

THE NEW HISTORY OF PSYCHOLOGY: Some (Different) Answers to Lovett's Five Questions

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The professionalization of the history of psychology from the 1960s led to significant changes in the way that history was written. Several authors tried to summarize these changes in the 1980s, and Laurel Furumoto's (1989) G. Stanley Hall lecture, "The new history of psychology" is the best-known example of this genre. This journal published a critique of the new history by Benjamin R. Lovett (2006) with the title, "The new history of psychology: A review and critique," and it is still being cited as an authoritative source. The article consists of 3 parts. First, the author attempts to show that the new history is not as different from the old as its proponents claim. He then discusses some problems that he considers to be unique to the new history, and he presents them in the form of 5 questions for the new historians, which he then goes on to answer himself. Finally, he discusses the problematic relationship between critical history and psychology. This article is a reply to Lovett's article. The author argues that the new history is different from the old in every way that Lovett claims that it is not. It critically analyzes Lovett's answers to his own 5 questions and offers some alternative answers to these questions. It also suggests that many psychologist-historians are opposed to new history of psychology, especially in its critical versions, and that this explains why Lovett's article has been uncritically received.

Keywords: historiography, internalism, presentism, Whig history, critical history

2006 was a difficult year for me. A close relative had died in the previous year after a long battle with cancer, and it left me with a backlog of publishing commitments that were long overdue. Thus when I saw the article by Benjamin J. Lovett (2006), "The new history of psychology: A review and critique," I thought that it merited a reply, and I was tempted to write one. On further reflection, I decided that my overdue commitments should take priority. I comforted myself with the thought that someone else would write a reply. Ten years on, nobody has. If the article had fallen into obscurity in the meantime, I would have given it no further thought, but it has now become standard reading.

The article itself can be downloaded from several websites, including the website of the

National University of Cordoba in Argentina.¹ Clearly, the people who have made it available in this way consider it to be of unusual importance. A search of the Internet will also show that it is required reading on course syllabi.² In their article, "History's mysteries demystified: Becoming a psychologist-historian," Vaughn-Blount, Rutherford, Baker, and Johnson (2009) list it without comment in their recommended readings.

Perhaps more interesting is its use as an authoritative source. Rappard (2008) quotes with approval Lovett's remarks that the new history has been accepted uncritically and his warning that its critical tendencies will only alienate psychologists. Rappard had already criticized the new history of psychology and had engaged in polemics with Kurt Danziger over the issue

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¹ <http://www.psyche.unc.edu.ar/wp-content/uploads/news-history-lovett.pdf>

² See http://individual.utoronto.ca/solovey/courses/HPS5012_Reading%20List%209-8-12.pdf and https://www.ndsu.edu/cule/pdfs/capstone/syllabi/pdf/PSYC_480_Syllabus_Spring_2010.pdf.

(Danziger, 1997; Rappard, 1997). He concludes by saying that his “sympathies clearly rest with the ‘old, uncritical history of psychology.’”

The article has also been cited in textbooks, always in a positive way. For example, Hergen-hahn and Henley (2013) write: “As Lovett (2006) observes, no matter how much historicism is emphasized, presentism cannot be completely avoided” (p. 2). They then quote extensively from the article. King, Woody, and Viney (2016) are even more approving:

Lovett (2006) believes the new history has been accepted uncritically despite few actual differences between works from the two camps. Indeed, Lovett cited works from scholars who embrace the new histories as little more than accounts of a succession of achievements of past luminaries. Further, he demonstrated that some older histories draw heavily from primary sources and archival materials. Based on Lovett’s analysis, any catalog of differences between the new and the old histories is a distinction without a distinction. (p. 11)

Even Walsh, Teo, and Baydala (2014), whose work is titled, *A Critical History and Philosophy of Psychology*, see Lovett’s article as an opportunity for self-criticism:

While critiquing the assumptions of traditional historians, critical histories of psychology should not be exempt from reflecting on their own historical context and premises (Lovett, 2006). Critical historians, for instance, could misuse history by attempting to over-compensate for conventional historians’ bias of using the past from the perspective of the rich, the famous and the successful. (p. 19)

The positive citations continue. Like van Rappard, Saulo Araujo is not enamored of the new history of psychology. In the introduction to his recent book, *Wundt and the Philosophical Foundations of Psychology* (Araujo, 2016), he provides an introduction to some of the recent literature on the history of psychology. He then writes:

