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Mikhail Katz
*Bar-Ilan University*



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# Mathematical Conquerors, Unguru Polarity, and the Task of History

Mikhail G. Katz

*Department of Mathematics, Bar Ilan University*
katzmik@macs.biu.ac.il

## Synopsis

I compare several approaches to the history of mathematics recently proposed by Blåsjö, Fraser–Schroter, Fried, and others. I argue that tools from both mathematics and history are essential for a meaningful history of the discipline.

In an extension of the Unguru–Weil controversy over the concept of *geometric algebra*, Michael Fried presents a case against both André Weil the "privileged observer" and Pierre de Fermat the "mathematical conqueror." Here I analyze Fried's version of Unguru's alleged polarity between a historian's and a mathematician's history. I identify some axioms of Friedian historiographic ideology, and propose a thought experiment to gauge its pertinence.

Unguru and his disciples Corry, Fried, and Rowe have described Freudenthal, van der Waerden, and Weil as Platonists but provided no evidence; here I provide evidence to the contrary. I also analyze how the various historiographic approaches play themselves out in the study of the pioneers of mathematical analysis including Fermat, Leibniz, Euler, and Cauchy.

## 1. Introduction

The recent literature features several approaches to the history of mathematics. Michael N. Fried [47] and Guicciardini [54] argue for versions of Unguru's approach (described below). Blåsjö [14] advocates a rational history as opposed to an "idiosyncraticist" one. Fraser and Schroter propose something of a middle course that defines the task of the history of mathematics as "our attempt to explain why a certain mathematical development happened" [39, page 16].[1] For a different perspective see [9]. In this article I analyze the perception of mathematical historiography that posits a polarity between a historical and a mathematical view. Such a perception is often associated with Sabetai Unguru. Against such Unguru polarity, I argue that tools from both disciplines are both useful and essential.

### *1.1. Unguru, Weil, van der Waerden, Freudenthal*

Sabetai Unguru [84] and André Weil [93] famously battled one another over the relation between Greek mathematics and the concept of *geometric algebra*, a term introduced by H. G. Zeuthen in 1885 (see Blåsjö [15, page 326], Høyrup [58, pages 4–6]).

B. L. van der Waerden [90] and Hans Freudenthal [45] published responses to Unguru earlier than Weil. A clarification is in order concerning the meaning of the term *geometric algebra*. Van der Waerden explained the term as follows:

> We studied the wording of [Euclid's] theorems and tried to reconstruct the original ideas of the author. We found it *evident* that these theorems did not arise out of geometrical problems. We were not able to find any interesting geometrical problem that would give rise to theorems like II 1–4. On the other hand, we found that the explanation of these theorems as arising from algebra worked well. Therefore we adopted the latter explanation. Now it turns out ... that what we, working mathematicians, found evident, is not evident to Unguru. Therefore I shall state more clearly the reasons why I feel that theorems like Euclid II 1–4 did not arise from geometrical considerations. [90, pages 203–204] (emphasis in the original)

Further details on van der Waerden's approach can be found in Section 2.5.

I refrain from taking a position in the debate on the narrow issue of geometric algebra as applied to Greek mathematics, but will simply point out that the debate has stimulated the articulation of various approaches to the history of mathematics. In this article I will analyze how the various approaches play themselves out in the study of the pioneers of mathematical analysis including Fermat, Leibniz, Euler, and Cauchy; see Section 3. On whether other historians endorse Unguru polarity, see Section 5.

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[1] I illustrate the latter approach in Section 3.6, in the context of certain developments in mathematical analysis from Euler to Cauchy, following Fraser–Schroter [40].

### 1.2. Returning from escapades

Readers familiar with the tenor of the Unguru–Weil controversy will not have been surprised by the tone of the "break-in" remark found in the 2001 book by Fried and Unguru ([48], henceforth FU):

> The mathematical and the historical approaches are antagonistic. Whoever breaks and enters typically returns from his *escapades* with other spoils than the peaceful and courteous caller. (Fried–Unguru [48, page 406]; emphasis on *escapades* added)

For readers less familiar with the controversy, it may be prudent to clarify that the unpeaceful and uncourteous caller allegedly involved in the break-in is, to be sure, the mathematician, not the historian.

The break-in remark is duly reproduced in a recent essay in the *Journal of Humanistic Mathematics* by Unguru's disciple Fried [47, page 7]. And yes, the "escapades" are in the original, both in FU and in Fried solo.

That Fried's mentor Unguru does not mince words with regard to Weil is not difficult to ascertain. Thus, one finds the following phrasing: "Betrayals, Indignities, and Steamroller Historiography: André Weil and Euclid" (Unguru [87, page 26, end of Section I]).

Clearly, neither unpeaceful break-ins nor uncourteous escapades represent *legitimate* relationships to the mathematics of the past. The break-in remark makes the reader wonder about the precise meaning of Fried's assurance that his plan is to catalog some of the attitudes toward the history of mathematics "*without judgment* as to whether they are necessarily correct or legitimate" (Fried [47, abstract, page 3]; emphasis added).

### 1.3. Who is open-minded?

A further telling comment appears on the back cover of the FU book on Apollonius of Perga:

> Although this volume is intended primarily for historians of ancient mathematics, its approach is fresh and engaging enough to be of interest also to historians, philosophers, linguists, and *open-minded mathematicians.* (Fried–Unguru [48]; emphasis added)

Readers will not fail to notice that, of the four classes of scholars mentioned, the mathematician is the only class limited by the qualifier *open-minded*;

here FU don't appear to imply that a mathematician is typically characterized by the qualifier. Such a polarizing approach (see further in Section 4.2) on the part of FU is hardly consistent with Fried's professed idea of cataloguing attitudes toward history "without judgment as to whether they are ... correct or legitimate." I analyze the ideological underpinnings of the FU approach in Section 2.

## 2. FU axiomatics: Discontinuity, *tabula rasa*, antiplatonism

In this section, I identify several axioms of historiography according to the Unguru school. In the Friedian scheme of things, it is axiomatic that the proper view of the relation between the mathematics of the past and that of the present is that of a *dis*continuity. It is indeed possible to argue that it is (see Section 3.5 for an example in Fermat). However, Fried appears to take it for granted that a contrary view of *continuity* between past and present necessarily amounts to whig history or, more politely, engaging in what Oakeshott described as a "practical past" and Grattan-Guinness [53] described as "heritage" (see Fried [47, page 7]). Fried's attitude here is at odds with the idea of historiography as seeking to "explain why a certain mathematical development happened" (Fraser–Schroter, [39, page 16]).

### *2.1. Axiom 1: Discontinuity*

What emerges is the following axiom of Friedian historiographic ideology:

> **Axiom 1 (discontinuity).** The proper attitude of a historian toward the mathematics of the past is that of a discontinuity with the mathematics of the present.

We note that, while the discontinuity view may be appropriate in certain cases, it is an assumption that needs to be argued rather than posited as an axiom as in Fried.

Fried presents a taxonomy of various attitudes toward the history of mathematics. He makes it clear that he means to apply his taxonomy rather broadly, and not merely to the historical work on Greek mathematics:

> [I]n most of the examples the mathematical past being considered by one person or another is that of Greece. ... Nevertheless, as I hope will be clear, the relationships evoked in the context of these

> examples have little to do with the particular character of Greek mathematics. [47, page 8]

Let us examine the effectiveness of the FU approach in such a broader context.

