
SPOTLIGHT

The New History of Psychology II: Some (Different) Answers to Watrin's Four Questions

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This article is mainly a response to the article by João Paulo Watrin, “The Ambiguous ‘New History of Psychology’: Some New Questions to Brock (2017)” (Watrin, 2017), which was itself a reply to my article, “The New History of Psychology: Some (Different) Answers to Lovett’s Five Questions” (Brock, 2017). Watrin (2017) suggested that previous writers have conflated the terms “critical history” and “new history.” They are said to differ, in that although the former is merely a name for a loose collection of approaches to the history of psychology, the latter involves rhetoric about the historiographical commitments of critical history. He also disputed the validity of the distinction between “old” and “new” history. I suggest that he is wrong on all these points. Watrin then poses and answers four rhetorical questions on Whig history, textbooks, critical thinking, and ad hominem arguments, and I provide alternative answers to all of them. After suggesting that our different views can be attributed to different agendas, I conclude with some reflections on how professional historians and psychologists can work together.

Keywords: new history, critical history, rhetoric, textbooks, ceremonial or celebratory history

Having discussed Lovett’s (2006) arguments and his rhetorical questions in some detail in my original article (Brock, 2017), I am going to leave it at that. I will simply say that I stand by every word that I wrote. Lovett’s main complaint is that I misrepresented his views, and yet anyone who compares what he wrote in his original article with what he now claims that he did and did not write will find some significant differences between the two. For example, he claims that he did not say that because someone has primary sources in their bibliography, they must have read them, and yet this is what he wrote in relation to E. G. Boring’s work: “Certainly, the bibliography sections of *AHEP* [A History of Experimental Psychology] demonstrate a mastery of a tremendous amount of primary source material, much of it in French or German and unavailable in translation” (Lovett, 2006, p. 21). I could provide a list of these differences, but I doubt that it would be of much interest to the reader. More importantly, my article was not directed at the views of one person. I began by referring to several authors who have cited Lovett’s article as an authoritative source, and I returned to this topic at the end. The article was directed at the views of all these authors and not just those of Lovett himself. Watrin (2017) offers some

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different arguments, and a different set of rhetorical questions, and I would like to respond to those before they are picked up by others.

Conceptual Distinctions

Watrin (2017) begins by suggesting that everyone who has previously written on this topic has “conflated” critical history and new history. The person who is most closely associated with the term “new history” is Laurel Furumoto, and it is clear that she regarded them as synonyms. In her G. Stanley Hall Lecture on the subject, she referred to “the new history, known as ‘critical history’ to some of its advocates” (Furumoto, 1989, p. 13). Both Lovett and I followed this practice in treating them as synonyms as well (Brock, 2017; Lovett, 2006). Synonyms are a common feature of our language, and so it is nothing out of the ordinary. Why does Watrin believe they are different? This takes us to another conceptual distinction that he wants to make. He suggests that critical history is not a particular approach but a loose collection of different approaches. He therefore describes it as an “umbrella term.” Given that “new history” has been used as a synonym for “critical history,” it, too, is an umbrella term, but he considers it to be rhetoric as well. What he is specifically referring to here is what he calls its “historiographical commitments”: things like externalism, historicism, opposition to Whig history, and so forth. He makes a big play on the fact that the literature on the new history typically provides a “point-by-point” account of the differences between the old and the new history, whereas the critical history literature does not. He calls this “dichotomizing rhetoric.” It is this dual aspect of the new history that provides the basis for the “ambiguity” that he refers to in his title and throughout the piece:

Leahey (1986) and Furumoto (1989) took a step further. They not only proposed “new history” as another name for critical narratives, but also draw a point-by-point distinction between “new” and “old” histories. This dichotomizing rhetoric thus specifies the particular commitments of the “new history,” suggesting that critical approaches share features such as externalism, historicism, and anti-Whiggism as their fundamental commitments. And this is where “new history” becomes distinct from “critical history.” “Critical history” is just an umbrella term, an all-encompassing name for different histories sharing a critical attitude toward psychology and its traditional narratives. “New history,” for its part, is an ambiguous term because it simultaneously refers to critical narratives in general and to that particular rhetoric about the commitments of critical historiography. (pp. 227–228)

There is a sense in which all written work that argues in favor of a point of view can be called “rhetoric,” but in suggesting that the new history is rhetoric but critical history is not, Watrin (2017) is using the term differently. He is using it in the more conventional sense of empty or meaningless rhetoric. In other words, there is no such thing as a new history. He exempts critical history from this claim by defining it in terms of a “critical attitude.” What he is suggesting is that there have been no changes in the historiography of psychology or the way in which the history is written. It is a surprising claim given that so many authors have written on the subject of the new history, and it has been generally taken for granted that there was a new history until now.

