

Toward a History of Mathematics Focused on Procedures

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Abstract Abraham Robinson's framework for modern infinitesimals was developed half a century ago. It enables a re-evaluation of the procedures of the pioneers of mathematical analysis. Their procedures have been often viewed through the lens of the success of the Weierstrassian foundations. We propose a view without passing through the lens, by means of proxies for such procedures in the modern theory of infinitesimals. The real accomplishments of calculus and analysis had been based primarily on the elaboration of novel techniques for solving problems rather than a quest for ultimate foundations. It may be hopeless to interpret historical *foundations* in terms of a punctiform continuum, but arguably it is possible to interpret historical *techniques and procedures* in terms of modern ones. Our proposed formalisations do not mean that Fermat, Gregory, Leibniz, Euler, and

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Cauchy were pre-Robinsonians, but rather indicate that Robinson's framework is more helpful in understanding their procedures than a Weierstrassian framework.

1 Introduction

We propose an approach to the history of mathematics as organic part of the history of science, based on a clearer distinction between practice/procedure and ontology than has been typically the custom of historians of mathematics, somewhat taken in with the success of the Weierstrassian foundations as developed starting around 1870. Today a grounding in such foundations is no longer viewed as a *sine-qua-non* of mathematics, with category theory playing an increasingly important foundational role.

The distinction between procedure and ontology was explored by philosophers Benacerraf (1965), Quine (1968), and Wartofsky (1976) but has been customarily paid scant attention to by historians of mathematics. We diverge from such custom already in the case of Stevin; see Sect. 3.

2 Methodological Issues

Interpreting historical mathematicians involves a recognition of the fact that most of them viewed the continuum as *not* being made out of points. Rather they viewed points as marking locations on a continuum. The latter was taken more or less as a primitive notion. Modern foundational theories starting around 1870 are based on a continuum made out of points and therefore cannot serve as a basis for interpreting the thinking of the earlier mathematicians as far as the foundations are concerned.

2.1 Procedures Versus Foundations

What one can however seek to interpret are the *techniques* and *procedures* (rather than foundations) of the earlier authors, using techniques and procedures available in modern frameworks. In short, it may be hopeless to interpret historical *foundations* in terms of a punctiform continuum, but arguably it is possible to interpret historical *techniques and procedures* in terms of modern techniques and procedures.

In the case of analysis, the modern frameworks available are those developed by K. Weierstrass and his followers around 1870 and based on an Archimedean continuum, as well as more recently those developed starting around 1960 by A. Robinson and his followers, and based on a continuum containing infinitesimals.¹ Additional frameworks were developed by W. Lawvere, A. Kock, and others.

¹ Some historians are fond of recycling the claim that Robinson used *model theory* to develop his system with infinitesimals. What they tend to overlook is not merely the fact that an alternative construction of the hyperreals via an ultrapower requires nothing more than a serious undergraduate course in algebra (covering the existence of a maximal ideal), but more significantly the distinction between *procedures* and *foundations*, as discussed in this Sect. 2.1, which highlights the point that whether one uses Weierstrass's foundations or Robinson's is of little import, procedurally speaking.

2.2 Parsimonious and Profligate

J. Gray responds to the challenge of the shifting foundations as follows:

Recently there have been attempts to argue that Leibniz, Euler, and even Cauchy could have been thinking in some informal version of rigorous modern non-standard analysis, in which infinite and infinitesimal quantities do exist. However, a historical interpretation such as the one sketched above that aims to understand Leibniz on his own terms, and that confers upon him both insight and consistency, has a lot to recommend it over an interpretation that has only been possible to defend in the last few decades. (Gray 2015, p. 11)

To what he apparently feels are profligate interpretations published in *Historia Mathematica* Laugwitz (1987), *Archive for History of Exact Sciences* Laugwitz (1989), and elsewhere, Gray opposes his own, which he defends on the grounds that it is

parsimonious and requires no expert defence for which modern concepts seem essential and therefore create more problems than they solve (e.g. with infinite series). The same can be said of non-standard readings of Euler; ... (ibid.)

Is this historian choosing one foundational framework over another in interpreting the techniques and procedures of the historical authors? We will examine the issue in detail in this section.

2.3 Our Assumptions

Our assumptions as to the nature of responsible historiography of mathematics are as follows.

- (1) Like other exact sciences, mathematics evolves through a continual clarification of the issues, procedures, and concepts involved, resulting in particular in the correction of earlier errors.
- (2) In mathematics as in the other sciences, it is inappropriate to select any particular moment in its evolution as a moment of supreme clarification above all other such moments.
- (3) The best one can do in any science is to state intuitions related to a given scientific problem as clearly as possible, hoping to convince one's colleagues or perhaps even *all* of one's colleagues of the scientific insight thus provided.

Unlike many historians of the natural sciences, historians of mathematical analysis often attribute a kind of supreme status to the clarification of the foundations that occurred around 1870. Some of the received scholarship on the history of analysis is based on the dual pillar of the *Triumvirate Agenda* (TA) and *Limit Fetishism* (LF); see Sect. 2.4.

2.4 Triumvirate and Limit

Historian C. Boyer described Cantor, Dedekind, and Weierstrass as *the great triumvirate* in (Boyer 1949, p. 298); the term serves as a humorous characterisation of both traditional scholars focused on the heroic 1870s and their objects of adulation.

Newton already was aware of, and explicitly mentioned, the fact that what he referred to as the *ultimate ratio* was not a ratio at all. Following his insight, later mathematicians may have easily introduced the notation “ult” for what we today denote “lim” following Cauchy’s progression

limite \rightsquigarrow lim. \rightsquigarrow lim

later assorted with subscripts like $x \rightarrow c$ by other authors. In such alternative notation, we might be working today with definitions of the following type:

a function f is continuous at c if $\text{ult}_{x \rightarrow c} f(x) = f(c)$

and similarly for the definitions of other concepts like the derivative:

$$f'(x) = \text{ult}_{h \rightarrow 0} \frac{f(x+h) - f(x)}{h}.$$

The point we wish to make is that the occurrence of the term *limit* itself (in whatever natural language) is of little significance if not accompanied by genuine *mathematical* innovation, reflected in mathematical practice in due course.

