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A Critique of the Modern Consensus in the Historiography of Mathematics[1]

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Synopsis

The history of mathematics is nowadays practiced primarily by professional historians rather than mathematicians, as was the norm a few decades ago. There is a strong consensus among these historians that the old-fashioned style of history is "obsolete," and that "the gains in historical understanding are incomparably greater" in the more "historically sensitive" works of today. I maintain that this self-congratulatory attitude is ill-founded, and that the alleged superiority of modern historiographical standards ultimately rests on a dubious redefinition of the purpose of history rather than intrinsic merit.

Much has changed in the historiography of mathematics in the last generation or two. The field has "evolved," as the current incumbents would have it, to the point where "the gains in historical understanding are incomparably greater" in modern work than those based on the "obsolete style of historiography" of old ([7, pages 107, 109], [4, page 781]). The object of derision here is that "good deal of early 20$^{\text{th}}$-century scholarship in the history of mathematics" that was "carried out by mathematicians rather than historians," who translated ancient mathematics "into the symbols and concepts of modern mathematics." The highest praise the modern historiographers can spare for these scholars is that their "motives ... were not in themselves reprehensible" [7, page 108].

[1] Editor's note: A thoughtful response to this essay, written by Michael Fried, is included in this issue of the *Journal of Humanistic Mathematics*; see http://scholarship.claremont.edu/jhm/vol4/iss2/13/.

These quotations are taken from two recent survey books on the history of mathematics—the *Oxford Handbook* and the *Very Short Introduction*—but the sentiment (and even the formulations, almost verbatim) dates back to Unguru [9], a founding document of sorts for the now dominant school of historiography. Indeed, "Unguru's position could now be regarded as the accepted orthodoxy," as Rowe [5, page 37] accurately reports. "It is clear that the old historiography has been overcome," another recent publication explains, referring to much the same thing [6, page 43]. And this goes not only for Greek geometry, which was Unguru's immediate concern, but for the history of mathematics generally. So for example a recent history of 17^{th} century mechanics notes that "only comparatively recently has the practice of translating seventeenth-century works into modern notation become unacceptable," praising the "new and sophisticated literature" that accords with these "historically sensitive" standards [1, page 9].

This "accepted orthodoxy" shapes our field and yet it is rarely if ever subjected to critical analysis. In my opinion the self-image of the modern historiographers is greatly inflated. In this paper I intend to show that many of the arguments for the supposed superiority of modern historiography ultimately come down to confusion, convention, and lack of critical thought, as one may well expect from a consensus that has never been seriously challenged.

One of the main sources of confusion, I believe, is that the modern historiographical consensus has no clearly articulated rival. Let me therefore try to spell out what I see as the core of the old-fashioned conception of history as a counterpoint. It goes as follows. In the first instance the subject matter of history is a sequence of events ordered chronologically: ABCDEFGH. . . In the history of mathematics these units are theorems and problems and so on. The purpose of mathematics is to understand these units; the purpose of history is to understand this sequence *qua* sequence. That is to say: Why does it begin at A and not at C or F? Why is ABC followed by D instead of going straight to H? The mathematician has no such qualms; he allows himself to pick and choose as he sees fit—to treat B through G as special cases of Q, and to give a course on vowels—but the historian wants to understand why things happened the way they did, i.e., to be able to feel for any given letter why the next one was this rather than that. This kind of history may be called "rational history," as the historian assumes that everything happened for a reason and makes it his duty to uncover these reasons.

The rational history of mathematics, then, is about understanding the development of mathematical ideas: to uncover the motivating forces behind their genesis, the interplay between them, and the ways in which they were understood and applied by the people who explored them. To many it would seem obvious that this is a sensible way of studying the history of mathematics. After all, it concerns the greatest accomplishments of the human mind in the domain of mathematics, which might seem like the commonsensical *sine qua non* of history of mathematics. Furthermore, the rational history of mathematics is directly relevant to many audiences beside historians: teachers and students whose daily preoccupations often mirror historical currents of thought, mathematicians who may know a theory in its modern definition-theorem-proof presentation but wonder how and why anyone arrived at it in the first place, philosophers trying to understand the nature of mathematical knowledge, and so on. Indeed, for any given mathematical topic, it is by definition the goal of rational history to reveal a natural path of thought leading to it and a context in which it is has meaningful and immediate value, since rational history cannot "look ahead" ten or twenty steps, as modern curriculum planners are wont to do.