These new approaches to the history of psychology have brought to light important aspects of psychological theory and practice, such as the influence of psychological discourse on society and the political and ideological uses of mental testing. Despite all their merits, however, many of these studies leave much to be desired in methodological terms. For example, Lovett (2006) has shown that the dichotomies created by historians (e.g., naive vs. critical, amateur vs. professional, use of primary vs. secondary sources etc.) are exaggerated, if not invalid. More, he identifies in the new history an uncritical acceptance of technical assumptions (e.g., Kuhn’s philosophy of science) and similar errors attributed to the old history (e.g., forms

of Whiggism). He adds that “the new history is not so different from the old as its proponents would argue” (Lovett, 2006, p. 26) and sometimes “it is difficult to distinguish between careful judgement and careful pre-judgment.” (Lovett, 2006, p. 30) (p. 13)

I doubt that historians of psychology have uncritically accepted the technical assumptions of the new history of psychology. What I do know is that Lovett’s article has been uncritically accepted, and it is this uncritical acceptance that has finally provoked me into writing a reply.

The Background

Most of the institutions that we associate with the history of psychology were founded in the three decades from the 1960s to the 1980s. This includes most of the professional societies, journals, archives, and graduate programs in the field. What lay behind it was the professionalization of the area. The name E. G. Boring is so closely identified with the history of psychology that it is easy to forget that this was not his main interest. He was an experimental psychologist who specialized in the areas of sensation and perception, and most of his publications were in this field. He wrote in his autobiography that he mainly worked on his textbook during the summer vacations (Boring, 1952). This was typical of all the psychologists who worked on the history of psychology prior to the 1960s. Another notable feature of this work is that it was centered on textbooks. Original research papers were relatively rare.

During the 1960s, a small number of historians of science moved away from the traditional emphasis of their discipline on the physical and biological sciences and began to take an interest in the history of psychology. There were also psychologists who came to regard it as their main or only area of teaching and research. Many of these had obtained their qualifications in other branches of psychology and were self-taught. The graduate programs that were established from the 1960s onwards began to produce a different kind of psychologist who identified with the area at an early stage of their career and had received training in historical research. The archives, the professional societies, and the journals that were established helped to facilitate their research and to provide outlets for it.

Needless to say, these changes led to changes in the content of the history of psychology itself. The fact that research was being carried out on an unprecedented scale led not only to new knowledge, but also to corrections of mistakes in the work of the past, most notably in relation to the so-called founder of psychology, Wilhelm Wundt. Largely due to the influence of historians of science like Robert Young (1966), scholarly standards began to improve, and a whole new way of writing history began to appear.

As significant as these changes were, the number of specialist historians of psychology was small and most psychology departments continued in the traditional way by regarding the history of psychology as a pedagogical field that was usually taught by someone whose research was in another branch of psychology, often with the help of textbooks that were written by psychologists who were not specialists in the area either. The end result was that a gulf developed between the scholarly and pedagogical aspects of the field, and some of the new specialists tried to remedy this situation in oral presentations and publications that were designed to inform the nonspecialists, especially those who were teaching the history of psychology, about the new developments that had taken place. The best known example of this genre is a G. Stanley Hall lecture with the title, "The new history of psychology" that was given by Laurel Furumoto at the annual meeting of the American Psychological Association in 1988 and published in the following year (Furumoto, 1989).

Lovett's article is a reply to this literature. By his own account, the article is in three parts. First, he compares the old and the new. He accepts that some differences between them exist but, as the quotation from Araujo's book shows, he goes on to suggest that "the new history is not so different from the old as its proponents would argue" (Araujo, 2016, p. 26). The second part consists of some problems or limitations that Lovett considers to be unique to the new history, and he expresses them in the form of "Five Questions for the New Historians" (p. 24). They are actually rhetorical questions since Lovett goes on to answer them himself. The third part deals with the critical aspects of the new history, and Lovett expresses his views on the kind of criticism that is acceptable

and the kind of criticism that is not. I will deal with each one of them in turn. First, the comparison of the new history and the old.

