for it is provided, with respect to which it appears to be merely an expression of a potential infinity by division. This last point is explicitly made by Barrow himself, with reference to arithmetical series, in his ninth lecture[58]:

> Nay it is plainly taught and demonstrated by Arithmeticians, that an infinite Series of Fractions decreasing in a certain Proportion, is equal to a certain Number, or to Unite [*sic*], or to a part of an Unity; *ex. gr.* that such a Series of Fractions decreasing in a *subsesquialter Proportion* is equal to *Two*, in *subduble Proportion* to *Unity*, in a *subtriple* to *one half*; from whence it is not inconsistent for something finite to contain in it an Infinity of Parts; especially since nothing agrees with Number, which does not with more right with Magnitude, which Number represents and denominates.

As Barrow's reference to arithmeticians reveals, the opposition between this potential conception of infinity and an actual one has nothing to do, as such, with the subsumption of the domain of arithmetic under that of geometry.

3.4 Two last proofs

My argument could proceed further, by mentioning several other non explicitly geometric proofs of Barrow's theorem that, though possibly not known to him in 1664, became so later, or would have been easily understood by him. I shall limit myself to two of them.

The first one—of which Barrow certainly became aware in the second half of the sixties—depends on the so-called "method of division of Mercator". This name depends on the fact that the first printed source where it is exposed is Mercator's *Logarithmotechnia*, published in 1668[59]. Still, Newton used it, in 1664 and 1665, in the context of his own reformulation of Wallis' method of quadrature[60].

The proposition 2 included in a note that Newton wrote in Cambridge, in the winter 1664-1665, after his second reading of Wallis's *Arithmetica infinitorm*[61], consists of the following equality:

$$\frac{aa}{b+x} = \frac{aa}{b} - \frac{aax}{bb} + \frac{aaxx}{b^3} - \frac{aax^3}{b^4} + \frac{aax^4}{b^5} - \frac{aax^5}{b^6} + \frac{aax^6}{b^7} + \&c. \tag{25}$$

[58]Cf. [7], p. 157.
[59]Cf. [25], par. XV, pp. 29-30.
[60]Cf. [28], ch. 4.
[61]Cf. [27], vol. 1, **1**, 3, § 4, [2], pp. 126-134, specially p. 127.

This is the first statement of a particular case of what would become, quite soon, Newton's binomial theorem. The proof of this equality offered by Newton on such an occasion cannot be reconstructed here. I shall limit myself to remark that it is far from being plain, simple and compact, and is based on a quite conjectural extension of Pascal's triangle or Wallis's table of figurate numbers. Still, Newton comes back to this matter a few months later, presumably at the end of summer 1665, in a sketch of a treatise on quadratures and series expansions[62], whose propositions 7 and 8 consist of the following equalities:

$$\begin{aligned}\overline{a+b}\}^{\frac{m}{n}} \quad &= \quad a^{\frac{m}{n}} + \frac{m}{n}\times\frac{b}{a}\times a^{\frac{m}{n}} + \frac{m}{n}\times\frac{m-n}{2n}\times\frac{bb}{aa}\times a^{\frac{m}{n}} + \\ &\qquad + \frac{m}{n}\times\frac{m-n}{2n}\times\frac{m-2n}{3n}\times\frac{b^3}{a^3}\times a^{\frac{m}{n}}.\ \&c.; \\ \frac{1}{\overline{a+b}\}^{\frac{m}{n}}} \quad &= \quad \frac{1}{a^{\frac{m}{n}}} - \frac{m}{n}\times\frac{b}{a}\times\frac{1}{a^{\frac{m}{n}}} - \frac{m}{n}\times\frac{-m-n}{2n}\times\frac{bb}{aa}\times\frac{1}{a^{\frac{m}{n}}} + \\ &\qquad - \frac{m}{n}\times\frac{-m-n}{2n}\times\frac{-m-2n}{3n}\times\frac{b^3}{a^3}\times\frac{1}{a^{\frac{m}{n}}}.\ \&c.\end{aligned}$$