Now I am not saying that rational history is the *only* kind of history that should be written. My goal in giving this definition is not to exclude other approaches but only to save the sensible core of the traditional approach before the baby is thrown out with the bathwater. It seems to me that since rational history has not been an articulated historiographical alternative, it has often been mistaken for things that are, such as Whiggism and presentism (i.e., the notion of history as a deterministic path from ignorance to the current state of knowledge, in which the historian is more concerned with who was the first to "get it right" than in understanding past thought in its own right). Distinctions between old and new historiography along such lines is commonplace and is spelled out in detail for example in [3]. But once articulated it is clear that rational history is independent of the modern state of mathematical knowledge. There is nothing in the definition of rational history that restricts it to only those ideas that are precursors of modern ones, and nothing that compels it to see only progress and ignore "false starts." Narrow-minded internalism is another trait associated with traditional scholarship, but again it is obvious that rational history as defined above does not ignore social context insofar as it is relevant to the development of mathematical ideas. It may very well be that certain authors sympathetic to the ideal of rational history have committed the sins of Whiggism, presentism and excessive internalism, but "guilt by association" is not a valid manner of dismissing rational history and all its intrinsic merit along with it.

Then there is the matter of notation, always a focal point of these debates. Modern historians take great pride in their zero-tolerance policy toward anachronistic notation and terminology. From the point of view of rational history, however, using modern modes of expression to describe historical works is often a sound methodological choice; at the very least because it can remove needless obstacles to clarity and understanding. Modern historians often allege that such an approach is incompatible with the goal of trying to understand a historical text in the manner of its author. This strikes me as a poor argument, for obviously history is full of mathematicians doing equivalent things in different garbs who nevertheless understood each other's works perfectly and considered themselves to be doing the exact same thing. In the same way, many accounts of historical mathematics in modern terms could be easily understood and recognised as his own by the historical mathematician in question, perhaps after supplying him with some trivial definitions of terms.

Certainly it *could* happen that a particular use of modern notions is misleading, for example by suggesting or assuming connotations and connections that were not conceived by the original author. But the question is not whether anachronistic notation comes with such connotations, which in a sense it always does. The question is rather whether its use undermines the purpose of the historian introducing it. If it does not, then there is no problem. If it does, then that is an individual mistake of the individual historian, comparable to other ways in which a historical account can be misleading, e.g. by failing to take into account a relevant source. The only meaningful way to criticise such mistakes is by showing how they discredit the specific conclusions and intentions of that specific study, not by appealing to some general methodological principle as if it were an end in itself.

Modern historians take the blanket objection route instead. For example:

> The use of modern algebraic notation … should never be mistaken for what the original writer was "really" trying to do, or what he would have done with the advantage of a good modern education. At best, such modernization obscures the original method and at worst can lead to serious misunderstandings [7, page 35].

One would like to ask these modern historians: Have you never had an idea that you could not quite formulate or work out to your satisfaction? And has it never happened that someone else, perhaps someone with experience and expertise in this area, was able to put his finger on precisely what you were "really" trying to say? And if you then had to explain your original idea to someone else, would you not do it with the aid of this better formulation? Surely this has happened to us all, both in mathematics and elsewhere. Obviously, then, modern notation does not "at best obscure" but rather at best makes diffuse matters clear.

Another version of the blanket objection has it that it is "impossible" to think in one way and write in another:

> Different ways of thinking imply different ways of expression. It is, therefore, impossible for a system of mathematical thought (like Greek algebra) to display such a discrepancy between its alleged underlying algebraic character and its purely geometric mode of expression [9, page 80].

Such a discrepancy is obviously not "impossible"—if anything it is commonplace. For example in calculus we often use infinitesimal reasoning as a behind-the-scenes heuristic and then write up our findings in the completely different language of epsilon-delta formalism.

This last quotation is from Sabetai Unguru's *locus classicus* of modern historiography [9]. His goal with this argument was to disprove the assumption, commonplace among mathematical readers for centuries, that certain parts of Euclid's *Elements* (such as much of Book II) is really algebra in geometrical dress. Although this is not the place to delve into this debate, it is instructive to note some absurd consequences that go hand in hand with Unguru's categorical stance regarding notation. Unguru repeatedly tries to disprove the "geometrical algebra" hypothesis by arguments of this form: "As a matter of fact, if we use modern algebraic symbolism, this ceases altogether to be a proposition" (page 99) or "In its algebraic form, the triviality of the entire enterprise becomes striking. The lemma becomes nothing but an inane, vapid, banal illustration of the simplification of fractions!" (page 102).

