A BURGESSIAN CRITIQUE OF NOMINALISTIC TENDENCIES IN CONTEMPORARY MATHEMATICS AND ITS HISTORIOGRAPHY

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Abstract. We analyze the developments in mathematical rigor from the viewpoint of a Burgessian critique of nominalistic reconstructions. We apply such a critique to the reconstruction of infinitesimal analysis accomplished through the efforts of Cantor, Dedekind, and Weierstrass; to the reconstruction of Cauchy’s foundational work associated with the work of Boyer and Grabiner; and to Bishop’s constructivist reconstruction of classical analysis. We examine the effects of an ontologically limitative disposition on historiography, teaching, and research.

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Supported by the Israel Science Foundation grant 1294/06
2000 Mathematics Subject Classification. Primary 01A85; Secondary 26E35, 03A05, 97A20, 97C30.

Key words and phrases. Abraham Robinson, adequality, Archimedean continuum, Bernoullian continuum, Burgess, Cantor, Cauchy, completeness, constructivism, continuity, Dedekind, du Bois-Reymond, epsilontics, Errett Bishop, Felix Klein, Fermat-Robinson standard part, infinitesimal, law of excluded middle, Leibniz-Löb transfer principle, nominalistic reconstruction, nominalism, non-Archimedean, rigor, Simon Stevin, Stolz, Weierstrass.
1. Introduction

Over the course of the past 140 years, the field of professional pure mathematics (analysis in particular), and to a large extent also its professional historiography, have become increasingly dominated by a particular philosophical disposition. We argue that such a disposition is akin to nominalism, and examine its ramifications.

In 1983, J. Burgess proposed a useful dichotomy for analyzing nominalistic narratives. The starting point of his critique is his perception that a philosopher’s job is not to rule on the ontological merits of this or that scientific entity, but rather to try to understand those entities that are employed in our best scientific theories. From this viewpoint, the problem of nominalism is the awkwardness of the contortions a nominalist goes through in developing an alternative to his target scientific practice, an alternative deemed ontologically “better” from his reductive perspective, but in reality amounting to the imposition of artificial strictures on the scientific practice.

Burgess introduces a dichotomy of hermeneutic versus revolutionary nominalism. Thus, hermeneutic nominalism is the hypothesis that science, properly interpreted, already dispenses with mathematical objects (entities) such as numbers and sets. Meanwhile, revolutionary nominalism is the project of replacing current scientific theories by alternatives dispensing with mathematical objects, see Burgess [30, p. 96] and Burgess and Rosen [32].

Nominalism in the philosophy of mathematics is often understood narrowly, as exemplified by the ideas of J. S. Mill and P. Kitcher, going back to Aristotle. However, the Burgessian distinction between hermeneutic and revolutionary reconstructions can be applied more broadly, so as to include nominalistic-type reconstructions that vary widely in their ontological target, namely the variety of abstract objects (entities) they seek to challenge (and, if possible, eliminate) as being merely conventional, see [30, p. 98-99].

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1See for example S. Shapiro [141].
Burgess quotes at length Yu. Manin’s critique in the 1970s of mathematical nominalism of constructivist inspiration, whose ontological target is the classical infinity, namely,

abstractions which are infinite and do not lend themselves to a constructivist interpretation [112, p. 172-173].

This suggests that Burgess would countenance an application of his dichotomy to nominalism of a constructivist inspiration.

The ontological target of the constructivists is the concept of Cantorian infinities, or more fundamentally, the logical principle of the Law of Excluded Middle (LEM). Coupled with a classical interpretation of the existence quantifier, LEM is responsible for propelling the said infinities into a dubious existence. LEM is the abstract object targeted by Bishop’s constructivist nominalism, which can therefore be called an anti-LEM nominalism. Thus, anti-LEM nominalism falls within the scope of the Burgessian critique, and is the first of the nominalistic reconstructions we wish to analyze.

The anti-LEM nominalistic reconstruction was in fact a re-reconstruction of an earlier nominalistic reconstruction of analysis, dating from the 1870s. The earlier reconstruction was implemented by the great triumvirate of Cantor, Dedekind, and Weierstrass. The ontological target of the triumvirate reconstruction was the abstract entity called the infinitesimal, a basic building block of a continuum, according to a line of investigators harking back to the Greek antiquity.

To place these historical developments in context, it is instructive to examine Felix Klein’s remarks dating from 1908. Having outlined the developments in real analysis associated with Weierstrass and his followers, Klein pointed out that

The scientific mathematics of today is built upon the series of developments which we have been outlining. But an essentially different conception of infinitesimal calculus has been running parallel with this [conception] through the centuries [95, p. 214].

Such a different conception, according to Klein,

\[A \text{ more detailed discussion of LEM in the context of the proof of the irrationality of } \sqrt{2} \text{ may be found in Section 2, see footnote 15.}\]

\[C. \text{ Boyer refers to Cantor, Dedekind, and Weierstrass as “the great triumvirate”, see [25, p. 298].}\]

\[D \text{For an entertaining history of infinitesimals see P. Davis and R. Hersh [46, p. 237-254]. For an analysis of a variety of competing theories of the continuum, see P. Ehrlich [51] as well as R. Taylor [157].}\]

\[E \text{See also footnote 62 on Cauchy.}\]
harks back to old metaphysical speculations concerning
the structure of the continuum according to which this
was made up of [...] infinitely small parts [95, p. 214]
[emphasis added—authors].

The significance of the triumvirate reconstruction has often been mea-
sured by the yardstick of the extirpation of the infinitesimal.

The infinitesimal ontological target has similarly been the motivat-
ing force behind a more recent nominalistic reconstruction, namely a
nominalistic re-appraisal of the meaning of Cauchy’s foundational work
in analysis.

We will analyze these three nominalistic projects through the lens
of the dichotomy introduced by Burgess. Our preliminary conclusion
is that, while the triumvirate reconstruction was primarily revolu-
tionary in the sense of Burgess, and the (currently prevailing) Cauchy re-
construction is mainly hermeneutic, the anti-LEM reconstruction has
combined elements of both types of nominalism. We will examine the
effects of a nominalist disposition on historiography, teaching, and re-
search.

A traditional view of 19th century analysis holds that a search for
rigor inevitably leads to epsilontics, as developed by Weierstrass in the
1870s; that such inevitable developments culminated in the establish-
ment of ultimate set-theoretic foundations for mathematics by Cantor;
and that eventually, once the antinomies sorted out, such foundations
were explicitly expressed in axiomatic form by Zermelo and Fraenkel.
Such a view entails a commitment to a specific destination or ultimate
goal of scientific development as being pre-determined and intrinsically
inevitable. The postulation of a specific outcome, believed to be the
inevitable result of the development of the discipline, is an outlook
specific to the mathematical community. Challenging such a belief
appears to be a radical proposition in the eyes of a typical professional
mathematician, but not in the eyes of scientists in related fields of

6Thus, after describing the formalisation of the real continuum in the 1870s,
on pages 127-128 of his retiring presidential address in 1902, E. Hobson remarks
triumphantly as follows: “It should be observed that the criterion for the con-
vergence of an aggregate [i.e. an equivalence class defining a real number] is of
such a character that no use is made in it of infinitesimals” [80, p. 128] [emphasis
added–authors]. Hobson reiterates: “In all such proofs [of convergence] the only
statements made are as to relations of finite numbers, no such entities as
infinites-
imals being recognized or employed. Such is the essence of the $\epsilon, \delta$
proofs with
which we are familiar” [80, p. 128] [emphasis added–authors]. The tenor of Hobson’s
remarks is that Weierstrass’s fundamental accomplishment was the elimination
of infinitesimals from foundational discourse in analysis.
the exact sciences. It is therefore puzzling that such a view should be accepted without challenge by a majority of historians of mathematics, who tend to toe the line on the mathematicians’ belief. Could mathematical analysis have followed a different path of development? Related material appears in Alexander [2], Giordano [65], Katz and Tall [92], Kutateladze [99, chapter 63], Mormann [116], Sepkoski [139], and Wilson [165].

2. AN ANTI-LEM NOMINALISTIC RECONSTRUCTION

This section is concerned with E. Bishop’s approach to reconstructing analysis. Bishop’s approach is rooted in Brouwer’s revolt against the non-constructive nature of mathematics as practiced by his contemporaries.\(^7\)

Is there meaning after LEM? The Brouwer–Hilbert debate captured the popular mathematical imagination in the 1920s. Brouwer’s crying call was for the elimination of most of the applications of LEM from meaningful mathematical discourse. Burgess discusses the debate briefly in his treatment of nominalism in [31, p. 27]. We will analyze E. Bishop’s implementation of Brouwer’s nominalistic project.\(^8\)

It is an open secret that the much-touted success of Bishop’s implementation of the intuitionistic project in his 1967 book [16] is due to philosophical compromises with a Platonist viewpoint that are resolutely rejected by the intuitionistic philosopher M. Dummett [49]. Thus, in a dramatic departure from both Kronecker\(^9\) and Brouwer, Bishopian constructivism accepts the completed (actual) infinity of the integers \(\mathbb{Z}\).\(^10\)

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\(^7\)Similar tendencies on the part of Wittgenstein were analyzed by H. Putnam, who describes them as “minimalist” [126, p. 242]. See also G. Kreisel [98].

\(^8\)It has been claimed that Bishopian constructivism, unlike Brouwer’s intuitionism, is compatible with classical mathematics, see e.g. Davies [44]. However, Brouwerian counterexamples do appear in Bishop’s work; see footnote 17 for more details. This could not be otherwise, since a verificational interpretation of the quantifiers necessarily results in a clash with classical mathematics. As a matter of presentation, the conflict with classical mathematics had been de-emphasized by Bishop. Bishop finesses the issue of Brouwer’s theorems (e.g., that every function is continuous) by declaring that he will only deal with uniformly continuous functions to begin with. In Bishopian mathematics, a circle cannot be decomposed into a pair of antipodal sets. A counterexample to the classical extreme value theorem is discussed in [158, p. 295], see footnote 17 for details.

\(^9\)Kronecker’s position is discussed in more detail in Section 4 in the main text around footnote 34.

\(^10\)Intuitionists view \(\mathbb{N}\) as a potential totality; for a more detailed discussion see, e.g., [58, section 4.3].
Bishop expressed himself as follows on the first page of his book:

in another universe, with another biology and another
physics, [there] will develop mathematics which in essence
is the same as ours [16 p. 1].

Since the sensory perceptions of the human body are physics- and
chemistry-bound, a claim of such trans-universe invariance amounts
to the positing of a disembodied nature of the infinite natural number
system, transcending physics and chemistry.

What type of nominalistic reconstruction best fits the bill of Bishop’s
constructivism? Bishop’s rejection of classical mathematics as a “de-
basement of meaning” [20 p. 1] would place him squarely in the camp
of revolutionary nominalisms in the sense of Burgess; yet some elements
of Bishop’s program tend to be of the hermeneutic variety, as well.

As an elementary example, consider Bishop’s discussion of the irra-
tionality of the square root of 2 in [20 p. 18]. Irrationality is defined
constructively in terms of being quantifiably apart from each rational
number. The classical proof of the irrationality of \( \sqrt{2} \) is a proof by
contradiction. Namely, we assume a hypothesized equality \( \sqrt{2} = \frac{m}{n} \),
examine the parity of the powers of 2, and arrive at a contradiction. At

\[\text{Footnotes:}
11\text{a claim that we attribute to a post-}
Sputnik\text{ fever}
12\text{The muddle of realism and anti-realism in the anti-LEM sector will be discussed}
in more detail in Section 3. Bishop’s disembodied integers illustrate the awkward
philosophical contorsions which are a tell-tale sign of nominalism. An alternative
approach to the problem is pursued in modern cognitive science. Bishop’s disem-
bodied integers, the cornerstone of his approach, appear to be at odds with modern
cognitive theory of embodied knowledge, see Tall [156], Lakoff and Núñez [100],
Sfard [140], Yablo [166]. Reyes [128] presents an intriguing thesis concerning an al-
legedly rhetorical nature of Newton’s attempts at grounding infinitesimals in terms
of moments or nascent and evanescent quantities, and Leibniz’s similar attempts in
terms of “a general heuristic under [the name of] the principle of continuity” [128]
p. 172]. He argues that what made these theories vulnerable to criticism is the
reigning principle in 17th century methodology according to which abstract objects
must necessarily have empirical counterparts/referents. D. Sherry points out that
“Formal axiomatics emerged only in the 19th century, after geometry embraced
objects with no empirical counterparts (e.g., Poncelet’s points at infinity...)” [144]
p. 67]. See also S. Feferman’s approach of conceptual structuralism [58], for a view
of mathematical objects as mental conceptions.
13\text{Bishop diagnosed classical mathematics with a case of a “debasement of mean-
ing” in his Schizophrenia in contemporary mathematics (1973). Hot on the heels of}
Schizophrenia came the 1975 Crisis in contemporary mathematics [19], where the
same diagnosis was slapped upon infinitesimal calculus à la Robinson [131].