### *2.2. Apollonius and* mathematical conquerors

Fried notes that his piece "has been written in a light and playful spirit" (ibid., page 8). Such a light(-headed) spirit is reflected in Fried's attitude toward Fermat. Fermat's reconstruction of Apollonius' *Plane Loci* prompts Fried to place Fermat in a category labeled "mathematical conqueror" rather than that of a historian. The label also covers Descartes and Viète, whose "sense of the past has the unambiguous character of a 'practical past', again to use Oakeshott's term" [47, pages 11–12]. Fried does not spare the great magistrate the tedium of Mahoney's breezy journalese:

> "Fermat was no antiquarian interested in a faithful reproduction of Apollonius' original work; ... The Plane Loci was to serve as a means to an end rather than an end in itself." (Mahoney as quoted by Fried in [47, page 11])

Fried appears to endorse Mahoney's dismissive attitude toward Fermat's historical work. Regrettably, Fried ignores Recasens' more balanced evaluation of Fermat's work on Apollonius' *Plane Loci*, emphasizing its classical geometric style, and contrasting it with van Schooten's:

> Fermat's demonstration of Locus II-5 is presented in the classical geometrical style of the day, though his conception was already algebraic; [van] Schooten's is a pure exercise of analytic geometry. (Recasens [76, page 315])

What is the source of Fried's facile dismissal of Fermat's historical scholarship? While it is difficult to be certain, a clue is found in the attitude of his mentor Unguru who wrote:

> [Fermat] took the Greek problems away from their indigenous territory into new and foreign lands. Interestingly (and again, I think, quite typically), Fermat did not see in his novel and revolutionary methods strategies intrinsically alien to Greek mathematics, thus contributing to the creation of the pervading and pernicious myth

> that there are not indeed any substantive differences between the geometrical works of the Greeks and the algebraic treatment of Greek mathematics by post-Viètan mathematicians. (Unguru [85, page 775])

Unguru describes Fermat's reading of Apollonius as contributing to a "pernicious myth," and Fermat's reliance on Vieta's theory of equations as "alien" to Greek mathematics; Fried apparently follows suit.

By page 12, Fried takes on a group he labels "mathematician-historians" whose fault is their interest in historical continuity:

> The mathematics of the past is still understood by them as continuous with present mathematics. (Fried [47, page 12])

Again, adherence to the continuity view is cast without argument as a fault (see Section 2.1). Yet positing discontinuity as a working hypothesis can make scholars myopic to important aspects of the historical development of mathematics, and have a chilling effect on attempts to explain why certain mathematical developments happened (the Fraser–Schroter definition of the task of a historian); see Section 3 for some examples.

### *2.3. Axiom 2: Tabula rasa*

Unguru's opposite number Weil makes a predictable appearance in [47] on page 14, under the label *privileged observer*. The label also covers Zeuthen and van der Waerden. The fault for this particular label is the desire to take advantage of "their mathematical ideas ... to piece together the past" (ibid.). Fried's posture on Weil brings us to the next axiom of Friedian historiographic ideology:

> **Axiom 2 (tabula rasa).** It is both possible and proper for historians to refrain from using modern mathematical ideas.

By page 16 we learn what authentic historians of mathematics do: they

> take as their working assumption, a kind of *null-hypothesis*, that there is a discontinuity between mathematical thought of the past and that of the present. [47, page16] (emphasis added)

This formulation of Fried's discontinuity axiom (see Section 2.1) has the advantage of being explicitly cast as a *hypothesis*. Yet nothing about Fried's tone here suggests any intention of actually exploring the *validity* of such a hypothesis. A related objection to Fried's posture was raised by Blåsjö and Hogendijk [17, page 775], who argue that ancient treatises may contain meanings and intentions that go beyond the surface text, based on a study of Ptolemy's *Almagest*. I will analyze Fried's Axiom 2 in Section 3.

### *2.4. Axiom 3: Uprooting the Platonist deviation*

The following additional axiom is discernible in the writings of Unguru and his students.

> **Axiom 3 (Mathematicians as Platonists)** Mathematicians interested in history are predominantly Platonists; furthermore, their beliefs (for example, that mathematics is eternally true and unchanging) interfere with their functioning as competent historians.

Mathematicians interested in history are repeatedly described as Platonists (see Figure 1) in the writings of Fried, Rowe, and Unguru. Thus, one finds the following comments (emphasis on "Platonic" added throughout):

1. "[M]athematically minded historians... assume tacitly or explicitly that mathematical entities reside in the world of *Platonic* ideas where they wait patiently to be discovered by the genius of the working mathematician" (Unguru–Rowe [88, page 3], quoting [86]). Unguru and Rowe go on at length (pages 5 through 10) to attack van der Waerden's interpretation of cuneiform tablet BM 13901.[2]

2. "It has been argued that most contemporary historians of mathematics are *Platonists* in their approach. They look in the past of mathematics for the eternally true, the unchanging, the constant" (Unguru–Rowe [89, page 47]). The problem with such an approach is diagnosed as follows: "If nothing changes there is no history" [88, page 48].

3. "[T]he methodology embodied in 'geometric algebra' ... is the outgrowth of a *Platonic* metaphysics that sees mathematical ideas as disembodied beings, pure and untainted by any idiosyncratic features" (Fried–Unguru [48, page 37]).

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[2] For a rebuttal of the Unguru–Rowe critique see Blåsjö [15, Section 3.4].

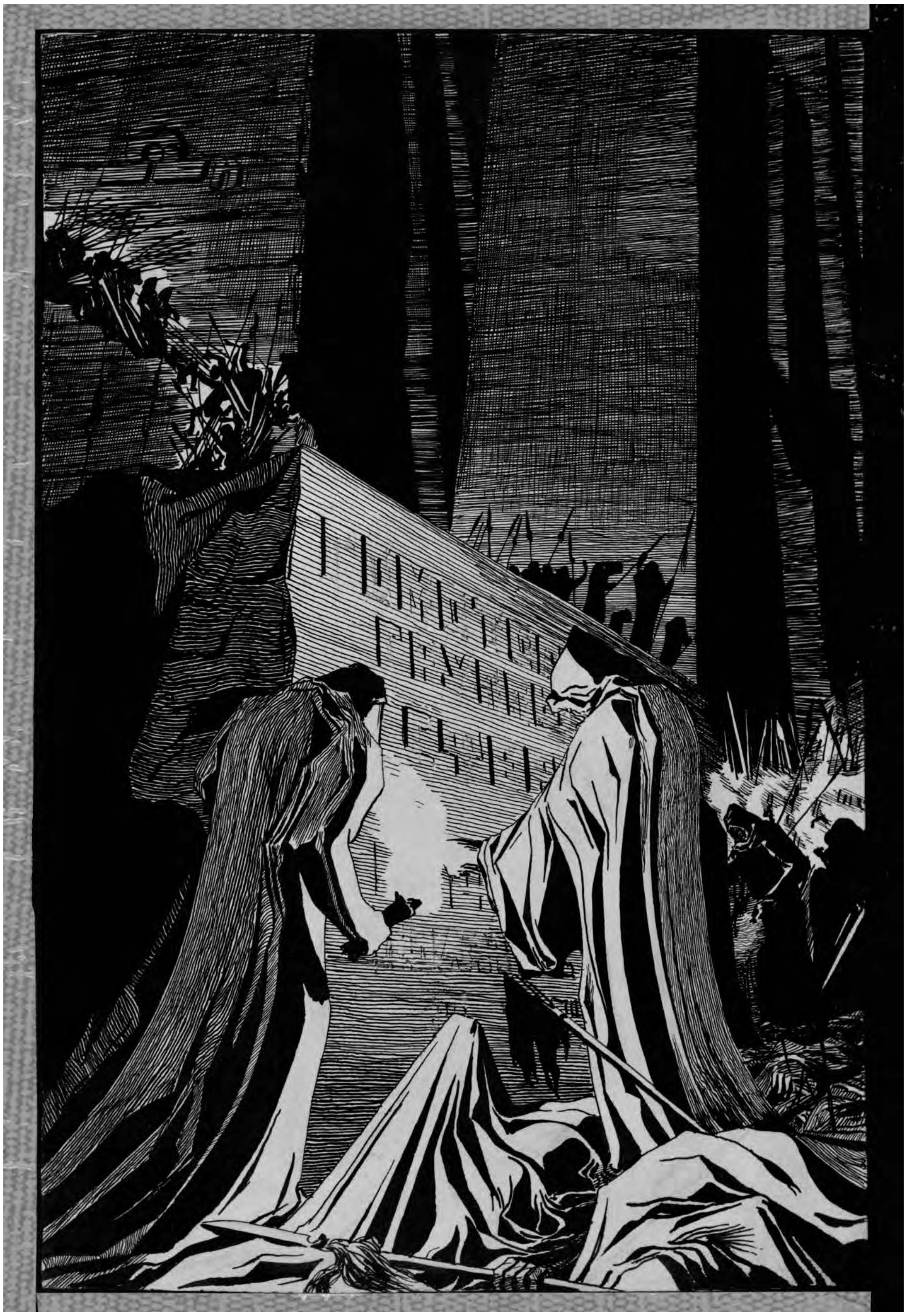

Figure 1: Mathematical Platonism. A humorous illustration from the book *Homotopic Topology*. The writing on the wall reads (in Russian): "Homotopy groups of spheres." Created by Professor A. T. Fomenko, academician, Moscow State University. Reproduced with permission of the author.