Differences That Lovett Accepts

The new historians tended to refer to the older histories as internalist, presentist, and Whig. Lovett accepts all of these differences:

First, old history is notably internalist; some of these histories contain minute biographical details but do not mention any broad social or political changes of the times the psychologists lived in . . . In addition to internalism, these old histories tend to be presentist, a term that refers to the use of recent knowledge both to better understand historic events and to choose historical questions worthy of inquiry . . . Not only does the present serve as a lens with which to view the past, but also psychology is viewed as having made progress when compared with the past. This third feature common to old history texts is termed a "Whig" conception of history, being based on the assumption that over time, psychology progresses and that today's psychologists know more (in an objective sense) than their forebears. (p. 20)

So far so good. Lovett also criticizes Furumoto (1989) and Leahey (1986) for failing to mention what he considers to be a fourth difference:

The descriptions of Furumoto and Leahey failed to mention one last dichotomy thought by many (e.g., Stocking, 1965) to differentiate the old and new histories: the training of the historian. Old histories tend to be penned by psychologists without formal training in historiography ("amateur historians"), whereas new histories tend to be written by scholars whose training is in history of science or even general history (e.g., O'Donnell, 1979; Zenderland, 1998). . . . In sum, then, the new history of psychology can be seen as taking the history of psychology out of the psychology department, out of the hands of those amateur historians who are interested in worshipping psychological heroes and teaching their psychology majors to do the same, and bringing the discipline into the history department. (p. 22)

Lovett goes on to discuss Kurt Danziger's (1990) *Constructing the Subject* as an example of the new history, and he notes that Danziger is not a historian but a psychologist:

When it comes to other features of new history, it is less clear where Danziger (1990) falls. Although many primary sources from history were cited, Danziger's training is in psychology rather than history . . . *Constructing the Subject* is squarely within the new history framework but does not fulfill all of the criteria exactly. (p. 23)

These quotations show that Lovett has misunderstood what the new historians were saying about the professionalization of the field. It does not refer to a move out of the psychology department and into the history department. There are certainly some historians involved but most of the writers whose work Lovett discusses—Pickren and Dewsbury (2002), Furumoto (1989), Harris (1980), Jones and Elcock (2001), Leahey (1986), Samelson (1999)—are psychologists and one of them, Rose (1996) is a sociologist. What these authors are referring to when they talk about the professionalization of the field is the move away from part-time hobbyists like Boring to full-time specialists, irrespective of the discipline to which they belong.

Differences That Lovett Does Not Accept

Lovett bases his claim that the new history is not so different from the old as its proponents claim on three things. I will discuss each one of them in turn.

Both the New History and the Old Are Critical

Lovett writes

The new historians have suggested some other features but without providing much support for their claims. For instance, the claim that old histories of psychology are “celebratory” or “ceremonial” is made frequently by the new historians, including Furumoto (1989) and Harris (1980). No further elaboration of what these words mean is given by either author, but the words connote a lack of criticism. However, Boring’s (1950a) *AHEP* is hardly devoid of critical tendencies. Historical figures’ ideas are taken seriously, with parts of their doctrines elaborated in some detail for the purpose of criticism . . . Current undergraduate textbooks are certainly not without criticism . . . In this connection, we might note that it is difficult to write a Whiggish history without critique—to demonstrate progress, someone before us must have been wrong. (pp. 20–21)

The last statement is not strictly true. If someone believes in scientific progress and attributes that progress to “standing on the shoulders of giants,” they are not criticizing their predecessors. Quite the contrary. Something else that is not true is the claim that neither Harris (1980) nor Furumoto (1989) provide any further elaboration of what they mean by ceremonial and critical history. For example, Furumoto (1989) writes:

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, which instead of focusing on the personal characteristics and intentions of historical figures, such as J. B. Watson, for example, asks “*historical* questions about subjects such as Watson’s career, the acceptance of behaviorism in American psychology and its subsequent institutionalization.” (p. 17)

It stands to reason that Boring and other textbook writers will have their views on psychology, and this will lead them to agree with the views of some historical figures and disagree with others. This is not the point. What Harris (1980) is saying is that the history itself lacks “a critical focus,” which is not the same thing. Thus the issue is not about the presence or absence of criticism but the kind of criticism that is being made.

Lovett devotes almost the entire conclusion of his article to the critical aspects of the new history:

The problem comes when the word critical becomes an antonym of “ceremonial,” as it has in recent years (e.g., Furumoto, 1989). At this point, “critical” history takes on a purpose beyond increased accuracy—it must actively criticize psychology and its histories, disparaging psychology’s “pretensions” to scientific objectivity and deprecating past histories for not being “critical” enough. (p. 32)

Comments like these show that Lovett was well aware that the new history is critical in ways that the old history was not. Indeed, it seems that the critical aspects of the new history are what he objects to most. It is only by failing to mention these differences here that he can argue that the new history is not so different from the old in this respect.