this stage, irrationality is considered to have been proved, in classical logic.\footnote{The classical proof showing that $\sqrt{2}$ is \emph{not} rational is, of course, acceptable in intuitionistic logic. To pass from this to the claim of its \textit{irrationality} as defined above, requires LEM (see footnote \footnote{Such a proof may be given as follows. For each rational $m/n$, the integer $2n^2$ is divisible by an odd power of 2, while $m^2$ is divisible by an even power of 2. Hence $|2n^2 - m^2| \geq 1$ (here we have applied LEM to an effectively decidable predicate over $\mathbb{Z}$, or more precisely the law of trichotomy). Since the decimal expansion of $\sqrt{2}$ starts with 1.41... , we may assume $m/n \leq 1.5$. It follows that $|\sqrt{2} - \frac{m}{n}| = \frac{|2n^2 - m^2|}{n^2 (\sqrt{2} + \frac{m}{n})} \geq \frac{1}{n^2 (\sqrt{2} + \frac{m}{n})} \geq \frac{1}{3n^2}$, yielding a numerically meaningful proof of irrationality, which is a special case of Liouville’s theorem on diophantine approximation of algebraic numbers, see \cite{72}.}}

However, as Bishop points out, the proof can be modified slightly so as to avoid LEM, and acquire an enhanced \textit{numerical meaning}. Thus, \emph{without} exploiting the equality $\sqrt{2} = \frac{m}{n}$, one can exhibit effective positive lower bounds for the difference $|\sqrt{2} - \frac{m}{n}|$ in terms of the denominator $n$, resulting in a constructively adequate proof of irrationality.\footnote{Such a proof is merely a modification of a classical proof, and can thus be considered a hermeneutic reconstruction thereof. A number of classical results (though by no means all) can be reinterpreted constructively, resulting in an enhancement of their numerical meaning, in some cases at little additional cost. This type of project is consistent with the idea of a hermeneutic nominalism in the sense of Burgess, and related to the notion of \textit{liberal constructivism} in the sense of G. Hellman (see below).}

The intuitionist/constructivist opposition to classical mathematics is predicated on the philosophical assumption that “meaningful” mathematics is mathematics done without the law of excluded middle. E. Bishop (following Brouwer but surpassing him in rhetoric) is on record making statements of the sort

“Very possibly classical mathematics will cease to exist as an independent discipline” \cite{18}, p. 54]

(to be replaced, naturally, by constructive mathematics); and

“Brouwer’s criticisms of classical mathematics were concerned with what I shall refer to as ‘the debasement of meaning’ ” \cite{20}, p. 1].

Such a stance posits intuitionism/constructivism as an \textit{alternative} to classical mathematics, and is described as \textit{radical constructivism} by G. Hellman \cite{74}, p. 222]. Radicalism is contrasted by Hellman with a
A liberal brand of intuitionism (a companion to classical mathematics). Liberal constructivism may be exemplified by A. Heyting [77, 78], who was Brouwer’s student, and formalized intuitionistic logic.

To motivate the long march through the foundations occasioned by a LEM-eliminative agenda, Bishop [19] goes to great lengths to dress it up in an appealing package of a theory of meaning that first conflates meaning with numerical meaning (a goal many mathematicians can relate to), and then numerical meaning with LEM extirpation. Rather than merely rejecting LEM or related logical principles such as trichotomy which sound perfectly unexceptionable to a typical mathematician, Bishop presents these principles in quasi metaphysical garb of “principles of omniscience” Bishop retells a creation story of intuitionism in the form of an imaginary dialog between Brouwer and Hilbert where the former completely dominates the exchange. Indeed, Bishop’s imaginary Brouwer-Hilbert exchange is dominated by an unspoken assumption that Brouwer is the only one who seeks “meaning”, an assumption that his illustrious opponent is never given a chance to challenge. Meanwhile, Hilbert’s comments in 1919 reveal clearly his attachment to meaning which he refers to as internal necessity:

We are not speaking here of arbitrariness in any sense. Mathematics is not like a game whose tasks are determined by arbitrarily stipulated rules. Rather, it is a conceptual system possessing internal necessity that can only be so and by no means otherwise [79, p. 14] (cited in Corry [37]).

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16Such a reduction is discussed in more detail in Section 4.  
17Thus, the main target of his criticism in [19] is the “limited principle of omniscience” (LPO). The LPO is formulated in terms of sequences, as the principle that it is possible to search “a sequence of integers to see whether they all vanish” [19, p. 511]. The LPO is equivalent to the law of trichotomy: \((a < 0) \lor (a = 0) \lor (a > 0)\). An even weaker principle is \((a \leq 0) \lor (a \geq 0)\), whose failure is exploited in the construction of a counterexample to the extreme value theorem by Troelstra and van Dalen [158, p. 295], see also our Section 4. This property is false intuitionistically. After discussing real numbers \(x \geq 0\) such that it is “not” true that \(x > 0\) or \(x = 0\), Bishop writes:

In much the same way we can construct a real number \(x\) such that it is not true that \(x \geq 0\) or \(x \leq 0\) [16, p. 26], [17, p. 28].

An \(a\) satisfying \(\neg((a \leq 0) \lor (a \geq 0))\) immediately yields a counterexample \(f(x) = ax\) to the extreme value theorem (EVT) on \([0, 1]\) (see [16, p. 59, exercise 9]; [17, p. 62, exercise 11]). Bridges interprets Bishop’s italicized “not” as referring to a Brouwerian counterexample, and asserts that trichotomy as well as the principle \((a \leq 0) \lor (a \geq 0)\) are independent of Bishopian constructivism. See D. Bridges [28] for details; a useful summary may be found in Taylor [157].
A majority of mathematicians (including those favorable to constructivism) feel that an implementation of Bishop’s program does involve a significant complication of the technical development of analysis, as a result of the nominalist work of LEM-elimination. Bishop’s program has met with a certain amount of success, and attracted a number of followers. Part of the attraction stems from a detailed lexicon developed by Bishop so as to challenge received (classical) views on the nature of mathematics. A constructive lexicon was a sine qua non of his success. A number of terms from Bishop’s constructivist lexicon constitute a novelty as far as intuitionism is concerned, and are not necessarily familiar even to someone knowledgeable about intuitionism per se. It may be helpful to provide a summary of such terms for easy reference, arranged alphabetically, as follows.

- **Debasement of meaning** is the cardinal sin of the classical opposition, from Cantor to Keisler committed with LEM (see below). The term occurs in Bishop’s *Schizophrenia* [20] and *Crisis* [19] texts.

- **Fundamentalist excluded thirdist** is a term that refers to a classically-trained mathematician who has not yet become sensitized to implicit use of the law of excluded middle (i.e., excluded third) in his arguments, see [130, p. 249] [19].

- **Idealistic mathematics** is the output of Platonist mathematical sensibilities, abetted by a metaphysical faith in LEM (see below), and characterized by the presence of merely a peculiar pragmatic content (see below).

- **Integer** is the revealed source of all meaning (see below), posited as an alternative foundation displacing both formal logic, axiomatic set theory, and recursive function theory. The integers wondrously escape the vigilant scrutiny of a constructivist intelligence determined to uproot and nip in the bud each and every Platonist fancy of a concept external to the mathematical mind.

- **Integrity** is perhaps one of the most misunderstood terms in Errett Bishop’s lexicon. Pourciau in his *Education* [124] appears to interpret

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18 But see footnote [20].

19 This use of the term “fundamentalist excluded thirdist” is in a text by Richman, not Bishop. I have not been able to source its occurrence in Bishop’s writing. In a similar vein, an ultrafinitist recently described this writer as a “choirboy of infinitesimalism”; however, this term does not seem to be in general use. See also footnote [29].

20 By dint of a familiar oracular quotation from Kronecker; see also main text around footnote [10].
it as an indictment of the ethics of the classical opposition. Yet in his
*Schizophrenia* text, Bishop merely muses:

> [...] I keep coming back to the term “integrity”. [20, p. 4]

Note that the period is in the original. Bishop describes *integrity* as
the opposite of a syndrome he colorfully refers to as *schizophrenia*,
characterized

(a) by a rejection of common sense in favor of formalism,
(b) by *debasement of meaning* (see above),
(c) as well as by a list of other ills—
but *excluding* dishonesty. Now the root of

\[\text{integr-ity}\]

is identical with that of *integer* (see above), the Bishopian ultimate
foundation of analysis. Bishop’s evocation of *integrity* may have been
an innocent pun intended to allude to a healthy constructivist mindset,
where the *integers* are uppermost.\(^{21}\)

- *Law of excluded middle (LEM)* is the main source of the non-
  constructivities of classical mathematics.\(^{22}\) Every formalisation of *in-
  tuitionistic logic* excludes *LEM*; adding *LEM* back again returns us to
  *classical logic*.

- *Limited principle of omniscience (LPO)* is a weak form of *LEM*
  (see above), involving *LEM*-like oracular abilities limited to the context
  of integer sequences.\(^{23}\) The *LPO* is still unacceptable to a constructivist,
  but could have served as a basis for a *meaningful* dialog between
  Brouwer and Hilbert (see [19]), that could allegedly have changed the
  course of 20th century mathematics.

\(^{21}\)In Bishop’s system, the integers are uppermost to the exclusion of the continuum. Bishop rejected Brouwer’s work on an intuitionistic continuum in the following terms:

> Brouwer’s bugaboo has been compulsive speculation about the
nature of the continuum. His fear seems to have been that, unless
he personally intervened to prevent it, the continuum would turn
out to be discrete. [The result was Brouwer’s] semimystical theory
of the continuum [16, p. 6 and 10].

Brouwer sought to incorporate a theory of the continuum as part of intuitionistic
mathematics, by means of his free choice sequences. Bishop’s commitment to integr-
ity is thus a departure from Brouwerian intuitionism.

\(^{22}\)See footnote [15] and footnote [17] for some examples.

\(^{23}\)See footnote [17] for a discussion of LPO.
• *Meaning* is a favorite philosophic term in Bishop’s lexicon, necessarily preceding an investigation of *truth* in any coherent discussion. In Bishop’s writing, the term *meaning* is routinely conflated with *numerical meaning* (see below).

• *Numerical meaning* is the content of a theorem admitting a proof based on intuitionistic logic, and expressing computationally meaningful facts about the integers. The conflation of *numerical meaning* with *meaning* par excellence in Bishop’s writing, has the following two consequences:

(1) it empowers the constructivist to sweep under the rug the distinction between pre-LEM and post-LEM numerical meaning, lending a marginal degree of plausibility to a dismissal of classical theorems which otherwise appear eminently coherent and meaningful and

(2) it allows the constructivist to enlist the support of anti-realist philosophical schools of thought (e.g. Michael Dummett) in the theory of meaning, in spite of the apparent tension with Bishop’s otherwise realist declarations (see entry realistic mathematics below).

• *Peculiar pragmatic content* is an expression of Bishop’s that was analyzed by Billinge. It connotes an alleged lack of empirical validity of classical mathematics, when classical results are merely *inference tickets* used in the deduction of other mathematical results.

• *Realistic mathematics*. The dichotomy of “realist” versus “idealist” (see above) is the dichotomy of “constructive” versus “classical” mathematics, in Bishop’s lexicon. There are two main narratives of the Intuitionist insurrection, one anti-realist and one realist. The issue is discussed in the next section.

3. **Insurrection according to Errett and according to Michael**

The anti-realist narrative, mainly following Michael Dummett, traces the original sin of classical mathematics with *LEM*, all the way

24As an illustration, a numerically meaningful proof of the irrationality of $\sqrt{2}$ appears in footnote.

25See the main text around footnote for a discussion of the classical extreme value theorem and its LEMless remains.
back to Aristotle\textsuperscript{26}. The law of excluded middle (see Section 2) is
the mathematical counterpart of geocentric cosmology (alternatively,
of phlogiston, see \cite[299]{125}), slated for the dustbin of history.\textsuperscript{27} The
anti-realist narrative dismisses the Quine-Putnam indispensability the-
thesis (see Feferman \cite[Section IIB]{57}) on the grounds that a \textit{philosophy-
first} examination of first principles is the unique authority empow-
ered to determine the correct way of doing mathematics.\textsuperscript{28} Generally
speaking, it is this narrative that seems to be favored by a number of
philosophers of mathematics.

Dummett opposes a truth-valued, bivalent semantics, namely the
notion that truth is one thing and knowability another, on the grounds
that it violates Dummett’s \textit{manifestation requirement}, see Shapiro \cite[p. 54]{142}.
The latter requirement, in the context of mathematics, is merely
a \textit{restatement} of the intuitionistic principle that truth is tantamount
to verifiability (necessitating a constructive interpretation of the quan-
tifiers). Thus, an acceptance of Dummett’s manifestation requirement,
leads to intuitionistic semantics and a rejection of LEM.