We therefore feel that searching the eighteenth century literature for occurrences of the term *limit* (in authors like d'Alembert or L'Huilier) so as to attribute to its author visionary insight into the magical *limit concept*, conveniently conflated by the triumvirate historian with the Weierstrassian *Epsilontik*, amounts to a kind of *limit fetishism* (LF) and constitutes an unhelpful approach to historiography.

2.5 Adequately Say Why

The influence of the TA+LF mindset can be traced in recent publications like Gray (2015). Already on the first page we find the following comment concerning an attempt to provide a foundational account for the calculus:

It is due to Joseph-Louis Lagrange, and its *failure* opened the way for the radically different accounts that followed. (Gray 2015, p. 1) (emphasis added)

However, attributing failure to Lagrange's program of expressing each function by its Taylor series is symptomatic of viewing history of mathematics as inevitable progress toward the triumvirate triumph. In reality Lagrange's program was successful when considered in the context of what are referred to today as analytic functions. That it is not general enough to handle future applications is not a *failure* though it is certainly a *limitation*. Reading on, we find the following comment on the infinitesimal calculus:

At its core stood a painful paradox. The simple and invariably correct rules for differentiation and integration were established by arguments that invoked: the vanishing of negligible quantities; *arguments about infinitesimal quantities*; plausible limit arguments that nonetheless seemed close to giving rules for evaluating 0/0. In short, the calculus worked—but *no-one could adequately say why*. (Gray 2015, p. 2) (emphasis added)

This passage is problematic on a number of counts:

- (1) it involves a confusion of the logical and the metaphysical criticism of the calculus;²

² Sherry (1987) argued that Berkeley's criticism of the calculus actually consisted of two separate components that should not be conflated, namely a logical and a metaphysical one:

- (a) logical criticism: how can dx be simultaneously zero and nonzero?
- (b) metaphysical criticism: what are these infinitesimal things anyway that we can't possibly have any perceptual access to or empirical verification of?

- (2) it fails to appreciate the distinction between *discarding* a negligible quantity and setting it *equal* to zero;
- (3) it is explicit in its assumption that infinitesimals are necessarily mired in *paradox*;
- (4) it reveals an ignorance of Leibniz's transcendental law of homogeneity and the generalized relation of equality "up to" something negligible;
- (5) it is based on an assumption that today we are able *adequately to say why* it all works.³

At the level appropriate for his historical period, Leibniz did "adequately say why" (to borrow Gray's phrase) when he developed his theoretical strategy for dealing with infinitesimals; see Sect. 6.

2.6 Euler's Intuitions

On the same page in Gray we find the following surprising comment concerning Euler's attempts to justify the calculus:

This was not for the want of trying. Euler wrote at length on this, as on everything else, but his view was that the naïve intuitions could be trusted if they were stated as clearly as they could be. (ibid.)

As noted in Sect. 2.3, the best a scientist can strive for is, ultimately, "intuitions stated as clearly as could be." Assuming otherwise amounts to bowing down to the triumvirate. Gray's comment rests on a questionable assumption that there is a sharp dividing line between intuitive arguments and rigorous ones, based on the idea of inevitable progress toward triumvirate rigor. As noted in Sect. 2.3, such naiveté is generally not shared by historians of science, who would question the assumption that there is a defining moment in the history of mathematics when mere intuition was finally transcended.

On page 3 we find the following quote from Euler:

"§ 86 Hence, if we introduce into the infinitesimal calculus a symbolism in which we denote dx an infinitely small quantity, then $dx = 0$ as well as $a dx = 0$ (a an arbitrary finite quantity). Notwithstanding this, the geometric ratio $a dx : dx$ will be finite, namely $a:1$, and this is the reason that these two infinitely small quantities dx and $a dx$ (though both $= 0$) cannot be confused with each other when their ratio is investigated. Similarly, when different infinitely small quantities dx and dy occur, their ratio is not fixed though each of them $= 0$." (Gray quoting Euler)

Comments Gray:

Whatever this may mean, it cannot be said to do more than *gesture* at what might be involved in rigorising the calculus,⁴ (Gray 2015) (emphasis added)

³ Note that the modern Zermelo–Fraenkel (ZFC) framework definitely works as a foundational system, but no-one can adequately say why, for instance, ZFC is consistent to begin with (moreover, in a precise sense discovered by Goedel, this cannot even be answered in the positive).

⁴ In point of fact, Euler is not seeking to 'rigorise' the calculus here, contrary to what Gray implies. Moreover, there is little indication that Euler found it problematic. He merely goes on to develop the calculus, e.g., by expanding trigonometric functions into series. It was the task of later generations to reshape his theses in a different setting.

(the comma is in the original). Now “whatever this may mean” is apriori an odd thing for a historian to say about a master of Euler’s caliber. The natural reaction of a lay reader when reading a historical text is usually one of dismissal stemming from a predictable failure to understand a historical work using different language from what the reader is accustomed to. As a rule, a historian’s job is to dispel a lay reader’s prejudices and misconceptions, rather than to reinforce them. But apparently such a rule applies to everything except... infinitesimals.

U. Bottazzini and Gray make a poetic proposal in the following terms: “The best policy is to read on in a spirit of dialogue with the earlier authors. (Bottazzini and Gray 2013).” The proposal of such a conversation with, say, Euler sounds intriguing. Consider, however, Gray’s comment to the effect that

Euler’s attempts at explaining the foundations of calculus in terms of differentials, which are and are not zero, are *dreadfully weak*. (Gray 2008, p. 6) (emphasis added)

Isn’t such a comment as an opening line in a conversation likely to be a conversation-stopper? Such comments border on disdain for the great masters of the past; cf. Sect. 2.9.