The claim that the “new history” involved a particular set of historiographical commitments but “critical history” did not should immediately strike us as implausible and an examination of the literature on critical history will show that this is not the case. Not only did these authors refer to the historiographical commitments of critical history, they referred to the same historiographical commitments that are discussed in the literature on new history. Danziger (1985) wrote,

It is hardly possible to speak of critical history unless the critical attitude extends not only to traditional authorities and to the historian’s own perspectives but also to the discipline itself. This means that no assumption is made that the historical development of the discipline must necessarily have been a progressive one. In other words, the pursuit of critical history involves a conscious attempt to avoid taking current biases for granted and using them as an ahistorical standard against which the past is to be judged (cf. Harris, 1980). (p. 100)

This is clearly a reference to Whig history and presentism. The similarities in the two sets of literature are no coincidence. Most of the literature on critical history predates the literature on new history and, as has already been noted, Furumoto (1989) regarded them as synonyms. She consequently drew heavily on the critical history literature in identifying the characteristics of the new history:

Harris defined ceremonial history as “accounts without a critical focus, stories (or cautionary tales) that have a symbolic function but do not help us understand the social forces with which we interact daily” (p. 219). By way of contrast, “a socially informed critical history of psychology,” Harris argued, is a better method. (p. 17)

This is a reference to externalism. Harris (1997, 2009) himself subsequently published a book chapter on critical history in which he used the term “new history.” It is clear from this chapter that he viewed the two terms as synonyms as well.

It is certainly true that the style of writing in the literature on the new history differs from that in the literature on critical history, but this can be explained by the different audiences that they were written for. Virtually all of the literature on critical history was written for specialist historians of psychology and, in a few cases, historians of science. Leahey’s (1986) book review in which he discussed the new history was published in the American Psychological Association journal of book reviews, *Contemporary Psychology*. It was therefore intended to reach a broad audience of psychologists rather than specialists in the field. Furumoto (1989) leaves us in no doubt as to what her target audience was. It was the vast army of psychologists who teach courses on the history of psychology, without being specialists in the field. Her point was that the research had gone off in a new direction and the pedagogy had not caught up. The whole aim of her lecture was to narrow the gap. The book chapter in which Harris (1997, 2009) used the term “new history” was aimed at an audience of nonspecialists as well. This difference alone can explain the differences in the style of writing. These authors could not assume any background knowledge of the subject and so they provided a relatively simple point-by-point account of the differences between the old history and the new. This is not “dichotomizing rhetoric,” as Watrin (2017) suggests. It was the most appropriate way of addressing an audience of this kind. Addressing an audience of specialist historians of psychology in this way would have been viewed as inappropriate, if not insulting.

I would go even further than this and suggest that the term “new history” was used because it was directed at an audience of nonspecialists. Critical history was a piece of specialist jargon that they could not be expected to be familiar with. New history was more likely to attract their attention, and it was an accurate description given that critical history was relatively new at the time. It was also more in line with the aim of narrowing the gap between the research and pedagogy of the field and, more generally, making the approach better known.

The upshot of all this is that the attempt to make a distinction between critical history and new history has no basis. For something to be conflated, it must involve two different things, and we are not talking about two different things here but two different names for the same thing. There is also no basis to the claim that critical history and new history were “umbrella terms.” When Furumoto (1989) referred to “new or critical *histories*” (p. 13), she was referring to differences in their subject matter, not their historiography. As far as the latter is concerned, there was widespread agreement on what these terms meant. Finally, if we are going to refer to the historiographical commitments of the new history as “rhetoric,” consistency would require us to do the same with critical history because it involved the same set of commitments.

Whether or not they should be described as rhetoric is a different matter. The only other evidence that Watrin (2017) offers in support of this claim is a quotation from Lovett’s article: “Lovett (2006), however, also says that ‘*many scholars* [emphasis added] exag-

gerate the differences between old and new history of psychology' (p. 17). He thus implies that 'new history' is rhetoric" (p. 228).

