

## HISTORICAL INFINITESIMALISTS AND MODERN HISTORIOGRAPHY OF INFINITESIMALS

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ABSTRACT. In the history of infinitesimal calculus, we trace innovation from Leibniz to Cauchy and reaction from Berkeley to Mansion and beyond. We explore 19th century infinitesimal lores, including the approaches of Siméon-Denis Poisson, Gaspard-Gustave de Coriolis, and Jean-Nicolas Noël. We examine contrasting historiographic approaches to such lores, in the work of Laugwitz, Schubring, Spalt, and others, and address a recent critique by Archibald et al. We argue that the element of contingency in this history is more prominent than many modern historians seem willing to acknowledge.

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## 1. DEBATE OVER LEIBNIZ

Both Leibniz and Cauchy used the term *infinitesimal* in their work. The meaning of the term has been the subject of scholarly debates. In this section, we focus on Leibniz's use of the term. In Sections 2 and 3, we focus on Cauchy and his interpreters. In Section 4, we analyze certain historiographic assumptions underlying existing interpretations of these pioneers of infinitesimal analysis. In Section 5, we address a recent critique by Archibald et al. We summarize our conclusions in Section 6.

George Berkeley claimed to find shortcomings in both the Newtonian and the Leibnizian calculus. While modern scholars (both historians and mathematicians) also find shortcomings, their quibbles about Leibniz are not identical to Berkeley's. Berkeley's empiricism obscured from him the coherence of the procedures of the Leibnizian infinitesimal calculus. Specifically, Berkeley's logical criticism overlooked the coherence of Leibniz's relation of infinite proximity, as we detail in Section 1.1.

**1.1. Berkeley's criticisms; Transcendental Law of Homogeneity.** Berkeley was an English cleric whose *empiricist* (i.e., based on sensations) metaphysics tolerated no conceptual innovations, like infinitesimals, without an empirical counterpart. Berkeley was similarly opposed, on metaphysical grounds, to infinite divisibility of the continuum (which he referred to as *extension*), an idea widely taken for granted today (as it was already by Leibniz).

In addition to his *metaphysical criticism* of the infinitesimal calculus of Newton and Leibniz, Berkeley put forth a *logical criticism* in his pamphlet *The Analyst*.

He claimed to have detected a logical fallacy at the foundation of the method. The distinction between logical and metaphysical criticisms in Berkeley goes back to Sherry's 1987 article [111]; see further in [10]. In terms of Fermat's technique of adequality<sup>1</sup> exploiting an increment  $E$ , Berkeley's objection can be formulated as follows: the increment  $E$  is assumed to be nonzero at the beginning of the calculation, but zero at its conclusion, an apparent logical fallacy.

However, as noted by Fermat historian Strømholm [118, p. 51],  $E$  is not assumed to be zero at the end of the calculation, but rather is *discarded* at the end of the calculation. Such a procedure was the foundation of both Fermat's adequality and Leibniz's Transcendental Law of Homogeneity (TLH), involving the relation of infinite proximity. Leibniz discussed the TLH in texts from 1695<sup>2</sup> and in a 1710 text [92].<sup>3</sup> The TLH is closely related to a pair of modern procedures<sup>4</sup> in analysis:

- (1) passing to the *limit* of a typical expression such as  $\frac{f(A+E)-f(A)}{E}$  in the Weierstrassian approach, and
- (2) taking the *standard part* in infinitesimal analysis [106].

Meanwhile, Berkeley's own attempt to explain the calculation of the slope when  $y = x^2$  in Section XXIV of *The Analyst* contains a logical circularity. Namely, Berkeley's argument relies on the determination of the tangents of a parabola by Apollonius (which is equivalent to the calculation of the slope). The circularity in Berkeley's argument is analyzed by Andersen [1]. Far from exposing logical flaws in the Leibnizian calculus, Berkeley's *The Analyst* is itself logically flawed. Berkeley's character has been analyzed by Moriarty [98] and [99].

Berkeley's rhetorical flourishes such as the *ghosts of departed quantities* were popular with a generation of scholars who attributed exaggerated significance to his influence in the history of the calculus. These are the historians and Leibniz scholars of the period until around 1966, including Boyer and Kline. These scholars

- (1) believed Berkeley to have provided the motivation for the eventual success of the "great triumvirate" [30, p. 298] of Cantor, Dedekind, and Weierstrass in eliminating the ghosts that haunted the early calculus of Newton and Leibniz; and
- (2) sought to interpret Cauchy as anticipating the Weierstrassian *Epsilontik* with its alternating quantifiers.<sup>5</sup>

In short, Weierstrass-trained historians tended to attribute special significance to Berkeley's critique because the Weierstrassian real line is taken to exhibit purely Archimedean behavior admitting no relation of infinite proximity. But such an approach to the historical infinitesimal calculus risks walking royal roads; see Section 4. Received attitudes underwent a subtle but significant change in the second half of the 20th century.

**1.2. Changing attitudes toward Berkeley.** Abraham Robinson developed modern infinitesimal analysis in his 1966 book [106], building upon earlier work by

<sup>1</sup>Such a technique was used by Fermat to solve problems of finding tangents to curves, maxima and minima, and others; see further in [77] and [16].

<sup>2</sup>See Bella [24, p. 192, note 70].

<sup>3</sup>The 1710 text was analyzed by Bos [28] (see p. 33 and note 64 there); see further in [78]. See Bascelli et al. [21] and Dinis [41] for a study of modern formalisations.

<sup>4</sup>On procedures *vs* foundations, see Section 3.9.

<sup>5</sup>The *modus operandi* of such scholars can therefore be described in terms of a quest for the *ghosts of departed quantifiers*; see [8].

Skolem [114], Hewitt [60], Loś [95], and others. Attitudes among scholars toward Berkeley’s criticisms have undergone a perceptible change since the appearance of Robinson’s book.<sup>6</sup> The current generation of Leibniz scholars holding received opinions derives its impetus from the second, 1990 edition of Ishiguro’s book [63] on Leibniz, where she wrote:

Robinson’s success in introducing infinitesimals into the Weierstrassian analysis seemed to vindicate Leibniz from Berkeley’s famous attack, in which Berkeley claimed “to conceive a quantity infinitely small, that is, infinitely less than any sensible or imaginable quantity or any finite quantity however small is, I confess, beyond my capacity.” [63, p. 83]

Ishiguro went on to disagree with such a seeming “vindication,” arguing that Leibniz was more rigorous than historians believed him to be, in the following sense. Ishiguro and her followers claim that Leibniz never thought of infinitesimals as mathematical entities in the first place, and that occurrences of the term *infinitesimal* in Leibniz do not *refer* to a mathematical entity; they are mere stenography for a more long-winded argument à la Archimedean exhaustion.<sup>7</sup> Such scholars are mostly silent as to the pertinence of Berkeley’s critique. What about Berkeley the great influencer? One finds little about this other than in reprints of the old classics by Boyer [30] and Kline [81].<sup>8</sup>

**1.3. Potential infinity vs infinite wholes; *infinita terminata*.** An aspect of Leibniz’s thought at variance with modern usage is his rejection of infinite wholes. The dichotomy of potential infinity *vs* infinite wholes can be traced back to Aristotle, for whom potential infinity meant an iterative process of repeating a procedure over and over again ( $\alpha\varepsilon\iota$ ). Thus, one can only have finite lines, but one can envision a process of doubling them each time, again and again (similarly, dividing a line segment again and again will give a smaller and smaller segment). This is the meaning of the so-called *syncategorematic* infinite. But the process never leads to an infinite whole (for details see Ugaglia [119]).

Leibniz agreed with this conclusion. His analysis of the Galilean paradox (comparing integers and squares of integers) led him to reject infinite wholes as contradictory,<sup>9</sup> and more precisely contradicting Euclid’s part-whole principle (whether or not Euclid himself meant for the principle to apply to infinite pluralities is a separate issue); for details see [74, Section 4.1], [75].

In this connection, Leibniz elaborated an important distinction in Proposition 11 of his *De Quadratura Arithmetica* [94, pp. 520–676].<sup>10</sup> This is the distinction between bounded infinity (*infinitum terminatum*) and unbounded infinity (*infinitum*

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<sup>6</sup>Bockstaele’s article published in the same year (1966) presents the two sides of the Belgian debate over the teaching of infinitesimals (see Section 2.7 below), but chooses to describe only one side as “obstinate” [27, pp. 2, 8]. Noël, an advocate of infinitesimals, is described mockingly as a “never-desponding defender of the infinitely small” [27, p. 14]. Bockstaele concludes by mentioning “a definitive acknowledgment of the limit concept as the foundation of the calculus” [27, p. 16]. See Section 4 for an analysis of teleological aspects of this type of historical scholarship.

<sup>7</sup>Some Leibniz scholars are beginning to re-evaluate the claims of the Ishiguro school; see e.g., Esquisabel and Raffo Quintana [43] and Samuel Henry Eklund at the University of California at Irvine [42]. See further in [11] and [74].

<sup>8</sup>Boyer and Kline also jointly fabricated the “quote” from Cavalieri about rigor being the concern of philosophy rather than mathematics.

<sup>9</sup>Cauchy reached similar conclusions; see main text at note 29.

<sup>10</sup>A French translation by Parmentier is available [90].

*interminatum*). The latter, exemplified by an unbounded infinite line, is a contradictory notion. The former, exemplified by a segment with infinitely separated endpoints, is a useful concept in geometry and calculus; see further in [11, Section 2.2]. The distinction between bounded and unbounded infinity was mentioned in the 29 July 1698 letter to Bernoulli [50, III 523] as well as the 2 February 1702 letter to Varignon [91, p. 91]. The same letter to Varignon contains a definition of an infinitesimal as a “fraction infiniment petite, ou dont le denominateur soit un nombre infini”<sup>11</sup> [91, p. 93], i.e., the reciprocal of an *infinitum terminatum*.

It is therefore difficult to agree with Gert Schubring’s claims that

- (1) “the founders of the calculus’ had not at all created it with a non-Archimedean continuum in mind” [110, p. 6], or that
- (2) “Leibniz always refused to be identified with a foundation based on—rather vaguely conceived—infinitesimals” [110, p. 1].

While Schubring apparently believes infinitesimals to be “vaguely conceived,” Leibniz defined them as fictional inassignable quantities smaller than any assignable quantity, or magnitudes incomparable with 1 in the sense of the violation of Definition 4 of Euclid’s Book V; see further in [11] and [7].<sup>12</sup>

**1.4. Magnitudes vs multitudes; Maxima and Minima.** The significance of the Leibnizian distinction between bounded and unbounded infinity tends to be underplayed by Leibniz scholars who follow Ishiguro [63] in seeking to interpret Leibnizian infinitesimals as stenography for exhaustion procedures in the sense outlined in Section 1.3, and to relate such an approach to the Scholastic concept of syncategorematic infinity (closely related to potential infinity). But while syncategorematic infinity is a well-known and important concept in Leibnizian thought, the locution *syncategorematic infinitesimal* is nowhere to be found in known Leibnizian texts. Leibniz’s work allows for an interpretation that he worked with infinitesimals and bounded infinities as fictional mathematical entities, rejecting infinite wholes while adhering to the part-whole principle; see further in [11].

Leibniz used the term *Maxima* to refer to infinite wholes, and the term *Minima* to refer to points viewed as constituent parts of the continuum. Leibniz rejected both *Maxima* and *Minima* in the following terms:

*Scholium.* We therefore hold that two things are excluded from the realm of intelligibles: minimum and maximum; the indivisible, or what is entirely *one*, and *everything*; what lacks parts, and what cannot be part of another.<sup>13</sup> (Leibniz as translated by Arthur in [3, p. 13])

Leibniz’s rejection of *Maxima* amounts to the rejection of infinite wholes (e.g., unbounded lines) as inconsistent. The rejection of their counterparts, *Minima*, amounts to the rejection of putative simplest constituents of the continuum, i.e., the rejection of a punctiform continuum (see [74]). To Leibniz, points play only the

<sup>11</sup>“An infinitely small fraction, or one whose denominator is an infinite number.”

<sup>12</sup>Such approaches to infinitesimals admit straightforward formalisations in modern infinitesimal theories: an infinitesimal is a number smaller in absolute value than every positive standard number; for details see e.g., [46] or [62]. The viability of the application of nonstandard analysis to interpreting the procedures of the historical infinitesimalists depends on the procedure/foundation distinction; see Section 3.9. On Schubring see also note 25.