4. "That Apollonius was a skilled geometrical algebraist is clearly the considerate opinion of Zeuthen. It is an opinion based *exclusively* on a *Platonic* philosophy of mathematics, according to which one and the same mathematical idea remains the same irrespective of its specific manifestations" (Fried–Unguru [48, pages 47–48]; emphasis added).

5. "There is one mathematics, from its pre-historical beginnings to the end of time, irrespective of its changing appearances over the centuries. This mathematics grows by accumulation and by a sharpening of its standards of rigor, while, its rational, ideal, *Platonic* Kernel, remaining unaffected by the historical changes mathematics undergoes, enjoys, as Hardy put it, immortality. In short, proven mathematical claims remain proven forever, no matter what the changes are that mathematics is undergoing. And since it is always possible to present past mathematics in modern garb, ancient mathematical accomplishments can be easily made to look modern and, therewith seamlessly integrated into the growing body of mathematical knowledge" (Unguru [87, pages 19-20]).

6. "The *Platonic* outlook embodied in Weil's statements, according to which (1) mathematical entities reside in the world of *Platonic* ideas and (2) mathematical equivalence is tantamount to historical equivalence, is inimical to history" (Unguru [87, page 29]).

What is comical about this string of attempts to pin a *Platonist* label on scholars is the contrast between the extreme care Unguru and his students advocate in working with primary documents and sourcing every historical claim, on the one hand, and the absence of such sources when it comes to criticizing scholars they disagree with, on the other.

This is not to say that mathematicians interested in history are never Platonists. Thus, in a recent volume by Dani–Papadopoulos, one learns that

> [T]hinkers in colonial towns in Asia Minor, Magna Graecia, and mainland Greece, cultivated a love for systematizing phenomena on a rational basis ... They appreciated purity, universality, a certainty and an elegance of mathematics, the characteristics that all other forms of knowledge do not possess. [34, page 216]

While such attitudes do exist, it remains that claiming your opponents are Platonists without providing evidence is no more convincing than claiming that Greek geometry had an algebraic foundation without providing evidence, a fault Unguru and others impute to their opponents. Without engaging in wild-eyed accusations of Platonism against scholars he disagreed with, Grattan-Guinness [52] was able to enunciate a dignified objection to geometric algebra (for a response see Blåsjö [15, Section 3.10]).

With regard to Axiom 3, it is worth noting that the broader the spectrum of the culprits named by Unguru, the less credible his charge of Platonism becomes. Consider, for example, the claim above that Zeuthen's opinion is "based *exclusively* on a Platonic philosophy of mathematics" (emphasis added). Unguru attacks Heiberg in [84, page 107] with similar vehemence. But how credible would be a claim that the historiographic philosophy of the *philologist* Johan Ludvig Heiberg (of the *Archimedes Palimpsest* fame) is due to mathematical Platonist beliefs, especially if no evidence is provided?

### *2.5. Corry's universals*

Axiomatizing tendencies similar to those of Fried, Rowe, and Unguru manifest themselves in the writing by Unguru's student Corry, as well.

Engagement with Platonism and its discontents appears to be a constant preoccupation in Corry's work. He alludes to Platonism by using terms as varied as "eternal truth," "essence of algebra," and "universal properties." Thus, in 1997 he writes:

> [Bourbaki] were extending in an unprecedented way the domain of validity of the belief in the eternal character of mathematical truths, from the body to the images of mathematical knowledge. (Corry [30, page 253])

In 2004 (originally published in 1996) he writes:

> ... a common difficulty that has been manifest ... is the attempt to define, by either of the sides involved, the "essence" of algebraic thinking throughout history. Such an attempt appears, from the perspective offered by the views advanced throughout the present book, as misconceived. (Corry [31, page 396])

In 2013 we find:

> ... the question about the "essence of algebra" as an ahistorical category seems to me an ill-posed and uninteresting one. (Corry [33, page 639])

In 2007, Corry imputes to mathematicians a quest for "universal properties" at the expense of historical authenticity. He appears to endorse Unguru's view of mathematicians as Platonists when he writes:

> [I]n analyzing mathematics of the past mathematicians often look for underlying mathematical concepts, regularities or affinities in order to conclude about historical connection. Mathematical affinity necessarily follows from *universal properties* of the entities involved and this has often been taken to suggest a certain historical scenario that 'might be'. But, Unguru warns us, one should be very careful not to allow such mathematical arguments led [sic] us to mistake historical truth (i.e., the 'thing that *has* been') with what is no more than mathematically possible scenarios (i.e., the 'thing that *might* be'). The former can only be found by direct *historical evidence*. (Corry [32]; emphasis on "has" and "might" in the original; emphasis on "universal properties" and "historical evidence" added)

Granted one needs direct historical evidence, as per Corry. However, where is the evidence that instead of looking for evidence, mathematicians interested in history look for *universal properties*? Such a view of mathematicians who are historians is postulated axiomatically by Corry, similarly to Fried, Rowe, and Unguru. Corry goes on to claim that

> [Unguru's 1975] work immediately attracted *furious* reactions, above all from three prominent mathematicians interested in the history of mathematics: André Weil, Bartel L. van der Waerden, and Hans Freudenthal. ([32]; emphasis added)

Corry's remark is specifically characterized by the attitude of looking for "the thing that might be" rather than the "thing that has been", a distinction he mentions in the passage quoted above. Namely, once Corry postulates a *universal*-seeking attitude on the part of Weil, van der Waerden, and Freudental, it then naturally follows, for Corry, that they would necessarily react "furiously" to Unguru's work. Corry does present evidence of presentist attitudes in historiography in connection with interpreting the Pythagorean discovery of the incommensurability of the diagonal and the side of the square. However, Corry presents evidence of such attitudes not in the writings of Weil, van der Waerden, or Freudental, but rather those of... Carl Boyer [22, page

80]).[3] For a discussion of the shortcomings of Boyer's historiographic approach see Section 3.6. Note that Weil is just as sceptical as Corry about claims being made on behalf of the Pythagoreans; see (Weil [94, pages 5, 8]). I will analyze Corry's problematic criticisms of van der Waerden, Freudenthal, and Weil respectively in Sections 2.6, 2.7, and 2.8.

### *2.6. van der Waerden on Diophantus and Arabic algebra*

Contrary to the claims emanating from the Unguru school, some of Unguru's opponents specifically denied being Platonist. Thus, van der Waerden wrote:

> I am simply not a Platonist. For me mathematics is not a contemplation of essences but intellectual construction. (van der Waerden as translated in Schappacher [78, page 245])

It is instructive to contrast Unguru's attitude toward van der Waerden with Szabó's. Szabó's book [82] deals with van der Waerden at length, but there is no trace there of any allegation of Platonist deviation. On the contrary, Szabó's book relies on van der Waerden's historical scholarship, as noted also by Folkerts [42]. The book does criticize van der Waerden for what Szabó claims to be an over-reliance on translations. Szabó discusses this issue in detail in the context of an analysis of the meaning of the Greek term $\delta\acute{\upsilon}\nu\alpha\mu\iota\varsigma$ (*dynamis*),[4] as mentioned by Folkerts.