It may indeed be that, as per Bottazzini–Gray, “the best policy is to read on in a spirit of dialogue with the earlier authors.” However, the policy as stated does not clarify what the *content* of such a dialogue would be. For example, if Gray is interested in confronting Euler on allegedly “dreadfully weak” foundations, the dialogue is not likely to be productive. Once Bottazzini and Gray commit themselves to resolving issues through dialogue, the question still remains: what is on the agenda? Is it *foundations* (as Gray’s 2008 comment seems to suggest) or *procedures*? Bottazzini and Gray leave this crucial issue unresolved.

Euler’s profound insights here, including the distinction between the *geometric* and the *arithmetic* modes of comparison, were analyzed in Bair et al. (2016); see Sect. 7 on the two modes of comparison and their relation to Leibnizian laws. That’s a lot more than a *gesture* (to borrow Gray’s term). Gray’s myopism arguably stems from ideological triumvirate commitment and a Berkeley–Cantor tradition of anti-infinitesimal prejudice.

After singing praises of d’Alembert with respect to his allegedly visionary comments on *limits*, on page 4 Gray goes on to admit that d’Alembert was himself “confused at crucial points.” Therefore it is unclear why Gray wishes to attribute visionary status to d’Alembert’s confused remarks on limits, which, as Gray himself acknowledges, are derivative from Newton; see our comments on LF in Sect. 2.4.

2.7 Gray Parsimoniousness Toward Leibniz

On page 9, Gray cites a famously cryptic passage opening Leibniz’s first publication on the calculus dating from 1684, where Leibniz introduces differentials like dx and dv without much explanation. Gray goes on to quote an additional passage from Leibniz’s paper as follows:

“We have only to keep in mind that to find a *tangent* means to draw a line that connects two points of the curve at an infinitely small distance, or the continued side of a polygon with an infinite number of angles, which for us takes the place of the curve. This infinitely small distance can always be expressed by a known differential like dv , or by a relation to it, that is, by some known tangent.” (ibid., quoting Leibniz)

At this point, without much ado Gray cuts to the chase, namely an allegation of contradiction attributed to Leibniz:

Now it is presented as an infinitely small distance. Could it be that Leibniz did indeed think of there being infinitely small distances, or was that more a way of speaking, a useful fiction? It is already clear that they have contradictory properties, and why should $d(xv)$ not be written as $(x + dx)(v + dv) - xv = xdv + vdx + dxdv$? (Gray 2015, p. 10)

What Gray seems to find contradictory is Leibniz’s maneuver of replacing $xdv + vdx + dxdv$ by $xdv + vdx$. However contradictions are there only for those who wish to detect them. A relation of the form

$$xdv + vdx + dxdv \sqsupseteq xdv + vdx$$

is a reasonably valid one if interpreted in the context of Leibniz’s TLH (see Sect. 6).

On page 10, Gray trips right over one of the familiar *faux amis de traducteur* when he translates Leibniz’s *à la rigueur* by means of the English term *rigour* and claims that Leibniz “said that the infinite need not be taken rigorously.” However, Gray’s translation is inaccurate. The correct translation for this expression is not *rigorously* but rather *literally*, as in the following passage:

Et c’est pour cet effect que j’ay donné un jour des lemmes des incomparables dans les Actes de Leipzic, qu’on peut entendre comme on vent [sic], soit des infinis *à la rigueur*, soit des grandeurs seulement, qui n’entrent point en ligne de compte les unes au prix des autres. (Leibniz 1702, p. 92) (emphasis added)

Leibniz’s pair of “soit”s in this remark indicates that there is a pair of distinct methodologies involved, a duality acknowledged by Leibniz scholars H. Bos and D. Jesseph (see Sect. 6). In a chapter 5 added to the second edition of her book, Ishiguro (1990) argued otherwise, and claimed that Leibnizian infinitesimals are *logical fictions* à la Russell. The stated impetus for Ishiguro’s (arguably flawed) reading was a desire to defend Leibniz’s honor as an *unconfused* and *consistent* logician by means of her syncategorematic reading; see Bascelli et al. (2016) for details. With Gray’s latest book, the argument has come full circle, as he seeks *both* to attribute contradiction to Leibniz *and* to toe the line on R. Arthur’s endorsement of Ishiguro’s *logical fiction* reading. Arthur’s own errors are analyzed in Sect. 6.2. Gray goes on to the *parsimonious* passage (already cited in Sect. 2.2), of which we reproduce an extension:

It is *parsimonious* and requires no expert defence for which modern concepts seem essential and therefore create more problems than they solve (e.g., with infinite series). The same can be said of non-standard readings of Euler; for a detailed discussion of Euler’s ideas in this connection, see Schubring (2005). (Gray 2015, p. 11) (emphasis added)

A distinction between *procedure* and *ontology* is apparently not one that interests Gray. For a detailed analysis of Schubring’s errors see Błaszczuk et al. (2016b).

Gray’s “parsimonious” argument could be termed *the Gray sword* (analogously to *the Occam razor*), and if applied in the context of a proper focus on procedures would in fact yield the opposite result of the one Gray seeks.

Consider for example Cauchy’s definition of continuity, namely an infinitesimal change α in the variable x always produces an infinitesimal change $f(x + \alpha) - f(x)$ in the function. In a modern infinitesimal framework one copies this over almost verbatim to get a precise definition of continuity.

Meanwhile, if one wishes to work in a traditional Weierstrassian framework, one needs to interpret Cauchy's definition as "really" saying that, for example, for every epsilon there is a delta such that for every x , etc.