The Old History Was Not About Great Men (But the New History Is About Great Women and Great African Americans)

Another claim by the new historians is that the old histories are centered on “Great Men.” Lovett takes issue with this claim and writes:

AHEP, taken by so many to be the prototype of the old “Great Man” history, concludes with Boring (1929) telling us that “There have been no great psychologists. . . . Wundt was not a great man on the order of Helmholtz or Darwin.” (p. 660)

This might appear to resolve the matter. Boring's work was about psychologists, and he did not think that there were any great psychologists. Unfortunately for Lovett, it does not. As the ellipsis in the quotation indicates, there is a sentence missing and the sentence is so short that it is unlikely that it was excluded for reasons of brevity. This is what Boring (1929) wrote:

There have been no great psychologists. Psychology has never had a great man to itself. Wundt was not a great man on the order of Helmholtz or Darwin. (p. 660)

Putting the missing sentence back into the quotation gives a different impression. It is not that Boring did not base his history on "Great Men"; all he is saying is that the great men in his history were not exclusively psychologists but had connections with other disciplines.

It is not just the quotation that is selective. Having cited the second edition of Boring's textbook from 1950 twice in the preceding paragraph, Lovett took this quotation from the first edition of 1929. Why this sudden switch? Perhaps Schultz and Schultz (2004) can shed some light on the issue:

In 1929, E. G. Boring wrote in his textbook, *A History of Experimental Psychology*, that psychology had no truly great proponent of the stature of Darwin or Helmholtz. In his second edition, published 21 years later in 1950, Boring revised his opinion. (p. 433)

Boring (1950a) wrote in the second edition that he regretted having written the words, "There have been no great psychologists" in the first edition. He went on to speak of Freud in glowing terms: "Psychologists long refused him admission to their numbers, yet now he is seen as the greatest originator of all" (p. 743). Further down the same page, he tells us that he considers Helmholtz, Darwin, James and Freud to be "very great men" (p. 743).³

Boring's views on the subject can be clearly seen in an article with the title, "Great Men and Scientific Progress" that was published in the same year as the second edition of his textbook (Boring, 1950b). These are some of the words from the opening paragraph:

The Great-Man theory of history is as old as history . . . but its age does not render it obsolete. We still look to great men for a partial explanation of history . . . The Great man theory cannot be wrong. It exposes too obvious a truth about society. (p. 339)

In short, the claim that Boring did not write about "Great Men" is quite simply false.

Anyone who is familiar with Boring's textbook will know that he complemented the notion of the great man with that of the *Zeitgeist*. Thus, Samelson (1999) writes: "The 'Great Man vs. *Zeitgeist*' formula has seen a deserved demise (almost) everywhere" (p. 248). However, Lovett refers to a quotation by Leahey (1986) in which he says, "The new history favors *zeitgeist* interpretations" (p. 649) and suggests that this is evidence to show that the new history is not so different from the old. Why the contradiction? I can only guess what Leahey was trying to say here, but I suspect that he was referring to fact that the new history is more externalist than the old. Whatever he meant to say, it was an unfortunate choice of words. No self-respecting externalist historian would ever use the term, *Zeitgeist*. It has been rightly characterized as the lazy historian's way of contextualizing historical events. There is no need to do all the hard work that is involved in relating these events to the social and historical context in which they occurred. You just invoke the spirit of the *Zeitgeist*, and there is nothing more to do (Ross, 1969). Thus, there are genuine differences between the old and the new history in this respect as well.

In focusing the discussion on Boring in this section, Lovett seems to have forgotten the opening paragraph of his article:

In his preface to *The Great Psychologists*, Watson (1963, p. vii) wrote that "great men in psychology live on in the work for which they are an inspiration in the field" . . . But both the terms and the reverent tone of that passage would be difficult to find in the contemporary history of psychology literature; even the use of "great" in the book's title might be called into question. (p. 17)

This quotation shows that Lovett was aware that the notion of "Great Men" was the focus of at least some of the older work on the history of psychology. Indeed he complains that it has become increasingly difficult to write the history of psychology in this way.

³ Boring had some strange ideas on who was a psychologist and who was not. Freud's background was in medicine and psychiatry, not psychology, while Wundt held a chair in philosophy and wrote on traditional philosophical topics like logic and ethics. It is no coincidence that he named his journal, *Philosophische Studien* [Philosophical Studies].