<sup>13</sup>“Habemus ergo exclusa rebus intelligibilibus, duo: Minimum et Maximum; indivisibile, vel omnino unum, et omne; quod partibus careat, et quod pars alterius esse non possit” (A VI 3 98).

role of endpoints of line segments. Thus the rejected counterparts of the contradictory infinite wholes are not infinitesimals but rather *points* viewed as the simplest constituents of a continuum.<sup>14</sup>

It is therefore problematic to assimilate – as Rabouin and Arthur do in [103] – infinitesimals to infinite wholes in the matter of inconsistency. In 2022, Arthur again claims that

These unassignables, however, cannot be understood as actual infinitesimals, since the notion of the actually infinitely small contains a contradiction. [5, p. 320]

Arthur has repeatedly misinterpreted the contradiction involved. According to Leibniz, the contradiction inherent in the notion of an infinite whole, i.e., the *infinitum interminatum*, does not affect the *infinita terminata* and their reciprocals, the infinitesimals. The most significant difference is that the *infinita terminata* are magnitudes, whereas infinite wholes are multitudes. While Leibniz considered the latter to be contrary to the part-whole principle and therefore contradictory, infinite wholes will be formalized by Cantor under the name of cardinalities, or transfinite numbers. By assimilating infinitesimals to infinite wholes, Arthur and others are in essence attempting to invert a cardinality so as to obtain an infinitesimal – but to Leibniz, the contradictory counterparts of infinite wholes are (not infinitesimals but) *points* viewed as constituent parts of the continuum.<sup>15</sup>

**1.5. Fraenkel shocked by inversion of cardinalities.** Reacting to contemporary attempts to define infinitesimals, Abraham Fraenkel wrote:

I was deeply shocked at how infinity was treated in the Marburg school, ... wherein the infinitesimal is brought into direct correspondence with Georg Cantor's transfinite numbers. [47, p. 85]

Here Fraenkel was referring to the work of Paul Natorp (1854–1924). Fraenkel went on to describe Natorp's shocking treatment as follows:

If following Cantor an infinitely large 'number' is denoted as  $w$ , and if the ratio  $1 : w = x : 1$  is formulated, then  $x$  must be an 'infinitely small number'. (ad loc., note 18)

Accordingly, Natorp confused magnitudes and multitudes, i.e., transfinite numbers. What Fraenkel pointed out is that the latter cannot be inverted to obtain an infinitesimal. Fraenkel would have likely been just as shocked to discover that, a century later, a similar confusion persists in the writings of Arthur (see Section 1.4) and other Leibniz scholars in the Ishiguro school; see further in [66].

Both in Leibniz and Cauchy scholarship, preoccupation with the distinction between potential infinity and an infinite whole, sometimes called actual infinity, is

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<sup>14</sup>The reciprocal relationship between Maxima and Minima can be formalized in modern mathematics as follows. The unbounded infinity represented by the real line  $\mathbb{R}$  can be viewed as an increasing union of segments:  $\mathbb{R} = \bigcup_{n \in \mathbb{N}} [-n, n]$ . In the reciprocal picture, we have the decreasing intersection  $\bigcap_{n \in \mathbb{N}} [-\frac{1}{n}, \frac{1}{n}]$  which is a single point (the origin). Thus, the counterparts of infinite wholes are points (not infinitesimals).

<sup>15</sup>Some historians are aware of the distinction and avoid committing Ishiguro and Arthur's error of conflating infinite number and infinite whole. Thus, Spalt wrote: "Johann Bernoulli ... knew his friend Leibniz to be a philosophical thinker and, as such convinced that infinite 'wholes' do not exist. Therefore, he had to be careful in talking to Leibniz about 'infinitely large' numbers" [117, p. 53].

often a way of changing the subject so as to deny that they ever used genuine infinitesimals. Thus, Boyer writes:

Cauchy and Weierstrass saw only paradox in attempts to identify an actual or ‘completed’ infinity in mathematics, believing that the infinitely large and small indicated nothing more than the potentiality of Aristotle. [31, p. 612]

What is involved is a conflation of two senses of the adjective *actual*:

- (A) as in *actual infinity* (or infinite whole) versus *potential infinity*; and
- (B) as in *actual infinitesimal* number or quantity (i.e., a genuine infinitesimal in the sense of violating Euclid’s Definition V.4 when compared to 1), as opposed to a smaller and smaller “ordinary” number.

The term *actual infinity* as used in set theory can refer to an infinite multitude taken as a whole, whereas infinite number in the sense of Leibniz can also refer to magnitude or quantity, not multitude; for details see [74]. The distinction between potential and actual infinity is distinct and independent from the question whether Leibniz and Cauchy used genuine infinitesimals. We will examine the modern debate over Cauchy’s infinitesimals in Sections 2 and 3.

## 2. DEBATE OVER CAUCHY

Siméon-Denis Poisson (1781–1840) . . . was able to promote [the *infiniment petits*] to far-reaching dissemination and impact because of his central position within the French educational system.  
—Schubring (2005)

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**2.1. Variable quantities, infinitesimals and limits in the *Cours*.** In his *Cours d’Analyse* [33], Cauchy laid foundations for analysis that were characterized by the following features:

- (1) Cauchy did not give an  $\epsilon$ - $\delta$  definition of limit;
- (2) Cauchy did not define the notion of continuity of a function in terms of limits;
- (3) Cauchy’s notion of limit is similar to that of his teacher Lacroix (who is not generally thought of as a pioneer of the Weierstrassian *Epsilontik*);
- (4) Cauchy’s final definition of continuity of a function  $f$ , emphasized in italics in his *Cours d’Analyse*, stipulates that an infinitesimal increment  $\alpha$  must always produce an infinitesimal change  $f(x + \alpha) - f(x)$  in the function.

See further in [12] and [72]. There have been varying interpretations of what Cauchy meant by the term *infinitesimal*. He gave the following definition of infinitesimals:

When the successive numerical values of such a variable decrease indefinitely, in such a way as to fall below any given number, this variable becomes what we call *infinitesimal*, or an *infinitely small quantity*. A variable of this kind has zero as its limit.<sup>16</sup> [32, p. 7]

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<sup>16</sup>“Lorsque les valeurs numériques successives d’une même variable décroissent indéfiniment, de manière à s’abaisser au-dessous de tout nombre donné, cette variable devient ce qu’on nomme un *infiniment petit* ou une quantité *infiniment petite*. Une variable de cette espèce a zéro pour limite” Cauchy [33, p. 4].

Interpretations have varied with regard to Cauchy’s term *devient* (becomes). Grabiner [53] holds that Cauchy’s *becomes* is equivalent to *is*, so that an infinitesimal is nothing but a variable quantity tending to zero, ruling out any associated non-Archimedean phenomena. Others have argued that the term *becomes* implies a process involving a change of nature<sup>17</sup> and have, accordingly, interpreted Cauchy’s infinitesimals in non-Archimedean terms. Regardless of the meaning of Cauchy’s definition of infinitesimals in the *Cours*, he used genuine infinitesimals in his later books and research articles, as documented in [14] (see below).

Detlef Laugwitz, following Robinson [106, pp. 269–276], proposed an interpretation of Cauchy analysis in a series of articles starting in the 1980s (see e.g., [85] and [86]), sparking a historical debate that is still current.

In 2020, Bair et al. [14] explored several applications that Cauchy made of infinitesimals in his work beyond his *Ecole Polytechnique* textbooks, in fields ranging from centers of curvature (see Section 2.4 below) to convergence of series of functions (see Section 3.1) to integral geometry (see Section 3.4).

In 2022, Gert Schubring (GS) published a lengthy review [110] of Bair et al. [14] for MathSciNet. Such attention is surely appreciated by every researcher (even though the review is unrefereed, i.e., non-peer-reviewed); hopefully it can mark the beginning of a meaningful dialog or informed debate.

It turns out, however, that many of GS’s comments in the review are at odds with what he wrote in his 2005 book [108], which he would go on to describe in [109, p. 527] as his “key publication.” We will document multiple shifts in GS’s position between 2005 and 2022, including his book’s acknowledgment of Poisson’s broad influence in promoting Poisson’s version of infinitesimals, which rivaled the type of *infinitesimal lore* that GS chose exclusively to emphasize in his review. We will also compare GS’s reactions to the Cauchy scholarship of Laugwitz and that of Bair et al. The philosophical assumptions underpinning GS’s position are analyzed in Section 3.11.

**2.2. Members of a group.** In his opening remarks, GS makes it clear that he is targeting not merely the article under review, but an entire program of re-evaluation of the history of analysis pursued by a large group of scholars:

This paper is written by members of a group that for several years has been leading a *crusade against the historiography* of mathematics. . . . the group seems to consist of at least 22 mathematicians and philosophers, . . . [110, p. 1]<sup>18</sup> (emphasis on “crusade against the historiography” added)

With regard to GS’s choice of wording (“crusade against, etc.”) in describing the work of scholars he happens to disagree with, it is worth recalling his track record of colorful terminology targeting the work of Laugwitz, as for example the following sarcastic comment:

“[Giusti’s 1984 article] spurred Laugwitz to even more detailed attempts to banish the error and confirm that Cauchy had used hyper-real numbers. . . . (see Laugwitz 1990, 21).” (Schubring [108, p. 432])<sup>19</sup>

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<sup>17</sup>Such a process can be formalized in modern mathematics in terms of a suitable equivalence relation; see e.g., [29].

<sup>18</sup>Page numbers here and below refer to the pdf of GS’s review.

<sup>19</sup>For the record, we note that Laugwitz never attributed hyperreal numbers to Cauchy, either in his 1990 article [87] cited by GS or anywhere else. For an analysis of GS’s misrepresentation



Further epithets are sampled in Section 3.8. One of the works GS quotes in 2022 is the article Bair et al. [10] going back to 2013, indicating that he is targeting at least a decade of work by the 22 scholars he mentioned. GS explains that

To enable the reader to understand the issues at stake and to situate their development, this review is a bit more extended than usual. [110, p. 1]

We will examine GS's take on "the issues at stake," evaluate how successful his attempts to "situate their development" are, and compare his views as expressed in 2005 and in 2022.

**2.3. Far-reaching dissemination: Carnot, Coriolis, Poisson.** Concerning the historical period of Cauchy's activity, GS claims the following in 2022:

Nobody so far, including Cauchy himself, had thought of the calculus in terms of a non-Archimedean continuum. [110, p. 2]

However, an examination of GS's key publication [108] indicates that he did not always feel this way. In 2005, he acknowledged the following:

[T]here was a propagation of the *infiniment petits* quite deviant [sic] from Cauchy's conceptualization in France, an attempt to popularize them as *actually* infinitely small quantities. This version would hence have had a claim on *anticipating non-standard analysis* if its propagators had been able to know that the latter would some day exist. [108, p. 575] (emphasis on "actually" in the original; emphasis on "anticipating non-standard analysis" added)

GS goes on to identify the propagator:

The conceptions were those of Siméon-Denis Poisson (1781–1840), who was able to promote them to *far-reaching dissemination* and impact because of his central position within the French educational system. (ibid.; emphasis on "far-reaching dissemination" added)

As member of the *Conseil royal de l'instruction publique*, Poisson succeeded in promoting infinitesimals to officially prescribed status at the national level in 1837:

Poisson prevailed in having his favorite method of using the *infiniment petits* centrally prescribed as compulsory of all the *collèges*: "The two geometry lessons will remain appended to the *troisième* class; but this teaching will be based on the method of infinitely small quantities." (Schubring [108, p. 585] based on Belhoste [23, p. 147])

A decree issued during the following year was even more specific:

A subsequent decree dated October 9, 1838, specified, by presenting a first detailed mathematics curriculum, how geometry was to be taught on the basis of the *infiniment petits*:

In plane geometry for the *troisième*, curves were to be conceived of as polygons having an infinite number of sides; in particular, circles were to be conceived of as regular polygons.

(Schubring [108, p. 585] based on Belhoste [23, p. 148])

Such a conception of a curve as an infinite-sided polygon, prescribed for the French mathematics curriculum in 1838, goes back to Leibniz and even earlier (a point not mentioned by GS); see further in [11, Section 2.3].

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of Laugwitz, see [26, Section 6.1] and [19, Section 4.5, pp. 278–279]. See further in Section 3.9 on Laugwitz's take on procedures.

Nor was Poisson the only first-rate mathematician to promote the dissemination of genuine infinitesimals. As noted by Grattan-Guinness, Coriolis wrote the following:

“The approbation of the *Conseil Royal* of the *Université* will be equal to giving an appropriate direction to these works and to establishing everywhere the same language founded upon the infinitely small.” (Coriolis as translated by Grattan-Guinness in [54, p. 1262])

Similar remarks apply to Carnot.<sup>20</sup>

In short, GS’s key publication acknowledged a “far-reaching dissemination and impact” of genuine infinitesimals in France, which was moreover endorsed at the national level by the *Conseil Royal*.<sup>21</sup> Very little of the above information concerning the extent of such dissemination trickled down to his 2022 review. To deny such a 19th century lore of genuine infinitesimals is “to wrench Cauchy’s ideas out of their historical context” (cf. [5]).

Specifically, GS’s 2005 talk of 19th century anticipation of nonstandard analysis (NSA) undermines his 2022 claim that “nobody . . . had thought of the calculus in terms of a non-Archimedean continuum.” If GS changed his mind about the “far-reaching dissemination” of Poisson’s non-Archimedean approach, he did not tell his readers about such a change of heart.<sup>22</sup> The 2022 claim is hardly compatible with GS’s stated goal of properly situating the development of infinitesimals in their historical context (see Section 2.2).

**2.4. Epsilons, small and infinitesimal.** In a 1826 work on differential geometry, Cauchy develops a formula for the radius of curvature  $\rho$  of a plane curve parametrized by arclength  $s$ . Such a formula is equivalent to the formula  $\frac{1}{\rho} = \frac{d\tau}{ds}$  in modern notation, where  $\tau$  is similar to the modern polar coordinate angle  $\theta$  of the tangent vector to the curve.

Cauchy’s proof of the formula was analyzed in [14, Section 5] (for a summary see below, Section 3.10, item 5). The conclusion was that the symbol  $\varepsilon$  *as used there* denotes a genuine infinitesimal on par with the quantities  $\Delta\tau$ ,  $\Delta s$ ,  $\Delta x$ , and  $\Delta y$  also used there.