Such logical complexity involving multiple alternations of quantifiers will surely fall by the (Gray) sword. Alternatively, one could seek to interpret Cauchy by means of sequences, which is not much better because Cauchy explicitly says in defining an infinitesimal that a sequence *becomes* an infinitesimal (rather than an infinitesimal being a sequence). So apparently Gray should be saying the following, instead:

Since Boyer (at least) there have been attempts to argue that Leibniz, Euler, and even Cauchy could have been thinking in some informal version of rigorous modern Weierstrassian analysis. However, a historical interpretation such as the one sketched above that aims to understand Leibniz on his own terms, and that confers upon him both insight and consistency, has a lot to recommend it over an interpretation that has only been possible to defend since Weierstrass came along. It is parsimonious and requires no expert defence for which modern alternating quantifiers seem essential and therefore create more problems than they solve.

2.8 The Truth in Mind

Most recently, we came across the following comment concerning Euler:

...Euler (1768–1770, 1: § 5) did not condemn "the common talk" (*locutiones communes*) about differentials as if they were absolute quantities: this common talk could be tolerated, provided one had always the truth in the mind; namely, we could write $dy = 2x dx$ and use this formula in calculations, but we had to have in the mind that the *true meaning* of $dy = 2x dx$ was $dy/dx = 2x$. (Capobianco et al. 2016) (emphasis added)

The idea seems to be that something called a *true meaning* resides not in a relation between Leibniz–Euler differentials but rather in a formula for what is called today the derivative. Such an idea seems to stem from a vision of inevitable progress in analysis toward its familiar post-Weierstrassian form. Such a vision suffers from latent realist tendencies (cf. Błaszczyk et al. 2016a) and ignores repeated warnings (Bos 1974) that Leibnizian calculus relying as it did on analysis of differentials looked very different from the conceptual structure of analysis today which was *not* its inevitable outcome. It also ignores Hacking's seminal writings on a possible Latin rival to a butterfly model of scientific development; see Hacking (2014).

2.9 Did Euler Prove Theorems by Example?

In his 2014 book, G. Ferraro writes at beginning of chapter 1, section 1 on page 7:

Capitolo I

Esempi e metodi dimostrativi

1. Introduzione

In *The Calculus as Algebraic Analysis*, Craig Fraser, riferendosi all'opera di Eulero e Lagrange, osserva:

A theorem is often regarded as demonstrated if verified for several examples, the assumption being that the reasoning in question could be adapted to any other example one chose to consider (Fraser 1989, p. 328).

Le parole di Fraser colgono un aspetto poco indagato della matematica dell'illuminismo. (Ferraro 2014, p. 7)

The last sentence indicates that Ferraro endorses Fraser's position as expressed in the passage cited in the original English without Italian translation. The following longer passage places Fraser's comment in context:

The calculus of Euler and Lagrange differs from later analysis in its assumptions about mathematical existence. The relation of this calculus to geometry or arithmetic is one of correspondence rather than representation. Its objects are formulas constructed from variables and constants using elementary and transcendental operations and the composition of functions. When Euler and Lagrange use the term "continuous" function they are referring to a function given by a single analytical expression; "continuity" means continuity of algebraic form. A theorem is often regarded as demonstrated if verified for several examples, the assumption being that the reasoning in question could be adapted to any other example one chose to consider. (Fraser 1989, p. 328)

Fraser's hypothesis that in Euler and Lagrange, allegedly "a theorem is often regarded as demonstrated if verified for several examples" is at variance with much that we know about Euler's mathematics. Thus, (Pólya 1941, p. 454) illustrates how Euler checked no fewer than 40 coefficients of an identity involving infinite products and sums:

$$\prod_{m=1}^{\infty} (1 - x^m) = \sum_{m=-\infty}^{m=+\infty} (-1)^m x^{(3m^2+m)/2}$$

while clearly acknowledging that he had no proof of the identity.⁵

Euler's proof of the infinite product formula for the sine function may rely on hidden lemmas, but it is a sophisticated argument that is a far cry from anything that could be described as "verification for several examples;" see Bair et al. (2016) for details. Speaking of Euler in dismissive terms chosen by Fraser and endorsed by Ferraro borders on disdain for the great masters of the past; cf. Sect. 2.6. In a similar vein, Ferraro claims that "for eighteenth-century mathematicians, there was no difference between finite and infinite sums." (Ferraro 1998, footnote 8, p. 294). Far from being a side comment, the claim is emphasized a decade later in the Preface to his 2008 book: "a distinction between finite and infinite sums was lacking, and this gave rise to formal procedures consisting of the infinite extension of finite procedures." (Ferraro 2008, p. viii).

We hope to have given sufficient indication of the kind of historical scholarship we wish to distance ourselves from in the present work.

3 Simon Stevin

Simon Stevin (1548–1620) developed an adequate system for representing ordinary numbers, including all the ones that were used in his time, whether rational or not. Moreover his scheme for representing numbers by unending decimals works well for all of them, as is well known.

Stevin developed specific notation for decimals (more complicated than the one we use today) and did actual technical work with them rather than merely envisioning their possibility, unlike some of his predecessors like E. Bonfils in 1350. Bonfils wrote that "the

⁵ At <http://mathoverflow.net/questions/242379> the reader will find many other examples.

unit is divided into ten parts which are called Primes, and each Prime is divided into ten parts which are called Seconds, and so on into infinity” (Gandz 1936, p. 39) but his ideas remained in the realm of the potential and he did not develop any notation to ground them.

Even earlier, the Greeks developed techniques for solving problems that today we may solve using more advanced number systems. But to Euclid and Eudoxus, only 2,3,4,...were numbers: everything else was proportion. The idea of attributing algebraic techniques in disguise to the Greeks is known as *Geometric Algebra* and is considered a controversial thesis. Our paper in no way depends on this thesis.

Stevin dealt with unending decimals in his book *l'Arithmetique* rather than the more practically-oriented *De Thiende* meant to teach students to work with decimals (of course, finite ones).

As far as using the term *real* to describe the numbers Stevin was concerned with, the first one to describe the common numbers as *real* may have been Descartes. Representing common numbers (including both rational and not rational) by unending decimals was to Stevin not merely a matter of speculation, but the background of, for example, his work on proving the intermediate value theorem for polynomials using subdivision into ten subintervals of equal length.