These equalities represent the first general statement of the binomial theorem[63]. Among other arguments that Newton offers for justifying them—including a quite complex one based, once again, on Pascal's triangle or Wallis's table of figurate numbers—we find the following[64]:

> The truth of these two prop: is also thus demonstrated. If $\overline{a+b}\}^{\frac{-1}{1}} = \frac{1}{a+b}$ I divide 1 by $a+b$ as in decimall fractions & find the quote $\frac{1}{a} - \frac{b}{aa} + \frac{bb}{a^3} - \frac{b^3}{a^4} + \frac{b^4}{a^5}\&c.$ as appeareth also by multiplying both parts by $a+b$. So I extract the rote of a^2+b as if they were decimall numbers & find $\sqrt{a^2+b} = a + \frac{b}{2a} - \frac{bb}{8a^3} + \frac{b^3}{16a^5}\&c$, as also may appear by squareing both parts.

We are, of course, interested in the first of these arguments, specially concerned with the first of the previous equalities. It just relies on Mercator's method. Its idea is simple: just apply to the binomial $a+b$ the usual method to divide 1 by any other number expressed as a sum of two numbers; first, divide 1 by a, multiply the result for $a+b$ and subtract the product from 1, so to get the first remainder, then operate on this remainder as on 1, and continue in this way as far as you like. If one note by "A_i" and "R_i" $(i = 1, 2, \ldots)$ the successive terms of the development of $\frac{1}{a+b}$ and the respective remainders, and take R_1 to be 1, one easily gets:

$$A_i = \frac{R_i}{a} \quad \text{and} \quad R_{i+1} = R_i - A_i\,(a+b)\,,$$

[62]Cf. [27], vol. 1, 2, 5, § 1, [6], pp. 318-321, specially p. 321.
[63]Cf. [27], vol. 1, p. 321, note (111).
[64]Cf. [27], vol. 1, **2**, 5, § 1, [6], p. 321.

that is

$$
\begin{aligned}
A_1 = \frac{R_1}{a} = \frac{1}{a} \quad &; \quad R_2 = R_1 - A_1\,(a+b) = -\frac{b}{a} \\
A_2 = \frac{R_2}{a} = -\frac{b}{a^2} \quad &; \quad R_3 = R_2 - A_2\,(a+b) = \frac{b^2}{a^2} \\
A_3 = \frac{R_3}{a} = \frac{b^2}{a^3} \quad &; \quad R_4 = R_3 - A_3\,(a+b) = -\frac{b^3}{a^3} \\
\dots \quad & \quad \dots \\
A_n = \frac{R_n}{a} = -(-)^n \frac{b^{n-1}}{a^n} \quad &; \quad R_{n+1} = (-)^n \frac{b^n}{a^n} \\
\dots \quad & \quad \dots,
\end{aligned}
$$

or

$$
\frac{1}{a+b} = \frac{1}{a} - \frac{b}{a^2} + \frac{b^2}{a^3} - \dots - (-)^i \frac{b^{i-1}}{a^i} + \dots \tag{26}
$$

To get the equality (25) it is then enough to operate appropriate substitutions and multiplications. In the same way, by replacing, in the equality (26), a with α and b with $-\beta$, then by multiplying for α^2, one gets the equality (8), that is thus proved through an argument whose arithmetical origins are evident.

If Barrow did not know this argument in 1664, he certainly became aware of it later on, since Newton exposed it, in a quite general form, in the *De analysi*[65], a text that Barrow himself sent to Collins on July 31st, 1669[66].