In his 1977 foundational text \cite{49} originating from 1973 lecture notes,
Dummett is frank about the source of his interest in the intuition-
ist/classical dispute in mathematics \cite[p. ix]{49}:

\begin{quote}
This dispute bears a \textbf{strong resemblance} to other
disputes over realism of one kind or another, that is,
concerning various kinds of subject-matter (or types of
statement), including that over realism about the phys-
ical universe [emphasis added–authors]
\end{quote}

What Dummett proceeds to say at this point, reveals the nature of his
interest:

\begin{quote}
but intuitionism represents the only sustained attempt
by the opponents of a \textbf{realist view} to work out a coher-
ent embodiment of their philosophical beliefs [emphasis
added–authors]
\end{quote}

\textsuperscript{26}The entry under \textit{debasement of meaning} in Section 2 would read, accordingly,
“the classical opposition from Aristotle to Keisler”; see main text at footnote 18.

\textsuperscript{27}Following Kronecker and Brouwer, Dummett rejects actual infinity, at variance
with Bishop.

\textsuperscript{28}In Hellman’s view, “any [...] attempt to reinstate a ‘first philosophical’ theory
of meaning prior to all science is doomed” \cite[p. 439]{75}. What this appears to
mean is that, while there can certainly be a philosophical notion of meaning before
science, any attempt to \textit{prescribe} standards of meaning \textit{prior} to the actual practice
of science, is \textit{doomed}.
What interests Dummett here is his fight against the *realist view*. What endears intuitionists to him, is the fact that they have succeeded where the phenomenalists have not [49, p. ix]:

Phenomenalists might have attained a greater success if they had made a remotely comparable effort to show in detail what consequences their interpretation of material-object statements would have for our employment of our language.

However, Dummett’s conflation of the mathematical debate and the philosophical debate, could be challenged.

We hereby explicitly sidestep the debate opposing the realist (as opposed to the super-realist, see W. Tait [153]) position and the anti-realist position. On the other hand, we observe that a defense of indispensability of mathematics would necessarily start by challenging Dummett’s “manifestation”. More precisely, such a defense would have to start by challenging the extension of Dummett’s manifestation requirement, from the realm of philosophy to the realm of mathematics. While Dummett chooses to pin the opposition to intuitionism, to a belief [49, p. ix] in an interpretation of mathematical statements as referring to an independently existing and objective reality[,] (i.e. a Platonic world of mathematical entities), J. Avigad [6] memorably retorts as follows:

We do not need fairy tales about numbers and triangles prancing about in the realm of the abstracta.29

Meanwhile, the *realist* narrative of the intuitionist insurrection appears to be more consistent with what Bishop himself actually wrote. In his foundational essay, Bishop expresses his position as follows:

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29 Constructivist Richman takes a dimmer view of prancing numbers and triangles. In addition, he presents a proposal to eliminate the axiom of choice altogether from constructive mathematics (including countable choice). Since the ultrafilter axiom is weaker than the axiom of choice, one might have hoped it would be salvaged; not so:

We are all Platonists, aren’t we? In the trenches, I mean—when the chips are down. Yes, Virginia, there really are circles, triangles, numbers, continuous functions, and all the rest. Well, maybe not free ultrafilters. Is it important to believe in the existence of free ultrafilters? Surely that’s not required of a Platonist. I can more easily imagine it as a test of sanity: ‘He believes in free ultrafilters, but he seems harmless’ (Richman [129]).

For Richman’s contribution to constructivist lexicon see footnote [19].
As pure mathematicians, we must decide whether we are playing a game, or whether our theorems describe an external reality [19, p. 507].

The right answer, to Bishop, is that they do describe an external reality. The dichotomy of “realist” versus “idealist” is the dichotomy of “constructive” versus “classical” mathematics, in Bishop’s lexicon (see entry under idealistic mathematics in Section 2). Bishop’s ambition is to incorporate “such mathematically oriented disciplines as physics” [20, p. 4] as part of his constructive revolution, revealing a recognition, on his part, of the potency of the Quine-Putnam indispensability challenge.

N. Kopell and G. Stolzenberg, close associates of Bishop, published a three-page Commentary [97] following Bishop’s Crisis text. Their note places the original sin with LEM at around 1870 (rather than Greek antiquity), when the “flourishing empirico-inductive tradition” began to be replaced by the “strictly logico-deductive conception of pure mathematics”. Kopell and Stolzenberg don’t hesitate to compare the empirico-inductive tradition in mathematics prior to 1870, to physics, in the following terms [97, p. 519]:

[Mathematical] theories were theories about the phenomena, just as in a physical theory.

Similar views have been expressed by D. Bridges [29], as well as Heyting [77, 78]. W. Tait [152] argues that, unlike intuitionism, constructive mathematics is part of classical mathematics. In fact, it was Frege’s revolutionary logic [59] (see Gillies [64]) and other foundational developments that created a new language and a new paradigm, transforming mathematical foundations into fair game for further investigation, experimentation, and criticism, including those of constructivist type.

The philosophical dilemmas in the anti-LEM sector discussed in this section are a function of the nominalist nature of its scientific goals. A critique of its scientific methods appears in the next section.

4. A CRITIQUE OF THE CONSTRUCTIVIST SCIENTIFIC METHOD

We would like to analyze more specifically the constructivist dilemma with regard to the following two items:

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30Our purpose here is not to endorse or refute Bishop’s views on this point, but rather to document his actual position, which appears to diverge from Dummett’s.

31Billinge [14, p. 314] purports to detect “inchoate” anti-realist views in Bishop’s writings, but provides no constructive proof of their existence, other than a pair quotes on numerical meaning. Meanwhile, Hellman [74, p. 222] writes: “Some of Bishop’s remarks (1967) suggest that his position belongs in [the radical] category”.

(1) the extreme value theorem, and
(2) the Hawking-Penrose singularity theorem.

Concerning (1), note that a constructive treatment of the extreme value theorem (EVT) by Troelstra and van Dalen [158, p. 295] brings to the fore the instability of the (classically unproblematic) maximum by actually constructing a counterexample. Such a counterexample relies on assuming that the principle
\[(a \leq 0) \vee (a \geq 0)\]
fails.\(^{32}\) This is a valuable insight, if viewed as a companion to classical mathematics.\(^{33}\) If viewed as an alternative, we are forced to ponder the consequences of the loss of the EVT.

Kronecker is sometimes thought of as the spiritual father of the Brouwer/Bishop/Dummett tendency. Kronecker was active at a time when the field of mathematics was still rather compartmentalized. Thus, he described a 3-way partition thereof into (a) analysis, (b) geometry, and (c) mechanics (presumably meaning mathematical physics). Kronecker proceeded to state that it is only the analytic one-third of mathematics that is amenable to a constructivisation in terms of the natural numbers that “were given to us, etc.”, but readily conceded that such an approach is inapplicable in the remaining two-thirds, geometry and physics.\(^{34}\)

Nowadays mathematicians adopt a more unitary approach to the field, and Kronecker’s partition seems provincial, but in fact his caution was vindicated by later developments, and can even be viewed as visionary. Consider a field such as general relativity, which in a way is a synthesis of Kronecker’s remaining two-thirds, namely, geometry and physics. Versions of the extreme value theorem are routinely exploited here, in the form of the existence of solutions to variational principles, such as geodesics, be it spacelike, timelike, or lightlike. At a deeper level, S.P. Novikov [119, 120] wrote about Hilbert’s meaningful contribution to relativity theory, in the form of discovering a Lagrangian for Einstein’s equation for spacetime. Hilbert’s deep insight was to show that general relativity, too, can be written in Lagrangian form, which is a satisfying conceptual insight.

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\(^{32}\)This principle is a special case of LEM; see footnote 17.

\(^{33}\)Some related ground on pluralism is covered in B. Davies [44]. Mathematical phenomena such as the instability of the extremum tend to be glossed over when approached from the classical viewpoint; here a constructive viewpoint can provide a welcome correction.

\(^{34}\)See Boniface and Schappacher [22] p. 211. 
A radical constructivist’s reaction would be to dismiss the material discussed in the previous paragraph as relying on LEM (needed for the EVT), hence lacking numerical meaning, and therefore meaningless. In short, radical constructivism (as opposed to the liberal variety) adopts a theory of meaning amounting to an ostrich effect as far as certain significant scientific insights are concerned. A quarter century ago, M. Beeson already acknowledged constructivism’s problem with the calculus of variations in the following terms:

Calculus of variations is a vast and important field which lies right on the frontier between constructive and non-constructive mathematics [10, p. 22].

An even more striking example is the Hawking-Penrose singularity theorem, whose foundational status was explored by Hellman [75]. The theorem relies on fixed point theorems and therefore is also constructively unacceptable, at least in its present form. However, the singularity theorem does provide important scientific insight. Roughly speaking, one of the versions of the theorem asserts that certain natural conditions on curvature (that are arguably satisfied experimentally in the visible universe) force the existence of a singularity when the solution is continued backward in time, resulting in a kind of a theoretical justification of the Big Bang. Such an insight cannot be described as “meaningless” by any reasonable standard of meaning preceding nominalist commitments.

5. THE TRIUMVIRATE NOMINALISTIC RECONSTRUCTION

This section analyzes a nominalistic reconstruction successfully implemented at the end of the 19th century by Cantor, Dedekind, and Weierstrass. The rigorisation of analysis they accomplished went hand-in-hand with the elimination of infinitesimals; indeed, the latter accomplishment is often viewed as a fundamental one. We would like to state from the outset that the main issue here is not a nominalistic attitude on the part of our three protagonists themselves. Such an attitude is only clearly apparent in the case of Cantor (see below). Rather, we argue that the historical context in the 1870s favored the acceptance of their reconstruction by the mathematical community, due to a certain philosophical disposition.

Some historical background is in order. As argued by D. Sherry [143], George Berkeley’s 1734 polemical essay [13] conflated a logical criticism around footnote 57.
and a metaphysical criticism. In the intervening centuries, mathematicians have not distinguished between the two criticisms sufficiently, and grew increasingly suspicious of infinitesimals. The metaphysical criticism stems from the 17th century doctrine that each theoretical entity must have an empirical counterpart/referent before such an entity can be used meaningfully; the use of infinitesimals of course would fly in the face of such a doctrine.

Today we no longer accept the 17th century doctrine. However, in addition to the metaphysical criticism, Berkeley made a poignant logical criticism, pointing out a paradox in the definition of the derivative. The seeds of an approach to resolving the logical paradox were already contained in the work of Fermat, but it was Robinson who ironed out the remaining logical wrinkle.

Thus, mathematicians throughout the 19th century were suspicious of infinitesimals because of a lingering influence of 17th century doctrine, but came to reject them because of what they felt were logical contradictions; these two aspects combined into a nominalistic attitude that caused the triumvirate reconstruction to spread like wildfire.

The tenor of Hobson’s remarks as indeed of a majority of historians of mathematics, is that Weierstrass’s fundamental accomplishment was the elimination of infinitesimals from foundational discourse in analysis. Infinitesimals were replaced by arguments relying on real inequalities and multiple-quantifier logical formulas.

The triumvirate transformation had the effect of a steamroller flattening a $B$-continuum into an $A$-continuum. Even the ardent enthusiasts of Weierstrassian epsilontics recognize that its practical effect on mathematical discourse has been “appalling”; thus, J. Pierpont wrote as follows in 1899:
The mathematician of to-day, trained in the school of Weierstrass, is fond of speaking of his science as ‘die absolut klare Wissenschaft.’ Any attempts to drag in metaphysical speculations are resented with indignant energy. With almost painful emotions he looks back at the sorry mixture of metaphysics and mathematics which was so common in the last century and at the beginning of this [122, p. 406] [emphasis added–authors].

Pierpont concludes:

The analysis of to-day is indeed a transparent science. Built up on the simple notion of number, its truths are the most solidly established in the whole range of human knowledge. It is, however, not to be overlooked that the price paid for this clearness is appalling, it is total separation from the world of our senses” [122, p. 406] [emphasis added–authors].

It is instructive to explore what form the “indignant energy” referred to by Pierpont took in practice, and what kind of rhetoric accompanies the “painful emotions”. A reader attuned to 19th century literature will not fail to recognize infinitesimals as the implied target of Pierpont’s epithet “metaphysical speculations”.

Thus, Cantor published a “proof-sketch” of a claim to the effect that the notion of an infinitesimal is inconsistent. By this time, several detailed constructions of non-Archimedean systems had appeared, notably by Stolz and du Bois-Reymond.

When Stolz published a defense of his work, arguing that technically speaking Cantor’s criticism does not apply to his system, Cantor responded by artful innuendo aimed at undermining the credibility of his opponents. At no point did Cantor vouchsafe to address their publications themselves. In his 1890 letter to Veronese, Cantor specifically referred to the work of Stolz and du Bois-Reymond. Cantor refers to their work on non-Archimedean systems as not merely an “abomination”, but a “self contradictory and completely useless” one.