Stevin's accomplishment seems all the more remarkable if one recalls that it dates from before Vieta, meaning that Stevin had no notation beyond the tool inherited from the Greeks namely that of proportions $a:b::c:d$. He indeed proceeds to write down a cubic equation as a proportion, which can be puzzling to an unprepared modern reader. The idea of an *equation* that we take for granted was in the process of emerging at the time. Stevin presented a divide-and-conquer algorithm for finding the root, which is essentially the one reproduced by Cauchy 250 years later in *Cours d'Analyse*.

In this sense, Stevin deserves the credit for developing a representation for the real numbers to a considerable extent, as indeed one way of introducing the real number field \mathbb{R} is via unending decimals. He was obviously unaware of the existence of what we call today the transcendental numbers but then again Cantor and Dedekind were obviously unaware of modern developments in real analysis.

Cantor, as well as Méray and Heine, sought to characterize the real numbers axiomatically by means of Cauchy Completeness (CC). This property however is insufficient to characterize the real numbers; one needs to require the Archimedean property in addition to CC. Can we then claim that they (i.e., Cantor, Heine, and Méray) really knew what the real numbers are? Apparently, not any more than Stevin, if a sufficient axiom system is a prerequisite for *knowing the real numbers*.

Dedekind (1872) was convinced he had a proof of the existence of an infinite set;⁶ see (Ferreirós 2007, p. 111 and section 5.2, p. 244). Thus, Joyce comments on Dedekind's concept of things being objects of our thought and concludes:

That's an innocent concept, but in paragraph 66 it's used to justify the astounding theorem that infinite sets exist. (Joyce 2005)

⁶ The proof exploits the assumption that there exists a set S of all things, and that a mathematical thing is an object of our thought. Then if s is such a thing, then the thought, denoted s' , that “ s can be an object of my thought” is a mathematical object is a thing distinct from s . Denoting the passage from s to s' by ϕ , Dedekind gets a self-map ϕ of S which is some kind of blend of the successor function and the brace-forming operation. From this Dedekind derives that S is infinite, QED.

Do such aspects of the work of Cantor and Dedekind invalidate their constructions of the real number system? Surely not. Similarly, Stevin's proposed construction should not be judged by the yardstick of awareness of future mathematical developments.

In the approach to the real numbers via decimals, one needs to identify each terminating decimal with the corresponding string with an infinite tail of 9 s, as in $1.0 = 0.999\dots$. The more common approaches to \mathbb{R} are (1) via Dedekind cuts, or (2) via equivalence classes of Cauchy sequences, an approach usually attributed, rather whimsically, to Georg Cantor, even though the concept of an equivalence relation did not exist yet at the time. The publication of Cantor (1872) was preceded by Heine (1872) by a few months but Heine explicitly attributes the idea of *Fundamentalreihe* to Cantor.

Even earlier, Charles Méray published his "Remarques sur la nature des quantités définies par la condition de servir de limites à des variables données" (Méray 1869); see Dugac (1970) for a detailed analysis. However, Méray's paper seems to have been unknown among German mathematicians.

While Stevin had no idea of the set-theoretic underpinnings of the received *ontology* of modern mathematics, *procedurally* speaking his approach to arithmetic was close to the modern one, meaning that he envisioned a certain homogeneity among all numbers with no preferential status for the rationals; see Malet (2006), Katz and Katz (2012b), and Błaszczyk et al. (2013) for further details.

Stevin's decimals cannot be placed on equal footing with the 1872 constructions, when both representations and algebraic operations were developed as well as the continuity axioms, while Stevin only gave the representation.

In 1923, A. Hoborski, a mathematician involved, like Stevin, in applied rather than pure mathematics, developed an arithmetic of real numbers based upon unending decimal representations Hoborski (1923).

4 Pierre de Fermat

Pierre de Fermat (1601/1607–1665) developed a procedure known as *adequality* for finding maxima and minima of algebraic expressions, tangents to curves, etc. The name of the procedure derives from the παρισότης of Diophantus. Some of its applications amount to variational techniques exploiting a small variation E . Fermat's treatment of geometric and physical applications suggests that an aspect of approximation is inherent in *adequality*, as well as an aspect of smallness on the part of E . Fermat relied on Bachet's reading of Diophantus, who coined the term παρισότης for mathematical purposes and used it to refer to the way in which $1321 / 711$ is approximately equal to $11 / 6$. In translating Diophantus, Bachet performed a semantic calque, passing from *parisoō* to *adaequo*, which is the source for Fermat's term rendered in English as *adequality*.

To give a summary of Fermat's algorithm for finding the maximum or minimum value of an algebraic expression in a variable A , we will write such an expression in modern functional notation as $f(A)$. One version of the algorithm can be broken up into five steps in the following way:

- (1) Introduce an auxiliary symbol E , and form $f(A + E)$;
- (2) Set *adequal* the two expressions

$$f(A+E) \sqsupseteq f(A)$$

(the notation “ \sqsupseteq ” for adequality is ours, not Fermat’s);

- (3) Cancel the common terms on the two sides of the adequality. The remaining terms all contain a factor of E ;
- (4) Divide by E (in a parenthetical comment, Fermat adds: “or by the highest common factor of E ”);
- (5) Among the remaining terms, suppress all terms which still contain a factor of E . Solving the resulting equation for A yields the desired extremum of f .

In simplified modern form, the algorithm entails expanding the difference quotient $\frac{f(A+E)-f(A)}{E}$ in powers of E and taking the constant term.