In reality, van der Waerden's 1976 article contains no sign of the "fury" claimed by Corry (see Section 2.5). Perhaps the most agitated passage there is van der Waerden's rebuttal of a spurious claim by Unguru (which is echoed thirty years later by Corry in [32]):

> We (Zeuthen and his followers) feel that the Greeks started with algebraic problems and translated them into geometric language.

[3] In [32], Corry criticizes attempts to deduce a purely historical claim merely from "underlying mathematical affinity." Corry provides the following example: "It is thus inferred that the Pythagoreans proved the incommensurability of the diagonal of a square with its side exactly as we nowadays prove that $\sqrt{2}$ is an irrational number." Corry's example is followed by a reference to (Boyer [22, page 80]). On that page, Boyer wrote: "A third explanation [of the expulsion of Hippasus from the Pythagorean brotherhood] holds that the expulsion was coupled with the disclosure of a mathematical discovery of devastating significance for Pythagorean philosophy—the existence of incommensurable magnitudes."

[4] At the risk of committing precisely the type of inaccuracy criticized by Szabó, one could translate *dynamis* roughly as "the squaring operation".

> Unguru thinks that we argued like this: We found that the theorems of Euclid II can be translated into modern algebraic formalism, and that they are easier to understand if thus translated, and this we took as 'the proof that this is what the ancient mathematician had in mind'. Of course, this is nonsense. We are not so weak in logical thinking! The fact that a theorem can be translated into another notation does not prove a thing about what the author of the theorem had in mind. (van der Waerden [90, page 203])

What one *does* find in van der Waerden's article is a specific rebuttal of Corry's claim concerning an alleged search for *universal properties* (see Section 2.5). Here van der Waerden is responding to Unguru, who claims that algebraic thinking involves

> Freedom from any ontological questions and commitments and, connected with this, abstractness rather than intuitiveness. (Unguru [84, page 77])

Van der Waerden responds by rejecting Unguru's characterisation of the algebra involved in his work on Greek mathematics, and points out that what he is referring to is

> algebra in the sense of Al-Khwārizmī, or in the sense of Cardano's 'Ars magna', or in the sense of our school algebra. (van der Waerden [90, page 199])

Thus, van der Waerden specifically endorses a similarity between Arabic premodern algebra and Greek mathematics, and analyzed Diophantus specifically in [90, page 210]. A similarity between Arabic premodern algebra and the work of Diophantus is also emphasized by Christianidis [26]. The continuation of the passage from van der Waerden is more problematic from the point of view of [26]. Here he writes:

> Algebra, then, is: the art of handling algebraic expressions like $(a+b)^2$ and of solving equations like $x^2 + ax = b$. (van der Waerden [90, page 199])

The viewpoint expressed here is at odds with the emphasis in [26] on the fact that premodern algebra did not deal with equations, polynomials were not sums but rather aggregates, and the operations stipulated in the problem were performed before the statement of the equation. However, apart from these important points, van der Waerden's notion of Greek mathematics as close to Arabic premodern algebra is kindred to the viewpoint elaborated in [26] and [27].[5]

The article [26] elaborates a distinction between *modern algebra* and *premodern algebra*. The latter term covers both Arabic sources and Diophantus. The position presented in [26] is clearly at odds with Unguru, who wrote:

> With Viète algebra becomes the very language of mathematics; in Diophantus' *Arithmetica*, on the other hand, we possess merely a refined auxiliary tool for the solution of arithmetical problems … (Unguru [84, page 111, note 138])

Regardless of how close the positions of van der Waerden and [26] can be considered to be, there is no mention of *universal properties* in van der Waerden's work on Greek mathematics. The *universals* appear to be all Corry's, not van der Waerden's.

### *2.7. Corry's shift on Freudenthal*

Similarly instructive is Corry's—I argue—variable position on Freudenthal in connection with Platonism and Bourbaki. The standard story on Bourbaki is the one of mathematical Formalism and *structures*. In Corry's view, there is some question concerning how different this is, in Bourbaki's case, from Platonism. Here is what Corry wrote in his book in 1996:

> The above-described mixture [in Bourbaki] of a declared formalist philosophy with a heavy dose of *actual Platonic belief* is illuminating in this regard. The formalist imperative, derived from that ambiguous position, provides the necessary background against which Bourbaki's drive to define the formal concept of *structure* and to develop some immediate results connected with it can be conceived. The *Platonic* stand, on the other hand, which reflects

[5] It would be more difficult to bridge the gap between the positions of Weil and [26], since Weil claims that "there is much, in Diophantus and in Viète's *Zetetica*, which in our view pertains to algebraic geometry" [94, page 25], whereas [26] specifically distances itself from attempts to interpret Diophantus in terms of algebraic geometry.

> Bourbaki's true working habits and beliefs, has led the very members of the group to consider this kind of conventional, formal effort as superfluous. (Corry [29, page 311]; emphasis on "structure" in the original; emphasis on "actual Platonic belief" and "Platonic" added)

On page 336 in the same book, Corry quotes Freudenthal's biting criticism of Piaget's reliance on Bourbaki and their concept of structure as an organizing principle:

> The most spectacular example of organizing mathematics is, of course, Bourbaki. How convincing this organization of mathematics is! So convincing that Piaget could rediscover Bourbaki's system in developmental psychology. ... Piaget is not a mathematician, so he could not know how unreliable mathematical system builders are. (Freudenthal as quoted in Corry [29, page 336]).

The same passage is quoted in the earlier article (Corry [28, page 341]).

The index in Corry's book on Bourbaki contains an ample supply of entries containing the term *universal*, including *universal constructs* and *universal problems*, a constant preoccupation of Bourbaki's which can also be seen as a function of their Platonist background philosophy in the sense Corry outlined in [29, page 311], where Corry speaks of Bourbaki's "actual Platonic belief."

There is a clear contrast, in Corry's mind, between Platonism, universals, Bourbaki, and Piaget, on one side of the debate, and Freudenthal with his clear opposition to both Bourbaki and Piaget, on the other. Freudenthal's opposition tends to undercut the idea of Freudenthal as Platonist, which in any case is at odds with Freudenthal's pragmatic position on mathematics education; see for example La Bastide [63].

On the other hand, Corry's article [32] includes Freudenthal on the list of the Platonist mathematical culprits (van der Waerden, Freudenthal, Weil) that has been made standard by Unguru. According to Corry [32], these scholars are in search of *universal properties*; see Section 2.5. This fits with Unguru's take on Freudenthal, but is at odds with what Corry himself wrote about Freudenthal a decade earlier, as documented above.

In fact, Freudenthal specifically sought to distance himself from Platonism in [46, page 7]. Freudenthal's interest in Intuitionism is discussed in [63, page 42]. He published at least two papers in the area: [43] and [44]. This interest similarly points away from Platonism, contrary to Corry's claim.

*2.8. Weil: internalist or externalist?*

Corry attacks both Weil and Bourbaki as Platonist, and dismisses Bourbaki's volume on the history of mathematics [20] as "royal-road-to-me" historiography [32]. Paumier and Aubin make a more specific claim against the Bourbaki volume generally and Weil's historiography in particular. Namely, they refer to the volume as "internalist history of concepts" [75, page 185], and imply that the same criticism applies to Weil's historiography, as well. In a related vein, Kutrovátz casts Unguru and Szabó as externalists and Weil as internalist and Platonist in [62].

To evaluate such criticisms of Weil, the Bourbaki volume is of limited utility since it was of joint authorship. Let us examine instead Weil's own book *Number Theory. An Approach Through History. From Hammurapi to Legendre* [94]. Does the *internalist* criticism apply here?