If this shows that Euclid did not know algebra, then neither did, say, Viète or Cardano. In Viète, for example, one may read such things as: "Theorem. The sum of two magnitudes plus their difference is equal to twice the greater magnitude" [11, page 37]. Clearly this proves that Viète is ignorant of algebra, for in its algebraic form $A + B + (A - B) = 2A$ the theorem is “inane, vapid, banal” and “ceases altogether to be a proposition.” Or let us open Cardano’s *Ars Magna*, unquestionably a work of algebra par excellence. At the end of the first chapter we encounter a geometrical proof that a “true” (i.e. positive) root of $x^3 + 10 = 6x^2 + 8x$ is also a “fictitious” (i.e. negative) root of $x^3 + 6x^2 = 8x + 10$. This is of course algebraically obvious by making the substitution $x = -x$, and yet Cardano felt the need to give a geometrical demonstration. By Unguru’s nonsensical logic this would “show beyond any reasonable doubt that what [Cardano] is doing is not algebra, but geometry” (page 105; I have replaced Euclid by Cardano in this quotation, but the logic remains the same).

The reader may have noted a pattern in my arguments so far, namely that they draw on historical mathematical thought to provide counterexamples to the dogmatic statements of modern historians. I have used this form of argument because I think it gets to the heart of the matter. We all agree that as historians we strive to understand past mathematics in its own right, not to reinterpret it by modern standards. But from here it does not follow, as the modern historiographers assume, that any use of anachronistic notation is necessarily evil, and that slavish adherence to the original form of expression is the same thing as perfect sensitivity to the author’s thought. In fact, as my arguments are intended to show, such an approach is demonstrably *in*sensitive to numerous aspects of historical mathematical thought.

A rather straightforward benefit of describing past mathematics in standardised terms is that it can facilitate uncovering connections between different works. But Unguru and his followers are so militant that they do not even concede this point. On the contrary, Unguru even manages to construe this as a vice rather than a virtue:

> When the mathematician succeeds in showing that two apparently unrelated mathematical texts ... have the same algebraic content ... it becomes legitimate to inquire into possible influences, questions of priority, ways of transmission, etc. ... Is this an acceptable position? As a historian, I must answer the question with an emphatic “no!” This position ... is historically unacceptable [9, pages 73-75].

This objection plainly makes no sense from the point of view of rational history: questions of influence do not need to “become legitimate”—they are inherently legitimate, and indeed one of the core concerns of historians.

Note that at this point the debate over anachronistic notation has become a proxy for what is, I believe, the real issue at stake. We are no longer debating notation but the very nature of historical inquiry, as witnessed by Unguru's qualifiers "as a *historian*" and "*historically* unacceptable."

It is to Unguru's credit that he indeed provides a definition of history to underwrite such claims. It goes as follows.

> History is primarily, essentially interested in the event *qua* particular event ... History is not (or is primarily not) striving to bunch events together, to crowd them under the same heading by draining them of their individualities. ... The domain of history, then, is the idiosyncratic [10, page 562].

This kind of history may be called "idiosyncraticism." The manner in which it contrasts with rational history may be illustrated by an analogy. When Newton brought out the underlying laws of mechanical phenomena, he certainly did not do so by studying "the event qua particular event." On the contrary, falling apples must be "drained of their individualities" and seen as abstract point masses before any meaningful scientific investigation can begin. Similarly, in rational history one must abstract away from idiosyncratic details, such as incidental matters of notation, before one gets to the real subject matter of history.

It would make no sense to criticise Newtonian mechanics for failing to take into account whether the apple is red or green, sweet or sour. Yet when it comes to the history of mathematics, modern historians proudly make the analogous objection to rational history. Turning to the *Oxford Handbook* again, we read that it is based on editorial principles such as "to limit the history of mathematics to the history of mathematicians is to lose much of the subject's richness" and "the ways in which people have chosen to express themselves ... are as historically meaningful as the mathematical content itself" [4, pages 2, 3]. Modern historians seem to consider these principles to be virtual truisms. Doing good history, it is assumed, is to "paint a complex and rich picture" "sensitive to its cultural context" [4, pages 311, 798]. Had humanity taken the same approach to the study of nature, we would still be in the middle ages today, but even so, the notion apparently never suggests itself to the modern historiographers that their principles and assumptions are in need of justification.

To be sure, the right level of abstraction is not always obvious, neither in physics nor in rational history. For instance, in a rational history of the 17^{th} century, it may seem at first sight that the difference between "x cubus" and "x^3" is precisely the kind of thing one ought to abstract away, until one realises that only the latter notation lends itself to the discovery of the binomial theorem. Thus in this case "the colour of the apple" turned out to be important after all. Nevertheless the fact remains that a substantial amount of abstraction is needed to reach the kinds of insights one wants, both in physics and in rational history. Both enterprises will fail to get off the ground if one insists that any one fact is "as meaningful" as any other.