There are two crucial points in trying to understand Fermat’s reasoning: first, the meaning of “adequality” in step (2); and second, the justification for suppressing the terms involving positive powers of E in step (5). As an example consider Fermat’s determination of the tangent line to the parabola. To simplify Fermat’s notation, we will work with the parabola $y = x^2$ thought of as the level curve

$$\frac{x^2}{y} = 1$$

of the two-variable function $\frac{x^2}{y}$. Given a point (x, y) on the parabola, Fermat seeks the tangent line through the point, exploiting the geometric fact that by convexity, a point (p, q) on the tangent line lies *outside* the parabola. He therefore obtains an inequality equivalent in our notation to $\frac{p^2}{q} > 1$, or $p^2 > q$. Here $q = y - E$, and E is Fermat’s magic symbol we wish to understand. Thus, we obtain

$$\frac{p^2}{y-E} > 1. \quad (4.1)$$

At this point Fermat proceeds as follows:

- (i) he writes down the inequality $\frac{p^2}{y-E} > 1$, or $p^2 > y - E$;
- (ii) he invites the reader to *adégaler* (to “adequate”);
- (iii) he writes down the adequality $\frac{p^2}{p^2} \sqsupseteq \frac{y}{y-E}$
- (iv) he uses an identity involving similar triangles to substitute $\frac{x}{p} = \frac{y+r}{y+r-E}$ where r is the distance from the vertex of the parabola to the point of intersection of the tangent to the parabola at y with the axis of symmetry,
- (v) he cross multiplies and cancels identical terms on right and left, then divides out by E , *discards* the remaining terms containing E , and obtains $y = r$ as the solution.

What interests us are steps (i) and (ii). How does Fermat pass from an inequality to an adequality? Giusti observes: “Comme d’habitude, Fermat est autant détaillé dans les exemples qu’il est réticent dans les explications. On ne trouvera donc presque jamais des justifications de sa règle des tangentes.” (Giusti 2009, p. 80) In fact, Fermat provides no explicit explanation for this step. However, what he does is to apply the defining relation for a curve to points on the tangent line to the curve. Note that here the quantity E , as

in $q = y - E$, is positive: Fermat did not have the facility we do of assigning negative values to variables.

Fermat says nothing about considering points $y + E$ “on the other side”, i.e., further away from the vertex of the parabola, as he does in the context of applying a related but different method, for instance in his two letters to Mersenne (see Strømholm 1968, p. 51), and in his letter to Brûlart Fermat (1643). Now for positive values of E , Fermat’s inequality (4.1) would be satisfied by a *transverse ray* (i.e., secant ray) starting at (x, y) and lying outside the parabola, just as much as it is satisfied by a tangent ray starting at (x, y) . Fermat’s method therefore presupposes an additional piece of information, privileging the tangent ray over transverse rays. The additional piece of information is geometric in origin: he applies the defining relation (of the curve itself) to a point on the tangent ray to the curve. Such a procedure is only meaningful when the increment E is small.

In modern terms, we would speak of the tangent line being a “best approximation” to the curve for a small variation E ; however, Fermat does not explicitly discuss the size of E .

The procedure of “discarding the remaining terms” in step (v) admits of a proxy in the hyperreal context in terms of the standard part principle (every finite hyperreal number is infinitely close to a real number). Fermat does not elaborate on the justification of this step, but he is always careful to speak of the *suppressing* or *deleting* the remaining term in E , rather than setting it equal to zero. Perhaps his rationale for suppressing terms in E consists in ignoring terms that don’t correspond to a possible measurement, prefiguring Leibniz’s *inassignable quantities*. Fermat’s inferential moves in the context of his adequacy are akin to Leibniz’s in the context of his calculus.

While Fermat never spoke of his E as being *infinitely small*, the technique based on what eventually came to be known as infinitesimals was known both to Fermat’s contemporaries like Galileo (see Bascelli 2014a, b) and Wallis (see (Katz and Katz 2012a, Section 13)) as well as Fermat himself, as his correspondence with Wallis makes clear; see (Katz et al. 2013, Section 2.1).

Fermat was very interested in Galileo’s treatise *De motu locali*, as we know from his letters to Marin Mersenne dated apr/may 1637, 10 august, and 22 october 1638. Galileo’s treatment of infinitesimals in *De motu locali* is discussed in Settle (1966) and (Wisn 1974, p. 292).

The clerics in Rome forbade the doctrine of *indivisibles* on 10 august 1632 (a month before Galileo was summoned to stand trial over heliocentrism); this may help explain why the catholic Fermat may have been reluctant to speak of them explicitly.

The problem of the parabola could of course be solved purely in the context of polynomials using the idea of a double root, but for transcendental curves like the cycloid Fermat does *not* study the order of multiplicity of the zero of an auxiliary polynomial. Rather, Fermat explicitly stated that he applied the defining property of the curve to points on the tangent line: “Il faut donc adégaler (à cause de la propriété spécifique de la courbe qui est à considérer sur la tangente)” (see Katz et al. 2013 for more details).

Fermat’s approach involves applying the defining relation of the curve, to a point on a *tangent* line to the curve where the relation is *not* satisfied exactly. Fermat’s approach is therefore consistent with the idea of approximation. His method involves a negligible distance (whether infinitesimal or not) between the tangent and the original curve when one is near the point of tangency. This line of reasoning is related to the ideas of the differential calculus. Fermat correctly solves the cycloid problem by obtaining the defining equation of the tangent line.

5 James Gregory

In his attempt to prove the irrationality of π , James Gregory (1638–1675) broadened the scope of mathematical *procedures* available at the time by introducing what he called a sixth operation (on top of the existing four arithmetic operations as well as extraction of roots). He referred to the new procedure as the *termination* of a (convergent) sequence: “And so by imagining this [sequence] to be continued to infinity, we can imagine the ultimate convergent terms to be equal; and we call those equal ultimate terms the termination of the [sequence].” (Gregory 1667, p. 18–19) Referring to sequences of inscribed and circumscribed polygons, he emphasized that

if the abovementioned series of polygons can be terminated, that is, if that ultimate inscribed polygon is found to be equal (so to speak) to that ultimate circumscribed polygon, it would undoubtedly provide the quadrature of a circle as well as a hyperbola. But since it is difficult, and in geometry perhaps unheard-of, for such a series to come to an end [lit.: be terminated], we have to start by showing some Propositions by means of which it is possible to find the terminations of a certain number of series of this type, and finally (if it can be done) a general method of finding terminations of all convergent series.

