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The Discipline of History and the "Modern Consensus in the Historiography of Mathematics"

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Synopsis

Teachers and students of mathematics often view history of mathematics as just mathematics as they know it, but in another form. This view is based on a misunderstanding of the nature of history of mathematics and the kind of knowledge it attempts to acquire. Unfortunately, it can also lead to a deep sense of disappointment with the history of mathematics itself, and, ultimately, a misunderstanding of the historical nature of mathematics. This kind of misunderstanding and the disappointment following from it—both raised to the level of resentment—run through the paper "A Critique of the Modern Consensus in the Historiography of Mathematics." My review of that paper, sent to me blind, became a response to it. In particular, this essay attempts to clarify the nature of the historical discipline and to show that author of the Critique ends up, in effect, wanting and not wanting history at the same time.

Viktor Blåsjö, the author of "A Critique of the Modern Consensus in the Historiography of Mathematics,"[1] says much that I accept wholeheartedly. For example, he says:

> ... the historian wants to understand why things happened the way they did... (VB, page 114).
>
> We all agree that as historians we strive to understand past mathematics in its own right, not to reinterpret it by modern standards (VB, page 118).

[1] Editor's note: Blåsjö's essay is included in this issue of the *Journal of Humanistic Mathematics*. The page numbers denoted by VB refer to the pagination in [2].

How can I disagree? This is my own historian's credo. As for his "rational history," I cannot object to it insofar as it is true 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" (VB, page 115).

And, again, that:

> 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 (VB, page 115).

Nor can I dismiss out of hand his criticism of *The Oxford Handbook of the History of Mathematics* [16] that its index contains "... over 40 entries for 'weaving and mathematics'," and not one for "calculus." Though I must express some reservations here, for judging a book by its index can be misleading: if I were to judge from the index of *The Princeton Companion to Mathematics* [10], edited by Field Medalist Timothy Gowers, for example, I would have to conclude that only 12 pages of 1034 are dedicated to calculus! Still, it may well be that by including the chapter "Algorithms and Automation: Mathematics and Textiles" (accounting for all 40 index entries on the subject, by the way), Robson and Stedall gave far more emphasis than I or Blåsjö would give to weaving and mathematics.

I will leave it to readers of *The Oxford Handbook* to decide whether or not the presence of 40 entries for weaving and mathematics is symptomatic of the entire book and to judge it accordingly. But Blåsjö goes beyond the simple complaint that *The Oxford Handbook* lacks balance; his claim is that the entire field of the history of mathematics has gone astray. And, by his own statements of what "we all agree about as historians," this can only mean that scholars such as Eleanor Robson, Jacqueline Stedall, Nathan Sidoli, and, above all, Sabetai Unguru are no longer "striving to understand past mathematics in its own right" or "to understand why things happened the way they did." Rather, in his view, they are only protecting their own peculiar predilections and, worse yet, they have turned their misguided tastes into the very standards of the field so that "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" (VB, page 122).

But despite the plight of the poor apprentice I am afraid it is true that "historians *are* [my emphasis] free to define their own excellence" (cf. VB, page 122), as Blåsjö says, though this is not because, as Blåsjö implies, they have usurped the command of the field and have set themselves up as a "self-appointed priesthood." Historians do not define their own excellence by bypassing some other objective tribunal of rational non-historians. Does Blåsjö believe that, say, the excellence of mathematicians could be determined by well-intentioned chemists or perhaps a congressional panel (unbiased, naturally, by the advice of mathematicians!)? Who else besides a number theorist could determine the excellence of work in number theory, who else besides a topologist in topology, or a historian in history? I grant that decision makers in government, representatives of industry, and funding organizations may influence the direction of academic fields; yet, ultimately, how a discipline like history defines itself rests with its practitioners. And the process of definition is not one of decree or consensus, as if a poll were taken, but of continual reflection. So when Blåsjö says that "[t]he definition of history ... is the real crux of the matter" (VB, page 120), once again, I tend to agree.

I would go further and say that it is part and parcel of any intellectual discipline to think about the nature of the discipline almost as much and as seriously as pursuing the actual business of it. Thus besides his purely historical investigations, an historian such as G. R. Elton found time to write a book called *The Practice of History* [8]; E. H. Carr wrote *What is History?* [6]; R. G. Collingwood, *The Idea of History* [7]; Marc Bloch, *The Historian's Craft* [3]. And if there is any doubt as to the importance of such works to the historians who write them, remember Bloch worked on the *Historian's Craft* as he awaited execution by the Gestapo. That there are so many accounts of what history is and what is at the heart of the historian's craft underlines of course the lack of complete agreement about the question. Still, there are some commonalities.