P. Ehrlich [52, p. 54] analyzes the errors in Cantor’s “proof” and documents his rhetoric.

The effect on the university classroom has been pervasive. In an emotionally charged atmosphere, students of calculus today are warned against taking the apparent ratio \( \frac{dy}{dx} \) literally. By the time one
reaches the chain rule \( \frac{dy}{dt} = \frac{dy}{dx} \frac{dx}{dt} \), the awkward contorsions of an obstinate denial are palpable throughout the spectrum of the undergraduate textbooks.\(^{41}\)

Who invented the real number system? According to van der Waerden, Simon Stevin’s general notion of a real number was accepted, tacitly or explicitly, by all later scientists [159, p. 69].

D. Fearnley-Sander writes that the modern concept of real number [...] was essentially achieved by Simon Stevin, around 1600, and was thoroughly assimilated into mathematics in the following two centuries [56, p. 809].

D. Fowler points out that Stevin [...] was a thorough-going arithmetizer: he published, in 1585, the first popularization of decimal fractions in the West [...]; in 1594, he described an algorithm for finding the decimal expansion of the root of any polynomial, the same algorithm we find later in Cauchy’s proof of the intermediate value theorem [62, p. 733].

The algorithm is discussed in more detail in [147, §10, p. 475-476]. Unlike Cauchy, who halves the interval at each step, Stevin subdivides the interval into ten equal parts, resulting in a gain of a new decimal digit of the solution at every iteration of the algorithm.\(^{42}\)

At variance with these historical judgments, the mathematical community tends overwhelmingly to award the credit for constructing the real number system to the great triumvirate,\(^{43}\) in appreciation of the successful extirpation of infinitesimals as a byproduct of the Weierstrassian epsilontic formulation of analysis.

To illustrate the nature of such a reconstruction, consider Cauchy’s notion of continuity. H. Freudenthal notes that “Cauchy invented our notion of continuity” [60, p. 136]. Cauchy’s starting point is a description of perceptual continuity of a function in terms of “varying by

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\(^{41}\)A concrete suggestion with regard to undergraduate teaching may be found at the end of Section 8.

\(^{42}\)Stevin’s numbers were anticipated by E. Bonfils in 1350, see S. Gandz [63]. Bonfils says that “the unit is divided into ten parts which are called Primes, and each Prime is divided into ten parts which are called Seconds, and so on into infinity” [63, p. 39].

\(^{43}\)See footnote 3 for the origin of this expression.
imperceptible degrees”. Such a turn of phrase occurs both in his letter to Coriolis of 1837, and in his 1853 text [36, p. 35].

Cauchy transforms perceptual continuity into a mathematical notion by exploiting his conception of an infinitesimal as being generated by a null sequence (see [26]). Both in 1821 and in 1853, Cauchy defines continuity of \( y = f(x) \) in terms of an infinitesimal \( x \)-increment resulting in an infinitesimal change in \( y \).

The well-known nominalistic residue of the perceptual definition (a residue that dominates our classrooms) would have \( f \) be continuous at \( x \) if for every positive epsilon there exists a positive delta such that if \( h \) is less than delta then \( f(x + h) - f(x) \) is less than epsilon, namely:

\[
\forall \epsilon > 0 \exists \delta > 0 : |h| < \delta \implies |f(x + h) - f(x)| < \epsilon.
\]

This can hardly be said to be a hermeneutic reconstruction of Cauchy’s infinitesimal definition. In our classrooms, are students being dressed to perform multiple-quantifier Weierstrassian epsilonlic logical stunts, on the pretense of being taught infinitesimal calculus?

Lord Kelvin’s technician, wishing to exploit the notion of continuity in a research paper, is unlikely to be interested in 4-quantifier definitions thereof. Regardless of the answer to such a question, the revolutionary nature of the triumvirate reconstruction of the foundations of analysis is evident. If one accepts the thesis that elimination of ontological entities called “infinitesimals” does constitute a species of nominalism, then the triumvirate recasting of analysis was a nominalist project. We will deal with Cantor and Dedekind in more detail in Section 6.

6. CANTOR AND DEDEKIND

Cantor is on record describing infinitesimals as the “cholera bacillus of mathematics” in a letter dated 12 December 1893, quoted in Meschkowski [113, p. 505] (see also Dauben [42, p. 353] and [43, p. 124]). Cantor went as far as publishing a purported “proof” of their logical

44Both Cauchy’s original French “par degr´es insensibles”, and its correct English translation “by imperceptible degrees”, are etymologically related to sensory perception.

45One can apply here Burgess’s remark to the effect that “[t]his is educational reform in the wrong direction: away from applications, toward entanglement in logical subtleties” [30, pp. 98-99].

46In analyzing Chihara’s and Field’s nominalist reconstructions, Burgess [30, p. 96] is sceptical as to the plausibility of interpreting what Lord Kelvin’s technician is saying, in terms of tacit knowledge of such topics in foundations of mathematics as predicative analysis and measurement theory.
inconsistency, as discussed in Section 5. Cantor may have extended numbers both in terms of the complete ordered field of real numbers and his theory of infinite cardinals; however, he also passionately believed that he had not only given a logical foundation to real analysis, but also simultaneously eliminated infinitesimals (the cholera bacillus).

Dedekind, while admitting that there is no evidence that the “true” continuum indeed possesses the property of completeness he championed (see M. Moore [115, p. 82]), at the same time formulated his definition of what came to be known as Dedekind cuts, in such a way as to rule out infinitesimals.

S. Feferman describes Dedekind’s construction of an complete ordered line $(\mathbb{R}, <)$ as follows:

Dedekind’s construction of such an $(\mathbb{R}, <)$ is obtained by taking it to consist of the rational numbers together with the numbers corresponding to all those cuts $(X_1, X_2)$ in $\mathbb{Q}$ for which $X_1$ has no largest element and $X_2$ has no least element, ordered in correspondence to the ordering of cuts $(X_1, X_2) < (Y_1, Y_2)$ when $X_1$ is a proper subset of $Y_1$. Dedekind himself spoke of this construction of $\mathbb{R}$ as individual cuts in $\mathbb{Q}$ for which $X_1$ has no largest element and $X_2$ no least element as the creation of an irrational number \footnote{Dedekind in 1872, translation in Ewald [55, p. 773].} though he did not identify the numbers themselves with those cuts \footnote{Dedekind in 1872, translation in Ewald [55, p. 773].}.

In this way the “gappiness” of the rationals is overcome, in Dedekind’s terminology.

Now requiring that an element of the continuum should induce a partition of $\mathbb{Q}$ does not yet rule out infinitesimals. However, requiring that a given partition of $\mathbb{Q}$ should correspond to a unique element of the continuum, does have the effect of ruling out infinitesimals. In the context of an infinitesimal-enriched continuum, it is clear that a pair of quantities in the cluster (halo) infinitely close to $\pi$, for example, will define the same partition of the rationals. Therefore the clause of “only one” forces a collapse of the infinitesimal cluster to a single quantity, in this case $\pi$.

Was rigor linked to the elimination of infinitesimals? E. Hobson in his retiring presidential address [80, p. 128] in 1902 summarized the advances in analysis over the previous century, and went on explicitly to make a connection between the foundational accomplishments in analysis, on the one hand, and the elimination of infinitesimals, on the other, by pointing out that an equivalence class defining a real number...
“is of such a character that no use is made in it of infinitesimals,” suggesting that Hobson viewed them as logically inconsistent (perhaps following Cantor or Berkeley). The matter of rigor will be analyzed in more detail in the next section.

7. A critique of the Weierstrassian scientific method

In criticizing the nominalistic aspect of the Weierstrassian elimination of infinitesimals, does one neglect the mathematical reasons why this was considered desirable?

The stated goal of the triumvirate program was mathematical rigor. Let us examine the meaning of mathematical rigor. Conceivably rigor could be interpreted in at least the following four ways, not all of which we endorse:

(1) it is a shibboleth that identifies the speaker as belonging to a clan of professional mathematicians;
(2) it represents the idea that as the field develops, its practitioners attain greater and more conceptual understanding of key issues, and are less prone to error;
(3) it represents the idea that a search for greater correctness in analysis inevitably led Weierstrass to epsilontics in the 1870s;
(4) it refers to the establishment of ultimate foundations for mathematics by Cantor, eventually explicitly expressed in axiomatic form by Zermelo and Fraenkel.

Item (1) may be pursued by a fashionable academic in the social sciences, but does not get to the bottom of the issue. Meanwhile, item (2) could apply to any exact science, and does not involve a commitment as to which route the development of mathematics may have taken. Item (2) could be supported by scientists in and outside of mathematics alike, as it does not entail a commitment to a specific destination or ultimate goal of scientific development as being pre-determined and intrinsically inevitable.

48 See footnote 6 for more details on Hobson.
49 It would be interesting to investigate the role of the Zermelo–Fraenkel axiomatisation of set theory in cementing the nominalistic disposition we are analyzing. Keisler points out that “the second- and higher-order theories of the real line depend on the underlying universe of set theory [...] Thus the properties of the real line are not uniquely determined by the axioms of set theory” [94, p. 228] [emphasis in the original–the authors]. He adds: “A set theory which was not strong enough to prove the unique existence of the real line would not have gained acceptance as a mathematical foundation” [94, p. 228]. Edward Nelson [118] has developed an alternative axiomatisation more congenial to infinitesimals.
On the other hand, the actual position of a majority of professional mathematicians today corresponds to items (3) and (4). The crucial element present in (3) and (4) and absent in (2) is the postulation of a specific outcome, believed to be the inevitable result of the development of the discipline. Challenging such a belief appears to be a radical proposition in the eyes of a typical professional mathematician, but not in the eyes of scientists in related fields of the exact sciences. It is therefore particularly puzzling that (3) and (4) should be accepted without challenge by a majority of historians of mathematics, who tend to toe the line on the mathematicians’ belief. It is therefore necessary to examine such a belief, which, as we argue, stems from a particular philosophical disposition akin to nominalism.

Could mathematical analysis have followed a different path of development? In an intriguing text published a decade ago, Pourciau examines the foundational crisis of the 1920s and the Brouwer–Hilbert controversy, and argues that Brouwer’s view may have prevailed had Brouwer been more of an... Errett Bishop. While we are sceptical to Pourciau’s main conclusions, the unmistakable facts are as follows:

(1) a real struggle did take place;
(2) some of the most brilliant minds at the time did side with Brouwer, at least for a period of time (e.g., Hermann Weyl);
(3) the battle was won by Hilbert not by mathematical means alone but also political ones, such as maneuvering Brouwer out of a key editorial board;
(4) while retroactively one can offer numerous reasons why Hilbert’s victory might have been inevitable, this was not at all obvious at the time.

We now leap back a century, and consider a key transitional figure, namely Cauchy. In 1821, Cauchy defined continuity of $y = f(x)$ in terms of “an infinitesimal $x$-increment corresponding to an infinitesimal $y$-increment.” Many a practicing mathematician, brought up on an alleged “Cauchy-Weierstrass $\epsilon, \delta$” tale, will be startled by such a revelation. The textbooks and the history books routinely obfuscate the nature of Cauchy’s definition of continuity.

Fifty years before Weierstrass, Cauchy performed a hypostatization by encapsulating a variable quantity tending to zero, into an individual/atomic entity called “an infinitesimal”.

50 More specifically, Pourciau would have wanted Brouwer to stop “wasting time” on free choice sequences and the continuum, and to focus instead on developing analysis on a constructive footing based on \(\mathbb{N}\).

51 See Section 9 for more details on Cauchy’s definition.
Was the naive traditional “definition” of the infinitesimal blatantly self-contradictory? We argue that it was not. Cauchy’s definition in terms of null sequences is a reasonable definition, and one that connects well with the sequential approach of the ultrapower construction. Mathematicians viewed infinitesimals with deep suspicion due in part to a conflation of two separate criticisms, the logical one and the metaphysical one, by Berkeley, see Sherry [143]. Thus, the emphasis on the elimination of infinitesimals in the traditional account of the history of analysis is misplaced.

Could analysis have developed on the basis of infinitesimals? Continuity, as all fundamental notions of analysis, can be defined, and were defined, by Cauchy in terms of infinitesimals. Epsilontics could have played a secondary role of clarifying whatever technical situations were too awkward to handle otherwise, but arguably they needn’t have replaced infinitesimals. As far as the issue of rigor is concerned, it needs to be recognized that Gauss and Dirichlet published virtually error-free mathematics before Weierstrassian epsilontics, while Weierstrass himself was not protected by epsilontics from publishing an erroneous paper by S. Kovalevskaya (the error was found by Volterra, see [127, p.568]).