Another possible, non explicitly geometric, proof of Barrow's theorem could be done by relying on the so-called method of indeterminate coefficients. This method was exposed, in its finitary form, in Descartes' *Geometry*[67], where it was used to solve the problem of tangents. Later on, and especially after its massive use by Newton, the infinitary version of this method became a crucial tool of the analysis. Such a method can be quite easily applied to the problem of looking for the sum of $\frac{\alpha^2}{\alpha-\beta}$. Suppose that:

$$
\frac{\alpha^2}{\alpha-\beta} = A + B\beta + C\beta^2 + D\beta^3 + \dots,
$$

where A, B, C, D, $\dots$ are indeterminate coefficients to be determined. Then

$$
\begin{aligned}
\alpha^2 &= \left(A + B\beta + C\beta^2 + D\beta^3 + \dots\right)(\alpha-\beta) \\
&= A\alpha + B\alpha\beta + C\alpha\beta^2 + D\alpha\beta^3 + \dots - A\beta - B\beta^2 - C\beta^3 - D\beta^4 - \dots,
\end{aligned}
$$

and thus, by separately equating the coefficients of the successive powers of β to zero:

[65] Cf. [27], vol. II, **2**, 3, pp. 212-214.

[66] Cf. [26], vol. I, p. 14 and [27], vol. II, **2**, *Introduction*, note (11), p. 166.

[67] Cf. [10], p. 347.

$$\begin{aligned}
\alpha^2 - A\alpha = 0 \quad &\rightarrow \quad A = \alpha, \\
A - B\alpha = 0 \quad &\rightarrow \quad B = \tfrac{A}{\alpha} = 1, \\
B - C\,\alpha = 0 \quad &\rightarrow \quad C = \tfrac{B}{\alpha} = \tfrac{1}{\alpha}, \\
C - D\alpha = 0 \quad &\rightarrow \quad D = \tfrac{C}{\alpha} = \tfrac{1}{\alpha^2}, \\
\ldots \qquad &\qquad \ldots,
\end{aligned}$$

according to the equality (15).

Both these proofs have a double disadvantage compared with on Viète's and Barrow's: they require a previous conjecture about the sum of the geometric series, that is, they have no heuristic power; and they do not provide any argument for warranting the infinite reiteration of the procedure on which they depend. The second one supposes, moreover, that the ratio $\frac{\alpha^2}{\alpha-\beta}$ has a unique expansion in a power series and that this power series can be manipulated term by term. If Barrow had known them when he gave his *Mathematical Lectures*, he could have advanced these reasons to prefer his own proof to them. But, if so, he would have implicitly opposed an ideal of perspicuity of mathematical arguments to an ideal of extension of mathematical techniques, rather than showing any reason for the domain of arithmetic to be subsumed under that of geometry.

4 Conclusions

The consideration of Barrow's examples should show that, when compared with the actual state of mathematical research in the middle of the 17th century, his opposition of geometry and arithmetic is out of place. These examples could have only been used in order to support the thesis according to which only a geometrical interpretation of infitary arguments would be able to license these same arguments, since, as Aristotle had already argued, the only legitimate sort of infinity would be the infinity of the division of an already given magnitude.

Still, the second example, namely the proof that Barrow offered for the theorem with which this example is concerned, possibly had an unexpected influence on the evolution of geometry. In a note he wrote at the time of his priority quarrel with Leibniz, probably in 1714, Newton claimed that "Dr Barrows Lectures might put [...][him] upon considering the generation of figures by motion[68]". Though he immediately added that he was not able to remember it exactly, this statement should be enough to attest that Newton followed at least some of Barrow's *Mathematical Lectures*, namely the parts of them dealing with a kinematic approach to geometry, and thus, quite probably, the lecture where Barrow exposed his second example.

[68]Cf. [27], vol. 1, **2**, 5, § 4, footnote (4), p. 344. Cf. also: *ibid.*, vol. 1, **2**, *Introduction*, App. 1, p. 150.

It is thus possible to conjecture that this example had some influence on the development of Newton's ideas about geometry. This is in any case what Hofmann has suggested[69].