To answer the question, we would need to agree first on the meaning of *internalist* and *externalist*. If we posit that historical work is *externalist* if it is written by Unguru, his disciples, and their cronies, then there is little hope for Weil. There is perhaps hope with a less partisan definition, such as "historiography that takes into account the contingent details of the historical period and its social context, etc." It is clear that historical and social factors are important. For instance, one obtains a distorted picture of the mathematics of Gregory, Fermat, and Leibniz if one disregards the fierce religious debates of the 17th century (see references listed in Section 3.1).

Now it so happens that Weil's book [94] does contain detailed discussions of the historical context. Weil's book is not without its shortcomings. For instance, when Weil mentions that Bachet "extracted from Diophantus the conjecture that every integer is a sum of four squares, and asked for a proof" [94, page 34], the reader may well feel disappointed by the ambiguity of the verb "extracted" and the absence of references. However, what interests us here is the validity or otherwise of the contention (implicit in Unguru and Corry and explicit in Paumier–Aubin and Kutrovátz) that Weil was *internalist*. Was Weil internalist as charged?

Weil mentions, for instance, that Euler was first motivated to look at the problem of Fermat primes $2^{2^n}+1$ by his correspondent Goldbach [94, page 172]. To give another example, Weil mentions that Fermat learned Vieta's symbolic algebra through his visits to d'Espagnet's private library in Bordeaux in the 1620s [94, page 39]. Such visits took place many years before Vieta's works were published in 1646 by van Schooten. In particular, the Fermat–

d'Espagnet contact was instrumental in Fermat's formulation of his method of adequality (see Section 3.2) relying as it did on Vieta's symbolic algebra. Such examples undermine the Paumier–Aubin claims, such as the following:

1. the charge of "an 'internalist history of concepts' which has only little to say about the way in which mathematics emerged from the interaction of groups of people in specific circumstances" [75, page 187];

2. the claim that "The focus on ideas erased much of the social dynamics at play in the historical development of mathematics" [75, page 204].

As we have seen, Weil does take the interactions and the dynamics into account.

## 3. Some case studies

I identified Fried's *tabula rasa* axiom in Section 2.3, and will analyze it in more detail in this section. It seems that while the axiom may be appropriate in certain cases, it is an assumption that needs to be argued rather than merely postulated. Such a need to argue the case applies to the very possibility itself of a "tabula rasa" attitude in the first place:

1. Can historians of mathematics truly view the past without the lens of modern mathematics?

2. Have historians been successful in such an endeavor?

Whereas it may be difficult to rule out the theoretical possibility of an affirmative answer to question 1, a number of recent studies suggest that in practice, the answer to question 2 is often negative, as I will discuss in Section 3.1.

### *3.1. History of analysis*

Some historians of $17^{\text{th}}$ to $19^{\text{th}}$ century mathematical analysis, while claiming to reject insights provided by modern mathematics in their interpretations, turn out themselves to be *privileged observers* in Fried's sense (see Section 2.3) though still in denial. Namely, they operate within a conceptual scheme dominated by the mathematical framework developed by Weierstrass at the end of the $19^{\text{th}}$ century, as argued in recent studies in the following cases:

- Fermat, in Katz *et al.* [61] and Bair *et al.* [8];
- Gregory, in Bascelli *et al.* [12];
- Leibniz, in Sherry–Katz [79], Bascelli *et al.* [10], Blåsjö [16], and Bair *et al.* [2];
- Euler, in Kanovei *et al.* [60], and Bair *et al.* [4];
- Cauchy, in Bair *et al.* [3], Bair *et al.* [6], Bascelli *et al.* [11], and Bair *et al.* [5].

The pattern that emerges from these studies is that some modern historians, limited in their knowledge of modern mathematics, tend to take a narrow view, that in some cases borders on naivete, of the work of the great mathematical pioneers of the $17^{\text{th}}$–$19^{\text{th}}$ centuries (see Sections 3.7 and 3.8 for examples).

The Fermat historian Mahoney is a case in point. Weil pointed out numerous historical, philological, and mathematical errors in Mahoney's work on Fermat; see [92]. Yet in the Friedian scheme of things, Weil is neatly shelved away on the *privileged observer* shelf, whereas Mahoney's work, breezy journalese and all, is blithely assumed to reside in that rarefied stratum called authentic history of mathematics, and relied upon to pass judgments on the value of the historical work by the great Pierre de Fermat (see Section 2.1).

### *3.2. Fermat's adequality*

Fermat used the method of *adequality* to find maxima and minima, tangents, and solve other problems.

To illustrate Fermat's method, consider the first example appearing in his *Oeuvres* [37, page 134]. Fermat considers a segment of length $B$, splits it into variable segments of length $A$ and $B-A$, and seeks to maximize the product $A(B-A)$, i.e.,

$$BA - A^2. \tag{3.1}$$

Next, Fermat replaces $A$ by $A+E$ (and $B-A$ by $B-A-E$). There is a controversy in the literature as to exact nature of Fermat's $E$, but for the purposes of following the mathematics it may be helpful to think of $E$ as small. Fermat goes on to expand the corresponding product as follows:

$$BA - A^2 + BE - 2AE - E^2. \tag{3.2}$$

In order to compare the expressions (3.1) and (3.2), Fermat removes the terms independent of $E$ from both expressions, and forms the relation

$$BE \sqcap 2AE + E^2, \tag{3.3}$$

also referred to as *adequality*. In the original, the term *adæquabitur* appears where we used the symbol $\sqcap$.[6] I will present the final part of Fermat's solution in Section 3.4.

### *3.3. van Maanen's summary*

Fermat's method is described as follows by van Maanen:[7]

> Fermat seems to have based his method for finding a maximum or minimum for a certain algebraic expression $I(x)$ on a double root argument, but in practice the algorithm was used in the following slightly different form. Fermat argued that if the extreme value is attained at $x_M$, $I(x)$ is constant in an infinitely small neighborhood[8] of $x_M$. Thus, if $E$ is very small, $x_M$ satisfies the equation $I(x + E) = I(x)$. [91, page 52]

Fermat never actually formed an algebraic relation (using Vieta's symbolic algebra) of *adæquabitur* between the expressions $I(x + E)$ and $I(x)$. The kind of relation he did form is illustrated in formula (3.3) in Section 3.2. Van Maanen provides the following additional explanations:

> This expression states that close to the extreme value,[9] lines parallel to the $x$-axis will intersect the graph of $I$ in two different points, but the extreme is characterised by the fact that these … parallels turn into the tangent [line] and the points of intersections reduce to one point which counts twice. The common terms in $x$ are removed from the equation $I(x + E) = I(x)$ and the resulting equation divided by $E$. Any remaining terms are deleted, and $x_M$ is solved from the resulting equation. (ibid.)

[6] A symbol similar to $\sqcap$ was used several decades later by Leibniz, interchangeably with =, to denote a relation of generalized equality.

[7] In place of Fermat's $E$, van Maanen uses a lower-case $e$. The pieces of notation $I(x)$, $x_M$, and = are van Maanen's.

[8] Describing Fermat's method in terms of the infinitely small is not entirely uncontroversial and is subject to debate; for details see Bair *et al.* [8].

[9] I added the comma for clarity.

While the summary by van Maanen does not mention the possibility of dividing by $E^2$, it is important to note that in Fermat's descriptions of the method, Fermat does envision the possibility of dividing by higher powers of $E$ in the process of obtaining the extremum.

### *3.4. Squaring both sides*

In the example presented in Section 3.2, the term $BE$ and the sum $2AE+E^2$ originally both appeared in the expression (3.2), but appear on different sides in relation (3.3) (all with positive sign). The remainder of Fermat's algorithm is more familiar to the modern reader: one divides both sides by $E$ to produce the relation $B \sqcap 2A+E$, and discards the summand $E$ to obtain the solution $A = \frac{B}{2}$.