In a final parting shot, Lovett suggests that the work of the new historians can be characterized as “Great Women” and “Great African American” history:

Furumoto (1989) noted that one emphasis of the new history of psychology is to highlight contributions of underrepresented social groups to psychology. To this end, Furumoto and Scarborough (1986) have discussed some of the first women psychologists, and Guthrie (1998) has written an entire book on early African American psychologists. These are certainly contributions to the history of psychology, but of what kind? They read like stereotypes of old history of psychology, often being “Great Woman” or “Great African American” history. (p. 31)

The basis of this claim is that these biographers have portrayed their subjects in a positive light. This is not how the term, “great” is usually understood. As any dictionary will show, it is usually applied to people who are unusually eminent or distinguished and whose work has had an extraordinary influence. For example, the *Oxford English Dictionary* defines it as “exceptional in ability or achievement; outstanding in activity, field or context specified; eminent or important.”⁴ This is clearly the way in which Boring was using the term when he reserved it for luminaries like Helmholtz, Darwin, James, and Freud. While it might suit Lovett’s polemical purposes to characterize the work of the new historians in this way, it relies on using the term, “great” in a novel and idiosyncratic way.

Both the Old and the New History Are Based on Primary Sources

Lovett writes

Finally, new historians have sought to distinguish their approach to scholarly methodology from that of old historians (Furumoto, 1989; Stocking, 1965). Writers of old history are said to use secondary sources, whereas new historians use primary sources. In looking at current undergraduate textbooks, the validity of this claim is difficult to assess. The primary sources are certainly present in reference lists, and the text is almost always sprinkled with primary source quotations. Certainly, the bibliography sections of *AHEP* demonstrate a mastery of a tremendous amount of primary source material, much of it in French or German and unavailable in translation. We may be skeptical that all of the authors of undergraduate textbooks have consumed the four volumes of Titchener’s laboratory manual (cited in many contemporary textbooks as well as *AHEP*), but it is likely that Boring read them more than once. (p. 21)

This quotation shows that Lovett is not so naive as to think that because a primary source appears in someone’s bibliography, they must have read it, though he seems to be making an exception in the case of Boring. His bibliography is said to provide evidence of “a mastery of a tremendous amount of primary source material” (p. 21). Bibliographies do not usually come with guarantees that their authors have read every work in them, and the ethics of the situation are unclear. The aim of a bibliography or “reference list” is to refer the reader to work that the author presumes to exist. Far from assuming that someone has read a primary source because it appears in their bibliography, we can turn this logic on its head and suggest that if someone does not appreciate the importance of consulting primary sources when writing the text, they are not likely to appreciate the importance of consulting primary sources when preparing their bibliographies.

I would like to relate an anecdote at this point. Many years ago, I became interested in Wundt’s *Völkerpsychologie*. Wundt’s main work on the subject consists of 10 large volumes that were published over a period of 20 years (Wundt, 1900–1920). Only one chapter from one of the 10 volumes had been translated into English, but that was not a problem for me since I speak German. The problem came when I tried to get a copy of the work. It proved to be impossible to get a complete set of the 10 volumes by interlibrary loan in North America. Not even the Library of Congress has a complete set. I eventually managed to look at a complete set of the 10 volumes in the library of the Humboldt University in Berlin.

In spite of this, the work appeared in the bibliographies of many American textbooks, including that of Boring, and still does. It seems implausible to suggest that their authors have the fluency in German needed to read them, the interest in reading 10 large volumes, and the ability to travel to Germany in order to do it. Boring (1929) mentions in the endnotes to the chapter on Wundt that he got his information about Wundt’s publications from a complete bibliography of Wundt’s publications that had been compiled by Wundt’s daughter, Eleanore,

⁴ “great, adj., n., adv., and int.” *OED Online*. New York, NY: Oxford University Press, March 2016.

after his death (p. 341). The authors of other textbooks had not been so cautious with the result that they could not agree on the name of the publisher, the date or dates of its publication, how many volumes it consisted of, or even the title of the work. A common mistake was to confuse the 10-volume *Völkerpsychologie* with *Elements of Folk Psychology*, which is an English translation of a different work (Wundt, 1916).