In my view, a crucial idea that Newton could have drawn from Barrow's proof is that of associating to a pair of segments, supposed to be generated in equal time by a moving point, the ratio of the speeds of the generating motions. This is more than merely admitting that a segment is generated through motion, which was quite a widespread conception in the middle of the 17th century. It rather entails that the speed of this motion is an essential feature to be considered in geometry. This is no more a philosophical option. It is rather a view on the intrinsic mathematical nature of geometric objects and their relations that constituted one of the crucial elements on which Newton's theory of fluxions was founded.

5 Annex 1: An arithmetical proof of Barrow's first theorem through a result of Ramanujan

Ramanujan's result is the following:

$$\sum_{i=1}^{n} \sqrt{i} = C + \left[\frac{2}{3}n + \frac{1}{2}\right]\sqrt{n} + \frac{1}{6}\sum_{j=0}^{\infty}\left[\sqrt{n+j} + \sqrt{n+j+1}\right]^{-3}, \tag{27}$$

where C is an appropriate constant.

To prove it, Ramanujan relies on the definition of the following function $\phi : \mathbb{N}^{+} \to \mathbb{R}$:

$$\phi(n) = \sum_{i=1}^{n} \sqrt{i} - \left(C + \frac{2}{3}n\sqrt{n} + \frac{1}{2}\sqrt{n}\right) - \frac{1}{6}\sum_{j=0}^{\infty}\left[\sqrt{n+j} + \sqrt{n+j+1}\right]^{-3}, \tag{28}$$

where C is such that

$$\phi(1) = 1 - C - \frac{2}{3} - \frac{1}{2} - \frac{1}{6}\sum_{j=0}^{\infty}\left[\sqrt{1+j} + \sqrt{2+j}\right]^{-3} = 0. \tag{29}$$

This is a finite constant, since

$$\sum_{j=0}^{\infty}\left[\sqrt{1+j} + \sqrt{2+j}\right]^{-3}$$

converges.

[69] Cf. [16], p. 115. Notice however that Whiteside has remarked that Newton could have also drawn his ideas about the generation of geometric objects by motion from some other sources [cf. [27], vol. 1, **2**, 5, § 4, footnote (4), p. 344]. Possibly, Newton's sources were many, but Barrow's example was probably one of them.

Once the function (28) is defined, the proof reduces to a quite easy calculation that Barrow could have performed himself. Thus, its difficulty consists in imagining of the appropriate form of the function $\phi(n)$: only the extraordinary numerical sense of someone like Ramanujan could have had this insight.

The first step in this calculation is the establishment of the following equality:

$$\phi(n)-\phi(n+1)=-\sqrt{n+1}+\frac{2}{3}\left[(n+1)\sqrt{n+1}-n\sqrt{n}\right]+\frac{1}{2}\left[\sqrt{n+1}-\sqrt{n}\right]+\frac{1}{6}\left[\sqrt{n}-\sqrt{n+1}\right]^{3}. \tag{30}$$

This is an immediate consequence of the other equality

$$\frac{1}{(a+b)^{\nu}}=\frac{(a-b)^{\nu}}{(a^2-b^2)^{\nu}},$$

that holds for any ν, a and b. It is enough, indeed, to put $\nu=3$, $a=\sqrt{n}$ and $b=\sqrt{n+1}$, for getting:

$$\frac{1}{\left(\sqrt{n}+\sqrt{n+1}\right)^{3}}=-\left(\sqrt{n}-\sqrt{n+1}\right)^{3}. \tag{31}$$

From (30), it is then easy to draw, by a simple calculation, that

$$\phi(n)-\phi(n+1)=0.$$

And, as $\phi(1)=0$, this entails that $\phi(n)=0$, for any (positive natural) n, which is equivalent to the equality (1)[70].