As a scientific discipline develops, its practitioners gain a better understanding of the conceptual issues, which helps them avoid errors. But assigning a singular, oracular, and benevolent role in this to epsilontics is philosophically naive. The proclivity to place the blame for errors on infinitesimals betrays a nominalistic disposition aimed against the ghosts of departed quantities, already dubbed “charlatanerie” by d’Alembert [40] in 1754.

8. A QUESTION SESSION

The following seven questions were formulated by R. Hersh, who also motivated the author to present the material of Section 4, as well as that of Section 7.

Question 8.1. Was a nominalistic viewpoint motivating the triumvirate project?

Answer. We argue that the answer is affirmative, and cite two items as evidence:

(1) Dedekind’s cuts and the “essence of continuity”, and
(2) Cantor’s tooth-and-nail fight against infinitesimals.

52 See discussion around formula (15.3) in Section 15 for more details.
53 The issue of alternative axiomatisations is discussed in footnote 49.
Concerning (1), mathematicians widely believed that Dedekind discovered such an essence. What is meant by the essence of continuity in this context is the idea that a pair of “cuts” on the rationals are identical if and only if the pair of numbers defining them are equal. Now the “if” part is unobjectionable, but the almost reflexive “only if” part following it has the effect of a steamroller flattening the B-continuum into an A-continuum. Namely, it collapses each monad (halo, cluster) to a point, since a pair of infinitely close (adequal) points necessarily define the same cut on the rationals (see Section 6 for more details). The fact that the steamroller effect was gladly accepted as a near-axiom is a reflection of a nominalistic attitude.

Concerning (2), Cantor not only published a “proof-sketch” of the non-existence of infinitesimals, he is on record calling them an “abomination” as well as the “cholera bacillus” of mathematics. When Stolz meekly objected that Cantor’s “proof” does not apply to his system, Cantor responded by the “abomination” remark (see Section 6 for more details). Now Cantor’s proof contains an error that was exhaustively analyzed by Ehrlich [52]. As it stands, it would “prove” the non-existence of the surreals! Incidentally, Ehrlich recently proved that “maximal” surreals are isomorphic to “maximal” hyperreals. Can Cantor’s attitude be considered as a philosophical predisposition to the detriment of infinitesimals?

**Question 8.2.** It has been written that Cauchy’s concern with clarifying the foundations of calculus was motivated by the need to teach it to French military cadets.

**Answer.** Cauchy did have some tensions with the management of the *Ecole Polytechnique* over the teaching of infinitesimals between 1814 and 1820. In around 1820 he started using them in earnest in both his textbooks and his research papers, and continued using them throughout his life, well past his teaching stint at the *Ecole*. Thus, in his 1853 text [36] he reaffirms the infinitesimal definition of continuity he gave in his 1821 textbook [33].

**Question 8.3.** Doesn’t reasoning by infinitesimals require a deep intuition that is beyond the reach of most students?

Kathleen Sullivan’s study [151] from 1976 shows that students enrolled in sections based on Keisler’s textbook end up having a better conceptual grasp of the notions of calculus than control groups following the standard approach. Two years ago, I taught infinitesimal

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54 See Section [13]
calculus to a group of 25 freshmen. I also had to train the TA who was new to the material. According to persistent reports from the TA, the students have never been so excited about learning calculus. On the contrary, it is the multiple-quantifier Weierstrassian epsilontic logical stunts that our students are dressed to perform (on pretense of being taught infinitesimal calculus) that are beyond their reach. In an ironic commentary on the nominalistic ethos reigning in our departments, not only was I relieved of my teaching this course the following year, but the course number itself was eliminated.

**Question 8.4.** It may true that “epsilontics” is in practice repugnant to many students. But the question is whether an issue that is really a matter of technical mathematics related to pedagogy is being misleadingly presented as a question of high metaphysics.

Answer. Berkeley turned this into a metaphysical debate. Generations of mathematicians have grown up thinking of it as a metaphysical debate. Such a characterisation is precisely what we contest.

**Question 8.5.** Isn’t “a positive number smaller than all positive numbers” self-contradictory? Is a phrase such as “I am smaller than myself”, intelligible?

Answer. Both Carnot and Cauchy say that an infinitesimal is generated by a variable quantity that becomes smaller than any fixed quantity. No contradiction here. The otherwise excellent study by Ehrlich [52] contains a curious slip with regard to Poisson. Poisson describes infinitesimals as being “less than any given magnitude of the same nature” [123, p. 13-14] (the quote is reproduced in Boyer [25, p. 283]). Ehrlich inexplicably omits the crucial modifier “given” when quoting Poisson in footnote 133 on page 76 of [52]. Based on the incomplete quote, Ehrlich proceeds to agree with Veronese’s assessment (of Poisson) that “[t]his proposition evidently contains a contradiction in terms” [160, p. 622]. Our assessment is that Poisson’s definition is in fact perfectly consistent.

**Question 8.6.** Infinitesimals were one thorny issue. Didn’t it take the modern theory of formal languages to untangle that?

Answer. Not exactly. A long tradition of technical work in non-Archimedean continua starts with Stolz and du Bois-Reymond, Levi-Civita, Hilbert, and Borel, see Ehrlich [52]. The tradition continues uninterrupted until Hewitt constructs the hyperreals in 1948. Then came Loś’s theorem whose consequence is a transfer principle, which is a mathematical implementation of the heuristic “law of continuity”
of Leibniz (“what’s true in the finite domain should remain true in the infinite domain”). What Łoś and Robinson untangled was the transfer principle. Non-Archimedean systems had a long history prior to these developments.

**Question 8.7.** There is still a pedagogical issue. I do understand that Keisler’s calculus book is teachable. But this says nothing about the difficulty of teaching calculus in terms of infinitesimals back around 1800. Keisler has Robinson’s non-standard analysis available, as a way to make sense of infinitesimals. Cauchy did not. Do you believe that Cauchy used a definition of infinitesimal in terms of a null sequence of rationals (or reals) in teaching introductory calculus?

**Answer.** The historical issue about Cauchy is an interesting one. Most of his course notes from the period 1814-1820 have been lost. His predecessor at the École Polytechnique, L. Carnot, defined infinitesimals exactly the same way as Cauchy did, but somehow is typically viewed by historians as belonging to the old school as far as infinitesimals are concerned (and criticized for his own version of the “cancellation of errors” argument originating with Berkeley). As far as Cauchy’s textbooks from 1821 onward indicate, he declares at the outset that infinitesimals are an indispensable foundational tool, defines them in terms of null sequences (more specifically, “a variable quantity becomes an infinitesimal”), defines continuity in terms of infinitesimals, defines his “Dirac” delta function in terms of infinitesimals (see [103]), defines infinitesimals of arbitrary real order in [35, p. 281], anticipating later work by Stolz, du Bois-Reymond, and others.

The following eight questions were posed by Martin Davis.

**Question 8.8.** How would you answer the query: how do you define a “null sequence”? Aren’t you back to epsilons?

**Answer.** Not necessarily. One could define it, for example, in terms of “only finitely many terms outside each given separation from zero”. While epsilontics has important applications, codifying the notion of a null sequence is not one of them. Epsilontics is helpful when it comes to characterizing a Cauchy sequence, if one does not yet know the limiting value. If one does know the limiting value, as in the case of a null sequence, a multiple-quantifier epsilontic formulation is no clearer than saying that all the terms eventually get arbitrarily small. To be more specific, if one describes a Cauchy sequence by saying that “terms eventually get arbitrarily close to each other”, the ambiguity can lead and has led to errors, though not in Cauchy (the sequences are rightfully named after him as he was aware of the trap). Such an ambiguity is
just not there as far as null sequences are concerned. Giving an epsilon-tic definition of a null sequence does not increase understanding and does not decrease the likelihood of error. A null sequence is arguably a notion that’s no more complex than multiple-quantifier epsilon-tics, just as the natural numbers are no more complex than the set-theoretic definition thereof in terms of 0 = ∅, 1 = {∅}, 2 = {∅, {∅}}, . . . which requires infinitely many set-theoretic types to make the point. Isn’t a natural number a more primitive notion?

**Question 8.9.** You evoke Cauchy’s use of variable quantities. But whatever is a “variable quantity”?

The concept of variable quantity was not clearly defined by mathematicians from Leibniz and l’Hopital onwards, and is today considered a historical curiosity. Cauchy himself sometimes seems to think they take discrete values (in his 1821 text [33]) and sometimes continuous (in his 1823 text [34]). Many historians agree that in 1821 they were discrete sequences, and Cauchy himself gives explicit examples of sequences. Now it is instructive to compare such variable quantities to the procedures advocated by the triumvirate. In fact, the approach can be compared to Cantor’s construction of the real numbers. A real number is a Cauchy sequence of rational numbers, modulo an equivalence relation. A sequence, which is not an individual/atomic entity, comes to be viewed as an atomic entity by a process which in triumvirate lexicon is called an equivalence relation, but in much older philosophical terminology is called *hypostatisation*. In education circles, researchers tend to use terms such as *encapsulation*, *procept*, and *reification*, instead. As you know, the ultrapower construction is a way of hypostatizing a hyperreal out of sequences of reals. As far as Cauchy’s competing views of a variable quantity as discrete (in 1821) or continuous (in 1823), they lead to distinct implementations of a B-continuum in Hewitt (1948), when a “continuous” version of the ultrapower construction was used, and in Luxemburg (1962), where the discrete version was used (a more recent account of the latter is in Goldblatt [68]).

**Question 8.10.** Isn’t the notion of a variable quantity a pernicious notion that makes time an essential part of mathematics?

Answer. I think you take Zeno’s paradoxes too seriously. I personally don’t think there is anything wrong with involving time in mathematics. It has not led to any errors as far as I know, *pace* Zeno.

**Question 8.11.** Given what relativity has taught us about time, is it a good idea to involve time in mathematics?
Answer. What did relativity teach us about time that would make us take time out of mathematics? That time is relative? But you may be confusing metaphysics with mathematics. Time that’s being used in mathematics is not an exact replica of physical time. We may still be influenced by 17th century doctrine according to which every theoretical entity must have an empirical counterpart/referent. This is why Berkeley was objecting to infinitesimals (his metaphysical criticism anyway). I would put time back in mathematics the same way I would put infinitesimals back in mathematics. Neither concept is under any obligation of corresponding to an empirical referent.

**Question 8.12.** Didn’t the triumvirate show us how to prove the existence of a complete ordered field?

Answer. Simon Stevin had already made major strides in defining the real numbers, represented by decimals. Some essential work needed to be done, such as the fact that the usual operations are well defined. This was done by Dedekind, see Fowler [62]. But Stevin numbers themselves were several centuries older, even though they go under the soothing name of numbers so real.

**Question 8.13.** My NSA book [45] does it by forming the quotient of the ring of finite hyper-rational numbers by the ideal of infinitesimals. . .

The remarkable fact is that this construction is already anticipated by Kästner (a contemporary of Euler’s) in the following terms: “If one partitions 1 without end into smaller and smaller parts, and takes larger and larger collections of such little parts, one gets closer and closer to the irrational number without ever attaining it”. Kästner concludes:

“Therefore one can view it as an infinite collection of infinitely small parts” [3], cited by Cousquer [38].

**Question 8.14.** . . . but that construction was not available to the earlier generations.

But Stevin numbers were. They kept on teaching analysis in France throughout the 1870s without any need for “constructing” something that had already been around for a century before Leibniz, see discussion in Laugwitz [104, p. 274].

**Question 8.15.** Aren’t you conflating the problem of rigorous foundation with how to teach calculus to beginners?

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55 This theme is developed in more detail in Section 5.
56 Kästner’s suggestion is implemented by the surjective leftmost vertical arrow in our Figure 5 in Section 15.
Figure 1. Differentiating $y = f(x) = x^2$ at $x = 1$ yields

\[
\frac{\Delta y}{\Delta x} = \frac{f(.9\ldots) - f(1)}{.9\ldots - 1} = \frac{(.9\ldots)^2 - 1}{.9\ldots - 1} = \frac{.9\ldots - 1)(.9\ldots + 1)}{.9\ldots - 1} = .9\ldots + 1 \approx 2.
\]

Here $\approx$ is the relation of being infinitely close (adequal). Hyperreals of the form $.9\ldots$ are discussed in [85].

As far as rigorous foundations are concerned, alternative foundations to ZF have been developed that are more congenial to infinitesimals, such as Edward Nelson’s [118]. Mathematicians are accustomed to thinking of ZF as “the foundations”. It needs to be recognized that this is a philosophical assumption. The assumption can be a reflection of a nominalist mindframe.

The example of a useful application of infinitesimals analyzed at the end of this section is quite elementary. A more advanced example is the elegant construction of the Haar measure in terms of counting infinitesimal neighborhoods, see Goldblatt [68]. Even more advanced examples such as the proof of the invariant subspace conjecture are explained in your book [45]. For an application to the Boltzmann equation, see Arkeryd [4, 5].