GS rejects such a conclusion in the following terms:

[T]he use of  $\varepsilon$  is known as being due to Cauchy himself, and this occurred apparently for the first time in his 1823 textbook on the differential calculus,

<sup>20</sup>See note 21.

<sup>21</sup>In 2022, GS claims that “[Bair et al.] call [Poisson], wrongly, a member of the CP [*Conseil de Perfectionnement* of the Ecole Polytechnique], while he was in fact an omnipotent member of the ministry’s *Conseil royal d’instruction publique*” [110, p. 6]. GS’s claim is in error. Indeed, Gilain mentions the “examineurs de mathématiques, *Poisson et de Prony*, qui animaient en général la commission programme du CP” [51, §32]. GS’s error is particularly surprising given his emphasis on Poisson’s perceived vice of the *cumul des mandats* in [108, p. 576], so that his being on the *Conseil Royal* is not inconsistent with the fact that he “championed the use of infinitesimals through [his] influence on the *Conseil de Perfectionnement* (CP)” as mentioned by Bair et al. [14, p. 142].

<sup>22</sup>In 2022, GS writes: “Lazare Carnot devoted himself, in his pre-infinitesimal periods, to elaborating the concept of null sequences for variables with limit zero” [110, p. 2]. If there was a pre-infinitesimal period, there must have been a subsequent infinitesimal period, as well. GS seems to acknowledge implicitly that genuine infinitesimals (or the *pseudo-infinitement petits* in GS’s parlance; see Section 2.7) were practiced by Carnot, as well. For an analysis of Carnot’s conception in relation to Leibniz’s, see Barreau [18].

in which he used it for the derivative, demanding ‘Let  $\delta, \varepsilon$  be two very small numbers’, clearly understanding them to have finite values. [110, p. 2]

The problem with GS’s claim is two-fold.

- (1) GS’s remark with regard to the 1823 textbook is in error, as Cauchy had already used  $\varepsilon$  in this sense in his 1821 textbook [33, Section 2.3, Theorem 1]; see further in [13, Section 3.8] (see also Section 2.5 below for an analysis of GS’s oversight).
- (2) Cauchy used the notation  $\varepsilon$  on more than one occasion. But if “infinitesimal” and “very small” meant the same thing to Cauchy, why did he use different terms, especially given the existence of a widely disseminated Poissonian infinitesimal tradition (see Section 2.3)?

Here GS seems to have overlooked the possibility that  $\varepsilon$  may *not* have had the same meaning in Cauchy’s 1826 text on differential geometry as in his 1821 and 1823 calculus textbooks.

**2.5. Schubring vs Grabiner.** GS’s oversight mentioned in Section 2.4 is all the more puzzling since this particular 1821 occurrence of  $\varepsilon$  is one of the key pieces of evidence used by Grabiner to argue Cauchy’s pioneering work on “manipulating algebraic inequalities.” Grabiner translates the *Cours d’Analyse* as follows:

“Designate by  $\varepsilon$  a number as small as desired. Since the increasing values of  $x$  will make the difference  $f(x + 1) - f(x)$  converge to the limit  $k$ , we can give to  $h$  a value sufficiently large so that,  $x$  being equal to or greater than  $h$ , the difference in question is included between  $k - \varepsilon$  and  $k + \varepsilon$ .” (Cauchy as translated by Grabiner in [53, p. 8])

Grabiner goes on to conclude: “This is hard to improve on” (*ibid.*), indicating that she views Cauchy’s calculation as a convincing example of an  $\varepsilon, \delta$  argument (a point already mentioned by Freudenthal [49, p. 137]). In 2016, GS sharply disagreed with Grabiner in the following terms:

I am criticizing historiographical approaches like that of Judith Grabiner where one sees epsilon-delta already realized in Cauchy. [109, p. 530]

It emerges that Schubring criticized Grabiner without properly examining the evidence she had presented.

**2.6. Non-standard numbers.** While in his key publication GS acknowledged the existence of a widely disseminated non-Archimedean conception of infinitesimal and infinite numbers as advocated by Poisson (see Section 2.3), by 2022 we find GS claiming the following:

Cauchy used for them the term of traditional *lore* to speak of arbitrarily large numbers — as mathematicians did throughout the 18th century, and likewise used by Weierstraß [see, for instance, K. Viertel . . . — none of them ever thinking of *non-standard numbers*. [110, p. 3] (emphasis on “lore” and “non-standard numbers” added)

GS’s reference to Weierstrass is particularly revealing. If there did exist a widely disseminated non-Archimedean tradition of genuine infinitesimals in Cauchy’s time (as GS acknowledged in his key publication), why should we automatically assume that when mathematicians used the terms *infinitely small* or *infinitely large*, they necessarily meant it in the Weierstrassian sense, as shorthand for more long-winded non-infinitesimal arguments?

While GS emphasizes his student Viertel’s work, Weierstrass’ use of the term *infinitesimal* in [120, p. 74] in a figurative sense was quoted in Bascelli et al. [20, Section 2.1], a fact not acknowledged by GS. The existence of such traditional lore was mentioned by Laugwitz in 1989 in the following terms:

In 1815 [Cauchy] does not use infinitesimals but only ‘very small numbers’, in a naive and pragmatic manner. [86, p. 232]

Laugwitz goes on to argue that the change occurred in the early 1820s, when Cauchy started using genuine infinitesimals.

Appealing to a “traditional lore” as evidence that Cauchy’s infinite numbers were merely large “ordinary” numbers is a non-sequitur, given the dual nature of the said lore, as we elaborate further in Section 2.7.

**2.7. Dual nature of 19th century lore.** Even though the term *lore* was absent from the analysis of the 18th and 19th centuries in his key publication from 2005, by 2022 GS speaks of infinitesimal and infinite numbers as parts of a “traditional lore to speak of arbitrarily large numbers — as mathematicians did throughout the 18th century” (see Section 2.6). Accordingly, he describes Cauchy’s phrase

“si l’on désigne par  $\varepsilon$  un nombre infiniment petit”

(if one denotes by  $\varepsilon$  an infinitely small number) as “standard lore for expressing an arbitrarily small number” [110, p. 3].

What was the nature of the said *lore*? One significant development that GS failed to mention is the 19th century debate about infinitesimals in Belgium and Luxembourg. Jean-Nicolas Noël (1783–1867) at the University of Liège and Jean Joseph Manilius (1807–1869) at the University of Ghent (Gand) [27, p. 8] were advocates of the use of genuine infinitesimals. They introduced both the Leibnizian distinction between assignable and inassignable quantities, and Leibniz’s definition of infinitesimal as smaller than any assignable quantity (see Section 1.3). Their opponents were led by Ernest Lamarle (1806–1875) similarly at the University of Gand; see further in [27], [17], and [68]. It emerges that there were distinct and *rival* infinitesimal lores in both France and Belgium at the time.

In 2022, GS does mention a criticism of genuine infinitesimals “in Belgium in 1887 by Paul Mansion” [110, p. 7], but fails to clarify its context. The context was indeed the debate opposing Noël/Manilius and Lamarle; Mansion [97] (at the University of Gand like Manilius and Lamarle) was presenting a rebuttal of the position held by Noël and Manilius, a fact GS fails to mention.

The Belgian debate is significant in the context of GS’s references to an infinitesimal *lore*, which he claims to amount to viewing an infinitesimal as an arbitrarily small number. There were surely infinitesimal lores in France, Belgium, and elsewhere as he claims, but GS appears to be only reporting one of them in 2022. In sum, by seeking to portray a monolithic lore, GS fails to clarify the dual nature of the infinitesimal lore.<sup>23</sup>

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<sup>23</sup>It is therefore ironic that GS should accuse the authors of [14] of holding such monolithic views, when he writes: “The authors are so enraptured by their conviction that Cauchy conceived of numbers within a non-Archimedean continuum that they consider historical mathematics anachronistically only in terms of this continuum” [110, p. 2]. Actually, it is GS who painted a reductionist monolithic picture of infinitesimal lore in his review, whereas the authors of [14] recognize the dual nature thereof.

GS was more forthcoming in 2005 with details on such a rival approach to infinitesimals, and even gave it a name: *pseudo-infinitement petits* (pseudo-infinitely small quantities), adopting Mansion’s terminology:

Mansion called quantities thus conceived of as *pseudo-infinitement petits*, with the intention of recalling the *obvious contradiction* this *definition* contained. [108, p. 583] (emphasis on “obvious contradiction” and “definition” added)

The definition in question was formulated as follows by Mansion:

Certain geometers have given yet another meaning to the word *infinitely small*. According to them, there exist quantities different from zero which are yet smaller than every assignable magnitude.<sup>24</sup>

The definition criticized by Mansion is essentially the Leibnizian definition of infinitesimals as smaller than every assignable quantity (see Section 1.3).<sup>25</sup> GS’s key publication devotes a number of pages to the subject in [108, pp. 583–593] where the term *pseudo-infinitement petits* is mentioned six times. Alas, very little of such pseudo-infinitesimal lore trickled down to his 2022 text, which therefore fails to situate the development of infinitesimals in their proper historical context with all its complexity.

**2.8. What is a number, ontologically?** The *Cours d’Analyse* includes a lengthy appendix called *Note I* [33, pp. 403–437]. GS claims that Cauchy only recognized finite numbers in his *Note I*:

... the authors still refuse to take notice of Cauchy’s very explicit discussion of what the number system means for him, in the introduction and in the extensive *Note I* in his *Cours d’analyse* of 1821 ... The status of “number” is attributed exclusively to positive integer numbers, called by Cauchy absolute numbers. ... In their crusade to capture Cauchy as a non-standard analysis forerunner, they systematically ignore Cauchy’s own explicit affirmations that for him only *finite numbers* are admitted as numbers. [110, p. 3] (emphasis on “Note I” in the original; emphasis on “finite numbers” added)

The problem with GS’s claim is four-fold.

- (1) A reading of Cauchy’s *Note I* reveals not only that he never claimed that all numbers are finite, but on the contrary that he envisioned the possibility that they may not be. Indeed, following the introduction of exponentiation  $A^B$ , Cauchy presents the following formula and comment:

$$A^0 = 1.$$

We assume however that the value of the number  $A$  remains finite and differs from zero.<sup>26</sup>

<sup>24</sup>“Certains géomètres ont donné un autre sens encore au mot *infinitement petit*. D’après eux, il existe *des quantités différentes de zéro et qui sont cependant inférieures à toute grandeur assignable*” [97, p. 214].

<sup>25</sup>The fact that this definition of infinitesimals admits a modern formalisation (see note 12) provides evidence against the Mansion–Schubring opinion that the definition involved an “obvious contradiction.” Mansion’s opinion is perhaps understandable as it may have been common in the 1880s, but Schubring’s is less so.

<sup>26</sup>“Nous supposons toutefois que la valeur du nombre  $A$  reste finie et diffère de zéro” [33, p. 416].

Thus, Cauchy finds it necessary to stipulate the *condition* that the number  $A$  should be finite (possibly because he is thinking of indeterminate forms of type  $\infty^0$ ) to ensure the validity of the formula.

- (2) GS's claim that in *Note I* all numbers are positive integers is contradicted by Cauchy's analysis of both rational and irrational numbers in *Note I* [33, p. 409].
- (3) GS claims that

Cauchy followed Carnot<sup>27</sup> in interpreting numbers ontologically and admitting only absolute numbers. [110, p. 3]

However, Cauchy's "absolute numbers" are simply *unsigned* numbers, as is evident from Cauchy's Turin lectures where they are referred to as *numeri assoluti* [35, p. 152]. Little can be derived from Cauchy's comments on unsigned numbers as regards the *ontology* of his numbers.

- (4) In 2022, GS discusses the passage from the text on the centers of curvature where Cauchy writes "si l'on désigne par  $\varepsilon$  un nombre infiniment petit." GS's claim that Cauchyan numbers are necessarily positive integers is at odds with Cauchy's reference to an infinitesimal as a *number*, a reference GS himself quotes.

What Cauchy did affirm, similarly to Leibniz (see Section 1.3), was that infinite wholes are contradictory, being contrary to the part-whole principle. Thus, in the *Sept Leçons*, Cauchy summarizes Galileo's paradox (comparing numbers and their squares), and concludes:

The proof that we just recalled was given for the first time by Galileo.<sup>28</sup>

GS's claim that "[Cauchy] says that one can prove mathematically that the assumption of a number 'infinite' would lead to manifest contradictions" [108, p. 449] equivocates on the meaning of the term *number*: Cauchy was referring to the impossibility of *infinite wholes*.<sup>29</sup> See further in Laugwitz [86, p. 201].

In his key publication, GS acknowledged [108, pp. 445, 448] that Abbot Moigno edited and published Cauchy's *Sept Leçons de Physique Générale* already after Cauchy's death. Yet in 2022 GS is willing to rely upon the good Abbot to represent Cauchy's views faithfully when Moigno claims in an Appendix (written by himself) to *Sept Leçons* that "A number being actually infinite is impossible; every number is essentially finite" [110, p. 4].<sup>30</sup> Moigno believed this, but did Cauchy (see Section 3.1)? GS has much to say about Moigno's and Mansion's criticism of Poisson's genuine infinitesimals,<sup>31</sup> but there is one name conspicuously absent from GS's list of the critics of Poisson: Cauchy himself. Significantly, Moigno's lengthy introduction was deleted when the *Sept Leçons* were included in the *Oeuvres Complètes* under the direction of the French Academy of Sciences; see [36, p. 412]. Similarly left out was Moigno's lengthy appendix pompously entitled

"Sur l'impossibilité du nombre actuellement infini, l'antiquité de l'homme, la science dans ses rapports avec la foi" (ibid.).