Regrettably, I did not keep a record of these discrepancies, and I no longer have access to the relevant textbooks. Fortunately, we need not look for mistakes in someone's bibliography to be aware that primary sources have not been used. No one to the best of my knowledge has ever questioned Boring's account of Titchener. He was after all a former student of Titchener and knew him well. His portrait of Wundt is a different matter. As is well known, Boring's account of Wundt has been severely criticized. The main authors here were Blumenthal (1979) and Danziger (1979). While much of the criticism of the traditional accounts of the history of psychology has remained buried in the specialist literature and is relatively unknown, this work came at a time when the attention of psychologists all over the world was centered on Leipzig to commemorate the centennial of the establishment of Wundt's laboratory in 1879.⁵ It could not, therefore, be so easily ignored. Lovett writes:

Criticism of Boring's work began as soon as it was published. Samelson (1980) discussed various early reviews of the first edition of *AHEP*, noting that the book was criticized as too narrow and selective in its definition of experimental psychology and as little more than "a long string of alternating biographical and doxographic sketches" (p. 468). These are normal criticisms of a history book, of course, and they pale in comparison to the complaints of the later critics, who attacked Boring's accuracy and even his motives in writing a history text. (p. 19)

There is no suggestion that the criticism was justified. These authors had simply "attacked Boring's accuracy" and "his motives in writing a history text." The last statement refers to the work of O'Donnell (1979), who suggested that Boring's aim had been to present psychology as an experimental science to prevent the subject being taken over by applied psychologists like clinicians and mental testers. Lovett's remarks suggest that there is some-

thing disreputable about writing history with the aim of influencing the course of psychology and that there are other motives that are "pure." Anyone who is familiar with the notion of "insider history" will be aware that there is nothing unusual about writing history with this aim (e.g., Danziger, 1990). It is quite simply what insider historians do.

It is more than obvious that Lovett does not want to believe the revisionist accounts. He complains that the new history "has been accepted somewhat uncritically by professional historians and textbook writers" (p. 17) and notes that "this revision has been widely incorporated into pedagogical material" (p. 19). This is true (Brock, 1993). Schultz and Schultz (1987), for example, wrote:

Thus, generations of students may have been offered a portrait of Wundt's psychology that may be more myth than fact, more legend than truth. For 100 years after the event, texts in the history of psychology, including the previous editions of this one, and teachers of the history of psychology course, may have been compounding and reinforcing the error under the imprimatur of their alleged expertise. (p. 59)

Lovett had previously suggested that textbooks use primary sources because they have primary sources in their bibliographies. The obvious question here is why did the authors of all the major textbooks think it necessary to revise their accounts? If they had based their work on primary sources, they would have dismissed the revisionist histories as mistaken and left their accounts as they were. The fact that they did not suggests that primary sources had not been used. In fact the quotation from Schultz and Schultz is more revealing than the authors intended. Their repeated use of the word, "may," which incidentally disappeared from the later editions of the text, suggests that they were not sure even then. All that they had was conflicting secondary accounts. Of course they could have resolved the issue by comparing Wundt's original work with the different secondary accounts in order to see who was right but they were no more willing to do that than was Lovett was. Apart from making the point that textbooks have primary sources in their bibliographies, he offers no evidence in support of his refusal to believe the revisionist accounts.

⁵ See, for example, Thomas (2007).

Other than that, the issue of primary versus secondary sources is a simple matter of common sense. How many lifetimes would it take to become familiar with the original works of Helmholtz, Fechner, Darwin, Wundt, James, Pavlov, Freud, Piaget, Skinner, and all the rest? This is before we start on the issue of acquiring the fluency in French, German, and Russian that would be needed to do it. Much of their work is still untranslated and, in any case, translations are not primary sources in the strict sense of the term because they rely on the interpretations of the translator. It is also worth noting that Bor-ing, along with Schultz and Schultz and the authors of many other textbooks, are not even full-time specialists. They are what Dewsbury (2003) called, “dabblers”; that is, psychologists whose main interests are in other branches of the subject and who have written textbooks on the history of psychology in their spare time. This makes it all the more unbelievable that they would have read the primary sources in their bibliographies.

The only way that historians of psychology have been able to base their work on primary sources is to narrow its focus so that they become specialists on a limited subject, such as the work of a single psychologist. The neglect of primary sources in “textbook history” is not the fault of the authors concerned. It is a problem of the genre; in particular, the view that all the important information about the history of psychology should be included in the pages of a single book. This tells us more about the limited tolerance that psychologists have for the history of psychology than it does about the most appropriate way to conduct historical research.