As a concrete example of what consequences a correction of the nominalistic triumvirate attitude would entail in the teaching of the calculus, consider the problem of the unital evaluation of the decimal .999\ldots, i.e., its evaluation to the unit value 1. Students are known overwhelmingly to believe that the number $a = .999\ldots$ falls short of 1 by an infinitesimal amount. A typical instructor believes such student intuitions to be erroneous, and seeks to inculcate the unital evaluation of $a$. An alternative approach was proposed by Ely [54] and Katz & Katz [84]. Instead of refuting student intuitions, an instructor could build upon them to calculate the derivative of $y = x^2$ at $x = 1$ by
choosing an infinitesimal $\Delta x = a - 1$ and showing that

$$\frac{\Delta y}{\Delta x} = \frac{a^2 - 1^2}{a - 1} = \frac{(a - 1)(a + 1)}{a - 1} = a + 1$$

is infinitely close (adequal) to 2, yielding the desired value without either epsilonics, estimates, or limits, see Figure 1. Here $a$ is interpreted as an extended decimal string with an infinite hypernatural’s worth of 9s, see [105]. Instead of building upon student intuition, a typical calculus course seeks to flatten it into the ground by steamrolling the B-continuum into the A-continuum, see Katz and Katz [84, 85]. A nominalist view of what constitutes an allowable number system has produced an ostrich effect whereby mathematics educators around the globe have failed to recognize the legitimacy, and potency, of students’ nonstandard conceptions of .999..., see Ely [54] for details.

9. The battle for Cauchy’s lineage

This section analyzes the reconstruction of Cauchy’s foundational work in analysis usually associated with J. Grabiner, and has its sources in the work of C. Boyer. A critical analysis of the traditional approach may be found in Hourya Benis Sinaceur’s article [145] from 1973. To place such work in a historical perspective, a minimal chronology of commentators on Cauchy’s foundational work in analysis would have to mention F. Klein’s observation in 1908 that

since Cauchy’s time, the words *infinitely small* are used in modern textbooks in a somewhat changed sense. One never says, namely, that a quantity *is* infinitely small, but rather that it becomes infinitely small” [95, p. 219].

Indeed, Cauchy’s starting point in defining an infinitesimal is a null sequence (i.e., sequence tending to zero), and he repeatedly refers to such a null sequence as becoming an infinitesimal.

P. Jourdain’s detailed 1913 study [82] of Cauchy is characterized by a total absence of any claim to the effect that Cauchy may have based his notion of infinitesimal, on limits.

C. Boyer quotes Cauchy’s definition of continuity as follows:

the function $f$ is continuous within given limits if between these limits an infinitely small increment $i$ in the variable $x$ produces always an infinitely small increment, $f(x + i) - f(x)$, in the function itself [25, p. 277].

57 Such an effect is comparable to a constructivist’s reaction to the challenge of meaningful applications of a post-LEM variety, see main text in Section 4 around footnote 35.
Next, Boyer proceeds to interpret Cauchy’s definition of continuity as follows: “The expressions infinitely small are here to be understood [...] in terms of [...] limits: i.e., \( f(x) \) is continuous within an interval if the limit of the variable \( f(x) \) as \( x \) approaches \( a \) is \( f(a) \), for any value of \( a \) within this interval” [emphasis added–authors]. Boyer feels that infinitesimals are to be understood in terms of limits. Or perhaps they are to be understood otherwise?

In 1967, A. Robinson discussed the place of infinitesimals in Cauchy’s work. He pointed out that “the assumption that [infinitesimals] satisfy the same laws as the ordinary numbers, which was stated explicitly by Leibniz, was rejected by Cauchy as unwarranted”. Yet,

Cauchy’s professed opinions in these matters notwithstanding, he did in fact treat infinitesimals habitually as if they were ordinary numbers and satisfied the familiar rules of arithmetic\(^{58}\)\[132\ p. 36\], \[133\ p. 545\].

T. Koetsier remarked that, had Cauchy wished to extend the domain of his functions to include infinitesimals, he would no doubt have mentioned how exactly the functions are to be so extended\(^{59}\). Beyond the observation that Cauchy did, in fact, make it clear that such an extension is to be carried out term-by-term\(^{60}\), Koetsier’s question prompts a similar query: had Cauchy wished to base his calculus on limits, he would no doubt have mentioned something about such a foundational stance. Instead, Cauchy emphasized that in founding analysis he was unable to avoid elaborating the fundamental properties of infinitely small quantities, see \[33\]. No mention of a foundational role of limits is anywhere to be found in Cauchy, unlike his would-be modern interpreters.

\(^{58}\)Freudenthal, similarly, notes a general tendency on Cauchy’s part not to play by the rules: “Cauchy was rather more flexible than dogmatic, for more often than not he sinned against his own precepts” \[60\ p. 137\]. Cauchy’s irrevent attitude extended into the civic domain, as Freudenthal reports the anecdote dealing with Cauchy’s stint as social worker in the town of Sceaux: “he spent his entire salary for the poor of that town, about which behavior he reassured the mayor: ‘Do not worry, it is only my salary; it is not my money, it is the emperor’s’ ” \[60\ p. 133\].

\(^{59}\)Here Koetsier asks: “If Cauchy had really wanted to consider functions defined on sets of infinitesimals, isn’t it then highly improbable that he would not have explicitly said so?” \[96\ p. 90\].

\(^{60}\)Namely, an infinitesimal being generated by a null sequence, we evaluate \( f \) at it by applying \( f \) to each term in the sequence. Bråting \[26\] analyzes Cauchy’s use of the particular sequence \( x = \frac{1}{n} \) in \[36\].
L. Sad et al have pursued this matter in detail in [136], arguing that what Cauchy had in mind was a prototype of an ultrapower construction, where the equivalence class of a null sequence indeed produces an infinitesimal, in a suitable set-theoretic framework.

To summarize, a post-Jourdain nominalist reconstruction of Cauchy’s infinitesimals, originating no later than Boyer, reduces them to a Weierstrassian notion of limit. To use Burgess’ terminology borrowed from linguistics, the Boyer-Grabiner interpretation becomes the hypothesis that certain noun phrases [in the present case, infinitesimals] in the surface structure are without counterpart in the deep structure [30, p. 97].

Meanwhile, a rival school of thought places Cauchy’s continuum firmly in the line of infinitesimal-enriched continua. The ongoing debate between rival visions of Cauchy’s continuum echoes Felix Klein’s sentiment reproduced above.

Viewed through the lens of the dichotomy introduced by Burgess, it appears that the traditional Boyer-Grabiner view is best described as a hermeneutic, rather than revolutionary, nominalistic reconstruction of Cauchy’s foundational work.

Cauchy’s definition of continuity in terms of infinitesimals has been a source of an on-going controversy, which provides insight into the nominalist nature of the Boyer-Grabiner reconstruction. Many historians have interpreted Cauchy’s definition as a proto-Weierstrassian definition of continuity in terms of limits. Thus, Smithies [146, p. 53, footnote 20] cites the page in Cauchy’s book where Cauchy gave the infinitesimal definition, but goes on to claim that the concept of limit was Cauchy’s “essential basis” for his concept of continuity [146, p. 58]. Smithies looked in Cauchy, saw the infinitesimal definition, and went on to write in his paper that he saw a limit definition. Such automated translation has been prevalent at least since Boyer [25, p. 277]. Smithies cites chapter and verse in Cauchy where the latter gives an infinitesimal definition of continuity, and proceeds to claim that Cauchy gave a modern one. Such awkward contortions are a trademark of a nominalist. In the next section, we will examine the methodology of nominalistic Cauchy scholarship.

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61 See formula (15.3) in Section 15.
62 See discussion of Klein in the main text around footnote 5. The two rival views of Cauchy’s infinitesimals have been pursued by historians, mathematicians, and philosophers, alike. The bibliography in the subject is vast. The most detailed statement of Boyer’s position may be found in Grabiner [69]. Robinson’s perspective was developed most successfully by D. Laugwitz [102] in 1989, and by K. Bråting [26] in 2007.
10. A subtle and difficult idea of adequality

The view of the history of analysis from the 1670s to the 1870s as a 2-century triumphant march toward the yawning heights of the rigor of Weierstrassian epsilontics has permeated the very language mathematicians speak today, making an alternative account nearly unthinkable. A majority of historians have followed suit, though some truly original thinkers differed. These include C. S. Peirce, Felix Klein, N. N. Luzin [107], Hans Freudenthal, Robinson, Lakatos [108], Laugwitz, Teixeira [136], and Bråting [26].

Meanwhile, J. Grabiner offered the following reflection on the subject of George Berkeley’s criticism of infinitesimal calculus:

“Since an adequate response to Berkeley’s objections would have involved recognizing that an equation involving limits is a shorthand expression for a sequence of inequalities—a subtle and difficult idea—no eighteenth century analyst gave a fully adequate answer to Berkeley [70, p. 189].”

This is an astonishing claim, which amounts to reading back into history, feedback-style, developments that came much later. Such a claim amounts to postulating the inevitability of a triumphant march, from Berkeley onward, toward the radiant future of Weierstrassian epsilontics (“sequence of inequalities—a subtle and difficult idea”). The claim of such inevitability in our opinion is an assumption that requires further argument. Berkeley was, after all, attacking the coherence of infinitesimals. He was not attacking the coherence of some kind of incipient form of Weierstrassian epsilontics and its inequalities. Isn’t there a simpler answer to Berkeley’s query, in terms of a distinction between “variable quantity” and “given quantity” already present in l’Hôpital’s textbook at the end of the 17th century? The missing ingredient was a way of relating a variable quantity to a given quantity, but that, too, was anticipated by Pierre de Fermat’s concept of adequality, as discussed in Section 13.

Grattan-Guinness enunciates a historical reconstruction project in the name of H. Freudenthal [61] in the following terms:

“It is mere feedback-style ahistory to read Cauchy (and contemporaries such as Bernard Bolzano) as if they had read Weierstrass already. On the contrary, their own pre-Weierstrassian muddles need historical reconstruction [71, p. 176].”

The term “muddle” refers to an irreducible ambiguity of historical mathematics such as Cauchy’s sum theorem of 1821.
We will analyze the problem in more detail from the 19th century, pre-Weierstrass, viewpoint of Cauchy’s textbooks. In Cauchy’s world, a variable quantity \( q \) can have a “limiting” fixed quantity \( k \), such that the difference \( q - k \) is infinitesimal. Consider Cauchy’s decomposition of an arbitrary infinitesimal of order \( n \) as a sum

\[
k\alpha^n(1 + \epsilon)
\]

(see [33, p. 28]), where \( k \) is fixed nonzero, whereas \( \epsilon \) is a variable quantity representing an infinitesimal. If one were to set \( n = 0 \) in this formula, one would obtain a representation of an an arbitrary finite quantity \( q \), as a sum

\[
q = k + k\epsilon.
\]

If we were to suppress the infinitesimal part \( k\epsilon \), we would obtain “the standard part” \( k \) of the original variable quantity \( q \). In the terminology of Section [13] we are dealing with a passage from a finite point of a B-continuum, to the infinitely close (adequal) point of the A-continuum, namely passing from a variable quantity to its limiting constant (fixed, given) quantity.

Cauchy had the means at his disposal to resolve Berkeley’s query, so as to solve the logical puzzle of the definition of the derivative in the context of a B-continuum. While he did not resolve it, he did not need the subtle and difficult idea of Weierstrassian epsilontics; suggesting otherwise amounts to feedback-style ahistory.

This reader was shocked to discover, upon his first reading of chapter 6 in Schubring [137], that Schubring is not aware of the fact that Robinson’s non-standard numbers are an extension of the real numbers.

Consider the following three consecutive sentences from Schubring’s chapter 6:

“[A] [Giusti’s 1984 paper] spurred Laugwitz to even more detailed attempts to banish the error and confirm that Cauchy had used hyper-real numbers.

[B] On this basis, he claims, the errors vanish and the theorems become correct, or, rather, they always were correct (see Laugwitz 1990, 21).

[C] In contrast to Robinson and his followers, Laugwitz (1987) assumes that Cauchy did not use nonstandard numbers in the sense of NSA, but that his infinitesimal petitis were infinitesimals representing an extension of the field of real numbers” [137, p. 432].
These three sentences, which we have labeled [A], [B], and [C], tell a remarkable story that will allow us to gauge Schubring’s exact relationship to the subject of his speculations. What interests us are the first sentence [A] and the last sentence [C]. Their literal reading yields the following four views put forth by Schubring: (1) Laugwitz interpreted Cauchy as using hyperreal numbers (from sentence [A]); (2) Robinson assumed that Cauchy used “nonstandard numbers in the sense of NSA” (from sentence [C]); (3) Laugwitz disagreed with Robinson on the latter point (from sentence [C]); (4) Laugwitz interpreted Cauchy as using an extension of the field of real numbers (from sentence [C]). Taken at face value, items (1) and (4) together would logically indicate that (5) Laugwitz interpreted Cauchy as using the hyperreal extension of the reals; moreover, if, as indicated in item (3), Laugwitz disagreed with Robinson, then it would logically follow that (6) Robinson interpreted Cauchy as not using the hyperreal extension of the reals; as to the question what number system Robinson did attribute to Cauchy, item (2) would indicate that (7) Robinson used, not Laugwitz’s hyperreals, but rather “nonstandard numbers in the sense of NSA”.