<sup>27</sup>Schubring's position on Carnot is obscure; see note 22.

<sup>28</sup>"[L]a démonstration que nous venons de rappeler a été donnée pour la première fois par Galilée" [36, p. 422].

<sup>29</sup>Leibniz's position was similar; see main text at note 9.

<sup>30</sup>As noted by Redondi, "Parmi tous les mystères de la raison, Moigno retient celui de l'impossibilité logique d'un nombre actuellement infini" [104, p. 217].

<sup>31</sup>I.e., the *pseudo-infiniment petits* in GS's parlance; see Section 2.7.

GS's assumption that Moigno and Mansion expressed Cauchy's view remains in the realm of opinion rather than a supported position. Describing Moigno as Cauchy's "alter religious-philosophical ego" as GS does in [110, p. 3] carries little persuasive force. It must be noted that already in his key publication, GS expressed his appreciation of the good Abbot:

Moigno not only rejects Poisson on the basis of the concepts shared at that time; he simultaneously discloses the *massive contradiction* in the French mathematical community. [108, p. 455] (emphasis added)

For an analysis of the Moigno–Schubring “massive contradiction” see note 25.

### 3. SUM THEOREM, INTEGRAL GEOMETRY, AND CONTINUITY

**3.1. Sum theorem.** Cauchy's sum theorem concerning conditions for continuity of the sum of a series of continuous functions has long been the subject of a controversy. In 1821, Cauchy published a version of the theorem in his *Cours d'Analyse* [33]. Abel and others eventually pointed out that the theorem seems to “suffer exceptions.” In 1853, Cauchy published a version of the theorem with an apparently modified hypothesis in the article [37]. The issue is summarized in Section 3.6. Some scholars have argued that the modified hypothesis is equivalent to uniform convergence. Robinson's interpretation includes the following two items:

- (1) the hypothesis of the 1821 theorem required only convergence at standard points, and therefore without additional assumptions, the theorem was incorrect as stated [106, pp. 271–272];
- (2) the hypothesis of the 1853 theorem required convergence at all points, including infinitesimals, resulting in a correct theorem when interpreted in nonstandard analysis [106, p. 273].<sup>32</sup>

The sum theorem was mentioned by Bair et al. in [14, Section 2]. Reacting to this mention, Schubring claims the following:

[T]he authors' principal aim here is to show that in these applications, Cauchy used infinitesimals as numbers, hence in the non-standard meaning. Yet, right at the beginning, this intention leads the authors to *falsify* a text by Cauchy. In their initial section on summation of series (Bair et al. 2020, p. 130), they give a truncated quotation: “He then states his convergence theorem modulo a hypothesis that the sum  $u_n + u_{n+1} + \dots + u_{n'-1}$  should be *toujours infiniment petite pour des valeurs infiniment grandes des nombres entiers  $n$  et  $n' > n$  . . .* (Cauchy [18], 1853, p. 457)” . . . Cauchy's text had been, however: . . . *la somme devient toujours infiniment petite, quand . . .* Cauchy had, hence, unequivocally expressed that the sum becomes as small as one wishes, thus dealing with a limit process. [110, pp. 2–3] (emphasis of “falsify” added)

In what way has Cauchy's text been allegedly falsified? Consider the following two items.

- (1) Note the difference in wording: [14] quoted the Cauchyan passage as saying “*toujours infiniment petite pour etc.*” whereas GS quotes it as saying “*devient toujours infiniment petite, quand etc.*” Thus allegedly Cauchy's *quand* was replaced by *pour*.

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<sup>32</sup>See Section 3.2 on Spalt's coverage of Robinson's interpretation.

- (2) GS stresses that the Cauchyan passage was truncated through the deletion of the verb *devient*, which, as he claims, represents a limiting process.

As far as item (1) is concerned, note that on page 457 Cauchy writes

*pour des valeurs infiniment grandes des nombres entiers  $n$  et  $n' > n$ ,*

using *pour*, exactly as quoted in [14]. On the next page 458 (*not* cited in [14]), there is a complex variable version of the theorem where Cauchy writes

*quand on attribue des valeurs infiniment grandes aux nombres entiers  $n$  et  $n' > n$ ,*

using *quand* as quoted by GS. It emerges that GS was merely looking at the wrong page in Cauchy's paper when reviewing Bair et al. [14].

As far as item (2) is concerned, the interpretation of Cauchy's *devient* was discussed in detail in Błaszczyk et al. [26] and elsewhere. GS's suggestion that the issue is being ignored by the group of scholars in question (see Section 2.2) is therefore baseless and misleading. Cauchy's *devient* was also discussed in a 2011 article in *Perspectives on Science* [70], and specifically in reference to Cauchy's sum theorem; and more recently in Bascelli et al. ([19], 2018). The rival interpretations are summarized in [72]. The substantive issue with regard to item (2) was outlined in Section 2.1.

If GS wishes to go with an Archimedean interpretation, he would have to interpret *becomes* as simply *is*. Then GS would find Cauchy asserting that

“the sum  $u_n + u_{n+1} + \dots + u_{n'-1}$  *is* always infinitely small for infinitely large values of the integers  $n$  et  $n' > n$ .”

This is a reasonable interpretation of the passage that ultimately hinges on the meaning of the term *infinitely small*; see Section 2.1. However, GS's assumption that the word *devient* “represents a limiting process” in a necessarily Archimedean context, remains in the realm of opinion rather than a position supported by evidence.

**3.2. Procedures and foundations in Spalt.** In a 2022 book, Detlef Spalt claims that

[Robinson's] result was: Cauchy's theorem is correct if we add one of the two additional assumptions: (a) the series is uniformly convergent or (b) the family  $(s_n(x))_n$  of partial sums is equicontinuous in the interval. [117, p. 239]

However, Spalt's claim is true only with regard to Robinson's interpretation of the 1821 formulation of the sum theorem. As mentioned in Section 3.1, item (1), Robinson assumes that convergence was required only at standard  $x$  in 1821. Meanwhile, as mentioned in item (2), Robinson goes on to address Cauchy's 1853 formulation, and states clearly that Cauchy's term “*toujours*” refers to the extension of the convergence condition from standard  $x$  to all  $x$  (including infinitesimal values):

If we interpret this theorem in the sense of Non-standard Analysis, so that ‘infiniment petite’ is taken to mean ‘infinitesimal’ and translate ‘*toujours*’ by ‘for all  $x$ ’ (and not only ‘for all standard  $x$ ’), then the condition introduced by Cauchy ... amounts precisely to uniform convergence. [106, p. 273]



Spalt's misrepresentation of Robinson's Cauchy scholarship (see further in Section 3.3) is symptomatic of a deeper problem. A colorful instance of conflation of procedures and foundations occurs in Spalt, who wrote:

Nonstandard-analysis needs to perform some acts of conceptual acrobatics in order to construct “hyper-real” numbers in a mathematically acceptable way. Here Robinson's construction stands out, but without studying (at least) one semester of modern logic, one is not able to follow his construction. It is, however, unlikely that Cauchy should have anticipated such a stilted concept in 1821 in any possible sense. [117, p. 134]

This “acrobatics” passage overlooks the fact that the issue is not how hard Robinson's construction is, but rather how hard it is for Spalt (and Schubring) to appreciate the distinction between procedures and foundations; see further in Section 3.9.

**3.3. Spalt on Robinson.** Oddly, two decades earlier Spalt did recognize that in Robinson's interpretation, the 1853 result was correct:

Nachdem Robinson die spätere Formulierung des Summensatzes durch Cauchy aus dem Jahr 1853 angeführt hat, kommt er zu dem Schluss, die dort gegebene Formulierung laufe ‘genau auf die gleichmäßige Konvergenz in Übereinstimmung mit (i) oben hinaus’. [116, p. 297]

In general, inaccuracies abound in Spalt's reporting on nonstandard analysis. Thus, he claimed that the intermediate value theorem is false in nonstandard analysis:

Take for example the Intermediate value theorem. Every continuous function which has two different values takes on each intermediate value in between. This is a basic theorem of classical analysis. But it is not true in non-standard analysis, . . . [115, p. 167]

Of course the intermediate value theorem is in the scope of the transfer principle and therefore is as true in nonstandard analysis as in classical analysis, and moreover can be proved using infinitesimals; see e.g., [106, p. 67].

**3.4. Integral geometry.** One of Cauchy's goals in [34] is a formula in integral geometry expressing the length of a curve in terms of an average of the lengths of its projections to the pencil of lines through the origin. There are two aspects of the problem, described in [14] as “the curve” and “the Grassmannian,” i.e., circle of directions from a point. Cauchy subdivides the curve into infinitesimal subsegments (*éléments infiniment petits*). By contrast, he approximates the Grassmannian circle by a regular  $n$ -gon, and studies the asymptotic behavior of the resulting approximation as  $n$  tends to infinity. If an infinitesimal merely meant a sequence to Cauchy, then there shouldn't be any difference in Cauchy's treatment of the curve and the Grassmannian; both should be sequences. The fact that Cauchy does treat them differently suggests that his infinitesimals are not merely sequences.

Concerning the analysis in [14] of Cauchy's theorem in integral geometry, GS writes the following:

*Lengths.* In a section on integral geometry, the authors refer to a publication by Cauchy where he “exploits a decomposition of a curve into infinitesimal length elements (respectively, of a surface into infinitesimal area elements)” (Bair et al. 2020, p. 130). The key formulation for them is Cauchy's statement (Bair et al. 2020, p. 132):

Le théorème II étant ainsi démontré pour le cas particulier où la quantité  $S$  se réduit à une longueur rectiligne  $s$ , il suffira, pour le démontrer dans le cas contraire, de décomposer  $S$  en *éléments infiniment petits*. (Cauchy [17], 1850, p. 171; emphasis added)

As *there is no conceptual analysis* of this issue and as their putting “infinitely small elements” in italics proves, it is sufficient for their intended appropriation of Cauchy as a forerunner of non-standard analysis to find the term “infinitesimal” or “infiniment petit” in his texts. [110, p. 4] (emphasis on “there is no conceptual analysis” added)

Here GS claims that

- (1) there is “no conceptual analysis of this issue” in [14], and that
- (2) the conclusion is based merely on the use of the term “infinitely small elements” by Cauchy.

However, both claims are inexact. Not only does [14] provide an analysis to justify its conclusion, but there is an almost page-long subsection 4.2 entitled “Analysis of Cauchy’s argument” [14, pp. 132–133] where the conclusion is justified (along the lines of the summary provided at the beginning of this section).

GS is free to disagree with the interpretation given in [14], but he is less free to misrepresent it for the readers of MathSciNet.

**3.5. Infinitesimals and null sequences.** In a further comment on Bair et al. [14], GS claims that

[T]he paper reveals several points where the authors withdraw claims made in earlier publications of the group” [110, p. 1]

He attempts to illustrate his claim by contrasting two passages:

- (1) a passage from 2013 to the effect that “In Cauchy, any variable quantity  $q$  that does not tend to infinity is expected to decompose as the sum of a given quantity  $c$  and an infinitesimal  $\alpha$ :  $q = c + \alpha$ ” [10, pp. 900–991], and
- (2) a passage from 2020 to the effect that “Cauchy’s presentation of infinitesimal techniques [in the calculation of the radius of curvature] contains no trace of the variable quantities or sequences exploited in his textbooks in the definitions of infinitesimals” [14, p. 135].

Comparing the two passages, GS claims that

in [the 2013] presentation of the essentials of their claims, the notion of null sequences was apodictically excluded. In [2020], *the authors admit* such a notion for Cauchy, . . . [110, p. 1] (emphasis on “the authors admit” added)

The problem with GS’s claim is two-fold:

- (1) Contrary to Schubring’s claim, null sequences were indeed mentioned in 2013, in the following terms: “Cauchy handles the said notion [related to uniform convergence] using infinitesimals, including one generated by the null sequence  $(\frac{1}{n})$ ” [10, p. 891].
- (2) Null sequences were discussed in detail in 2011 (and elsewhere) in the following terms: “infinitesimals themselves are defined in terms of variable quantities becoming arbitrarily small (which have often been interpreted as null sequences). Cauchy writes that such a null sequence ‘becomes’ an infinitesimal  $\alpha$ ” [70, p. 428]

By seeking to contrast the 2013 and the 2020 articles, Schubring commits himself to evaluating the work of “at least 22 mathematicians and philosophers” as a whole (see Section 2.2). He attempts to present the discussion of null sequences in 2020 as some kind of late afterthought (“the authors admit, etc.”), but ends up misrepresenting the work by the group. His odd claim concerning “the authors withdraw[ing] claims made in earlier publications of the group” is groundless, and does not improve with repetition:

*Admitting* that Cauchy had exploited the null-sequences notion in his textbooks, the authors aim to verify whether their infinitesimalist interpretation is at least viable in certain applications, ... [110, p. 2] (emphasis on “Admitting” added)

As documented above, Cauchy’s notion of null sequence was a constant presence in the work by the group, rather than some kind of late “admission” as claimed misleadingly by GS.