For future reference, let us note that a relation of type (3.3) can be squared to produce a relation of type $(BE)^2 \sqcap (2AE+E^2)^2$. In this particular example, the relation need not be squared. However, in an example involving square roots one needs to square both sides at a certain stage to eliminate the radicals; see Section 3.5. Meanwhile, once one passes to the difference $I(x+E) - I(x)$ (to use van Maanen's notation), such an opportunity is lost.

Fermat never performed the step of carrying all the terms to the left-hand side of the relation so as to form the difference $I(x+E) - I(x)$;[10] nor did he ever form the quotient $\frac{I(x+E)-I(x)}{E}$ familiar to the modern reader. In Section 3.5 I will compare the treatment of this aspect of Fermat's method by a historian and a mathematician.

### *3.5. Experiencing $E^2$*

The perspective of Unguruan polarity can lead historians to devote insufficient attention to the actual mathematical details and ultimately to historical error. Thus, Mahoney claimed the following:

> In fact, in the problems Fermat worked out, the proviso of repeated division by $y$ [i.e., $E$] was unnecessary. But, thinking in terms of the theory of equations, Fermat could imagine, *even if he had not experienced*, cases in which the adequated expressions contained nothing less than higher powers of $y$. (Mahoney [68, page 165]; emphasis added)

[10] Fermat historian Breger did in [24, page 27]; for details see Bair *et al.* [8, Section 2.6, page 573].

Mahoney assumed that Fermat "had not experienced" cases where division by $E^2$ was necessary. Meanwhile, Giusti analyzes an example "experienced" by Fermat which involves radicals, and which indeed leads to division by $E^2$. The example (Fermat [37, page 153]) involves finding the maximum of the expression $A + \sqrt{BA - A^2}$ (here $B$ is fixed). In the process of solution, a suitable relation of adequality, as in formula (3.3), indeed needs to be *squared* (see Section 3.4). Giusti concludes:

> Ce qui nous intéresse dans ce cas est qu'il donne en exemple une adégalité où les termes d'ordre le plus bas sont en $E^2$. Comme on sait, dans l'énonciation de sa règle Fermat parlait de division par $E$ ou par une puissance de $E$ ... Plusieurs commentateurs ont soutenu ... que Fermat avait commis ici une *erreur* ... On doit donc penser que dans un premier moment Fermat avait traité les quantités contenant des racines avec la méthode usuelle, qui conduisait parfois à la disparition des termes en $E$, et qui ait tenu compte de cette éventualité dans l'énonciation de la règle générale. (Giusti [51, Section 6, pages 75–76]; emphasis added).

What Giusti is pointing out is that in this particular application of adequality in a case involving radicals, division by $E^2$ (and not merely by $E$) is required. Thus, the error is Mahoney's, not Fermat's.

A first-rate analyst and differential geometer, Giusti was able to appreciate the *discontinuity* between Fermat's method of adequality, on the one hand, and the modern $\frac{I(x+E)-I(x)}{E}$ perspective, on the other, better than many a Fermat historian. More generally, a scholar's work should be evaluated on the basis of its own merits rather than which class he primarily belongs to, be it historian, mathematician, or philologist.

Appreciating discontinuity is not the prerogative of Unguru's adepts, contrary to strawman accounts found in Unguru [87] and Guicciardini [54]. The portrait of a mathematician's view of his discipline dominated by mathematical Platonism as found in Unguru and his students (as detailed in Section 2.4) as well as Guicciardini is similarly a strawman caricature, as when Guicciardini elaborates on "the perfect embodiment of the immutable laws of mathematics written in the sky for eternity" [54, page 148] and claims that "[t]he mathematician's world is the world of Urania" [54, page 150].

*3.6. Why certain developments happened: Euler to Cauchy*

Analyzing the differences between 18th and 19th century analysis, Fraser and Schroter observe:

> The decline of [Euler's] formalism stemmed mainly from its limitations as a means of generating useful results. Moreover, as methods began to change, an awareness of formalism's apparent difficulties and even contradictions lent momentum to efforts to rein it in. (Fraser–Schroter [40, Section 3.3])

Note that Fraser and Schroter are analyzing Euler's own work itself here, rather than its reception by Cauchy. Fraser and Schroter continue:

> Euler had been confident that the "out-there" objectivity of algebra secured the generality of his formal techniques, but Cauchy demanded that generality be found within mathematical methods themselves. In his [textbook] *Cours d'analyse* of 1821 Cauchy rejected formalism in favour of a fully quantitative analysis. [40]

Fraser and Schroter feel that the limitations and difficulties of Euler's variety of algebraic formalism can be fruitfully analyzed from the standpoint of considerably later developments, notably Cauchy's "quantitative analysis." In their view, it is possible to comment on the shortcomings of Euler's algebraic formalism and the reasons for this particular development from Euler to Cauchy without running the risk of anachronism. Meawhile, it is clear that the Fraser–Schroter approach may run afoul of both the discontinuity axiom (see Section 2.1) and the tabula rasa axiom (see Section 2.3).

The issue of anachronism was perceptively analyzed by Ian Hacking [55] in terms of the distinction between the butterfly model and the Latin model for the development of a scientific discipline. Hacking contrasts a model of a deterministic (genetically determined) biological development of animals like butterflies (the egg–larva–cocoon–butterfly sequence), with a model of a contingent historical evolution of languages like Latin. Emphasizing determinism over contingency can easily lead to anachronism; for more details see Bair *et al.* [5].

Similarly to Hacking, Fraser notes the danger for a historian in the adoption of a model based on an analogy with the pre-determined evolution of a biological organism. In his review of Boyer's book *The Concepts of the Calculus*, Fraser comments on the risks of anachronism:

> [Boyer's] focus on the development of concepts through time may reflect as well an embrace of the metaphor of a plant or animal organism. The concept undergoes a progressive development, moving in a directed and pre-determined way from its origins to an adult and completed form. ... The possibility of introducing anachronisms is almost inevitable in such an approach, and to a certain degree this is true of Boyer's book. (Fraser [38, page 18])

Fraser specifically singles out for criticism Boyer's teleological view of mathematical analysis as inexorably progressing toward the ultimate *Epsilontik* achievement:

> [Boyer] seemed to view the eighteenth-century work as exploratory or approximative as the subject moved inexorably in the direction of the arithmetical limit-based approach of Augustin-Louis Cauchy and Karl Weierstrass. [38, page 19]

I will report on two additional cases of such teleological thinking in the historiography of mathematics in Sections 3.7 and 3.8.

### *3.7. Leibnizian infinitesimals*

Boyer-style, *Epsilontik*-oriented teleological readings of the history of analysis (see Section 3.6) are common in the literature. Thus, Ishiguro interprets Leibnizian infinitesimals as follows:

> It seems that when we make reference to infinitesimals in a proposition, we are not designating a fixed magnitude incomparably smaller than our ordinary magnitudes. Leibniz is saying that whatever small magnitude an opponent may present, one can assert the existence of a smaller magnitude. In other words, we can paraphrase the proposition with a universal proposition with an embedded existential claim. (Ishiguro [59, page 87])

What is posited here is the contention that when Leibniz wrote that his incomparable (or inassignable) $dx$, or $\epsilon$, was smaller than every given (assignable) quantity $Q$, what he really meant was an alternating-quantifier clause (universal quantifier $\forall$ followed by an existential one $\exists$) to the effect that for each given $Q > 0$ there exists an $\epsilon > 0$ such that $\epsilon < Q$. Such a logical sleight of hand goes under the name of the *syncategorematic interpretation*. Here the author is interpreting Leibniz as thinking like Weierstrass (see also Section 3.10). For details see Bascelli *et al.* [10] and Bair *et al.* [2].

### *3.8. Cauchyan infinitesimals*

In a similar vein, Siegmund-Schultze views Cauchy's use of infinitesimals as a step *backward*:

> There has been ... an intense historical discussion in the last four decades or so how to interpret certain apparent remnants of the past or – as compared to J. L. Lagrange's (1736–1813) rigorous 'Algebraic Analysis' – even *steps backwards* in Cauchy's book, particularly his use of infinitesimals ... (Siegmund-Schultze [80]; emphasis added)

Siegmund-Schultze's reader will have little trouble reconstructing exactly which direction a *step forward* may have been in. Grabiner similarly reads Cauchy as thinking like Weierstrass; for details see Bair *et al.* [5].