One of these commonalities—really a *sine qua non* of history—is an acute awareness of the tension between past and present. History wants to understand the past but the historians' materials, the objects of their study, are things that have made their way into the present. For this reason Elton [8] defines history as being "concerned with all those human sayings, thoughts, deeds and sufferings which occurred in the past and have left present deposit;" and, Elton continues, "it deals with them from the point of view of happening, change, and the particular" (page 23). It is accordingly not just past or present that is essential, according to Elton, but how these are treated, namely, "from the point of view of happening, change, and the particular." The historical mode of thinking demands treating these "survivals" from the past, as Michael Oakeshott calls them (see [14]), precisely as survivals. A survival is a survival from another world. One interrogates survivors to understand where they came from—a world not conditioned by the existence of ours, yet one out of which ours grew. In this spirit, Oakeshott [15] sets out the historian's task as follows:

> What the historian is interested in is a dead past; a past unlike the present. The *differentia* [emphasis in the original] of the historical past lies in its very disparity from what is contemporary. The historian does not set out to discover a past where the same beliefs, the same actions, the same intentions obtain as those which occupy his own world. His business is to elucidate a past independent of the present, and he is never (as an historian) tempted to subsume past events under general rules. He is concerned with a particular past. It is true, of course, that the historian postulates a general similarity between the historical past and the present, because he assumes the possibility of understanding what belongs to the historical past. But his particular business lies, not with this bare and general similarity, but with the detailed dissimilarity of past and present. He is concerned with the past as past, and with each moment of the past in so far as it is unlike any other moment" (page 106).

For historians, then, understanding the past means engaging with thoughts and actions from a different time and, therefore, from a different world. This is the task of history—all history, though particularly intellectual history, which includes the history of mathematics. History is not mere chronology. Even "old-fashioned" history was never, as Blåsjö says, taking a sequence of events ABCDEFGH... and then understanding the "sequence *qua* sequence" (VB, page 114). Collingwood, whom we can take as a representative of "old-fashioned" history or at least one who is not a representative of "fashionable modern history," said all of this much better than I can:

> History, then, is not, as it has so often been mis-described, a story of successive events or an account of change. Unlike the natural scientist, the historian is not concerned with events as such at all. He is only concerned with those events which are the outward expression of thoughts, and is only concerned with these in so far as they express thoughts [7, page 217].

The point is that the "modern consensus in the historiography of mathematics," if there is consensus as clear and peremptory as Blåsjö claims, is little more than a consensus to take seriously history as historians, old and new, have thought about it. Unguru and the others did not invent a new historiography. All Unguru did in his "*locus classicus* of modern historiography" [18] was to point out—polemically I admit—that history of mathematics was being pursued as if it did not need to take into account what historians thought about history. And he was not claiming that history of mathematics had to proceed according to a specific historiography—for there is more than one (some, in fact, which would give weaving and mathematics greater weight and some which would give it less)—only that it begin with those broad commonalities without which history would not be history. His message in a nutshell was, as he put it, "[t]he history of mathematics is history not mathematics" [19, page 563].

Blåsjö's statement that "as historians we strive to understand past mathematics in its own right, not to reinterpret it by modern standards" (VB, page 118) is in fact perfectly in line with Unguru's position. This is why, as a historian, I said above that I agree with it. Unfortunately, Blåsjö seems to miss the meaning of what he wrote, and he seems to forget that history of mathematics is not mathematics, or, in his case, physics. Thus in clarifying his own view of history he writes, by way of analogy:

> When Newton brought out the underlying laws of mechanical phenomena, he certainly did not do so by studying "the event qua particular event" [he is quoting Unguru]. On the contrary, falling apples must be "drained them 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 (VB, page 119).

Then a few lines later he goes on more emphatically:

> Doing good history, it is assumed [by his antagonists, Eleanor Robson and Jacqueline Stedall], is to "paint a complex and rich picture" "sensitive to its cultural context" [forgetting that only a few pages earlier on page 115 he had said that his own rational history "does not ignore social context"!]. Had humanity taken the same approach to the study of nature we would still be in the middle ages today (VB, page 119).

We could perhaps say that Blåsjö is simply pressing for a more rigorous methodology in historical research, sharing the dream of old 19th century positivist historians like Henry Thomas Buckle (1821-1862). The passages above do have something in common with Buckle's ambition to place history on a par with the natural sciences:

> I hope to accomplish for the history of man something equivalent, or at all events analogous, to what has been effected by other inquirers for the different branches of natural science. In regard to nature, events apparently the most irregular and capricious have been explained, and have been shown to be in accordance with certain fixed and universal laws. This has been done because men of ability, and, above all, men of patient, untiring thought, have studied natural events with the view of discovering their regularity: and if human events were subjected to a similar treatment, we have every right to expect similar results [5, page 6].