The definition of history is, I believe, the real crux of the matter, and the actual foundation of the modern historiographical revolution. Once this is realised, it becomes clear that *the alleged superiority of modern historiographical standards rests on a redefinition of the purpose of history rather than intrinsic merit.* If one *defines* history in such a way that idiosyncratic details are "as historically meaningful as the mathematical content itself" or even "*the* domain of history," then, sure enough, modern historiographical standards are indeed vastly superior to older ones, and to those associated with rational history.

But of course this begs the question: Why should we define history in this way? While rational history is of obvious interest to many people, it is much less clear why studying "the event qua particular event" is a worthwhile pursuit. To this I know of no answer from the modern historiographers. Unguru presents his definition as if it were a matter of objective fact or divine decree, while his enthusiastic followers do not even acknowledge the need for a definition in the first place, let alone a justification for it.

In this way the modern historiography is a revolution under a blank banner, as it were. But this has not stopped its recruits from cheerfully obliterating any and all remnants of rational history, and with them virtually every reason anyone ever had to take an interest in the history of mathematics in the first place, other than to further their own academic careers.

To show that I am not exaggerating by much, imagine that a teacher or student of calculus wishes to know more about the subject's history. Perhaps his naiveté will be forgiven if he assumes that a recent 800-page *Oxford Handbook of the History of Mathematics* [4] ought to contain something on the matter. Alas, he will be disappointed. In the book's detailed 28-page index, there is not a single entry for the calculus. Perhaps he knows that Leibniz was one of the creators of the calculus. He might be relieved to find that Leibniz at least exists in the index, with three entries (as many as Pablo Picasso). These three entries concern his letters to Peter I, his priority dispute with Newton, and Rolle's posturing against his calculus in the Paris Academy. In other words: patronage, power struggles, and rhetoric—fashionable topics that fascinate the modern historian of mathematics far more than that antiquated substrate that is mathematics itself. And so it goes. Although neither the calculus nor any other branch of mathematics receives a sustained discussion, the "Handbook" does find room for dedicated chapters on Vietnam, the Balkans, and "Mathematics in fourteenth-century theology." The index includes 40 entries for "weaving and mathematics."

The casual reader might conclude that this is not really a "handbook" at all as much as an excuse for historians to expound on any esoteric topic of their fancy under the imprimatur of Oxford University Press. But that would be underestimating the magnitude of the recent revolution in the historiography of mathematics. For it is not the notion of handbook that has been redefined, but the notion of history of mathematics. The *Oxford Handbook* really *is* a handbook—not of history of mathematics in its traditional sense, but of the history of mathematics as defined by the historians populating the field today. Under Unguru's idiosyncraticist definition of history—where history is the study of "the event qua particular event"—this is as much of a handbook as there could ever be.

As another recent History of Mathematics from Oxford University Press—the *Very Short Introduction* [7]—puts it:

> Focusing on big discoveries rules out the mathematical experience of most of the human race: women, children, accountants, teachers, engineers, factory workers, and so on, often entire continents and centuries of them. Clearly this will not do (page *xvi*).

Presumably these people also sketched a few drawings and perhaps even kept a diary, so should they displace Rembrandt and Shakespeare from histories of art and literature as well? Evidently so, for anything else would be "elite history" (page 12). Unless our hypothetical reader thinks this is "clearly" a good idea, he will most likely be left with the impression that this thing called history of mathematics is of no use to people like him, who enjoy mathematics and are curious about how and why it developed.

In alienating all their traditional audiences in this way, it seems to me that modern historians are all too eager to embrace the incestuous utopia envisaged by I. B. Cohen when he wrote:

> It is surely no longer necessary to justify the study of history of science. ... Not far off is the time when historians of science will be so numerous that they may produce scholarly works which need satisfy only the members of their own profession, the only requirement being that of high standards [2, page 773].

To my mind, this is not a desirable goal but rather a sure-fire recipe for disaster. The very idea of having a field of study with "high standards" but no justification or purpose is madness, and its outcome predictable. Since the field has no purpose or justification, there are no grounds on which to evaluate or question the standards. Thus the "high standards" cannot be meaningfully defined except circularly, as synonymous with the style of work of the leaders of the field. Since historians answer to no one they are free to define their own excellence, whence their self-congratulatory attitude is perhaps unsurprising.

Furthermore, by logical necessity, sycophancy becomes an entrance requirement for any aspiring apprentice: if he does not accept the framework of "standards" sanctioned by the self-appointed priesthood in control of the field, then he is by definition not doing history of mathematics and will be turned away. And since the field acknowledges no higher purpose, there can be no court of appeal. "Before jumping on the bandwagon, the common guildsman should wait the nod of the hogen-mogens to be sure it joins the right parade," as Truesdell [8, page 589] aptly observed. This spiral of insularity, then, is the ultimate foundation of the modern historiography.

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