Lovett’s Five Questions for the New Historians

The second section allegedly discusses some problems or limitations that exist with the new history but not with the old. Lovett presents them in the form of five questions, which he says are for the new historians but which he goes on to answer himself. They are therefore rhetorical questions in the usual sense of the term. They are also loaded questions in that they are based on assumptions which I do not share. In spite of this, I will discuss Lovett’s answers to these questions and offer some different answers of my own.

Q1. Is the New History Merely Whiggish in a New Way?

As might be expected, Lovett answers this question in the affirmative and goes on to talk about “this new Whiggish history of the history of psychology” (p. 25). What is the basis for the claim? It is that the new historians regard certain aspects of the new history as an improvement on the old. Does this constitute Whig history? Only if the term is used in a novel and idiosyncratic way. Just as Lovett redefined the word, “great” to suit his polemical purposes in the previous section, he is redefining the word, “Whiggish” here.

It might be appropriate to revisit the *Oxford English Dictionary* at this point. It defines a Whig historian as “a historian who interprets history as the continuous and inevitable victory of progress over reaction.”⁶ When the British political historian, Herbert Butterfield (1965) originally coined the term, he was referring to a view that was common in the 19th century whereby British political history was seen a story of continuous and inevitable progress, a view that became increasingly difficult to maintain amid the carnage of the First World War. When it was subsequently adopted by historians of science, it was used in a similar way to refer to the view that the history of science is a story of continuous and inevitable progress. No implication was ever intended that it was “Whiggish” to say that progress had occurred in a limited area of knowledge over a limited period of time. This is what the new historians are saying. They are referring to the professionalization of the field that occurred in the period from the 1960s to the 1980s. The earliest work on the history of psychology was published at the beginning of the 19th century, and the new historians of psychology were not saying that any significant progress occurred in the field during its first 150 years. They were also referring only to its scholarly aspects. The whole point behind the literature on the new history of psychology was to point out that these developments had not affected its pedagogical aspects.

Lovett’s use of the term “Whiggish” bears no relationship to the way in which it has been

⁶ “Whig, n.2 and adj.” *OED Online*. New York, NY: Oxford University Press, March 2016.

traditionally used. According to Lovett's definition, anyone who believes that progress has occurred in any aspect of life is a Whig historian and the only way to avoid the label to make all discussion of progress taboo. If that is the definition of a Whig historian, then we are all Whig historians, and the term is effectively meaningless since it serves no useful purpose in distinguishing one kind of historian from another. In making these points, I am of course aware that the negative connotations of the label make it a convenient term of abuse.

Lovett continues

Who would argue that history done by experts (historians) is not superior to the efforts of amateurs (psychologists), or that primary sources are not preferable to secondary sources in scholarship? But what makes external history better than internal history? What is inherently wrong about studying the past for the sake of the present? And why is newer philosophy of science better than older philosophy of science (another sort of Whiggishness)? The new historians clearly have work left to do if they wish to claim (as they do) that their approach is not just different, but better. (p. 25)

The first sentence is another example of Lovett's misunderstanding of what the professionalization of the field has involved. Assuming that these are genuine questions, and I am not entirely sure that they are, I will attempt to answer them in reverse order.

The third question was: "why is newer philosophy of science better than older philosophy of science (another sort of Whiggishness)?" (p. 25). The answer is that some progress has occurred in this field as well. No one—and here I mean *no one*—takes seriously the positivist views of science that were popular in the early part of the 20th century. It is now recognized that they are not an accurate account of how science works. Of course no one is suggesting here that the newer philosophies of science represent the last word on the subject, just that some progress in the field has occurred. Recognizing this progress is only Whiggish if the term is used in Lovett's novel and idiosyncratic way whereby anyone who acknowledges that progress has occurred in some limited sphere is automatically a "Whig."

With regard to the question, "What is wrong with studying the past for the sake of the present?", there is nothing wrong with it, and no new historian has to the best of my knowledge ever suggested that there is. The question is

based on a misunderstanding of what the new historians are saying when they criticize presentist history. Lovett's fourth question is on the subject of presentism and so I will postpone any further discussion of it until there.