Once this last equality is admitted, to prove Barrow's first theorem it is enough to remark that from it and the other equality (31), it follows that

$$\frac{\sum_{i=1}^{n}\sqrt{i}}{(n+1)\sqrt{n}}=\frac{C}{(n+1)\sqrt{n}}+\frac{4n+3}{6(n+1)}-\frac{1}{6}\sum_{j=0}^{\infty}\frac{\left[\sqrt{n+j}-\sqrt{n+j+1}\right]^{3}}{(n+1)\sqrt{n}}$$

[70] Let us note that from (29) and (31), it follows that:

$$\begin{aligned}
C &= -\frac{1}{6}\left[1-\sum_{j=0}^{\infty}\left[\sqrt{1+j}-\sqrt{2+j}\right]^{3}\right]\\
&= -\frac{1}{6}\left[1-\left[\sqrt{1}-\sqrt{2}\right]^{3}-\left[\sqrt{2}-\sqrt{3}\right]^{3}-\left[\sqrt{3}-\sqrt{4}\right]^{3}-\ldots\right]\\
&= -\frac{1}{6}\left[1-\left[7-5\sqrt{2}\right]-\left[11\sqrt{2}-9\sqrt{3}\right]-\left[15\sqrt{3}-13\sqrt{4}\right]-\ldots\right]\\
&= 1+\sqrt{2}+\sqrt{3}+\ldots+\sqrt{n}-\frac{1+4n}{6}\sqrt{n+1}+\ldots
\end{aligned}$$

and thus[71]:

$$\begin{aligned}
\lim_{n\to\infty}\frac{\sum\limits_{i=0}^{n}\sqrt{i}}{(n+1)\sqrt{n}} &= \lim_{n\to\infty}\left[\frac{C}{(n+1)\sqrt{n}}+\frac{4n+3}{6(n+1)}-\frac{1}{6}\sum_{j=0}^{\infty}\frac{\left[\sqrt{n+j}-\sqrt{n+j+1}\right]^3}{(n+1)\sqrt{n}}\right] \\
&= \lim_{n\to\infty}\frac{C}{(n+1)\sqrt{n}}+\lim_{n\to\infty}\frac{4n+3}{6(n+1)}-\sum_{j=0}^{\infty}\lim_{n\to\infty}\frac{\left[\sqrt{n+j}-\sqrt{n+j+1}\right]^3}{6(n+1)\sqrt{n}} \\
&= 0+\frac{2}{3}-0-0-0-\ldots=\frac{2}{3}.
\end{aligned}$$

6 Annex 2: A proof of Barrow's first theorem through integration

According to the Cauchy-Riemann definition of a definite integral, one gets:

$$\lim_{\varepsilon\to 0}\sum_{i=1}^{n}\varepsilon\sqrt{i\varepsilon}=\int_0^1\sqrt{x}dx.$$

If one supposes that $\varepsilon=\frac{1}{n}$, it follows that

$$\lim_{n\to\infty}\sum_{i=1}^{n}\frac{1}{n}\sqrt{\frac{i}{n}}=\lim_{n\to\infty}\sum_{i=1}^{n}\frac{\sqrt{i}}{n\sqrt{n}}=\int_0^1\sqrt{x}dx.$$

As

$$\lim_{n\to\infty}\sum_{i=1}^{n}\frac{\sqrt{i}}{n\sqrt{n}}=\lim_{n\to\infty}\sum_{i=0}^{n}\frac{\sqrt{i}}{n\sqrt{n}}=\lim_{n\to\infty}\sum_{i=0}^{n}\frac{\sqrt{i}}{(n+1)\sqrt{n}},$$

this is the same as:

$$\lim_{n\to\infty}\sum_{i=0}^{n}\frac{\sqrt{i}}{(n+1)\sqrt{n}}=\int_0^1\sqrt{x}dx.$$

---

[71] From the binomial theorem it follows, for any natural j, that

$$\begin{aligned}
\frac{\left[\sqrt{n+j}-\sqrt{n+j+1}\right]^3}{(n+1)\sqrt{n}} &= \frac{[4n+4j+3]\sqrt{n+j}-[4n+4j+1]\sqrt{n+j+1}}{(n+1)\sqrt{n}} \\
&= \frac{[4n+4j+3]\sum\limits_{i=0}^{\infty}\binom{1/2}{i}(\sqrt{n})^{1-2i}j^i-[4n+4j+1]\sum\limits_{i=0}^{\infty}\binom{1/2}{i}(\sqrt{n})^{1-2i}(j+1)^i}{(n+1)\sqrt{n}}
\end{aligned}$$

and so:

$$\lim_{n\to\infty}\frac{\left[\sqrt{n+j}-\sqrt{n+j+1}\right]^3}{(n+1)\sqrt{n}}=\lim_{n\to\infty}\frac{2\sqrt{n}}{(n+1)\sqrt{n}}=\lim_{n\to\infty}\frac{2}{n+1}=0.$$