Needless to say, historians since the 19th century, while still deeply concerned with the problem of rigor, have come to see the naiveté in the hope for a scientific history as Buckle and others described it.[2] Be that as it may, I do not think that this is what Blåsjö had in mind. For his remarks about Newton's apples follow on the heels of his discussion of symbolic notation in analyzing historical mathematical texts. His point is not that we should be rigorous in trying to grasp Newton's idiosyncratic thought—he deems *that* uninteresting and unimportant from the start—rather, we should abstract Newton's *mathematics* from any idiosyncratic tendencies of Newton and his

[2] Berlin's "History and Theory: The Concept of Scientific History" [1] is a very engaging discussion of the subject.

time and treat it just as we would any other mathematics today, using any tools modern or otherwise at our disposal. What would remain of history in the "history of mathematics" is not clear; however, mathematics it would be.

It is no wonder, then, that Blåsjö fails to grasp why modern historians of mathematics like Unguru studiously avoid modern mathematical symbolism in their accounts of historical mathematical texts. He thinks that such symbolism can "remove *needless obstacles* [emphasis added] to clarity and understanding" (VB, page 116). But clarity and understanding of *what*? Of the pure abstracted unhistorical (i.e. time-independent) mathematics as Blåsjö understands it? If so (as I think that it *is* so), then naturally the older mathematicians' formulations will get in the way. However, if one is interested in clarity and understanding of "the past mathematics *in its own right* [emphasis added]," can one really say that such older formulations are needless obstacles? Are they not at the very core of the historical challenge? Is not their own difficulty the precise difficulty of imagining how past mathematicians actually conceived their subject? Using modern symbolic notation sweeps away this essential historical difficulty: it allows us only to say, "in terms of our own conceptualization what so-and-so was doing comes down to this ..." It clarifies only our own mathematical mode of thought while hopelessly obscuring that of the older mathematicians.[3]

It is this, I think, that Blåsjö misses in Unguru's arguments. He claims that, by rejecting modern symbolic notation categorically, Unguru's arguments lead to "absurd consequences." Blåsjö quotes [18]: "As a matter of fact, if we use modern algebraic symbolism, this ceases altogether to be a proposition." It is worth looking at the example Unguru is referring to here. It is Euclid's *Elements*, IX.8, which states:

> If as many numbers as we please beginning from a unit be in continued proportion, the third from the unit will be square, as will also those which successively leave out one; the fourth will be cube, as will also all those which leave out two; and the seventh will be at once cube and square, as will also those which leave out five. (Translation from [12, page 390 of volume II]).

[3]Klein's famous book *Greek Mathematical Thought and Origin of Algebra* [13] gives a deep account of the rift between the non-symbolic mathematical thought of the Greeks and the symbolic mode of thought that has characterized mathematics since the 17^{th} century.

The proof is not terribly long, but it relies crucially on Euclid's definition of a part which in turn relies on the fundamental idea of "measuring" (*katametrein*). As Unguru points out (as does Heath in his commentary [12, page 292 of volume II]), if we express Euclid's proposition symbolically, what there is to prove is hard to see—the proposition becomes a mere observation. For, when one makes the transition to a modern algebraic framework, "numbers in continued proportion from a unit" is just this:

$$1, a, a^2, a^3, a^4, a^5, a^6, \ldots$$

So,

$$1, a^2, a^4, a^6, \ldots$$

are squares,

$$1, a^3, a^6, a^9, \ldots$$

are cubes, and a^6 is both a square and a cube, and so on.

Blåsjö argues that if this proves that Euclid had no algebra then we might as well assume—and this is the absurd consequence—that Viète had no algebra since Viète also makes statements that we take to be obvious in algebra, such as: "The sum of two magnitudes added to difference of the same is equal to twice the greater magnitude." But in a way, this example only strengthens Unguru's argument. For one, Viète makes it clear that these results are basic or at least preliminary. That's why he places them in a chapter entitled "Preliminary matters for calculating with species" (*Ad logisticen speciosam notae priores*). What was not basic was the idea of "calculating with species." He meant by that the revolutionary idea of calculating with symbolic entities as opposed to "calculating with numbers" (*logistice numerosa*) (see [13, pages 163ff]). As Bos rightly points out, "[using letter equivalents for general indeterminates] was not a self-evident step because it raised the question of the status and nature of these general indeterminate magnitudes and of the operations performed on them" [4, page 147]. Accordingly, what precedes the "theorem" cited above is a "proposition" which lays out the procedure, "To add the difference of two magnitudes to their sum"—and it is here that Viète gives us the symbolic statement explicitly: "*sit A+B addenda A-B: summa fit A bis*"—"Let A+B be added to A-B: the sum will be twice A". That Viète only then pronounces the theorem shows, I think, that he intended the reader to see how the new calculation of species makes this sort of relation immediate—this is exactly what algebra does best and what it was advertised to do by its early modern proponents. The use of symbolic notation for Euclid, then, not only conceals Euclid's procedures but also at the same time the revolutionary character of Viète's procedures: it does as much injustice to the early modern world as it does to the ancient world.