### *3.9. History, heritage, or escapade?*

Significantly, in his essay Fried fails to mention the seminal scholarship of Reviel Netz on ancient Greek mathematics (see, for example, [72, 73, 74]). Would, for example, Netz's detection of traces of infinitesimals in the work of Archimedes be listed under the label of history, heritage, or "escapade" (to quote Fried)? Would an argument to the effect that the *procedures* (see Section 3.10) of the Leibnizian calculus find better proxies in modern infinitesimal frameworks than in late 19$^{\text{th}}$ century Weierstrassian ones, rank as history, heritage, or escapade? Would an argument to the effect that Cauchy's definition of continuity via infinitesimals finds better proxies in modern infinitesimal frameworks than in late 19$^{\text{th}}$ century Weierstrassian ones, rank as history, heritage, or escapade? Unfortunately, there is little in Fried's essay that would allow one to explore such questions.

### *3.10. Procedures vs ontology*

The procedures / ontology distinction elaborated in Błaszczyk *et al.* [19] can be thought of as a refinement of Grattan-Guinness' history/heritage distinction. Consider for instance Leibnizian infinitesimal calculus. Without the procedure / ontology distinction, interpreting Leibnizian infinitesimals in terms of modern infinitesimals will be predictably criticized for utilizing history as *heritage*. What some historians do not appreciate sufficiently is that, in an ontological sense, interpreting Leibniz in Weierstrassian terms is just as much heritage. Surely talking about Leibniz in terms of ultrafilters[11] is not writing history; however, analyzing Leibnizian *procedures* in terms of those of Robinson's procedures is better history than a lot of what is written on Leibniz by received historians and philosophers (who have pursued a syncategorematic reading of Leibnizian infinitesimals; see Section 3.7), such as Ishiguro, Arthur, Rabouin, and others; for details see Bair *et al.* [2].

Let me summarize some of the arguments involved.

**1.** Leibniz made it clear on more than one occasion that his infinitesimals violate Euclid Definition V.5 (Euclid V.4 in modern editions), which is a version of what is known today as the Archimedean axiom; see, for example, (Leibniz [64, page 322]). In this sense, the procedures in Leibniz are closer to those in Robinson than those in Weierstrass.

**2.** If one follows Unguru's strictures and Fried's tabula rasa, one can't exploit *any* modern framework to interpret Leibniz; however, in practice the syncategorematic society interpret Leibniz in Weierstrassian terms, so the Unguruan objection is a moot point as far as the current debate over the Leibnizian calculus is concerned.

**3.** While modern foundations of mathematics were clearly not known to Leibniz, it is worth pointing out that this applies both to the set-theoretic foundational *ontology* of the classical Archimedean track, and to Robinson's non-Archimedean track. But as far as Leibniz's *procedures* are concerned, they find closer proxies in Robinson's framework than in a Weierstrassian one. For example, Leibniz's law of continuity is more readily understood in terms of Robinson's transfer principle than in any Archimedean terms.

**4.** The syncategorematic society seems to experience no inhibitions about interpreting Leibnizian infinitesimals in terms of alternating quantifiers (see Section 3.7), which are conspicuously absent in Leibniz himself. Meanwhile, Robinson's framework enables one to interpret them without alternating quantifiers in a way closer to Leibniz's own procedures.

**5.** On several occasions Leibniz mentions a distinction between *inassignable* numbers like $dx$ or $dy$, and (ordinary) *assignable* numbers; see, for example, his *Cum Prodiisset* [65] and *Puisque des personnes...* [66]. The distinction has no analog in a traditional Weierstrassian framework. Meanwhile, there is a ready analog of standard and nonstandard numbers, either in Robinson's [77] or in Nelson's [71] framework for analysis with infinitesimals.

[11] See, for example, Fletcher *et al.* [41] for a technical explanation.

*3.11. Are there gaps in Euclid?*

Some of the best work on ancient Greece would possibly fail to satisfy Fried's criteria for authentic history, such as de Risi's monumental work [35], devoted to the reception of Euclid in the early modern age. Here de Risi writes:

> Euclid's system of principles has been repeatedly discussed and challenged: A few gaps in the proofs were found ... [35, page 592].

This is a statement about Euclid and not merely its early modern reception. Now wouldn't the claim of the existence of a what is seen today as a "gap" in Euclid be at odds with Fried's *tabula rasa* axiom (see Section 2.3)?

*3.12. Philological thought experiment*

Fried's discussion is so general as to raise questions about its utility. Dipert notes in his review of the original 1981 edition of Mueller [70]:

> It will be difficult in the coming years for anyone doing serious research on Euclid, *outside of the narrowest philological studies*, not first to have come to grips with the present book, and it is to be hoped that this volume will inject new vigor into discussions of Euclid by contemporary logicians and philosophers of mathematics. (Dipert [36]; emphasis on "philological studies" added)

Inspired by Dipert's observation, let me propose the following thought experiment. Consider a hypothetical study of, say, the frequency of Greek roots in the texts of ancient Greek mathematicians. Surely this is a legitimate study in Philology. As far as Fried's requirements for authentic history, such a study would meet them with flying colors. Thus, the satisfaction of the *discontinuity* axiom (see Section 2.1) is obvious. The satisfaction of the *tabula rasa* axiom (see Section 2.3) is evident, seeing that no modern mathematics is used at all in such a study. The risk of a Platonist deviation (see Section 2.4) is infinitesimal. Freudenthal, van der Waerden, and Weil may well have written on interpreting the classics; but by Fried's ideological criteria, our hypothetical philological study would constitute legitimate mathematical history, surpassing anything that such "privileged observers" may have written. Yet it seems safe to surmise, following Dipert, that the audience for such a philological study among those interested in the history of mathematics would be limited.

## 4. Evolution of Unguru polarity

### *4.1. Fried's upgrade*

Fried admits in his 2018 essay that when he was a graduate student under Unguru, it was axiomatic that there are only two approaches to the history of mathemathics: that of a historian, and that of a mathematician. He writes:

> By the time I finished my Ph.D., I could make some distinctions: I could divide historians of mathematics into a mathematician type, such as Zeuthen or van der Waerden, a historian type, like Sabetai Unguru, and, perhaps, a postmodern type ... [47, page 4]

The latter "type" is quickly dismissed as "not in fact a serious option" leaving us with only two options, historian and mathematician. Fried goes on to relate in his essay that he came to appreciate that the historiographic picture is more complex, resulting in the novel labels of *mathematical conquerors*, *privileged observers*, and the like. Such a more complex picture is something of a departure from Unguruan orthodoxy, as I analyze in Section 4.2.

### *4.2. Unguru polarity*

Meanwhile, Unguru himself sticks to his guns as far as the original dichotomy of mathematician versus historian is concerned. In his 2018 piece, Unguru reaffirms the idea that there are only two approaches to the history of mathematics:

> The paper deals with two *polar-opposite* approaches to the study of the history of mathematics, that of the mathematician, tackling the history of his discipline, and that of the historian. (Unguru [87, page 17]; emphasis added)

Unguru proceeds to reveal further details on the alleged polarity:

> [S]ince it is always possible to present past mathematics in modern garb, ancient mathematical accomplishments can be easily made to look modern and, therewith seamlessly integrated into the growing body of mathematical knowledge. That this is a historical *calamity* is not the mathematician's worry ... Never mind that this procedure is tantamount to the *obliteration* of the history of

> mathematics as a *historical* discipline. Why, after all, should this concern the mathematician? ([87, page 20]; emphasis on *historical* in the original; emphasis on *calamity* and *obliteration* added)

It seems to me that Unguru's assumption that a mathematician does not care about a possible "obliteration" of the history of mathematics as a historical discipline is unwarranted. Note that "calamity" and "obliteration" are strong terms to describe the work of respected scholars such as van der Waerden, Freudenthal, and others. I will examine the issue in more detail in Section 4.3.