**3.6. Ambiguous continuity.** In the same vein, GS goes on to claim that

Regarding the concept of continuity, the paper under review shows another and even more remarkable change of position and withdrawal. ... They comment ‘that Cauchy’s definition of continuity is, from a modern viewpoint, somewhat ambiguous’ (Bair et al. 2020, p. 140). All former certainty is gone. And this is *not too far* from the result of the careful analysis of Umberto Bottazzini, who chose to speak of ‘ambiguous’ and to attribute to Cauchy a special meaning of continuity, ‘C-continuity’ [110, pp. 4–5] (emphasis on “not too far” added)

Here GS juxtaposes the use of the adjective *ambiguous* by Bair et al. [14] and by Bottazzini, and suggests that they are “not too far.” The problem with GS’s claim is two-fold:

- (1) Cauchy’s final definition of continuity requires  $f(x+\alpha)$  to be infinitely close to  $f(x)$  for all infinitesimal  $\alpha$ . The definition is ambiguous since it is unclear whether Cauchy meant this to apply at “ordinary”  $x$  or, in addition, at  $x$  generated by variable quantities.<sup>33</sup> This is the ambiguity referred to by Bair et al. in 2020 on page 140, involving no change of position relative to earlier articles by the group.
- (2) Declaring Cauchy’s continuity to be “C-continuity” (or for that matter Weierstrass’s continuity to be “W-continuity”) carries as little explanatory power as explaining the classical infinitesimalists’ work by their possession of “an unerring intuition” [52, p. 358].

While this is not the place to analyze Bottazzini’s concept, it is necessary to point out that it is unrelated to the issue discussed in item (1) above, contrary to GS’s suggestion. Thus GS’s claim of “remarkable change of position and withdrawal” is baseless.

**3.7. Their own non-standard-analysis concept.** GS lodges the following claim concerning Cauchy’s colleagues Poisson, de Prony, and Petit:

[T]he authors affirm that all of them [i.e., Cauchy’s colleagues] not only shared the same conceptions, but even that there had been just one unique conception of infinitesimals: *their own non-standard-analysis concept*:

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<sup>33</sup>For a modern formalisation see e.g., [29].

There seems to be little reason to doubt that the notion of infinitely small in the minds of Poisson, de Prony, Petit, and others was solidly in the Leibniz–l’Hôpital–Bernoulli–Euler school. (Bair et al. 2020, p. 142)

[110, p. 6] (emphasis added)

What exactly is the connection between GS’s claim and the indented quotation from Bair et al. [14]? The indented passage mentioning Poisson, de Prony,<sup>34</sup> Petit, Leibniz, l’Hôpital, Bernoulli, and Euler<sup>35</sup> is apparently quoted as evidence of an alleged positing of a “unique . . . non-standard-analysis concept.” It does not require great analytical skills on the part of the reader to ascertain that nonstandard analysis was not mentioned at all in the passage quoted as evidence for GS’s non-standard claim. Rather, the passage emphasized the 17th–18th century infinitesimalist tradition. GS’s claim that [14] attributed a “non-standard-analysis concept” to Cauchy’s colleagues is therefore as baseless as his rhetorical flourishes targeting Laugwitz quoted in Sections 2.2 and 3.8.

**3.8. Hermetic monologue?** In 1989, Laugwitz presented a detailed comparative analysis of the approaches of Fourier, Poisson, and Cauchy to the method of auxiliary multipliers for obtaining values of indeterminate series and integrals; see [86, Section 5, pp. 218–232]. Thus, Laugwitz was clearly aware of the fact that taking into account contemporary work by Fourier and Poisson is essential for understanding Cauchy himself. One would not guess as much from the portrayal of Laugwitz’ work as depicted by GS:

- (1) “[Laugwitz] reduced [Cauchy’s universe of discourse] to a hermetic monologue by Cauchy” [108, p. 4];
- (2) “they [Laugwitz and others] did not use Cauchy’s communication with contemporary mathematicians as a means to uncover what the respective concepts meant in their own period” [108, p. 4];
- (3) “. . . Laugwitz . . . had practically assigned a solipsistic mathematics to Cauchy” [108, p. 434];
- (4) “. . . Laugwitz’s approach of seeking meaning exclusively through the internal ‘conceiving’ of a text is typical” [108, p. 441].<sup>36</sup>

Given Laugwitz’s attention to the historical context of Cauchy’s work as mentioned above, GS’s “hermetic monologue, etc.” comments amount to a strawman criticism.

Bair et al. [14] commented in detail on the work of Cauchy’s contemporaries Poisson, de Prony, and Petit (see Section 3.7). Related comments on Fourier had

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<sup>34</sup>GS’s stance on de Prony is puzzling. Namely, GS attributes to de Prony “the exclusion and rejection of *infiniment petits* by the analytic method. In de Prony the *infiniment petits* were excluded from the foundational concepts of his teaching by simply not being mentioned; etc.” [108, p. 289]. GS’s claim flies in the face of de Prony’s detailed treatment of the problem of infinitesimal oscillations, as well as his derivation of the formula for  $\cos z$  in terms of the exponential function following Euler; see [13, Section 3.6].

<sup>35</sup>GS’s stance on Euler is puzzling. He claims that “Euler established a purely algebraising foundation, achieving its climax in Lagrange’s theory of functions” [110, p. 1]. The claim is meaningless without specifying what “algebraising” means exactly. GS’s intention here seems to be to minimize the importance of Euler’s infinitesimals; related remarks were made by Ferraro (see item (2) in Section 4.1). A rebuttal of such views appears in [9].

<sup>36</sup>While sharp criticisms of Laugwitz’s Cauchy scholarship by Schubring and by Spalt are well known, what is perhaps less known is Schubring’s sharp criticisms of Spalt’s Cauchy scholarship; in fact all of the criticisms cited here target both Laugwitz and Spalt.

already appeared in the work [10, p. 61] quoted by GS, and elsewhere. One would not guess as much from Schubring’s strawman depiction of [10]:

[T]hey had understood ‘context’ as referring just to other parts of the same text, and were defending Detlef Laugwitz’s earlier interpretations of Cauchy, attributing to him a proper ‘universe’, independent and free of all relation with contemporary mathematics: . . . [110, p. 5]

It is true that the article [10] contained a defense of Laugwitz from GS’s strawman criticisms already in 2013, but neither Laugwitz nor any of the scholars caricatured by GS in the above comments ever attributed to Cauchy “a proper ‘universe’ independent and free of all relation with contemporary mathematics.”

**3.9. Procedures, foundations, and misconceptions.** GS’s comments about Poisson, de Prony, Petit (see Section 3.7) and “non-standard numbers” (see Section 2.6) reveal his greatest misconception concerning formalisations of historical mathematics. He appears to believe that interpreting the work of historical infinitesimalists in terms of Robinson’s framework for infinitesimal analysis necessarily amounts to attributing “non-standard numbers” to those 19th century authors. If this meant attributing some kind of “anticipation” of ultrafilters<sup>37</sup> to 19th century authors, it would certainly amount to an absurd interpretation.

However, formalisation of the work of historical infinitesimalists in terms of modern theories of infinitesimals involves only providing suitable proxies for their procedures and inferential moves (see e.g., [14]; on procedures see further in [11]). Such formalisations presuppose no relation to the foundational aspect of the grounding of Robinson’s infinitesimal analysis in modern set-theoretic terms (see also [39]). The viability of applying NSA to interpreting the procedures of the historical infinitesimalists depends crucially on the procedure/foundation distinction.

The related procedure/ontology distinction was emphasized in our 2017 objection to GS’s position, published five years ago [26, p. 126]. Five years later, GS’s 2022 effort [110] reveals little awareness of our objections.

Unlike Schubring and Spalt (see Section 3.2), Detlef Laugwitz clearly realized this distinction. Thus, in his 1987 publication in *Historia Mathematica* [85] he carefully distinguished between

- (1) his analysis of Cauchy’s procedures presented in Sections 1 through 14, and
- (2) his proposed models of Cauchyan infinitesimals in terms of modern infinitesimal theories, in his Section 15.

See further in [26, Section 6.2].<sup>38</sup>

The key insight is that Robinson’s procedures provide better proxies than Weierstrassian ones. Thus, Robinson’s standard/nonstandard distinction<sup>39</sup> is a proxy for Leibniz’s assignable/inassignable distinction. The latter found 19th century echoes in the work of Noël and Manilius (see Section 2.7), ignored by GS.

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<sup>37</sup>For the benefit of the reader not familiar with the details of the construction of the hyperreal field  ${}^*\mathbb{R}$ , it may be useful to recall that  ${}^*\mathbb{R}$  is obtained as a quotient  $\mathbb{R}^{\mathbb{N}}/\mathcal{U}$  where  $\mathbb{R}^{\mathbb{N}}$  is the space of sequences of real numbers, and  $\mathcal{U}$  is a nonprincipal ultrafilter on  $\mathbb{N}$ . For details see e.g., [46]. Recently it turned out that ultrafilters are unnecessary for analysis with infinitesimals, which can be developed conservatively over ZF; see [62].

<sup>38</sup>A failure to appreciate such a distinction led GS to accuse Laugwitz of attributing hyperreal numbers to Cauchy; see note 19 and the main text there.

<sup>39</sup>For example, numbers come in two varieties: standard and nonstandard. Nonzero infinitesimals are necessarily nonstandard.

**3.10. Interpretations, lores, and crusades.** GS has pursued a different interpretation of Cauchy, and had already expressed himself in his key publication concerning Laugwitz’s alleged attribution of hyperreal numbers to Cauchy (see Section 2.2), and in 2016 concerning his opponents’ alleged “misconceptions” [109]. However, note the following five points.

1. GS arguably goes beyond a scholarly disagreement when he resorts to accusing his opponents of allegedly “leading a crusade against historiography” [110, p. 1] (see Section 2.8) and “falsifying Cauchy’s text” (see Section 3.1), as he did when he accused Laugwitz of attributing hyperreal numbers to Cauchy (see Section 2.2), as well as of attributing “a hermetic monologue” and “solipsistic mathematics” to Cauchy (see Section 3.8).

2. GS formulates a sweeping indictment not merely of the article [14] but of an entire research program (see Section 2.2). But then he goes on to accuse the authors of [14] of failing to mention Cauchy’s verb *devient*, while himself failing to mention the detailed analyses of *devient* in the earlier articles (see Section 3.1). Such selective coverage comes dangerously close to being mendacious by omission, and falls short of a legitimate scholarly criticism.

3. GS’s one-sided portrayal of 19th century *infinitesimal lore*, where allegedly infinitesimals were necessarily “standard lore for expressing an arbitrarily small number” [110, p. 3], is contrary to historical fact; see the Belgian debate in Section 2.7 and the analysis there of the dual nature of 19th century infinitesimal lore. His one-sided portrayal is also contrary to what GS wrote in his key publication concerning the “far-reaching” influence in France of Poisson’s genuine infinitesimals (or the “pseudo-*infiniment petits*” in GS’s parlance), clearly establishing a plurality of such *lores*. Thus, what GS claims in 2022 is at odds with his own interpretation as developed in his key publication.

4. GS’s contention that Bair et al. [14] attributed “non-standard numbers” to 19th century authors (see Sections 2.6 and 3.7) is a strawman criticism (parallel to a similar contention with regard to Laugwitz mentioned in item 1). Indeed, no such claim appeared in [14], where it was emphasized, on the contrary, that procedures based on modern infinitesimals are only proxies for the procedures of the historical infinitesimalists (see Section 3.9). Furthermore, [14] clearly acknowledged the profound differences in the background ontology of the historical infinitesimalists and the modern ones. There are further misrepresentations in GS’s 2022 review [110] but we will limit ourselves to the remarks already made, for lack of space.

5. GS claims that when Cauchy’s 1826 text on differential geometry (see Section 2.4) speaks of  $\varepsilon$  as “un nombre infiniment petit” he is *not* referring to a genuine infinitesimal (or a *pseudo-infiniment petit* in GS’s parlance). Here Cauchy develops a formula for the radius of curvature  $\rho$  at a point of a curve in terms of the variation  $\Delta\tau$  of its tangent vector. Significantly, this particular  $\varepsilon$  occurs in the same equation as Cauchy’s increment  $\Delta\tau$ :

$$\frac{\sin\left(\frac{\pi}{2} \pm \varepsilon\right)}{r} = \frac{\sin(\pm\Delta\tau)}{\sqrt{\Delta x^2 + \Delta y^2}}. \quad (3.1)$$

From (3.1), Cauchy derives the relation

$$\frac{1}{\rho} = \pm \frac{d\tau}{\sqrt{dx^2 + dy^2}}$$

involving the radius of curvature  $\rho$ , by passing to the limits [34, pp. 98–89]. Cauchy refers to his  $\Delta\tau$  as an *angle de contingence* (ibid.), sometimes translated as *hornlike angle*. This is a traditional term for an angle incomparable with ordinary rectilinear angles at least since the 16th century. As GS acknowledged in his key publication,

Klein has shown in detail that the hornlike angles form a model of non-Archimedean quantities. [108, p. 17]

The reference is to [80, p. 221] (see also [84]).<sup>40</sup> The occurrence of  $\varepsilon$  and the hornangle  $\Delta\tau$  in the same equation indicates that this particular  $\varepsilon$  shares the non-Archimedean nature of  $\Delta\tau$ . In more detail, since the hornangle  $\Delta\tau$  on the right-hand side of (3.1) is infinitesimal,  $\sin(\pm\Delta\tau)$  is also infinitesimal; therefore  $\sin(\frac{\pi}{2} \pm \varepsilon)$  on the left-hand side is infinitely close to 1; hence  $\varepsilon$  is also infinitesimal. In 2022, GS’s account of Cauchy’s 1826 text in differential geometry fails to clarify the non-Archimedean context of the  $\varepsilon$  as used by Cauchy. Schubring’s assumption that  $\Delta\tau$  is a finite difference betrays a presentist bias.