Note that in a modern infinitesimal framework like Robinson (1966), sequences possess terms with infinite indices. Gregory’s relation can be formalized in terms of the standard part principle in Robinson’s framework. This principle asserts that every finite hyperreal number is infinitely close to a unique real number.

If each term with an infinite index n is indistinguishable (in the sense of being infinitely close) from some real number, then we “terminate the series” (to exploit Gregory’s terminology) with this number, meaning that this number is the limit of the sequence. Gregory’s definition of the coincidence of lengths of inscribed (I_n) and circumscribed (C_n) polygons corresponds to a relation of infinite proximity in a hyperreal framework. Namely we have $I_n \approx C_n$ where \approx is the relation of being infinitely close (i.e., the difference is infinitesimal), and the common standard part of these values is what is known today as the *limit* of the sequence.

Our proposed formalisation does not mean that Gregory is a pre-Robinsonian, but rather indicates that Robinson’s framework is more helpful in understanding Gregory’s procedures than a Weierstrassian framework.

6 Gottfried Wilhelm von Leibniz

Gottfried Wilhelm Leibniz (1646–1716) was a co-founder of infinitesimal calculus. When we trace the diverse paths through mathematical history that have led from the infinitesimal calculus of the seventeenth century to its version implemented in Abraham Robinson’s framework in the twentieth, we notice patterns often neglected in received historiography focusing on the success of Weierstrassian foundations.

We have argued that the final version of Leibniz’s infinitesimal calculus was free of logical fallacies, owing to its *procedural* implementation in ZFC via Robinson’s framework.

6.1 Berkeley on Shakier Ground

Both Berkeley as a philosopher of mathematics, and the strength of his criticisms of Leibniz's infinitesimals have been overestimated by many historians of mathematics. Such criticisms stand on shakier ground than the underestimated mathematical and philosophical resources available to Leibniz for defending his theory. Leibniz's theoretical strategy for dealing with infinitesimals includes the following aspects:

- (1) Leibniz clearly realized that infinitesimals violate the so-called Archimedean property⁷ which Leibniz refers to as Euclid V.5;⁸ in a letter to L'Hospital he considers infinitesimals as non-Archimedean quantities, in reference to Euclid's theory of proportions (De Risi 2016, p. 64, note 15).
- (2) Leibniz introduced a distinction between assignable and inassignable numbers. Ordinary numbers are assignable while infinitesimals are inassignable. This distinction enabled Leibniz to ground the procedures of the calculus relying on differentials on the *transcendental law of homogeneity* (TLH), asserting roughly that higher order terms can be discarded in a calculation since they are negligible (in the sense that an infinitesimal is negligible compared to an ordinary quantity like 1).
- (3) Leibniz exploited a generalized relation of *equality up to*. This was more general than the relation of strict equality and enabled a formalisation of the TLH (see previous item).
- (4) Leibniz described infinitesimals as *useful fictions* akin to imaginary numbers. Leibniz's position was at variance with many of his contemporaries and allies who tended to take a more realist stance. We interpret Leibnizian infinitesimals as *pure fictions* at variance with a post-Russellian *logical fiction* reading involving a concealed quantifier ranging over ordinary values; see Bascelli et al. (2016).
- (5) Leibniz formulated a law of continuity (LC) governing the transition from the realm of assignable quantities to a broader one encompassing infinite and infinitesimal quantities: "il se trouve que les règles du fini réussissent dans l'infini... et que vice versa les règles de l'infini réussissent dans le fini." Leibniz (1702)
- (6) Meanwhile, the TLH returns to the realm of assignable quantities.

The relation between the two realms can be represented by the diagram of Fig. 1.

Leibniz is explicit about the fact that his *incomparables* violate Euclid V.5 (when compared to other quantities) in his letter to L'Hospital from the same year: "J'appelle *grandeurs incomparables* dont l'une multipliée par quelque nombre fini que ce soit, ne sauroit excéder l'autre, de la même façon qu'Euclide la pris dans sa cinquième définition du cinquième livre."⁹ (Leibniz 1695a, p. 288)

6.2 Arthur's Errors

The claim in (Arthur 2013, p. 562) that allegedly "Leibniz was quite explicit about *this Archimedean foundation* for his differentials as 'incomparables'" (emphasis added) is therefore surprising. Arthur fails to explain his inference of an allegedly Archimedean

⁷ In modern notation this can be expressed as $(\forall x, y > 0)(\exists n \in \mathbb{N})[nx > y]$.

⁸ In modern editions of *The Elements* this appears as Definition V.4.

⁹ This can be translated as follows: "I use the term *incomparable magnitudes* to refer to [magnitudes] of which one multiplied by any finite number whatsoever, will be unable to exceed the other, in the same way [adopted by] Euclid in the fifth definition of the fifth book [of the *Elements*]."

$$\left\{ \begin{array}{l} \textit{assignable} \\ \textit{quantities} \end{array} \right\} \xrightarrow{\text{LC}} \left\{ \begin{array}{l} \textit{assignable and inassignable} \\ \textit{quantities} \end{array} \right\} \xrightarrow{\text{TLH}} \left\{ \begin{array}{l} \textit{assignable} \\ \textit{quantities} \end{array} \right\}$$

Fig. 1 Leibniz’s law of continuity (LC) takes one from assignable to inassignable quantities, while his transcendental law of homogeneity (TLH) returns one to assignable quantities

nature of the Leibnizian continuum. Therefore we can only surmise the nature of Arthur’s inference, apparently based on the reference to Archimedes himself by Leibniz. However, the term *Archimedean axiom* for Euclid V.4 was not coined until the 1880s (see Stolz 1883), about two centuries after Leibniz. Thus, Leibniz’s mention of Archimedes could not refer to what is known today as the Archimedean *property* or *axiom*. Rather, Leibniz mentions an ancient authority merely to reassure the reader of the soundness of his methods. Arthur’s cryptic claim concerning the passage mentioning Archimedes (i.e., that it is indicative of an allegedly Archimedean foundation for the Leibnizian differentials) borders on obfuscation.