To prove the equality (1), it is thus enough to remark that

$$\int_0^1 \sqrt{x}dx = \frac{2}{3}\left[x^{\frac{3}{2}}\right]_0^1 = \frac{2}{3}.$$

Notice that the limits $\lim_{\varepsilon\to 0}\sum_{i=1}^{n}\varepsilon\sqrt{i\varepsilon}$, $\lim_{n\to\infty}\sum_{i=1}^{n}\frac{1}{n}\sqrt{\frac{i}{n}}$, $\lim_{n\to\infty}\sum_{i=1}^{n}\frac{\sqrt{i}}{n\sqrt{n}}$, and $\lim_{n\to\infty}\sum_{i=0}^{n}\frac{\sqrt{i}}{(n+1)\sqrt{n}}$ that enter this argument are essentially different from the limit $\lim_{n\to\infty}\frac{C}{(n+1)\sqrt{n}}$, $\lim_{n\to\infty}\frac{4n+3}{6(n+1)}$, and $\lim_{n\to\infty}\frac{1}{6}\sum_{j=0}^{\infty}\frac{\left[\sqrt{n+j}-\sqrt{n+j+1}\right]^3}{(n+1)\sqrt{n}}$ that enter Ramanujan's argument, since the former cannot commute with $\sum$. In the former, this last operator refers, indeed, to the same variable as the limit operator or to a function of this variable. This is a typical feature of the Cauchy-Riemann integral, and it is because of it that the argument based on such an integral is not purely arithmetic, while Ramanujan's one is so.

References

[1] Archimedes. *The Works of Archimedes.* Cambridge University Press, 1897. Edited in modern notation [. . .] by T. L. Heath.

[2] Archimedes. *Œuvres Complètes.* Les Belles Lettres, Paris, 1970-1972 (4 vols.). Texte établi et traduit par C. Mugler.

[3] Archimedes. *Opera Omnia cum Commentariis Eutocii.* Teubner, Stuttgart, 1972-1975 (4 vols.). Interum editit I. L. Heiberg, corrigenda adiecit E. S. Stamatis.

[4] Aristotle. *The complete works of Aristotle. The revised Oxford translation.* Princeton University Press, Princeton, 1984. Edited by J. Barnes.

[5] M. E. Baron. *The Origins of Infinitesimal Calculus.* Pergamon Press, Oxford, 1969.

[6] I. Barrow. *Lectiones Mathematicæ XXIII; Principia Matheseôs generalia exponuntur: Habitæ Cantabrigiæ A. D. 1664, 1665, 1666.* Typis J. Playford, pro G. Wells, in Cœmenterio D. Pauli, Londini, 1684-85. This is a unique volume in three parts with three separate title pages, the first dated 1685, and the two others 1684. The first part (whose title is that given above) contains Barrow's mathematical lectures 9-15 (numerated as I-VII). The second part (whose title is *Mathematicæ Professoris Lucasiani Lectiones Habitæ in Scholis Publicis Academiæ Cantabrigiensis An. Dom. M.DC.LXVI*) contains Barrow's mathematical lectures 16-23 (numerated as I-VIII). The third part (whose title is *Mathematicæ Professoris Lucasiani Lectiones Habitæ in Scholis Publicis Academiæ*

Cantabrigiensis Incerti temporis hæ sunt ultimæ Lectiones, neque aliquem ad annum affixæ) contains four more of Barrow's lectures (numerated as I-IV) on Archimedes' *Sphere and Cylinder*.