**3.11. Default number systems.** Our gentle reader may well wonder why explorations of Cauchy’s use of genuine infinitesimals in work by Laugwitz and Bair et al. should provoke GS to describe such work as anachronistic attempts to depict Cauchy as hermetic, solipsistic, and “free of all relation with contemporary mathematics” (see Section 3.8). This is especially puzzling since GS himself clearly enunciated his disagreement with Grabiner’s portrayal of Cauchy as a pioneer of the Weierstrassian *Epsilontik* (see Section 2.5). The explanation depends crucially on the distinction between the following two questions:

- (A) Did Cauchy’s work contain a significant *Epsilontik* component?
- (B) Did Cauchy use only Archimedean quantities?

The fact that GS identifies fully with 19th century critics of infinitesimals such as Moigno and Mansion suggests that, while GS would answer question (A) in the negative, he assumes Cauchy’s background continuum to be Archimedean as the *default* option that requires no further argument. Such an assumption (a natural product of modern undergraduate training in naive set theory and calculus/analysis) leads GS to assume further that Cauchy’s contemporaries would have been unable to understand Cauchy had the latter used genuine infinitesimals. Significantly, GS assigns ontological import to Cauchy’s expression *absolute numbers*, whereas in reality Cauchy merely referred to the convention of interpreting unsigned numbers  $x$  as positive ( $+x$ ) rather than negative ( $-x$ ) numbers (see Section 2.8). Products of such default thinking are the ‘hermetic’ and ‘solipsistic’ flourishes (see Section 3.8); no wonder GS believes infinitesimals to be “vaguely conceived” (see Section 1.3).<sup>41</sup>

Indeed, the outcome of Schubring’s 2022 analysis in [110] is predetermined by his historiographic assumptions, one of which seems to prohibit Cauchy from using

<sup>40</sup>On Klein see further in [15] and [66].

<sup>41</sup>In the same vein, Spalt appears to view genuine infinitesimals as bordering on the supernatural: “an ‘infinitely small quantity’ is for Leibniz nothing supernatural, inconceivable—but only a special case of a commonly used changing quantity: just one which decreases indefinitely” [117, p. 36].

genuine infinitesimals, in spite of the evidence provided by Laugwitz in his publications in *Historia Mathematica* [85] and *Archive for History of Exact Sciences* [86] and elsewhere, and by Bair et al. in *British Journal for the History of Mathematics* [12] and elsewhere. We will analyze some related historiographic issues in Section 4.

#### 4. ROYAL ROAD TO THE GREAT TRIUMVIRATE

**4.1. Teleology through examples.** One persistent theme in the historiography of the 19th century is the perception that mathematical analysis reached its teleological fulfillment with the development of what mathematicians often consider to be ultimate foundations of analysis during the period of Weierstrass and following. Such perspectives typically include the conception of an infinitesimal-free continuum as the true foundation of analysis, and come assorted with an enduring faith in a literal interpretation of the epithet *real* in the expression *real number* (an attitude that tends to overlook the fact that we only have a *theory* of real numbers, not an absolute standard *model* a.k.a. the *intended interpretation*),<sup>42</sup> and the accompanying enduring faith that the elimination of infinitesimals was an inevitable part of the teleological process. We provide some examples from the recent literature on Leibniz, Euler, and Cauchy.

- (1) There have been sustained efforts ranging from Ishiguro ([63], 1990) to Rabouin and Arthur ([103], 2020) to deny that Leibniz meant the term *infinitesimal* to refer to a mathematical entity (see Section 1.2). Most recently in 2022, Arthur persists in an error already diagnosed by Fraenkel over a century ago; see Section 1.5.
- (2) Commenting on Euler’s *Introductio in analysin infinitorum*, Ferraro perceives a causal connection between the use of infinitesimals and lack of success:
 

“Euler was not entirely successful in achieving his aim since he introduced infinitesimal considerations in various proofs.”<sup>43</sup> [44, p. 11]
- (3) Siegmund-Schultze suggests that infinitesimals in Cauchy are not merely a remnant of the past but actually constitute a step backward:
 

“There has been . . . an intense historical discussion in the last four decades or so how to interpret certain apparent remnants of the past or – as compared to J. L. Lagrange’s (1736–1813) rigorous ‘Algebraic Analysis’ – even *steps backwards* in Cauchy’s book, particularly his use of infinitesimals . . .” ([113]; emphasis added)

Attempts to present Cauchy as a precursor of Weierstrass in the received Cauchy literature are based on similar assumptions,<sup>44</sup> and ignore numerous works and applications where Cauchy used infinitesimals as numbers, as detailed in [14] (see Section 2.7 for an example).

Schubring claims that the group Bair et al.

has been trying to rewrite the history of the infinitesimal calculus as a forerunner of non-standard analysis [110, p. 1]

but overlooks the fact that Bair et al. only object to

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<sup>42</sup>Such attitudes are common among mathematicians who describe themselves as Platonists, such as Alain Connes; see further in [69], [76] [67], and [107, Section 3.5].

<sup>43</sup>As argued in [9], Euler was more successful in achieving his aim than some historians believe.

<sup>44</sup>See Section 2.1.



- (1) writing the history of the infinitesimal calculus as necessarily a forerunner of the Weierstrassian *Epsilontik*, and
- (2) Schubring's assumption that Cauchyan infinitesimals belong in an infinitesimal lore (see Section 2.7) that is unequivocally Archimedean.

Schubring's "old-fashioned" [110, p. 1] take on Leibniz is analyzed in Section 1.3, and his take on Cauchy, in Sections 2 and 3.

**4.2. Fraser on inevitable evolution.** Certainly, some historians are aware of the pitfalls of teleological fallacies analyzed in Section 4.1. Thus, Fraser writes:

Since the 1960s there has been a new wave of writing about the history of eighteenth-century mathematics. Authors such as Henk Bos, Steven Engelsman, Niels Jahnke, Giovanni Ferraro, Craig Fraser and Marco Panza have charted the development of calculus without interpreting this development as a first stage in the inevitable evolution of an arithmetic foundation. [48, p. 27]

Fraser appears to acknowledge that the traditional "arithmetic foundation" of classical analysis as developed around 1870 was not the inevitable result of the evolution of analysis. Yet he immediately goes on to reassure his readers in the following terms:

- (1) Of course, classical analysis developed out of the older subject and it remains a primary point of reference for understanding the eighteenth-century theories. [48, p. 27]
- (2) The relevance of modern non-Archimedean theories to an historical appreciation of the early calculus is a moot point. [48, p. 43]

Postulating the superiority of classical analysis over non-Archimedean theories as the basis for a historical appreciation of the early calculus involves precisely the type of teleological fallacy examined in Section 4.1. Fraser proceeds to succumb to a closely related fallacy of conflating procedures and foundations (see Section 3.9):

[N]onstandard analysis and other non-Archimedean versions of calculus emerged only fairly recently in somewhat *abstruse mathematical settings* that bear little connection to the historical developments one and a half, two or three centuries earlier. [48, p. 27] (emphasis added)

For the benefit of the reader not familiar with foundational subtleties, we hasten to point out that Robinson's framework is grounded in the traditional Zermelo–Fraenkel set theory, no longer considered abstruse by classically-trained mathematicians. We can agree with Fraser that the *foundational* aspects of grounding Robinson's infinitesimals in classical set theory bear little connection to the historical developments from Leibniz to Cauchy. However, the *procedures* of these pioneers of analysis do exhibit a strong connection to those developed by Robinson.<sup>45</sup>

A recent piece by Archibald et al. published in *The Mathematical Intelligencer* in response to the article "Two-track depictions of Leibniz's fictions" [75] enables a considerable extension of the list of scholars who apparently have difficulty separating the contention that Leibniz and others exploited procedures using genuine infinitesimals, from the idea of a "pervasive presence of nonstandard analysis in the history of mathematics." The overlap between Archibald's coauthors and Fraser's

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<sup>45</sup>See further in [8, Sections 4.2–4.6, pp. 123–128] and [19, Section 4.4, pp. 277–278] for an analysis of Fraser's text.

list of leading scholars (see above) includes Ferraro and Panza, apparently providing evidence against Fraser’s claim quoted at the beginning of this section. See Section 5 for more details.

**4.3. Grattan-Guinness on presentism.** In an influential 1990 article, Grattan-Guinness wrote:

[Mathematicians] usually view history as a record of a ‘royal road to me’ – that is, an account of how a particular modern theory arose out of older theories instead of an account of those older theories in their own right. In other words, they confound the question, ‘How did we get here?’, with a different question, ‘What happened in the past?’ [55, p.157] (cf. [56, p.165])

The critique of the “How did we get here?” attitude is on target.<sup>46</sup>

Grattan-Guinness’s *royal road to me* issue seems closely related to the issue of presentism. As an example, Grattan-Guinness criticized the approaches of Dieudonné and Birkhoff for presentism, but painted a more sympathetic picture of the approach of André Weil.<sup>47</sup>

For better sensitivity to the issues exhibited by another eminent mathematician, see the lecture delivered by A. Weil in 1978 to an international congress of mathematicians: “History of Mathematics - why and how”, . . . [55, note 19, p.170]

Grattan-Guinness’s criticism does apply to Bourbaki’s approach (to writing the history of mathematics), and specifically in reference to its teleological aspect; see further in Section 4.1.

**4.4. Hacking’s dichotomy.** In his last book, Ian Hacking ([59], 2014) presented an analysis closely related to Grattan-Guinness’ critique. Hacking introduces a distinction between the butterfly model and the Latin model for the development of a scientific discipline. Hacking contrasts a model of a deterministic (genetically determined) biological development of animals like butterflies (the egg–larva–cocoon–butterfly sequence), with a model of a contingent historical evolution of languages like Latin. Hacking notes that

If analysis had stuck to infinitesimals in the face of philosophical naysayers like Bishop Berkeley, analysis might have looked very different. Problems that were pressing late in the nineteenth century, and which moved Cantor and his colleagues, might have received a different emphasis, . . . This alternative mathematics might have seemed just as ‘successful’, just as ‘rich’, to its inventors as ours does to us. [59, p.119].

To borrow Hacking’s terminology, one could say that some historians of mathematics seem convinced that the butterfly of rigorous analysis needed to shed its infinitesimal cocoon in order to fly. Emphasizing determinism over contingency can easily lead to anachronism; see further in [13]. Dauben asks:

Is it anachronistic to use nonstandard analysis or transfinite numbers to “rehabilitate” or explain the works of Leibniz, Euler, Cauchy, or Peirce, for

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<sup>46</sup>Meanwhile, Grattan-Guinness’s stereotyping of mathematicians is unfortunate as it comes dangerously close to endorsing Unguru polarity; see further in [71].

<sup>47</sup>Subsequently Grattan-Guinness was more critical of Weil in [56, p.166].

example, as recent mathematicians, historians, and philosophers of mathematics have attempted? [40, p. 307]

He answers as follows:

Robinson succeeded in showing the reasonableness of “redrawing” the early history of the calculus to reinstate past views that, cast in the light of nonstandard analysis, could be seen more clearly. In these cases at least, an anachronistic explanation nevertheless serves to clarify, not confound, what had confused earlier defenders of theories based on infinitesimals like the calculus. [40, p. 327]

Dauben does not analyze the issue of anachronism in terms of the procedure/foundation distinction (see Section 3.9), but arguably an explanation addressing the procedures while acknowledging the differences in foundations, no longer needs to be described as anachronistic if it succeeds in accounting for the inferential moves of the historical authors.

**4.5. Contingency and determinism.** The contingency of the historical evolution of the mathematical sciences would entail in particular that the mathematical landscape today could have been different from what it currently is. Such a perspective is consonant both with Hacking’s Latin model (see Section 4.4) and with Grattan-Guinness’s critique of the “How did we get here?” approach (see Section 4.3). The shortcoming of the latter approach is its implied faith in the determinism of the historical evolution of mathematics. In the history of any science, it is philosophically problematic to claim a singular juncture when suddenly what was obscure becomes clarified.<sup>48</sup>

Some historians of mathematics tend to view the history of mathematical analysis as an exception in this regard, possibly under the influence of their undergraduate training in (naive) set theory and the *Epsilon*itik.<sup>49</sup> Such historians pursue a teleological reading of the history of analysis of the 17–19 centuries, giving credit to the “great triumvirate” [30, p. 298] of Cantor, Dedekind, and Weierstrass in this connection, in a version of the approach mocked by Grattan-Guinness that can be characterized as a *royal road to the great triumvirate*. In general, historians of natural science make no analogous “singular juncture” claims, and are on more solid ground historiographically speaking.

Viewed from this perspective, it is perhaps illusory, or more precisely circular, to insist that concepts like *infinitesimal* must be clarified before they can serve a useful purpose in an argument. Both the concepts and the arguments are part of an evolving effort by scholars to reach clearer understanding. Arguably this contention

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<sup>48</sup>A telling example of a postulation of such a singular juncture, following a millennial aspiration, is provided by Frank Quinn: “The breakthrough was development of a system of rules and procedures that really worked, in the sense that, if they are followed very carefully, then arguments without rule violations give completely reliable conclusions. . . . There is no abstract reason (i.e., apparently no proof) that a useful such system of rules exist, [sic] and no assurance that we would find it. However, it does exist and, after *thousands of years* of tinkering and under intense pressure from the sciences for substantial progress, we did find it” [102, pp. 31–32] (emphasis on “thousands of years” added). To illustrate the touted millennial breakthrough, Quinn provides the example of “Weierstrass’s nowhere-differentiable function (1872)” [102, p. 31]. We will not comment on Quinn’s assumption that the rigorisation of mathematical analysis occurred under “intense pressure from the sciences.”