### *4.3. Polarity-driven historiography*

What is the driving force behind Unguru's historiographic ideology, including his readiness to describe the two approaches as "antagonistic"? The ideological polarity postulated in Unguru's approach appears to involve a perception of class struggle, as it were, between historians (H-type, my notation) and mathematicians (M-type, my notation) with their "antagonistic" class interests. As already noted in Section 1.3, M-type as a class does not fare very well relative to the attribute of *open-mindedness* in the FU ideology. For a detailed study of polarity-driven historiography as applied to, or more precisely against, Felix Klein and (in Unguru's words) "the obliteration of the history of mathematics as a historical discipline" by Mehrtens, see Bair *et al.* [7].[12]

Unguru seeks to forefront the struggle between H-type and M-type as *the* fundamental "antagonism" in terms of which all historical scholarship must be evaluated, in an attitude reminiscent of the classic adage "The history of all hitherto existing society is the history of class struggles." How fruitful is such a historiographic attitude? Let us examine the issue in the context of a case study.

### *4.4. Is exponential notation faithful to Euclid?*

As a case study illustrating his historiographic ideology, Unguru proposes an examination of Euclid's Proposition IX.8, dealing with what would be called

[12] Mehrtens [69] in an avowedly marxist approach, postulates the existence of two polar-opposite attitudes among German mathematicians at the beginning of the 20$^{\text{th}}$ century: modern (M-type, my notation) and countermodern (C-type, my notation). Felix Klein has the bad luck of being pigeonholed as a C-type, along with unsavory types like Ludwig Bieberbach and the *SS-Brigadeführer* Theodor Vahlen. The value of such crude interpretive frameworks is limited.

today a geometric progression of lengths starting with the unity. Paraphrased in modern terms, the proposition asserts that in the geometric progression, every other term is a square, every third term is a cube, etc. Unguru [87] objects to reformulating the proposition in terms of the algebraic properties of the exponential notation $1, a, a^2, a^3, \ldots$, echoing the criticisms he already made forty years ago in [84].

Why does Unguru feel that exponential notation must not be used to reformulate Proposition IX.8? He provides a detailed explanation in the following terms:

> A proposition for the proof of which Euclid has to toil subtly and painstakingly, and in the course of whose proof he had to rely on many previous propositions and definitions (e.g., VIII.22 and 23, def. VII.20) becomes a *trivial commonplace*, which is an immediate outgrowth, a trite after-effect, of our symbolic notation: 1, $a$, $a^2$, $a^3$, $a^4, a^5, a^6, a^7, \ldots$ As a matter of fact, if we use modern symbolism, this ceases altogether to be a proposition and its truthfulness is an immediate and *trivial application* of the definition of a geometric progression in the particular case when the first member equals 1 and the ratio, $q = a$, is a positive integer (for Euclid)! ([87, page 27]; emphasis added)

Unguru claims that using exponential notation causes Euclid's proposition to become a trivial commonplace severed from Euclid's "previous propositions," and a trivial application of the definition of a geometric progression. In this connection, Blåsjö points out that

> Unguru ... mistakenly believes that certain algebraic insights are somehow built into the notation itself. (Blåsjö [15, page 330])

Namely,

> The fact that, for example, $a^4$ is a square is not by any means implied by the symbolic notation itself. The fact that $a^{xy} = (a^x)^y$ is a contingent fact, a result that needs proving. It is not at all obvious from the very notation itself ... [15]

Thus, contrary to Unguru's claim, Euclid's Proposition IX.8 is *not* severed from Euclid's "previous propositions" which are similarly more accessible to modern readers when expressed in modern notation, whose properties require proof just as Euclid's propositions do.

For instance, Proposition VIII.22 mentioned by Unguru asserts the following: "If three numbers are in continued proportion, and the first is square, then the third is also square." In modern terminology this can be expressed as follows: if $a^2 : b = b : c$, then $a^2c = b^2$, and therefore $c = x^2$ for some $x$. Put another way, $a^2rr = (ar)^2$.[13] This is not a triviality but rather an identity that requires proof. Such an identity could possibly be used in the proof of special cases of $a^{xy} = (a^x)^y$. For more details see Mueller [70]. Unguru's ideological opposition to using modern exponential notation in this case has little justification.

It is a pity that Unguru chose not to address Blåsjö's rebuttal of his objections to [84] in [87]. Note that the rebuttal [15] appeared two years earlier than Unguru's piece.

### *4.5. What is an acceptable meta-language?*

In his 2018 piece, Unguru reiterates a sweeping claim he already made in 1979:

> The only acceptable meta-language for a historically sympathetic investigation and comprehension of Greek mathematics seems to be ordinary language, not algebra. [87, page 30].

Given such a stance, it is not surprising that Unguru opposes any and all use of algebraic notation (including exponential notation) in dealing with Euclid (see Section 4.4) and Apollonius (see Section 2.2). However, Berggren notes in his review of [86] that the reason

> why modern words, with the concepts they embody, are acceptable as analytic tools where Renaissance (or even Arabic) algebra is forbidden is never explained [by Unguru]. (Berggren [13])

The position of some other historians with regard to Unguru's claims is discussed next, in Section 5.

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[13]Here the first term in the progression is a square $a^2$ by hypothesis. The second term is $a^2r$ and the third term is $(a^2r)r$. The identity $a^2rr = (ar)^2$ enables one to conclude that the third term is also a square.

## 5. Do historians endorse Unguru polarity?

Unguru's positing of a polarity of historian *vs* mathematician tends to obscure the fact that a number of distinguished *historians* have broken ranks with Unguru on the methodological issues in question, such as the following scholars.

1. Kirsti (Møller Pedersen) Andersen wrote a negative review of Unguru's polarity manifesto [84] for Mathematical Reviews, noting in particular that Unguru "underestimates the historians' [for example, Zeuthen's] understanding of Greek mathematics" [1].

2. C. M. Taisbak notes that Unguru and Rowe "are being ridiculously unfair, to say the least, towards Heath at this point [concerning interpretation of the *Elements*, items I44 and I45], to say nothing of others" [83].

3. Árpád Szabó receives the strongest endorsement from Unguru in [84, pages 78, 81]. Yet when Szabó analyzed *Elements* Book V [82, page 47], he employed symbolic notation introduced in the 19th century by Hermann Hankel.[14] Such a practice is clearly contrary to Unguru's position on modern algebraic notation; see Section 4.5. Unlike Unguru, Szabó treats van der Waerden's scholarship with respect and even relies on it (see Section 2.4).

4. Christianidis [26, page 36] proposes a distinction between premodern algebra and modern algebra and argues that Diophantus can be legitimately analyzed in terms of the former category (see Section 2.6).

The present article is *not* a defense of the mathematician as mathematical historian. The main thrust of this article is the following: The postulation of an ideological polarity of historian *vs* mathematician (the latter routinely suspected of a Platonist deviation) does more harm than good in that it obscures the only possible basis for evaluating work in the history of mathematics, namely competent scholarship. A mathematician who wishes to write about a historical figure, but is insufficiently familiar with the historical period and / or the primary documents, should be criticized as much as a historian insufficiently familiar with the mathematics to appreciate the fine points, and indeed

[14] In more detail, Hankel [56, pages 389–404] introduced algebraic notation in an account of Euclid's *Elements* book V. Furthermore, Heiberg [57, vol. II, s. 3] employed Hankel's notation in his translation of Book V into Latin. For more details see Błaszczyk [18, page 3, notes 5, 11].

the implicit aspects (as detailed for example in Blåsjö–Hogendijk [17]), of what the historical figure actually wrote. The axioms of discontinuity and tabula rasa and the positing of a polarity between mathematicians and historians are of questionable value to the task of the history of mathematics.

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