The 1695 letter to l’Hospital (with its explicit mention of violation of Euclid Definition V.4 by his incomparables) is absent from Arthur’s bibliography.

Leading Leibniz scholar Jesseph in Jesseph (2015) largely endorses Bos’ interpretation of Leibnizian infinitesimals as fictions, at variance with Ishiguro, Arthur, and surprisingly many other historians who back the syncategorematic reading in substance if not in name.

Modern proxies for Leibniz’s procedures expressed by LC and TLH are, respectively, the *transfer principle* and the *standard part principle* in Robinson’s framework. Leibniz’s theoretical strategy for dealing with infinitesimals and infinite numbers was explored in the articles (Katz and Sherry 2012, 2013; Sherry and Katz 2014; Bascelli et al. 2016).

7 Leonhard Euler

Leonhard Euler (1707–1783) routinely relied on procedures exploiting infinite numbers in his work, as in applying the binomial formula to an expression raised to an infinite power so as to obtain the development of the exponential function into power series.

Euler’s comments on infinity indicate an affinity with Leibnizian fictionalist views: “Even if someone denies that infinite numbers really exist in this world, still in mathematical speculations there arise questions to which answers cannot be given unless we admit an infinite number.” (Euler 2000, § 82).

Euler’s dual notion of *arithmetic* and *geometric* equality which indicate that, like Leibniz, he was working with generalized notions of equality. Thus, Euler wrote:

Since the infinitely small is actually nothing, it is clear that a finite quantity can neither be increased nor decreased by adding or subtracting an infinitely small quantity. Let a be a finite quantity and let dx be infinitely small. Then $a + dx$ and $a - dx$, or, more generally, $a \pm ndx$, are equal to a . Whether we consider the relation between $a \pm ndx$ and a as arithmetic or as geometric, in both cases the ratio turns out to be that between equals. The arithmetic ratio of equals is clear: Since $ndx = 0$, we have $a \pm ndx - a = 0$. On the other hand, the geometric ratio is clearly of equals, since $\frac{a \pm ndx}{a} = 1$. From this we obtain the well-known rule that the infinitely small vanishes in comparison with the finite and hence can be neglected. For this reason the objection brought up against the analysis of the infinite, that it lacks geometric rigor, falls to the ground under its own weight, since nothing is

neglected except that which is actually nothing. Hence with perfect justice we can affirm that in this sublime science we keep the same perfect geometric rigor that is found in the books of the ancients. (Euler 2000, § 87)

Like Leibniz, Euler did not distinguish notationwise between different modes of comparison, but we could perhaps introduce two separate symbols for the two relations, such as \approx for the arithmetic comparison and the Leibnizian symbol \sqsupset for the geometric comparison. See Bair et al. (2016) for further details.

8 Augustin-Louis Cauchy

A. L. Cauchy (1789–1857)’s significance stems from the fact that he is a transitional figure, who championed greater rigor in mathematics. Historians enamored of set-theoretic foundations tend to translate *rigor* as *epsilon-delta*, and sometimes even attribute an epsilon-delta definition of continuity to Cauchy.

In reality, to Cauchy rigor stood for the traditional ideal of *geometric* rigor, meaning the rigor of Euclid’s geometry as it was admired throughout the centuries. What lies in the background is Cauchy’s opposition to certain summation techniques of infinite series as practiced by Euler and Lagrange without necessarily paying attention to convergence. To Cauchy rigor entailed a rejection of these techniques that he referred to as the *generality of algebra*.

In his textbooks, Cauchy insists on reconciling rigor with infinitesimals. By this he means not the elimination of infinitesimals but rather the reliance thereon, as in his definition of continuity. As late as 1853, Cauchy still defined continuity as follows in a research article:

...une fonction u de la variable réelle x sera *continue*, entre deux limites données de x , si, cette fonction admettant pour chaque valeur intermédiaire de x une valeur unique et finie, un accroissement infiniment petit attribué à la variable produit toujours, entre les limites dont il s’agit, un accroissement infiniment petit de la fonction elle-même. (Cauchy 1853) [emphasis in the original]

In 1821, Cauchy denotes his infinitesimal α and requires $f(x + \alpha) - f(x)$ to be infinitesimal as the definition of the continuity of f . In differential geometry, Cauchy routinely defined the center of curvature of a plane curve by intersecting a pair of *infinitely close* normals to the curve. An approach to differential geometry exploiting infinitesimals was developed in Nowik and Katz (2015). These issues are explored further in Cutland et al. (1988), Katz and Katz (2011), Borovik and Katz (2012), Katz and Tall (2013), Bascelli et al. (2014), and Błaszczyk et al. (2016b).

9 Conclusion

We have argued that a history of mathematics that views the past through the lens of Weierstrassian foundations is misguided. Not only are these developments of 140 years ago less central to mathematical practice today, but a historical approach that focuses on foundations distorts the actual work of past mathematicians. A more fruitful approach is to examine the procedures mathematicians developed, which had little or nothing to do with

questions of foundations. Modern mathematical conceptions of quantity, approximation, and particularly infinitesimals, have roots in the procedures developed by leading mathematicians from the 16th through the nineteenth century.

By examining the procedures of a few mathematical masters of the past, we have argued that the real accomplishments of the calculus and analysis have been based primarily on the elaboration of new techniques rather than quest for ultimate foundations. The masters are best understood through the study of their procedures rather than their contribution to what some historians perceive to be a heroic march toward ultimate foundations.

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