<sup>49</sup>Thus, Knobloch [83, pp. 13–14] does not hesitate to appeal to  $\aleph_0$  [his notation] in a discussion of the Leibnizian calculus. See further in [73].

applies as much in mathematics as it does in natural science; the singular emphasis placed in some history of mathematics books on the foundational developments of the 1870s may amount to such a *royal road*. For all the seminal *mathematical* importance of the developments of the 1870s (known to all), their *philosophical* force was undercut by the mathematical developments of the 1970s when Karel Hrbacek [61] and Edward Nelson [100] developed axiomatic approaches to analysis with infinitesimals; see [68] for further details.

## 5. IS PLURALISM IN THE HISTORY OF MATHEMATICS POSSIBLE?

As elaborated in Sections 1 through 3, the authors of the present article have over the years developed the following perspective. Many mathematicians in the 17–19th centuries (Newton, Leibniz, Euler, Cauchy, . . .) employed one version or another of informal calculus with infinitesimals. When adhering to the internal rules of such calculi, they produced valid results and predictions in mathematics and physics.

Abraham Robinson’s Nonstandard Analysis (NSA for short; 1961), building upon earlier work by T. Skolem [114], E. Hewitt [60], J. Loś [95] and others, was a milestone in that it constituted the first fully rigorous approach to ‘calculus with infinitesimals’. In light of the groundbreaking nature of Robinson’s work, the role of pre-Robinson/pre-20th century infinitesimal calculi in the history of mathematics could be viewed as follows.

- (1) The procedures of pre-20th century infinitesimal calculi are formalized more successfully in NSA than in traditional mathematics based on the epsilon-delta framework in a purely Archimedean context.
- (2) The pre-20th century infinitesimal calculi should (at most) be viewed as an inspiration for NSA. In particular, classifying them as ‘forerunners’ of NSA (or similar concepts) is problematic in that such a classification projects a modern viewpoint onto 17–19th century mathematics.

We stand by this position, and in this section we defend the viability of using NSA in this sense to interpret the procedures of the historical infinitesimalists, against a recent attack by Tom Archibald et al. [2]. Furthermore, we feel that the possibility of pluralism in the historiography of mathematics has just received an unwarranted blow from Archibald et al.

**5.1. Depictions put in the pillory.** The article “Two-track depictions of Leibniz’s fictions” [75] was published in the september issue of the 2022 volume of *The Mathematical Intelligencer*. “Two-track depictions” analyzed rival interpretations of the procedures of the Leibnizian calculus, one of the issues being whether or not Leibniz used genuine infinitesimals. As emphasized in an earlier article [11] in the *British Journal for the History of Mathematics* and elsewhere, an analysis of the procedures of the historical infinitesimalists needs to be carefully distinguished from the foundational issues of the grounding of infinitesimals in modern set-theoretic frameworks (see Section 3.9).

The response by Archibald et al. to “Two-track depictions” was published online in *The Mathematical Intelligencer* on 29 september 2022. The response reveals that some historians of mathematics apparently have difficulty separating the contention that Leibniz and others exploited procedures using genuine infinitesimals,

from the idea of a “pervasive presence of nonstandard analysis in the history of mathematics.” Thus, Archibald et al. claim the following:

The aim [of “Two-track depictions” and other articles] is ... to put various scholars in the pillory – with accompanying abusive epithets – for not enthusiastically recognizing the pervasive presence of nonstandard analysis in the history of mathematics. [2, p. 1]

Indeed, there was no presence (pervasive or otherwise) of NSA in the history of mathematics before 1961 when it was first introduced by Robinson in [105], but the *procedures* of NSA provide better proxies for the procedures of the historical infinitesimalists than the procedures of Weierstrassian analysis in a purely Archimedean setting; see further in [25].

Archibald et al. do not provide any examples of alleged “abusive epithets” in “Two-track depictions” – for the simple reason that there are none – but see Section 5.2.

**5.2. Epithets and crusades.** The response by Archibald et al. was signed among others by Gert Schubring, the nature of whose epithets can be gleaned from comments he made about the work of Cauchy historian Detlef Laugwitz, sampled in Section 3.8. Given Laugwitz’s attention to the historical context of Cauchy’s work in his articles in *Historia Mathematica* [85], *Archive for History of Exact Sciences* [86], and elsewhere, Schubring’s comments about “hermetic monologue” and “solipsistic mathematics” amount to a strawman criticism already analyzed in [26].

It is no secret that, like Laugwitz, the authors of [11] and [75] have pursued an interpretation of Cauchy at variance with Schubring’s, in such venues as *Perspectives on Science* [70], *British Journal for the History of Mathematics* [12] and elsewhere. Schubring’s reaction to such work is on record. Referring to the work of “at least 22 mathematicians and philosophers” [110],<sup>50</sup> Schubring used the epithet “crusade against the historiography” to describe such work (see Section 2.2). Given such language, it is decidedly comical to find Schubring accusing scholars – who happen to disagree with his historical interpretations – of allegedly using “abusive epithets.”

**5.3. Sole aim?** Our aims in interpreting historical infinitesimalists were outlined at the beginning of Section 5. Archibald et al. claim that

these criticisms [contained in “Two-track depictions” and other articles] do not consist in reasoned historical argument, but rather in piecemeal confutation of isolated quotations of their opponents taken out of context, whose *sole aim* is to show them as enemies of the group’s understanding of infinitesimals. [2, p. 1] (emphasis added)

Contrary to such claims, the “22 mathematicians and philosophers” (see Section 5.2) did present reasoned historical arguments against what they see as untenable received interpretations, and proposed better alternatives.

Consider for example the case of Richard Arthur, one of Archibald’s coauthors. The 2021 article in *British Journal for the History of Mathematics* [11] argued in detail that

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<sup>50</sup>Schubring’s unrefereed opinion piece is endorsed by Archibald et al. [2, note 6].

- (1) Arthur’s attempt in [4] to interpret the Leibnizian calculus in terms of a modern theory of infinitesimals called *Smooth Infinitesimal Analysis* is unviable, and
- (2) proposed a more viable alternative (which is currently the target of [6]).

Another coauthor of Archibald’s, Jeremy Gray, wrote the following about Euler’s foundations:

Euler’s attempts at explaining the foundations of calculus in terms of differentials, which are and are not zero, are *dreadfully weak*. [58, p. 6] (emphasis added)

Unfortunately, Gray provided no context for his “dreadfully weak” claim concerning Euler’s foundations, suggesting that he assumes such views to be generally accepted. Gray’s assumptions were challenged in a detailed study of Euler in *Journal for General Philosophy of Science* [9]. Since there has been no follow-up by Gray, it is difficult to know how he would defend his assumptions concerning Euler.

The Euler scholarship of Archibald’s coauthor Giovanni Ferraro was analyzed in detail in [9], as well, similarly without follow-up, except perhaps for the following recent comment. Commenting on Euler’s *Introductio in analysin infinitorum*, Ferraro perceives a causal connection between the use of infinitesimals and lack of success:

Euler was not entirely successful in achieving his aim since he introduced infinitesimal considerations in various proofs. [44, p. 11]

Ferraro’s 2020 comment is disappointingly consistent with the presentist attitude in his earlier work analyzed in [9].

**5.4. Debate over Cauchy.** Gray also claimed the following about Cauchy’s definitions:

He defined what it is for a function to be integrable, to be *continuous*, and to be differentiable, using careful, if not altogether unambiguous, limiting arguments. [57, p. 62] (emphasis added)

Such a claim is inaccurate at least with regard to continuity, a point argued in [9]. A more successful alternative has been elaborated in a number of publications on Cauchy including a 2020 article in *British Journal for the History of Mathematics* [12].

Archibald’s coauthor Jesper Lützen has been more forthcoming than Gray with information about Cauchy. Note the following five points (summarizing the analysis in [8, Section 3]):

- (1) Lützen acknowledges that Cauchy’s definitions contain no quantifiers, writing: “We miss our quantifiers, our  $\varepsilon$ ’s,  $\delta$ ’s” [96, p. 161].
- (2) He acknowledges that Cauchy’s second definition of continuity used infinitesimals [96, p. 160].
- (3) However, Lützen misrepresents the work of Robinson and Laugwitz when he claims that they asserted that Cauchy’s variables go through infinitesimal values on their way to zero [96, p. 164]. Neither Robinson nor Laugwitz ever made such a claim to our knowledge, though it is found in a 1978 paper on Cauchy by Fisher [45, p. 316].

- (4) He claims that the truth is found in Grabiner [53], who explains that whatever Lützen and others “miss” (see item (1)) is actually found in Cauchy’s *proofs* (rather than definitions), which are “strikingly modern” [96, p. 161].
- (5) According to Lützen, it is a “fundamental lacuna” of Cauchy’s proof of intermediate value theorem that the result relies on completeness, which could not have been provided by Cauchy [96, p. 167–168]. But as Laugwitz already pointed out [86, p. 202], Cauchy did not need a construction of the reals because he had unending decimal expansions (ever since Simon Stevin). Criticizing Cauchy’s proof on the grounds of the missing property of completeness therefore risks being anachronistic.

A significant point concerns the disagreement between a pair of Archibald’s coauthors: Lützen endorses Grabiner’s analysis of Cauchy, whereas Schubring criticizes Grabiner’s approach, as detailed in Section 2.5.<sup>51</sup> The Lützen–Schubring disagreement leads us to an interesting question of who exactly is entitled to disagree without running the risk of being branded a crusader (see Section 5.2).

The list could be continued, but we hope to have illustrated the fact that believing the sweeping claims by Archibald et al. would entail canceling large parts of modern scholarship published in leading history and philosophy journals.

**5.5. Leibniz–Bernoulli correspondence.** Archibald et al. make a number of additional spurious claims, including the claim of having detected a contradiction in the work of the “22 mathematicians and philosophers” (see Section 5.2). Since Archibald’s coauthors Arthur and Rabouin elaborate in [103] on a spurious claim of having detected contradictions also in the notion of infinitesimal in Leibniz (as analyzed in “Two-track depictions” and in more detail in [74]),<sup>52</sup> we are satisfied to be in good company.

For the reader interested in the technical details, note that Archibald et al. claim to have detected a contradiction in our interpretation of Leibniz. The claim is based on Archibald et al.’s reading of the Leibniz–Bernoulli correspondence from 1698–1699:

In the case [Leibniz] discussed with Bernoulli in 1698–1699, the question was rather about whether there exists an infinitieth term in an infinite series, which in the case of a decreasing series would stand for an infinitesimal quantity. Bernoulli insisted that there would indeed be such an infinitieth (although not necessarily last) term, thus entailing the existence of an infinitesimal. According to Katz et al., this entails a conception of infinite series as consisting of an infinite sequence of standard numbers followed by an infinitesimal part. [2, p. 2]

However, the claim by Archibald et al. is based on a misreading of crucial aspects of the 1698–1699 correspondence. Basically, Archibald et al. are committing an elementary logical error, as we will now explain.

In their correspondence, Bernoulli tried to convince Leibniz, through the analysis of the behavior of an infinite series, that an infinitesimal term in the series must

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<sup>51</sup>Schubring’s position is closer to Grattan-Guinness’s, who, decades earlier, “warn[ed] against planting later refinements (especially the Weierstrassians’) onto that period [of Cauchy’s activity in the 1820s]” [54, note 1, p. 713].

<sup>52</sup>The alleged contradiction results from their tendency to assimilate infinitesimals to infinite wholes: essentially Rabouin and Arthur are trying to invert a cardinality to obtain an infinitesimal, a procedure that shocked Fraenkel over a century ago; see Section 1.5.

exist. To put it another way, Bernoulli tried to derive the existence of infinitesimals from the existence of infinite series. Archibald et al. appear to believe that, since we referred to a non-Archimedean continuum as a *Bernoullian continuum*, we must agree with Bernoulli’s reasoning. But note the following two points:

- (1) The fact that Leibniz did not agree with Bernoulli’s reasoning does not mean that Leibniz rejected infinitesimals as (fictional) mathematical entities; he merely found Bernoulli’s argument flawed because it was based on a conflation of magnitude and multitude.<sup>53</sup>
- (2) We similarly don’t agree with Bernoulli’s reasoning, and used the term *Bernoullian continuum* only because Bernoulli routinely used infinitesimals in his *mathematical* work (and not because we agree with his reasoning from series in favor of the existence of infinitesimals), quite apart from his *philosophical* attempts to convince Leibniz to adopt a more realistic position with regard to infinitesimals.

We have addressed this point concerning Bernoulli in detail because Archibald et al. apparently attach great importance to it, seeing that the name “Bernoulli” is mentioned no fewer than 15 times in their 4-page text.

**5.6. Alice and Bobs.** With regard to the Leibnizian calculus, the 2022 article “Two-track depictions” [75] presented and compared two interpretations, represented respectively by Alice and Bob. One of the aspects of Leibniz’s position highlighted by Bob was the notion of *infinitum terminatum* (lit. bounded infinity), contrasted by Leibniz with *infinitum interminatum* (unbounded infinity).