I hope this makes it clear that the "blanket rejection" of anachronism, as Blåsjö puts it, is not a matter of dogmatism or, worse, a kind of obscurantism; it is, rather, an uncompromising openness to what historical thinkers really thought, a commitment to listen to them as they made themselves heard, namely, through the texts they had written. This may be difficult—at times almost impossibly difficult—but it is the task historians set for themselves. Indeed, against the confidence that modern mathematics with all its harmonizing power—which is real—will explain all historical thought, the "dogmatism" of historians like Unguru goes always with a sober and humble acceptance that the mind of Euclid and Apollonius and Newton may ever hold mysteries for us.

All this, I say yet again, is consistent with Blåsjö's statement that "as historians we strive to understand past mathematics in its own right, not to reinterpret it by modern standards" (VB, page 118). If Blåsjö truly believed this, he would, no doubt, be more sympathetic to the historical scholarship of Unguru, Stedall, and Robson—scholarship which, I might add, has incontestably illuminated the history of Greek and medieval mathematics (Unguru), 16$^{\text{th}}$ and 17$^{\text{th}}$ century mathematics (Stedall), and ancient Mesopotamian mathematics (Robson).

But I think it is not truly history that interests Blåsjö. Towards the end of his critique, Blåsjö asks us to imagine "a teacher or student of calculus [who] wishes to know more about the subject's history" (VB, page 120), and then asks what such a student or teacher will gain from a book like Robson and Stedall's *Handbook*. His interest, in a way, is an educational one, and his concern is the frustration of mathematics teachers when they confront the kind of history historians read. He thinks, therefore, there should be an alternative history, one that mathematics teachers would find useful for their teaching of essentially modern mathematics. It is this, I believe, he is calling "rational history."

The clearest description Blåsjö gives of "rational history" is in the fifth paragraph, which begins:

> 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 (VB, page 115).

This is very much along the lines of Toeplitz's proposal for teaching calculus "genetically" [17]:

> The proposal presented here for curing all these [pedagogical] difficulties I have gradually developed and tested from my own lecture practice for the last nineteen years, and I hope someday to be able to present it in the form of a textbook; I should like to call it the *genetic method*. I started from the second of the three moments [the difficulty providing technical knowledge without killing students' interest in calculus] just described and said to myself: all these subjects of infinitesimal calculus studied today as the canonized requisites of the subject, the mean-value theorem, Taylor series, the concept of convergence, the definite integral, and above all the differential quotient itself, and about which questions as to "Why is that so?" "How were these arrived at?" are never asked—all these requisites, nevertheless, must have been at one time objects of absorbing investigation, exciting activity, namely, at the moment of their creation. If one were to go back to the roots of the concepts, the dust of time and the abrasions of long use would fall away from them, and they would once more stand before us full of life ([17, pages 92–93], translation from [9]).

Toeplitz was, like Blåsjö I think, seriously interested in what might help students learn mathematics and develop a taste for it. He thought that a historically-oriented approach, based on a view (as dubious as it is seductive) that the development of students' understanding of mathematics runs roughly parallel to the development of the subject itself, would address problems that were strictly pedagogical. At the same time, he was wise enough to distinguish what he was doing in his own classes and what he was proposing from history itself. He says he wants to prevent the misunderstanding that he is dealing with a "historical method":

> Not without reason, this catchphrase [i.e. "historical method"] is unpopular; the historical brings to mind the idea, which we, on the contrary, would particularly like to eliminate, of the old and antiquated, the roundabout paths often followed by research, the subjective and haphazard nature of scientific discoveries. It is especially important to me to draw a dividing line in this direction (page 94). The historian, including the historian of mathematics, has the task of recording all that has been, whether good or bad. But I want to select only the motives for those things in history which afterwards proved successful, and I want to make use of them directly or indirectly. Nothing could be further from me than to lecture about the history of infinitesimal calculus: I myself ran away from such a course when I was a student. My motive is not history, but the genesis of problems, facts, and proofs, about the decisive turning points within that genesis (pages 93–94).

Toeplitz was not always successful in keeping his "genetic approach" separate from "history" (see [9]) ; however, he was well aware that a distinction had to be made. There may be different ways of relating to the past or using the past. These are not necessarily illegitimate. At the same time, one must be careful not to fall into the error of deeming any reference to the past as history. This is why Oakeshott [15, 14] whom I mentioned above, carefully distinguished a "practical past" from a "historical past." The history of mathematics as history can be discussed and refined—and, as I stressed in this response, historians including those Blåsjö upbraids in his Critique do just that—however, one must always remember that it is history, like it or not.

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