[7] I. Barrow. *The Usefulness of Mathematical Learning explained and demonstrated: Being Mathematical Lectures [...]*. S. Austen, London, 1734. Translated by the Rev^d. Mr. J. Kirkby.

[8] I. Barrow. *The Mathematical Works of Isaac Barrow*. Cambridge University Press, Cambridge, 1860. Edited for Trinity College by W. Whewell.

[9] I. Barrow. *Mathematici Professoris Lectiones, pro Anno Millesimo Sexcentesimo Sexagesimo Quarto*. Without publisher (probably printed by J. Playford for G. Wells: cf. [14], p. 79), whithout year (probably 1683: cf. [14], p. 79). The volume contains the *Oratio Præfatoria* and Barrow's mathematical lectures 1-8 (numerated as I-VIII).

[10] R. Descartes. *Discours de la méthode [...]. Plus la Dioptrique.Les Météores. Et la Géométrie qui sont des essaies de cette Méthode*. I. Mirre, Leyde, 1637.

[11] J. Dhombres. The analysis of the synthesis of the analysis... two moments of a chiasmus: Viète and Fourier. In M. Otte and M. Panza, editors, *Analysis and Synthesis in Mathematics. History and Philosophy*, volume 196 of *Boston Studies in the Philosophy of Science*, pages 147–176. Kluwer A. P., Dordrecht, Boston, London, 1997.

[12] Euclid. *The Thirteen Books of the Elements*. Cambridge Univ. Press, Cambridge, 2nd edition, 1926. Translated with introduction and commentary by Sir Thomas L. Heath; 3 vols.

[13] M. Feingold. Isaac Barrow: divine, scholar, mathematician. In M. Feingold, editor, *Before Newton. The life and times of Isaac Barrow*, pages 1–104. Cambridge University Press, 1990.

[14] M. Feingold. Isaac Barrow's library. In M. Feingold, editor, *Before Newton. The life and times of Isaac Barrow*, pages 333–372. Cambridge University Press, 1990.

[15] K. Hill. Neither ancient nor modern: Wallis and Barrow on the composition of continua. *Notes and Records of the Royal Society of London*, 50, 51:165–178 (of vol. 50), and 13–22 (of vol. 51), 1996, 1997.

[16] J. E. Hofmann. *Studien zur Vorgeschichte des Prioritätstreites zwischen Leibniz und Newton [...]*. Number 2 in Abhandlungen der Preussischen Akademie der Wissenschaften. Verlag der Wissenschaften, Berlin, 1943.

[17] D. M. Jesseph. *Squaring the circle: The war between Hobbes and Wallis.* University of Chicago Press, Chicago and London, 1999.

[18] M. S. Mahoney. Barrow's mathematics: between ancients and moderns. In M. Feingold, editor, *Before Newton. The life and times of Isaac Barrow*, pages 179–249. Cambridge University Press, 1990.

[19] L. Maierù. *Fra Descartes e Newton: Isaac Barrow e John Wallis.* Rubbettino, Cosenza, 1994.

[20] A. Malet. *From Indivisibles to Infinitesimals. Studies on Seventeenth-Century Mathematization of Infinitely Small Quantities.* Servei de Publications de la Universitat Autònoma de Barcelona, Bellaterra, 1996.

[21] A. Malet. Barrow, Wallis, and the remaking of seventeenth century indivisibles. *Centaurus*, 39:67–92, 1997.

[22] A. Malet. Isaac Barrow on the mathematization of nature: Theological voluntarism and the rise of geometrical optics. *Journal of the History of Ideas*, 58:265–287, 1997.

[23] P. Mancosu. *Philosophy of Mathematics and Mathematical Practice in the Seventeenth Century.* Oxford Unversity Press, New York, Oxford, 1996.

[24] M. R. Massa. Mengoli on 'quasi proportions'. *Historia Mathematica*, 24:257–280, 1997.