I responded to this argument in some detail in my original article and suggested that the differences had not been exaggerated (Brock, 2017). Watrin (2017) does not respond to any of my points. More importantly, it is an unwarranted inference. Before differences can be exaggerated, some differences must exist. This was Lovett's (2006) view. Although he believed that the differences between the old and the new history had been exaggerated, he was still willing to acknowledge that there were some genuine differences between the two. Even if he is correct in this view, it does not imply that the new history is rhetoric. It would have a rhetorical component to it, but nothing more than that.

Watrin (2017) goes on to suggest that, although Lovett was not aware that he was dealing with rhetoric, it is less of a problem in his work because he focused on the work of the authors who use the term "new history." I, however, used the term "in its full ambiguity" and ended up repeating the rhetoric that Lovett criticized. Watrin is at his most concise in making this point in the abstract:

Lovett's article does present shortcomings, it is argued that many of his claims are still valid because his critique focused on the rhetoric of "new history." Brock, for his part, uses the term "new history" in its full ambiguity and attempts to defend critical histories with the rhetoric of "new history." As a result, Brock ends up reproducing many of the problems criticized by Lovett. (p. 225)

He then goes on to say that it is this alleged difference that led him to write a reply to me:

Even though some of Brock's (2017) criticisms are rightly justified, he reproduces problems pointed out by Lovett (2006) precisely because he does not properly handle this ambiguity. The present article is thus an answer to Brock (2017). (p. 226)

Quite apart from the questionable use of the term "rhetoric," there is another problem with this claim. Although Lovett wrote at the outset that he would focus on the authors who use the term "new history," it is not what he did in practice. Watrin (2017) acknowledges this point:

While he says that his focus is limited "to those aspects identified by those scholars who use the term 'new history'" (p. 22), he cites authors like Ash (1983) and Stocking (1965) as "new historians," authors who, at least in the works cited, never mention the label "new history." (p. 228)

Lovett (2006) also discussed Danziger's (1990) *Constructing the Subject* as a paradigmatic example of the new history, and yet Danziger did not use this term either. Lovett did not limit his discussion to the so-called "rhetoric" of the new history and neither did I. There are no differences between us in this respect. As for Watrin's decision to reply to me, the real reason for that will become apparent throughout the course of this essay: He shares Lovett's dislike of critical history and agrees with most of his views.

Lovett (2006) suggested that the differences between the old and the new history had been exaggerated. In dismissing the new history as "rhetoric," Watrin (2017) wants to take the argument one step further and deny the validity of the distinction itself. He even implicates me in this view. In my original article, I made the point that not all contemporary historians of psychology can be easily characterized as old and new historians (Brock, 2017). Watrin (2017) quotes my words on this subject and comments: "If the difference between 'old' and 'new' histories does not hold in practice, why would someone insist in defending it?" (p. 229).

I did not say that the difference does not hold in practice. I said that it holds in some cases but not in others. This does not make it invalid. Let me give another example to illustrate the point. It is common to find references to "historians" and "psychologists" in this literature, and yet this distinction is not always clear-cut. There are some people who have qualifications in both, and some have taught in history departments and psychology departments at different points in their career. The fact that not everyone can be easily classified in this way does not

make the distinction invalid, any more than it makes the distinction between old and new historians invalid. There is no logical connection between the two.

It might be worth remembering some of the other changes that occurred in the history of psychology in the three decades from the 1960s to the 1980s. One of the most significant changes was the entry of professional historians into the field. There was also a degree of professionalization in psychology, most notably in the establishment of graduate programs in the history of psychology. There were also a number of psychologists whose background was in another branch of the discipline but who began to commit to the area full time. All this led to research being conducted on an unprecedented scale, and numerous professional organizations and journals were established to provide an outlet for this research. What Watrin would have us believe is that all these social changes did not lead to any changes in the way that the history was written and that it was “business as usual” as far as the historiography of psychology was concerned. The claim flies in the face of all the available evidence. One only needs to compare [Boring’s \(1929, 1950a\) *A History of Experimental Psychology*](#) with [Danziger’s \(1990\) *Constructing the Subject*](#) in order to see the differences between the old history and the new. It is therefore unsurprising that no one else has made this claim.