We hasten to clarify that all of the items listed above are incoherent. Indeed, Robinson’s “non-standard numbers” and the hyperreals are one and the same number system (see Section 15 for more details; Robinson’s approach is actually more general than Hewitt’s hyperreal fields). Meanwhile, Laugwitz’s preferred system is a different system altogether, called Omega-calculus. We gather that Schubring literally does not know what he is writing about when he takes on Robinson and Laugwitz.

A reader interested in an introduction to Popper and fallibilism need look no further than chapter 6 of Schubring [137], who comments on the enthusiasm for revising traditional beliefs in the history of science and reinterpreting the discipline from a theoretical, epistemological perspective generated by Thomas Kuhn’s (1962) work on the structure of scientific revolutions. Applying Popper’s favorite keyword of fallibilism, the statements of earlier scientists that historiography had declared to be false were particularly attractive objects for such an epistemologically guided revision.

The philosopher Imre Lakatos (1922-1972) was responsible for introducing these new approaches into the history of mathematics. One of the examples he analyzed and published in 1966 received a great deal of
attention: Cauchy’s theorem and the problem of uniform convergence. Lakatos refines Robinson’s approach by claiming that Cauchy’s theorem had also been correct at the time, because he had been working with infinitesimals [137, p. 431–432].

One might have expected that, having devoted so much space to the philosophical underpinnings of Lakatos’ interpretation of Cauchy’s sum theorem, Schubring would actually devote a thought or two to that interpretation itself. Instead, Schubring presents a misguided claim to the effect that Robinson acknowledged the incorrectness of the sum theorem. Schubring appears to feel that calling Lakatos a Popperian and a fallibilist is sufficient refutation in its own right. Similarly, Schubring dismisses Laugwitz’s reading of Cauchy as “solipsistic” [137, p. 434]; accuses them of interpreting Cauchy’s conceptions as some hermetic closure of a private mathematics [137, p. 435] [emphasis in the original—the authors]; as well as being “highly anomalous or isolated” [137, p. 441]. Now common sense would suggest that Laugwitz is interpreting Cauchy’s words according to their plain meaning, and takes his infinitesimals at face value. Isn’t the burden of proof on Schubring to explain why the triumvirate interpretation of Cauchy is not “solipsistic”, “hermetic”, or “anomalous”? Schubring does nothing of the sort.

Why are Lakatos and Laugwitz demonized rather than analyzed by Schubring? The issue of whether or not Schubring commands a minimum background necessary to understand either Robinson’s, Lakatos’, or Laugwitz’s interpretation was discussed above. More fundamentally, the act of contemplating for a moment the idea that Cauchy’s infinitesimals can be taken at face value is unthinkable to a triumvirate historian, as it would undermine the nominalistic Cauchy-Weierstrass tale.

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65 Schubring’s quote of Robinson’s reference to Cauchy’s “famous error” is taken out of context. Note that the full quote is “a famous error of Cauchy’s, which has been discussed repeatedly in the literature” (Robinson [131, p. 271]). Robinson devotes a lengthy paragraph on pages 260-261 to a statement of a received view of the history of the calculus. Robinson then proceeds to refute the received view. Similarly, the received, and famous, view (and one that “has been discussed repeatedly in the literature”) is that Cauchy erred in his treatment of the sum theorem. Robinson proceeds to challenge such a view, by offering an interpretation that vindicates Cauchy’s sum theorem, along the lines of Cauchy’s own modification/clarification of 1853 (Cauchy [36]). Robinson’s interpretation is consistent with our position that Cauchy’s 1821 result is irreducibly ambiguous, see also footnote 64. For a summary of the controversy over the sum theorem, see Section 12.
that the received historiography is erected upon. The failure to appreciate the potency of the Robinson-Lakatos-Laugwitz interpretation is symptomatic of an ostrich effect conditioned by a narrow A-continuum vision.\textsuperscript{66} The Robinson-Lakatos-Laugwitz interpretation of Cauchy’s sum theorem is considered in more detail in Section 12.

11. A CASE STUDY IN NOMINALISTIC HERMENEUTICS

Chapter 6 in Schubring \cite{137} is entitled “Cauchy’s compromise concept”. Which compromise is the author referring to? The answer becomes apparent on page 439, where the author quotes Cauchy’s “reconciliation” sentence:

\begin{quote}
My main aim has been to reconcile rigor, which I have made a law in my Cours d’Analyse, with the simplicity that comes from the direct consideration of infinitely small quantities (Cauchy 1823, see \cite{34, p. 10}) [emphasis added—authors].
\end{quote}

Cauchy’s choice of the word “reconcile” does suggest a resolution of a certain tension. What is the nature of such a tension? The sentence mentions “rigor” and “infinitely small quantities” in the same breath. This led Schubring to a conclusion of Cauchy’s alleged perception of a “disagreement” between them:

\begin{quote}
In his next textbook on differential calculus in 1823, Cauchy points out expressly that he has adopted a compromise concept and that the “simplicity of the infinitely small quantities” [...] disagrees with the “rigor” that he wished to achieve in his 1821 textbook \cite{137, p. 439}.
\end{quote}

Schubring’s conclusion concerning such an alleged “disagreement”, as well as the “compromise” of his title, both hinge essentially on a single word concilier (reconcile) in Cauchy. Let us analyze its meaning. If it refers to a disagreement between rigor and infinitesimals, how do we account for Cauchy’s attribution, in 1821, of a fundamental foundational role of infinitesimals in establishing a rigorous basis for analysis? Had Cauchy changed his mind sometime between 1821 and 1823?

To solve the riddle we must place Cauchy’s “reconciliation” sentence in the context where it occurs. In the sentence immediately preceding it, Cauchy speaks of his break with the earlier texts in analysis:

\begin{footnotesize}
\textsuperscript{66} Such an effect is comparable to those occurring in the constructivist context, see footnote \textsuperscript{59} and in the educational context, see footnote \textsuperscript{57}.
\end{footnotesize}
Les méthodes que j’ai suivies diffèrent à plusieurs égards de celles qui se trouvent exposées dans les ouvrages du même genre [34, p. 10].

Could he be referring to his own earlier text? To answer the question, we must read on what Cauchy has to say. Immediately following the “reconciliation” sentence, Cauchy unleashes a sustained attack against the flawed method of divergent power series. Cauchy does not name the culprit, but clearly identifies the offending treatise. It is the Mécanique Analytique, see (Cauchy 1823, p. 11). The second edition of Lagrange’s treatise came out in 1811, when Cauchy was barely out of his teens. Here Lagrange writes:

Lorsqu’on a bien conçu l’esprit de ce système, et qu’on s’est convaincu de l’exactitude de ses résultats par la méthode géométrique des premières et dernières raisons, ou par la méthode analytique des fonctions dérivées, on peut employer les infiniment petits comme un instrument sûr et commode pour abréger et simplifier les démonstrations [101, p. iv].

Lagrange’s ringing endorsement of infinitesimals in 1811 is as unambiguous as that of Johann Bernoulli, l’Hôpital, or Varignon. In rejecting Lagrange’s flawed method of power series, as well as his principle of the “generality of algebra”, Cauchy was surely faced with a dilemma with regard to Lagrange’s infinitesimals, which had stirred controversy for over a century. We argue that it is the context of a critical re-evaluation of Lagrange’s mathematics that created a tension for Cauchy vis-à-vis Lagrange’s work of 1811: can he sift the chaff from the grain?

Cauchy’s great accomplishment was his recognition that, while Lagrange’s flawed power series method and his principle of the generality of algebra do not measure up to the standard of rigor Cauchy sought to uphold in his own work, the infinitesimals can indeed be reconciled with such a standard of rigor. The resolution of the tension between the rejection of Lagrange’s conceptual framework, on the one hand, and the acceptance of his infinitesimals, on the other, is what Cauchy is referring to in his “reconciliation” sentence. Cauchy’s blending of rigor and infinitesimals in 1823 is consistent with his approach in 1821. Cauchy’s sentence compromises Schubring’s concept of a Cauchyan ambivalence with regard to infinitesimals, and pulls the rug from under Schubring’s nominalistic and solipsistic reading of Cauchy.
12. Cauchy’s sum theorem

In this section, we summarize the controversy over the sum theorem, recently analyzed by Bråting [26]. The issue hinges on two types of convergence. To clarify the mathematical issues involved, we will first consider the simpler distinction between continuity and uniform continuity. Let \( x \) be in the domain of a function \( f \), and consider the following condition, which we will call microcontinuity at \( x \):

“If \( x' \) is in the domain of \( f \) and \( x' \) is infinitely close to \( x \), then \( f(x') \) is infinitely close to \( f(x) \)”.

Then ordinary continuity of \( f \) is equivalent to \( f \) being microcontinuous on the Archimedean continuum (A-continuum for short), i.e., at every point \( x \) of its domain in the A-continuum. Meanwhile, uniform continuity of \( f \) is equivalent to \( f \) being microcontinuous on the Bernoulian continuum (B-continuum for short), i.e., at every point \( x \) of its domain in the B-continuum.

Thus, the function

\[
\frac{1}{\sin \frac{1}{x}}
\]

for positive \( x \) fails to be uniformly continuous because microcontinuity fails at a positive infinitesimal \( x \). The function \( x^2 \) fails to be uniformly continuous because of the failure of microcontinuity at a single infinite member of the B-continuum.

A similar distinction exists between pointwise convergence and uniform convergence. The latter condition requires convergence at the points of the B-continuum in addition to the points of the A-continuum, see e.g. Goldblatt [68, Theorem 7.12.2, p. 87].

Which condition did Cauchy have in mind in 1821? This is essentially the subject of the controversy over the sum theorem.

Abel interpreted it as convergence on the A-continuum, and presented “exceptions” (what we would call today counterexamples) in 1826. After the publication of additional such exceptions by Seidel and Stokes in the 1840s, Cauchy clarified/modified his position in 1853. In his text [36], he specified a stronger condition of convergence on the B-continuum, including at \( x = 1/n \). The latter entity is explicitly mentioned by Cauchy as illustrating the failure of the error term to tend to zero. The stronger condition bars Abel’s counterexample. See our text [86] for more details.

\[67\] The relation of the two continua is discussed in more detail in Section 13.
13. FERMAT, WALLIS, AND AN “AMAZINGLY RECKLESS” USE OF INFINITY

A Leibnizian definition of the derivative as the infinitesimal quotient

$$\frac{\Delta y}{\Delta x}$$

whose logical weakness was criticized by Berkeley, was modified by A. Robinson by exploiting a map called the standard part, denoted “st”, from the finite part of a B-continuum (for “Bernoullian”), to the A-continuum (for “Archimedean”), as illustrated in Figure 2. Here two points of a B-continuum have the same image under “st” if and only if they are equal up to an infinitesimal.

This section analyzes the historical seeds of Robinson’s theory, in the work of Fermat, Wallis, as well as Barrow. The key concept here is that of adequality (see below). It should be kept in mind that Fermat never considered the local slope of a curve. Therefore one has to be careful not to attribute to Fermat mathematical content that could not be there. On the other hand, Barrow did study curves and their slope. Furthermore, Barrow exploited Fermat’s adequality in his work [9, p. 252], as documented by H. Breger [27, p. 198].

The binary relation of “equality up to an infinitesimal” was anticipated in the work of Pierre de Fermat. Fermat used a term usually translated into English as “adequality” [74]. André Weil writes as follows:

68 In the context of the hyperreal extension of the real numbers, the map “st” sends each finite point $x$ to the real point $\text{st}(x) \in \mathbb{R}$ infinitely close to $x$. In other words, the map “st” collapses the cluster (halo) of points infinitely close to a real number $x$, back to $x$.

69 While Barrow’s role is also critical, we will mostly concentrate on Fermat and Wallis.

70 In French one uses adégalité, adégal, see [81, p. 73].
Fermat [...] developed a method which slowly but surely brought him very close to modern infinitesimal concepts. What he did was to write congruences between functions of \( x \) modulo suitable powers of \( x - x_0 \); for such congruences, he introduces the technical term *adaequitas, adaequare*, etc., which he says he has borrowed from Diophantus. As Diophantus V.11 shows, it means an approximate equality, and this is indeed how Fermat explains the word in one of his later writings [163, p. 1146].