[25] N. Mercator. *Logarithmotechnia; sive Methodus construendi Logarithmos Nova, accuratis & facilis [. . .].* M. Pitt, Londini, 1668.

[26] I. Newton. *The Correspondence of Isaac Newton.* Cambridge University Press, Cambridge, 1959-1977. Edited by H. W. Turnbull. 7 vols.

[27] I. Newton. *The Mathematical Papers of Isaac Newton.* Cambridge University Press, Cambridge, 1967-1981. Edited by D. T. Whiteside (8 vols.).

[28] M. Panza. *Newton et les origines de l'*analyse*: 1664-1666.* Blanchard, Paris, 2005.

[29] M. Panza. 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. *Revue d'Histoire des Mathématiques*, forthcoming.

[30] A. De Pace. *Le matematiche e il mondo: ricerche su un dibattito in Italia nella seconda metà del Cinquecento.* Franco Angeli, Milano, 1999.

[31] H. Pycior. *Symbols, impossible numbers, and geometric entanglements: British algebra through the commentaries on Newton's Universal arithmetick.* Cambridge University Press, Cambridge, 1997.

[32] M. H. Pycior. Mathematics and Philosophy: Wallis, Hobbes, Barrow, and Berkeley. *Journal of the History of Ideas*, 48:265–286, 1987.

[33] S. Ramanujan. On the sum of the square roots of the first n natural numbers. *Journal Indian Mathematical Society*, 7:173–175, 1915. Also in: S. Ramanujan, *Collected Papers*, ed. G. H. Hardy, P. V. Seshu Aiyar, and B. M. Wilson, Cambridge Univ. Press, Cambridge, 1927, pp. 47-49.

[34] C. Sasaki. The acceptance of the theory of proportions in the sixteenth and seventeenth centuries: Barrow's reaction to the analytic mathematics. *Historia Scientiarum*, 29(83):83–116, 1985.

[35] R. F. Sluse. An extract of a letter from the excellent Renatus Franciscus Slusius [...] concerning his short and easie method of drawing tangents to all geometrical curves without any labour of calculation [...]. *Philosophical Transactions*, (90):5143–5146, January 20th 1672/73.

[36] R. F. Sluse. Illustrissimi Slusii modus, quo demonstrat methodus suam ducendi tangentes ad qualibet curvas. *Philosophical Tansactions*, (94):6059, May 19th 1673.

[37] F. Viète. *In artem analiticem isagoge*. Jamet Mettayer, Turonis, 1591. Also in [39], pp. 1-12.

[38] F. Viète. *Variorum de rebus mathematicis responsorum, liber VIII [...]*. apud Iamettium Mettayer Typographum Regium, Turonis, 1593. Also in [39], pp. 347-435.

[39] F. Viète. *Francisci Vietæ Opera Matematica, In unum Volumen congest, ac recognita, Operā atque studio Francisci à Schooten Leydensis [...]*. B. & A. Elzeviorum, Lugduni Batavorum, 1646.

[40] J. Wallis. *Arithmetica infinitorum [...]*. Included with separate pagination in: J. Wallis, *Operum Mathematicorum. Pars Altera [...]*, typis L. Lichfield, apud O. Pullein, Oxonii, 1656.

[41] J. Wallis. *Mathesis universalis, sive Arithmeticum opus integrum, tum numerosam arithmeticam tum speciosam complectens.* in: J. Wallis, *Operum mathematicorum pars prima [...]*. typis L. Lichfield, impensis T. Robinson, Oxonii, 1657.

[42] J. Wallis. *A Treatise of Algebra, both historical and practical [...]*. Playford, London, 1685.

[43] J. Wallis. *De Algebra tractatus historicus et praticus*, volume II (1693) of *Opera Mathematica*. Theatro Sheldoniano, Oxoniæ, 1693-1699. 3 vols.

[44] D. T. Whiteside. Pattern of mathematical thought in the later seventeenth century. *Archive for History of Exact Sciences*, 1:179–388, 1960-1962.