[Watrin \(2017\)](#) follows the lead of [Lovett \(2006\)](#) and ends by posing (and answering) some rhetorical questions. However, unlike Lovett, whose questions were directed at “the new historians” in general, Watrin’s questions are specifically directed at me. I am reluctant to provide more alternative answers to rhetorical questions at this point, but I will do it for what I hope will be the last time.

Why Can Only “Old” Histories Be Called “Whig”?

I resorted to the *Oxford English Dictionary* ([OED Online, 2016](#)) to show that Whig history does not merely involve the notion of progress; it involves a particular kind of progress that is thought to be continuous and inevitable. I then said that it was not “Whig” to acknowledge the existence of progress in a limited area of knowledge over a limited period of time ([Brock, 2017](#)). I was thinking here in particular about the professionalization of the history of psychology in the three decades from the 1960s to the 1980s. [Watrin \(2017\)](#) accuses me of having double standards:

[Brock \(2017\)](#) has double standards. In his view, historians of science can rightly use the word “Whig” to refer to views about progress in science (i.e., a limited area of knowledge that is often distinguished from philosophy, e.g.) as we know it since the early modern period to the present (i.e., a limited period of time). (p. 229)

I am happy to acknowledge that progress in science can occur. Whether or not it has occurred in specific instances is a different matter. A limited area of knowledge and a limited period of time are not on their own necessary and sufficient conditions for progress to have occurred.

With more than a touch of exaggeration, [Watrin \(2017\)](#) suggests that I contradict myself “again and again”: “[Brock \(2017\)](#), however, contradicts himself again and again. He gives other examples of how progress can occur in limited areas of knowledge over limited periods of time without being Whig” (p. 229).

These “examples” amount to the grand total of one: philosophy of science. He claims that if I accept that progress has occurred in this field, I must view it as continuous and inevitable:

When [Brock \(2017\)](#) says that positivist philosophies of science are now dead letters and that newer approaches are not the last word on the matter, he recognizes that there is a continuous and inevitable progress in the history of the philosophy of science, just like in any Whig narrative. (p. 230)

This is another unwarranted inference. He makes the same unwarranted inference elsewhere: “When [Brock \(2017\)](#) argues that externalism will naturally be better than inter-

nalism because psychologists are always immersed in a social context, is not this a view that progress in the historiography of psychology is inevitable?" (p. 230). Note that [Watrin \(2017\)](#) offers no arguments or evidence in support of these claims. He just states the first one dogmatically and presents the second in the form of rhetorical question.

There is a distinction to be made here between progress in general and a particular kind of progress that may or may not exist, but which is thought to be continuous and inevitable. Even Herbert Butterfield, who was responsible for coining the term "Whig history," made a distinction of this kind ([Butterfield, 1931/1965](#)). I do not know what it is about the distinction that Watrin finds so difficult to understand. Most people who accept the existence of progress acknowledge the possibility of regress and stagnation as well. I certainly do with respect to the history of psychology. It was out of a concern that the views of [Lovett \(2006\)](#) would lead to the field going backwards that I wrote my original critique.

What Should Historians of Psychology Do About Textbooks?

[Watrin \(2017\)](#) suggests that I argue "as if textbooks were an intrinsically flawed genre with respect to historical accuracy" (p. 231), and asks "Should textbooks be eliminated at all, as [Brock \(2017\)](#) seems to suggest?" (p. 231). This is a gross misrepresentation of my views. Prior to my recent retirement, I taught history of psychology courses for over 20 years. With the sole exception of an advanced course, in which only professional literature was used, I had at least one recommended textbook in all of these courses. The textbook that I used in later years was [Graham Richards's \(2010\) *Putting Psychology in Its Place*](#). I also wrote some enthusiastic comments for the jacket blurb of one textbook ([Jansz & van Drunen, 2004](#)), and I have more recently published a glowing review of another ([Brock, 2013a](#)). These are not the words of someone who believes that textbooks are intrinsically flawed and should be eliminated.

I made the point that the vast scope of history of psychology textbooks makes it impossible for them to be based in their entirety on primary sources. This is hardly earth-shattering news. We do not usually consider textbooks to be original contributions to knowledge. They are usually summaries of existing knowledge that are written for students.

These remarks were made in the context of discussing the accuracy of [Boring's \(1929, 1950a\)](#) textbook, and the state of the existing knowledge at the time is an inherent problem with it. Prior to the 1960s, the history of psychology was thought to be an exclusively pedagogical subject and very little research was carried out. There was consequently little reliable research on which the textbook could be based. This is why he got his account of Wundt so badly wrong.