Weil [163, p. 1146, footnote 5] then supplies the following quote from Fermat:

*Adaequetur, ut ait Diophantus* \(^{71}\) *aut fere aequetur*; in Mr. Mahoney’s translation: “adequal, or almost equal” (p. 246).

Here Weil is citing Mahoney [110, p. 246] (cf. [111, p. 247]). Mahoney similarly mentions the meaning of “approximate equality” or “equality in the limiting case” in [110, p. 164, end of footnote 46]. Mahoney also points out that the term “adequacy” in Fermat has additional meanings. The latter are emphasized in a recent text by E. Giusti [67], who is sharply critical of Breger [27]. While the review [163] by Weil is similarly sharply critical of Mahoney, both agree that the meaning of “approximate equality”, leading into infinitesimal calculus, is at least one of the meanings of the term *adequality* for Fermat.\(^ {72}\)

This meaning was aptly summarized by J. Stillwell. Stillwell’s historical presentation is somewhat simplified, and does not sufficiently distinguish between the seeds actually present in Fermat, on the one hand, and a modern interpretation thereof, on the other,\(^ {73}\) but he does a splendid job of explaining the mathematical background for the uninitiated. Thus, he notes that \( 2x + dx \) is not equal to \( 2x \) (see Figure [1]), and writes:

Instead, the two are connected by a looser notion than equality that Fermat called adequality. If we denote

\(^{71}\)The original term in Diophantus is \( \pi\alpha\rho\iota\sigma\delta\tau\gamma \), see Weil [164, p. 28].

\(^{72}\)Jensen similarly describes adequality as approximate equality, and describes neglected terms as *infinitesimals* in [81, p. 82]. Struik notes that “Fermat uses the term to denote what we call a limiting process” [150, p. 220, footnote 5].

\(^{73}\)K. Barner [8] compiled a useful bibliography on Fermat’s adequality, including many authors we have not mentioned here.

See main text around footnote [69] above for a discussion of Barrow’s role, documented by H Breger.
A BURGESSIAN CRITIQUE OF NOMINALISTIC TENDENCIES

Figure 3. Zooming in on Wallis’s infinitesimal $\frac{1}{\infty}$, which is adequal to 0 in Fermat’s terminology

adequality by $=_{ad}$, then it is accurate to say that

$$2x + dx =_{ad} 2x,$$

and hence that $dy/dx$ for the parabola is adequal to $2x$. Meanwhile, $2x + dx$ is not a number, so $2x$ is the only number to which $dy/dx$ is adequal. This is the true sense in which $dy/dx$ represents the slope of the curve [148, p. 91].

Stillwell points out that Fermat introduced the idea of adequality in 1630s but he was ahead of his time. His successors were unwilling to give up the convenience of ordinary equations, preferring to use equality loosely rather than to use adequality accurately. The idea of adequality was revived only in the twentieth century, in the so-called non-standard analysis [148, p. 91].

We will refer to the map from the (finite part of the) B-continuum to the A-continuum as the Fermat-Robinson standard part, see Figure 3.

As far as the logical criticism formulated by Rev. George is concerned, Fermat’s adequality had pre-emptively provided the seeds of an answer, a century before the bishop ever lifted up his pen to write The Analyst [13].

Fermat’s contemporary John Wallis, in a departure from Cavalieri’s focus on the geometry of indivisibles, emphasized the arithmetic of infinitesimals, see J. Stedall’s introduction in [162]. To Cavalieri, a plane figure is made of lines; to Wallis, it is made of parallelograms.
of infinitesimal altitude. Wallis transforms this insight into symbolic algebra over the $\infty$ symbol which he introduced. He exploits formulas like $\infty \times \frac{1}{\infty} = 1$ in his calculations of areas. Thus, in proposition 182 of *Arithmetica Infinitorum*, Wallis partitions a triangle of altitude $A$ and base $B$ into a precise number $\infty$ of “parallelograms” of infinitesimal width $\frac{A}{\infty}$, see Figure 4 (copied from [117, p. 170]).

He then computes the combined length of the bases of the parallelograms to be $\frac{B}{2} \infty$, and finds the area to be

$$A \infty \times \frac{B}{2} \infty = \frac{AB}{2}.$$  \hfill (13.1)

Wallis used an actual infinitesimal $\frac{1}{\infty}$ in calculations as if it were an ordinary number, anticipating Leibniz’s law of continuity.

Wallis’s area calculation (13.1) is reproduced by J. Scott, who notes that Wallis
treats infinity as though the ordinary rules of arithmetic could be applied to it [138, p. 20].

Such a treatment of infinity strikes Scott as something of a blemish, as he writes:

But this is perhaps understandable. For many years to come the greatest confusion regarding these terms persisted, and even in the next century they continued to be used in what appears to us an amazingly reckless fashion [138, p. 21].

What is the source of Scott’s confidence in dismissing Wallis’s use of infinity as “reckless”? Scott identifies it on the preceding page of his
book; it is, predictably, the triumvirate “modern conception of infinity” [138, p. 19]. Scott’s tunnel A-continuum vision blinds him to the potential of Wallis’s vision of infinity. But this is perhaps understandable. Many years separate Scott from Robinson’s theory which in particular empowers Wallis’s calculation. The lesson of Scott’s condescending steamrolling of Wallis’s infinitesimal calculation could be taken to heart by historians who until this day cling to a nominalistic belief that Robinson’s theory has little relevance to the history of mathematics in the 17th century.

14. Conclusion

Nominalism in the narrow sense defines its ontological target as the ordinary numbers. Burgess in his essay [30] suggests that there is also a nominalism in a broader sense. Thus, he quotes at length Manin’s criticism of constructivism, suggesting that LEM-elimination can also fall under the category of a nominalism understood in a broader sense. In a later text [31, p. 30], Burgess discusses Brouwer under a similar angle.

Infinitesimals were largely eliminated from mathematical discourse starting in the 1870s through the efforts of the great triumvirate. The elimination took place under the banner of striving for greater rigor, but the roots of the triumvirate reconstruction lay in a failure to provide a solid foundation for a B-continuum (see Section 13). Had actually useful mathematics been sacrificed on the altar of “mathematical rigor” during the second half of the 19th century?

Today we can give a precise sense to C.S. Peirce’s description of the real line as a pseudo-continuum.

Cantor’s revolutionary advances in set theory went hand-in-hand with his emotional opposition to infinitesimals as an “abomination”

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74 See footnote 3.
75 See Vicenti and Bloor [161] for an analysis of rigor in 19th century mathematics. See also our Section 7.
76 American philosopher Charles Sanders Peirce felt that a construction of a true continuum necessarily involves infinitesimals. He wrote as follows: “But I now define a pseudo-continuum as that which modern writers on the theory of functions call a continuum. But this is fully represented by […] the totality of real values, rational and irrational” [121] (see CP 6.176, 1903 marginal note. Here, and below, CP x.y stands for Collected Papers of Charles Sanders Peirce, volume x, paragraph y). Peirce used the word “pseudo-continua” to describe real numbers in the syllabus (CP 1.185) of his lectures on Topics of Logic. Thus, Peirce’s intuition of the continuum corresponded to a type of a B-continuum (see Section 13), whereas an A-continuum to him was a pseudo-continuum.
and the “cholera bacillus” of mathematics. Cantor’s interest in eliminating infinitesimals is paralleled nearly a century later by Bishop’s interest in eliminating LEM, and by a traditional nominalist’s interest in eliminating Platonic counting numbers. The automatic infinitesimal-to-limit translation as applied to Cauchy by Boyer and others is not only reductionist, but also self-contradictory, see [86].

15. RIVAL CONTINUA

This section summarizes a 20th century implementation of the B-continuum, not to be confused with incipient notions of such a continuum found in earlier centuries. An alternative implementation has been pursued by Lawvere, John L. Bell [11, 12], and others.

We illustrate the construction by means of an infinite-resolution microscope in Figure 3. We will denote such a B-continuum by the new symbol $\mathbb{I}$ (“thick-R”). Such a continuum is constructed in formula (15.4). We will also denote its finite part, by $\mathbb{I}_< = \{x \in \mathbb{I} : |x| < \infty\}$.

so that we have a disjoint union

$$\mathbb{I} = \mathbb{I}_< \cup \mathbb{I}_\infty,$$

where $\mathbb{I}_\infty$ consists of unlimited hyperreals (i.e., inverses of nonzero infinitesimals).

The map “st” sends each finite point $x \in \mathbb{I}$, to the real point $st(x) \in \mathbb{R}$ infinitely close to $x$, as follows:

$$\begin{array}{c}
\mathbb{I}_< \\
st \\
\mathbb{R}
\end{array}$$

Robinson’s answer to Berkeley’s logical criticism (see D. Sherry [143]) is to define the derivative as

$$st\left(\frac{\Delta y}{\Delta x}\right),$$

instead of $\Delta y/\Delta x$.

Note that both the term “hyper-real field”, and an ultrapower construction thereof, are due to E. Hewitt in 1948, see [76, p. 74]. In 1966, Robinson referred to the

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77This is the Fermat-Robinson standard part whose seeds in Fermat’s adequality were discussed in Appendix [13].
Figure 5. An intermediate field $\mathbb{Q}^N / \mathcal{F}_u$ is built directly out of $\mathbb{Q}$

theory of hyperreal fields (Hewitt [1948]) which ... can serve as non-standard models of analysis [131, p. 278].

The transfer principle is a precise implementation of Leibniz’s heuristic law of continuity: “what succeeds for the finite numbers succeeds also for the infinite numbers and vice versa”, see [131, p. 266]. The transfer principle, allowing an extension of every first-order real statement to the hyperreals, is a consequence of the theorem of J. Łoś in 1955, see [106], and can therefore be referred to as a Leibniz-Łoś transfer principle. A Hewitt-Łoś framework allows one to work in a B-continuum satisfying the transfer principle. To elaborate on the ultrapower construction of the hyperreals, let $\mathbb{Q}^N$ denote the ring of sequences of rational numbers. Let

$$\left(\mathbb{Q}^N\right)_C$$

denote the subspace consisting of Cauchy sequences. The reals are by definition the quotient field

$$\mathbb{R} := \left(\mathbb{Q}^N\right)_C / \mathcal{F}_{null},$$

where $\mathcal{F}_{null}$ contains all null sequences. Meanwhile, an infinitesimal-enriched field extension of $\mathbb{Q}$ may be obtained by forming the quotient

$$\mathbb{Q}^N / \mathcal{F}_u.$$  

Here a sequence $\langle u_n : n \in \mathbb{N} \rangle$ is in $\mathcal{F}_u$ if and only if the set of indices

$$\{n \in \mathbb{N} : u_n = 0\}$$

is a member of a fixed ultrafilter. See Figure 5.

To give an example, the sequence

$$\langle (-1)^n/n \rangle$$

In this construction, every null sequence defines an infinitesimal, but the converse is not necessarily true. Modulo suitable foundational material, one can ensure that every infinitesimal is represented by a null sequence; an appropriate ultrafilter (called a P-point) will exist if one assumes the continuum hypothesis, or even the weaker Martin’s axiom. See Cutland et al [39] for details.
represents a nonzero infinitesimal, whose sign depends on whether or not the set \(2\mathbb{N}\) is a member of the ultrafilter. To obtain a full hyperreal field, we replace \(\mathbb{Q}\) by \(\mathbb{R}\) in the construction, and form a similar quotient

\[
\mathbb{R} := \mathbb{R}^N / \mathcal{F}_u.
\]  

(15.4)

We wish to emphasize the analogy with formula (15.2) defining the A-continuum. Note that, while the leftmost vertical arrow in Figure 5 is surjective, we have

\[
(\mathbb{Q}^N / \mathcal{F}_u) \cap \mathbb{R} = \mathbb{Q}.
\]

A more detailed discussion of this construction can be found in the book by M. Davis [45]. See also Blasszyk [21] for some philosophical implications. More advanced properties of the hyperreals such as saturation were proved later, see Keisler [94] for a historical outline. A helpful “semicolon” notation for presenting an extended decimal expansion of a hyperreal was described by A. H. Lightstone [105]. See also P. Roquette [134] for infinitesimal reminiscences. A discussion of infinitesimal optics is in K. Stroyan [149], J. Keisler [93], D. Tall [154], and L. Magnani and R. Dossena [109, 48], and Bair & Henry [7].

Applications of the B-continuum range from aid in teaching calculus [54, 84, 85, 155, 156] (see illustration in Figure 1) to the Boltzmann equation (see L. Arkeryd [4, 5]), modeling of timed systems in computer science (see H. Rust [135]); mathematical economics (see Anderson [3]); mathematical physics (see Albeverio et al. [1]); etc.

ACKNOWLEDGMENTS

We are grateful to Martin Davis, Solomon Feferman, Reuben Hersh, David Sherry, and Steve Shnider for invaluable comments that helped improve the manuscript. Hilton Kramer’s influence is obvious throughout.

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