Leibniz’s position, as explained by the Leibniz historian Eberhard Knobloch [82, p. 97], is that the *infinitum interminatum*, corresponding to an infinite whole (such as an unbounded infinite line) is a contradictory notion (see Section 1.3), whereas, by contrast, the *infinitum terminatum* is a notion useful in geometry and calculus. Such a bounded infinity, as the name suggests, is exemplified by a subline (bounded by a pair of infinitely separated endpoints) of the (contradictory) unbounded infinite line.

Archibald et al. quote this 2022 article, as well as the 2012 article [112] and the 2013 article [79]. Such attention is surely appreciated by every researcher; hopefully it can mark the beginning of a meaningful dialog or informed debate. Archibald et al. proceed to label the three Bob2012, Bob2013, and Bob2021 (the latter seems to be a misdated reference to the 2022 article), and to claim that Bob2012 and Bob2021 contradict each other in their opinion of whether infinitesimals are contradictory notions or not. However, the formulation “contain a contradiction” in [112] (concerning infinitesimals but also negatives and imaginaries) was in a different context and must not be conflated with the contradictory nature of Leibnizian “infinite wholes.” Toward the end of [2], one finds an interesting footnote 4 to the effect that

Richard Arthur and David Rabouin, two of the authors of this paper, will dedicate a specific study to this, providing several sources in which Leibniz explicitly claimed that *lineae infinitae terminatae* are contradictory entities. [2, note 4]

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<sup>53</sup>A related misunderstanding occurs in Rabouin and Arthur [103]; for details see [74], note 24, pp. 12–13 and the main text there.



At the very least, it seems that Arthur and Rabouin owe thanks to Bob for raising such an interesting issue, if it led to a new “specific study” of theirs. We look forward to seeing their “specific study” and suspend judgment of the merits of the, frankly surprising, claim that the *infinitum terminatum* is a contradictory notion – a claim that does not square with several texts by Leibniz, as documented in recent articles [11] and [74]. Surprisingly, Archibald et al. expect the reader to accept their wholesale dismissal of the research of “22 mathematicians and philosophers” (to quote Schubring), published in leading journals, based on an as-yet-unpublished specific study by Arthur and Rabouin.

5.7. **Pacidius.** There is a Leibnizian passage from 1676 where both *infinita terminata* and contradictions are mentioned. However, the passage leads to the opposite conclusion from the one sought by Arthur and Rabouin:

“Pacidius: I would indeed admit these infinitely small spaces and times in geometry, for the sake of invention, even if they are imaginary. But I am not sure whether they can be admitted in nature. For there seem to arise from them infinite straight lines bounded at both ends, as I will show at another time; which is absurd.”<sup>54</sup> (Leibniz as translated by Arthur in [3, p. 207])

The structure of Leibniz’s argument, consistent with his fictionalist views developed elsewhere, is that there are no infinitesimals in nature because if there were some, then there would also be *infinita terminata*, which would be absurd (by an argument that Leibniz promises to provide elsewhere). Accordingly, it is the hypothesis of the existence of bounded infinities *in nature* that leads to an absurdity. On the other hand, their usefulness in geometry does not depend on their existence in nature. This is a powerful argument against the Rabouin–Arthur interpretation.

More generally, it is surprising that Archibald et al. should present the position of Arthur and Rabouin concerning Leibniz as allegedly universally accepted among Leibniz scholars.<sup>55</sup> Quite the contrary: the 2020 article by Rabouin and Arthur on Leibniz in *Archive for History of Exact Sciences* was followed in the same journal by the 2021 article by Esquisabel and Raffo Quintana [43], who explicitly reject the Rabouin–Arthur interpretation in the following terms:

- (1) “[U]nlike the infinite number or the number of all numbers, for Leibniz infinitary concepts do not imply any contradiction, although they may imply paradoxical consequences.” [43, p. 641]
- (2) “[W]e disagree with the reasons [Rabouin and Arthur] gave for the Leibnizian rejection of the existence of infinitesimals, and in our opinion the

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<sup>54</sup>“Ego spatia haec et tempora infinite parva in Geometria quidem admitterem, inventionis causa, licet essent imaginaria. Sed an possint admitti in natura delibero. Videntur enim inde oriri lineae rectae infinitae utrinque terminatae, ut alias ostendam; quod absurdum est” [3, p. 206].

<sup>55</sup>Thus, Archibald et al. claim that “there is no B-methodology sensu stricto in Leibniz. Leibniz’s main argument is that it is not possible to treat infinitesimals as existing entities because that amounts to the introduction of an infinite number, which he takes to be a contradictory notion” and go on to describe such a position as “fact” in their note 3: “<sup>3</sup>This had been a well-known fact among Leibniz scholars for some time” [2, note 3]. One of the works they cite is Bassler [22]. The shortcomings of Bassler’s reading are analyzed in [73, Section 2].

texts they refer to in order to support their interpretation are not convincing. Since we argue that Leibniz did not consider the concept of infinitesimal as *self-contradictory*, we try to provide an alternative conception of impossibility.” [43, p. 620]

Curiously, the article by Esquisabel and Raffo Quintana was communicated by no other than . . . Archibald’s coauthor Jeremy Gray (see [43, p. 613]). Apparently, Gray did not read carefully one of the two texts: either the article he communicated, or the piece by Archibald et al. before consenting to have his name added to its author list.

**5.8. Two methods in Leibniz.** Perhaps the most remarkable case of membership on the author list of Archibald et al. is Douglas Jesseph. Speaking of the law of continuity in 1989, Jesseph asserts that

Leibniz argues that, when applied to the calculus, this law yields a *new kind of quantity* which will provide the foundation for the reasonings which appear in the solution to geometrical problems. [64, pp. 241–242] (emphasis added)

Ideas such as “new kind of quantity” in Leibniz are incompatible with the Arthur–Rabouin reading. Jesseph concludes:

In the Leibnizian scheme, true mathematical principles will be found acceptable on any resolution of the metaphysical problems of the infinite. Thus, Leibniz’ concern with matters of rigor leads him to propound a very strong thesis indeed, namely no matter how the symbols “dx” and “dy” are interpreted, the basic procedures of the calculus can be vindicated. Such vindication could take the form of a new science of infinity, or it could be carried out along classical lines, but in either case the new methods will be found completely secure. [64, p. 243]

Jesseph’s conclusion is consonant with the idea of the presence of two methods in Leibniz, as argued in the 2013 *Erkenntnis* article “Leibniz’s infinitesimals: Their fictionality, their modern implementations, and their foes from Berkeley to Russell and beyond” [79]. In fact, Jesseph was roundly criticized by Bassler – one of the authors endorsed by Archibald et al. in their note 3.<sup>56</sup> An analysis of Bassler’s criticism of Jesseph appears in [73, Section 2.3].

Jesseph’s appeal to two methods in Leibniz re-emerged in his discussion in [65] of Leibniz’s method of computing integrals via transmutation of curves from *De Quadratura Arithmetica*, which requires knowledge of the tangent lines to the curve. For conic sections, the tangent lines were known classically, but for the method to apply more generally, the tangents can only be obtained via ratios of genuine infinitesimals. Here, at least, Jesseph endorses genuine infinitesimals as an irreducible part of the Leibnizian framework; see further in [20].

To his honor, Craig Fraser (whose earlier critique [48] of infinitesimal methodology was analyzed in [8, Sections 4.2–4.6, pp. 123–128] and [19, Section 4.4, pp. 277–278]) does not appear among Archibald’s coauthors.

**5.9. Non-Archimedean continuum?** Referring to the argument in the 2013 article [79], Archibald et al. assert the following:

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<sup>56</sup>See note 55.

The main claim was that Leibniz shared with Bernoulli a certain view of the continuum as consisting of infinitesimal numbers in addition to ordinary (or “assignable”) numbers. We may note in passing that this already involves anachronism at odds with a properly historical approach. For Leibniz did not conceive of numbers as constituting a continuum, nor did he allow infinite sets (infinite wholes, in his terminology). . . . There is no way that one can claim that Bernoulli defended a certain picture of the continuum “following Leibniz.” Accordingly, there is no B-methodology *sensu stricto* in Leibniz. [2, p. 2]

Archibald et al.’s objection here is two-fold:

- (1) that Leibniz “did not conceive of numbers as constituting a continuum” and rejected infinite wholes.
- (2) that there is no B-methodology (i.e., methodology involving genuine infinitesimals) in Leibniz.

There are two major problems with Archibald et al.’s claims.

First, using the term *continuum* does not imply either its punctiform structure or that of an infinite whole, any more so than does the term *extension*. Leibniz tended to use the latter term; he used it for example in the letter to Masson from 1716 analyzed in detail in the article in *Review of Symbolic Logic* [74]. One wonders how Archibald would evaluate the title *The Labyrinth of the Continuum* of the classical work [3] edited by . . . his coauthor Arthur. Archibald et al. are therefore attacking a strawman (if not themselves).

Second, Leibniz has two documents from 1695 where he makes it clear that his incomparables violate the notion of comparability expressed in Euclid V definition 4, which is a version of the Archimedean property; see [88, p. 288] and [89, p. 322]. Thus, Leibniz is rather explicit about non-Archimedean phenomena occurring for his incomparables.

**5.10. How many tracks?** Archibald et al. go on to claim that the 2013 article [79] argued that Leibniz had two methods: track A and track B, whereas in the 2022 “Two-track depictions,” Bob asserts on the contrary that Leibniz used only the track B method whereas Alice claims that Leibniz used only the track A method:

Katz et al. completely changed their position, but without acknowledging this change, *as if it did not ruin their previous argument*. In the above-cited paper published in this journal, which is supposed to give a survey of a long-standing debate, A and B are no longer presented as a pair of methodologies in Leibniz, but as positions endorsed by commentators to understand the term “fiction” in Leibniz. [2, p. 2] (emphasis added)

Contrary to the claim by Archibald et al., the 2022 article does not “ruin” the previous argument at all. The existence of two methods in Leibniz is established fact that was commented on in detail by Bos in the seminal study [28]. Furthermore, this fact was accepted by Archibald’s coauthor Jesseph; see Section 5.8. The two methods are the exhaustion method and the infinitesimal method. Bos mentioned that the method using infinitesimals exploited the law of continuity.<sup>57</sup> Furthermore, the existence of two methods in Leibniz is a strong argument against the Rabouin–Arthur interpretation, which makes it difficult to distinguish between the

<sup>57</sup>Leibniz’s law of continuity possesses an adequate proxy in Robinson’s transfer principle as explained in [11], and has no convincing analog in the Weierstrassian setting.

two methods, an “infinitesimal” being merely stenography for more exhaustion. The point is that in [75], Alice and Bob are arguing specifically about the interpretation of Leibniz’s term *infinitesimal* (rather than about Leibniz’s exhaustion method which involves no infinitesimals even nominally). Thus, we stand by both

- (1) our (and Henk Bos’s) position that there are two separate methods in the Leibnizian calculus, and
- (2) our position that Leibniz’s infinitesimal method involved genuine infinitesimals rather than stenography for exhaustion.

There is no contradiction between the two positions, contrary to the claim by Archibald et al. The cardinal point here is that Leibniz’s non-infinitesimal (“exhaustion”) method was indeed mentioned by Bob in the article “Two-track depictions” pilloried by Archibald et al.:

Bob argues that Archimedean paraphrases in exhaustion style constitute an alternative method rather than an unwrapping of the infinitesimal method. [75, p. 262]

Archibald et al. are certainly within their rights to disagree with our arguments, but their attempt to win the argument by misrepresenting our position does not amount to a helpful contribution to historical scholarship.

In closing, it is ironic that Archibald et al. should claim that

[O]ver the years, it became clearer and clearer that our interlocutors do not care much about rational discussion and scientific dialogue from *different perspectives*, but seek rather to disparage their alleged enemies, . . . The latest example of that approach is provided by a paper . . . “Two-Track Depictions of Leibniz’s Fictions.” [2, p. 2] (emphasis on “different perspectives” added)

For “Two-track depictions” is devoted specifically to making explicit a pair of *different perspectives* on Leibniz’s calculus, so as to stimulate rational discussion and scientific dialogue.

Archibald et al. do little to clarify the Question of Fundamental Methodology, namely that the history of mathematics, like mathematics itself, could benefit from a plurality of approaches.

## 6. CONCLUSION

We have examined the modern debate over the infinitesimal calculus from Leibniz to Cauchy. Scholars who seek to interpret the Leibnizian calculus in a purely Archimedean context while denying his infinitesimal the status of a mathematical entity, often do not adequately appreciate Leibniz’s distinction between infinite wholes and bounded infinities (the inverse of infinitesimals). Leibniz’s rejection of the former does not imply rejection of the latter.

To grant Leibniz and Cauchy the use of genuine infinitesimals is not to impute to them the anticipation of modern nonstandard analysis or nonstandard numbers. It only means to argue that, in line with the procedures/foundations distinction, one finds better proxies for their ideas and inferential moves in Robinson’s framework for analysis with infinitesimals than in Weierstrassian analysis.

Leibniz and Cauchy had systems of infinitesimal analysis that yielded correct predictions in analysis, geometry, physics and elsewhere when one adhered to the internal rules of those systems.

A historiography that wishes to see Leibniz and Cauchy as direct predecessors of Weierstrass, and therefore denies them any use of genuine infinitesimals, runs the risk of being teleological.

One should also recognize for mathematics that history is contingent, in line with the insights by Grattan-Guinness and Hacking, and the evolution of analysis did not necessarily have to result in the elimination of infinitesimals.

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