We have come a long way since Boring's textbook was regarded as standard reading. More research on the history of psychology has been carried out, and there are now full-time specialists in the field, something that Boring himself was not. Some of the modern textbooks have been written by respected figures in the history of psychology, and this is all to the good.

The problems of the genre have not gone away, however, and this is largely because professionalization has affected only a small corner of the field. Some of the most popular textbooks on both sides of the Atlantic have been written by people who are not specialists in the area. This is because most of the teachers of the history of psychology course are not specialists in the area either, and they can more easily identify with a textbook that has been written by someone who is in a similar position to themselves. What usually happens is that they read a few textbooks in preparation for the course and then add their own two cents to the story. The resulting lecture notes are then published in the form of another textbook. This is how the inaccurate view of Wundt that Boring propagated was repeated

in textbooks right up to the 1980s (Brock, 1993). It is because they were based on other textbooks rather than reliable research.

This is not “history” in the way that we usually understand the term. It has more in common with the oral histories that have been studied by anthropologists, in that it consists of stories that are passed on from one generation to the next. However, as anthropologists have noted, they are not just stories that are designed to entertain, though this can sometimes be a factor in their transmission as well. They often have the character of parables, in that they explain the origins of a moral rule and warn of the dire consequences that will ensue if this rule is broken. The various myths surrounding the demise of introspection in psychology are a good example of this phenomenon (Brock, 1991, 2013b).

One point that cannot be overlooked here is that publishers are primarily interested in fulfilling the demands of the market, and even respected historians of psychology who write textbooks have to take this into account. There are some topics whose inclusion is nonnegotiable, and the fact that most psychology departments are not willing to devote more than one course to the history of psychology means that they are usually expected to produce comprehensive accounts. Watrin (2017) might point out that most of them are called *a* history of psychology rather than *the* history of psychology, but this is mere hair-splitting. Most textbook authors are aware that they cannot include every last detail about the history of psychology in their books, but they still try to be as comprehensive as they can.

Comprehensive accounts give the false impression that we already know everything there is to know about the history of psychology, and yet there are still many topics for which there is no reliable research. One of the problems here is that the demand for history of psychology textbooks is vast compared with the number of active researchers in the field. It is more a matter of the tail wagging the dog rather than the other way around.

In response to the question “What should historians of psychology do about textbooks?” my answer is “Use them but use them wisely.” Textbooks in this area vary enormously in their scholarly merits compared with the textbooks in other fields. One way of overcoming this problem is to pay careful attention to the qualifications of the author(s). For example, have they contributed to the research of the field? It is unlikely that someone will be familiar with the research literature if they have not contributed to it in any way. Above all, do not regard textbooks as the be-all and end-all of the history of psychology. They are pedagogical instruments and should not be confused with the research.

Before I leave this section, I would like to address another misrepresentation of my views. Watrin (2017) has got the impression from somewhere that I believe that if the traditional topics are not covered in a history of psychology course, they will be covered by someone else:

Whether we like it or not, prominent people, methods, and theories maintain their relevance to psychologists’ current practices. Above all, these themes need to be addressed with the same historical rigor usually applied to marginalized psychologists, methods, and ideas. Contrary to Brock’s (2017) claims, there is absolutely no guarantee that their history—especially a critical history—will be a topic in other courses besides the traditional history of psychology course. (p. 231)

My remarks were made in relation to the view that the role of the history of psychology instructor is to socialize new recruits into psychology, and that the aims of the new history need to be reconciled with this role. I said that I did not consider this to be my role and that my colleagues were already doing a good job in this regard. It is possible that some psychologists believe that *only* the history of psychology can achieve this end. If they do, I have some bad news. There are many psychology departments around the world that do not offer the history of psychology in any way, shape, or form, and yet they still succeed

in socializing their students into the discipline. My point was that this process was going to occur with or without my help.

Why Is “True” Critical Thinking a Privilege of Critical History?

Rhetorical questions tend to put the respondent in a “no-win” situation and this one is no exception. The simple answer is that I do not believe that “true” critical thinking is a privilege of critical history and I have never suggested that it is. I did not even use the term “critical thinking”.

Watrin (2017) is not a reliable guide to my views. He claims that “[Brock] argued that authors such as Harris (1980) do offer a precise definition of the kind of critical thinking they intend” (p. 232). He then goes on to discuss Harris’s article and suggests that his definition of critical thinking is not precise. That led me to do a word search of my article. The word “precise” does not appear anywhere in it. The word “definition” appears once and it is in a quotation rather than the text (Brock, 2017). As for Harris himself, he did not use the term “critical thinking” either (Harris, 1980). The basis of this claim is a statement by Lovett (2006) to the effect that Harris (1980) and Furumoto (1989) do not provide “further elaboration” of what they mean by “ceremonial” or “celebratory” history (p. 20). This has somehow been transformed into a precise definition of critical thinking.

In faulting us for failing to provide a precise definition of critical thinking, Watrin (2017) is asking us to do the impossible. He provides no definition of critical thinking either, precise or otherwise. The only thing that is clear is that it has nothing to do with critical history in the way that we usually understand the term. He considers this kind of history to be “thoughtless” and finds support in Lovett’s (2006) article for this view:

If we are not careful, “critical history” becomes a thoughtless inversion of all of our current thought structures and bases its claim to validity on the simple fact that it is the opposite of what used to be thought. This may be critical in a certain sense, but it does not involve any critical thinking. (p. 32)

Lovett (2006) distinguished two kinds of critical history. One was based on good scholarship, and the other on challenging psychology and its traditional history. He approved of the former and disapproved of the latter. I suggested that it was impossible to separate the two. As we have seen with the revisionist accounts of Wundt, you cannot have good scholarship without challenging the traditional view (Brock, 2017). Watrin (2017) remarks,

[Brock] even criticizes Lovett (2006) for pointing out “the kind of critical history that he considers acceptable and the kind of critical history that he does not” (p. 212), as if there were only one “true” form of critical history. Nevertheless, how can any form of critical thinking be above criticism itself? Above all, how can critical thinking be a privilege of a particular trend in the historiography of psychology? How can a particular tradition of a particular field of human knowledge have monopoly over a quality that, in thesis, every human being should be allowed to develop? (p. 233)

I hope the reader can see a common pattern here. He repeats my words and then draws a totally unwarranted inference from them. The inference is then followed by a series of rhetorical questions containing even more unwarranted inferences. The result is that he is criticizing the views of a fictional straw man that do not even remotely resemble my own.

Danziger (1985) suggested that “no one wishes to be identified as an uncritical historian” (p. 99). The whole aim of this section is to enable Watrin (2017) to hijack the term “critical history” and replace it with a relatively toothless and less threatening version that is based on this fuzzy notion of “critical thinking,” which is effectively thinking that stays within accepted boundaries. If it strays outside them, it can be dismissed as “thoughtless.”

I do not believe that there is one “true” critical history. I do believe, however, that this use of the term “critical history” is so far removed from its usual meaning that it is

misleading to use it to refer to this kind of history. In refusing to challenge psychology and its traditional history, it is ceremonial or celebratory history by a different name.

How Can Ad Hominem Attacks Contribute to the History of Psychology?

I made the point that Lovett has no qualifications and very little background in the history of psychology (Brock, 2017). Watrin (2017) quotes my words on this subject and writes, "Anyone with some knowledge of logical fallacies will recognize that this is an ad hominem argument. It attacks Lovett, not his reasoning. Ad hominem attacks constitute a fallacy of relevance because their premises are logically irrelevant to their conclusion" (p. 233). The term "ad hominem fallacy," as it is used by philosophers, denotes criticism that is directed at the person *instead of* their views. This is what makes it fallacious. Anyone who has read my article will know that the vast majority of it is devoted to discussing Lovett's (2006) views. The comments about Lovett himself are confined to a few paragraphs at the end, and their sole purpose is to shed some light on the views that have already been discussed. A more appropriate expression in this context would be the "ad hominem taboo" (e.g., Brandt, 1971).

We freely discuss people's qualifications when we are considering them for a job. If we engage the services of anyone from a motor mechanic to a divorce lawyer, we will want to know that they have the appropriate qualifications in their field. We also discuss the qualifications of politicians and others who are standing for elected office. Despite this, discussing someone's qualifications in the context of academic debate is usually considered taboo. That I do not agree with this taboo will be obvious from the fact that I did not observe it. I consider the issue of qualifications to be important.

Most of us have doctoral degrees, but our doctoral degrees are in different subjects. My doctoral degree is in history and theory of psychology, and I would not dream of setting myself up as an expert on cognitive or developmental psychology. The same situation does not apply in reverse. When it comes to the history of psychology, qualifications are thought to be optional at best.

If we engage the services of a qualified specialist, we can usually expect a minimum standard of work. This is not the case when people have no qualifications. There are psychologists who have become competent, and in some cases, excellent, historians through self-study over a period of years, but this is by no means true of all. The result is that much of the work on the history of psychology is distinctly "amateurish." One of the problems here is that the people who are judging this work are no more qualified than the people who have written it.

We can rephrase the question so that it reads, "Would it help the history of psychology if it was like any other area of psychology in that it was mainly pursued by qualified specialists?" The answer is obviously "yes." However, the reluctance of psychology departments to support training programs in the area, or even to hire historians of psychology, makes it an unrealistic goal. I would therefore suggest that it would help the history of psychology if every psychologist who is not a trained specialist in the area would grasp one simple point: *history is not psychology*. It is a different subject and it requires a different set of knowledge and skills. Having accepted this point, they can then decide if they want to undergo the arduous process of retraining as a historian or if they prefer to stick to what they already know.

The subject of qualifications will always be off-limits if it is characterized in terms of "ad hominem attacks." Contrary to Watrin's (2017) assertions, I am not suggesting that anyone should be prevented from writing history or from expressing their views on historiography. What I am suggesting is that we have an inadequate system of quality control and that it is due to a lack of institutional support.

Watrin (2017) ends this section with an ad hominem argument of his own. He suggests that my qualifications in history and philosophy have not prevented me from making the

same mistakes and distortions that Lovett made. As with the contradictions that I made “again and again” (p. 229), these “mistakes” (p. 234) and “distortions” (p. 234) refer to a single case.

What he is specifically referring to is my point that Lovett (2006) omitted a sentence from a quotation from Boring (1929), and that its omission gives a misleading impression. The words “there have been no great psychologists” (p. 660) give the impression that he did not write about “great men,” but the missing sentence, “Psychology has never had a great man to itself” (p. 660), shows that he simply meant that the great men in his history were not exclusively psychologists and had connections to other disciplines. Watrin (2017) points out that I left out the last sentence of Lovett’s quotation, “[Wundt] ‘founded’ experimental psychology, but in that he was more the instrument of the times than an originator” (Boring, 1929; p. 660) and comments,

Boring (1950a, 1950b) thus developed a balanced and relational view about great men and the *Zeitgeist* in which they lived. His perspective does not fit the dichotomizing rhetoric of “new history” that opposes great men to their context and internalism to externalism. Boring’s (1929, 1950a, 1950b) theoretical refinement does not allow Brock (2017) to simplistically conclude that the former’s thought is an archetypal celebration of great men. (p. 234)

I left the last sentence out of Lovett’s quotation because it was not relevant to the topic that was then being discussed, namely, Boring’s use of the term “great men.” I then went on to discuss Boring’s use of the term “*Zeitgeist*” at length and suggested that it involved little more than paying lip service to the existence of a context (Brock, 2017). This is why Boring’s history has quite correctly been portrayed as a celebration of great men. It is Watrin’s (2017) failure to appreciate the difference between externalism and appeals to the workings of a mysterious “ghost” that makes the word “simplistic” more appropriate to his account.

Different Agendas

Although Watrin (2017) adopts the common rhetorical strategy of portraying himself as evenhanded, he and Lovett have a great deal in common. In addition to their shared predilection for rhetorical questions, they are both psychologists who dislike critical history and that is the source of their opposition to my views. Here I am using “critical history” in the usual sense of the term and not the kind of cross-dressing to which they refer.

They also have a shared interest in defending and rehabilitating the reputation of E. G. Boring and the traditional history of psychology that he represents. This is why Watrin (2017) claimed that the professionalization of the field from the 1960s onward did not lead to any changes in its historiography. He wants to suggest that the new history is not an improvement on the old in terms of its scholarly merits, and that the old history is at least its equal, if not better, in this respect. His comments on Boring in the previous section provide an illustration of this view. It also explains why he is so keen to defend the accuracy of textbooks.

Watrin (2017) ends with another rhetorical flourish. He portrays our differences in personal terms and accuses me of “Bulverism,” which involves assuming your opponent is wrong and then explaining the error. He apparently is on the side of cooperation and balance:

How can critical histories and the history of psychology as a whole benefit from positions like Brock’s (2017)? The field struggles with debates about its future for decades and there is still some insistence on a rhetoric that stimulates division rather cooperation, antinomies rather than balance. (p. 235)

There is nothing about the tone of his article that suggests cooperation and balance to me. It is a polemical attack on critical history and everything that it represents. If he and Lovett

want to criticize this kind of history, they can hardly expect a positive response from the people who identify with it. The irony here is that they themselves are defending psychology and its traditional history against the criticism of critical historians. As I put it in my original article, “If you criticize someone’s family, their home town or their country, the chances are that you will get a negative response. The situation will be no different if you criticize their discipline or their profession” (Brock, 2017, p. 213).

Historians and Psychologists

I have clearly offended [Watrin’s \(2017\)](#) professional sensibilities:

As seen before, for [Brock \(2017\)](#), the fault is always on Lovett and on psychologists in general. It is they who use the notion of Whiggism in a novel and idiosyncratic way. It is they who do not accept critical histories. And, above all, it is they who show a limited tolerance for the history of psychology. (p. 233)

He then suggests that my views overlook the possibility of collaboration between historians and psychologists and ends with another rhetorical question:

[Brock \(2017\)](#) also seems to neglect the value of the continuous debate about how psychologists and professional historians can write together the history of psychology (see, e.g., [Barnes & Greer, 2016](#); [Weidman, 2016](#)). If the future of the history of psychology lies in the hands of psychologists and historians, how could ad hominem attacks between them contribute to this future? (p. 234)

Once again, he has got the wrong end of the stick. The articles by [Barnes and Greer \(2016\)](#) and [Weidman \(2016\)](#) were published in a special issue of this journal that I guest edited and both were written at my request. I am very much in favor of historians and psychologists working together.

Another point that needs to be made here is that, although I have a Master’s degree in history and philosophy of science, my doctoral degree is in psychology and I spent my entire career teaching in psychology departments. Some people might be forgiven, therefore, in thinking that this is not a conflict between historians and psychologists but another chapter in the ongoing debate between the psychologists who favor critical history and the psychologists who favor a more celebratory approach (e.g., [Danziger, 2013](#); [Robinson, 2013](#)).

There is a sense in which it does represent a conflict between historians and psychologists. The older history of psychology that E. G. Boring represents is a psychologist’s history, and it has come to be known as “insider history” (e.g., [Danziger, 1990](#)). One of the most important developments that preceded the rise of the new history was the entry of professional historians into the field. The emphasis on primary sources is one of the basic scholarly standards of the history profession. Other characteristics that we associate with the new history, such as externalism and the critique of Whig history, also have their origins in history rather than psychology. Furthermore, historians do not have a vested interest in promoting psychology, either to their students or anyone else, and so they tend to adopt a more critical approach. In short, what has come to be known as the “new history” is very much a historian’s history, as opposed to the “insider history” that preceded it.

This is why [Lovett \(2006\)](#) came to the mistaken conclusion that the new history involves taking the subject out of the psychology department and into the history department. Despite his continued attempts to defend the mistake ([Lovett, 2017](#)), the psychologists who write critical history are not planning to abandon the field so that it can be taken over by historians. They have, however, adopted many of the views of historians with respect to writing history. Clearly, the psychologists who have taken this step will find it easier to make common cause with historians than the psychologists who reject these views and want to defend and rehabilitate the older forms of insider history.

One of the reasons why historians and psychologists can benefit from working together is that they bring different knowledge and skills to the field. As Weidman (2016) points out, historians are usually more skilled at writing history, whereas psychologists tend to have a deeper knowledge of the technical aspects of their discipline. Mentioning the knowledge and skills that each one possesses implies that there are also knowledge and skills that they might lack. This is not usually a problem for historians. If a historian wants to write about a technical subject, such as the history of statistics, he or she will usually be aware that a good knowledge of statistics is an essential prerequisite for the work. The same situation does not always apply in reverse. There are some psychologists who realize that history is a different subject and that it requires a different set of knowledge and skills, but there are also many who do not. They are either unaware that qualifications are needed or they assume that their qualifications in psychology qualify them to be historians as well. If the psychologists whose background in history is weak to nonexistent are genuinely interested in establishing a dialog with historians, lecturing them on how to write history is not a good way to start.

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