I have deliberately left the first question until last because it requires the most detailed response. The question is, "What makes external history better than internal history?" External history is better than internal history if and only if it is better in explaining the facts as we understand them. For example, there is a traditional view to the effect that introspection died out in psychology because it deals with evidence that is "private," and if two introspectors disagree, we have no way of knowing who is right. The example of introspectors disagreeing with each other that is usually given is the so-called "imageless thought controversy" between Titchener and his students at Cornell University and the psychologists of the Würzburg School. Because of this problem, J. B. Watson suggested that psychology should be a science of behavior and, in doing so, helped to place the subject on firm scientific foundations. This is an internalist account since it makes no reference to developments outside psychology. It is also compatible with the Whig view of history in that it suggests that the change represents progress. This version of events has a number of problems.

The first problem is that it was not a disagreement between individual introspectors but a disagreement between two different schools. The two schools disagreed with each other but they agreed among themselves. Another problem is that there is a great deal of evidence to suggest that American psychology had already gone "behaviorist" before this controversy occurred. The only major figure in the United States who still advocated an introspectionist approach was Titchener, and he is the only person who Watson mentions by name in his behaviorist manifesto (Watson, 1913). If we look at the manifesto itself, the controversy is mentioned only in one of the footnotes. The main point of the article is to point out that the people who are likely to provide support for psychology in areas such as the law, education, and advertising are not interested in the content of people's minds but they are interested in their behavior. Other problems for the internalist account are that the move to behavior did not result in

psychologists agreeing with each other. On the contrary, different schools of behaviorism began to emerge. It also fails to explain why some psychologists continued with introspection in the 1920s and 30s and why there have been repeated calls for a return to introspection ever since (Brock, 1991, 2012; Danziger, 1980).

The externalist account would suggest that the switch from introspection to behavior was not because of any intrinsic problems with the former. It was because of the realization that psychology would have a limited role in society if it concerned itself with the content of people's minds, but it would have a much bigger social role if it focused on behavior instead. This account is perfectly capable of explaining the facts as we understand them, whereas the internalist account is not. Whether or not this will happen in every case is an open question but the chances are that it will for the simple reason that psychologists do not live in a social vacuum. They participate in their social surroundings just like everyone else.

The rest of this section is taken up with references to three critics of recent developments in the history of science. One of them is Edward L. Harrison (1987), a physicist who wrote a short article for *Nature* in which he defended Whig history. He is not alone in this respect. Another physicist, Steven Weinberg (2015) has more recently come out in support of Whig history. That practicing scientists and historians of science should hold different views on the history of science is perhaps unsurprising. What is more surprising is that scientists should think that they can tell historians of science how they ought to do their work. The other two critics, David L. Hull (1979) and Larry Laudan (1992) were/are philosophers of science and philosophers of science have often had an uncomfortable relationship with historians of science, largely due to territorial disputes over which of them has the right to explain the theories and methods of science. Lovett writes:

One would never know of the existence of these views from reading the new historians of psychology who appear to have uncritically endorsed certain trends in historical scholarship, considering those trends to be the last word on the matter. (p. 25)

The flaw in the logic here is to assume that because someone has not discussed these views, they are not aware of them or have not consid-

ered them. Authors do not usually regard their publications as an opportunity to make the views of those who disagree with them better known. They might discuss these views if they want to criticize them but they are under no obligation to discuss the views of everyone who disagrees with them.

These comments also ignore the context in which the accounts of the new history of psychology were produced. They are for the most part attempts by specialist historians of psychology to introduce some recent developments in their field to the nonspecialists who are involved in teaching it. Anyone who has been in this situation will know that explaining concepts like "internalism," "presentism," and "Whig history" to people who have never heard of them is not easy and the last thing that anyone would want to do in his situation is complicate the issues even further by discussing dissenting views.

Q2. Is Science Less Capable of Progress Than Historical Scholarship?

Lovett writes

Why is it assumed that the history of psychology is capable of progressing but not the science? The new historians are known best for criticizing the idea that psychology has progressed, or that such "progress" has not done much good for the field. (p. 25)

The point should be made here that the discussion is about psychology, not science. I know that psychologists will protest endlessly that their subject is a science but there is no reason to suppose that progress in psychology and progress in physics or chemistry will occur in exactly the same way. That is an open question.

More importantly, none of the new historians has ever said that psychology is incapable of progress. The authors who Lovett quotes—Ash (1987); Harris (1997) and Samelson (1999)—are all criticizing the standard Whig view that the history of psychology is a story of continuous and inevitable progress. None of them are saying that progress in psychology is impossible or that it has not occurred. Indeed, Lovett acknowledges this in his account of the work of Ash (1983): "Ash never actually demonstrated that psychology has not progressed, nor is it clear that he thinks no progress has taken place" (p. 26). So who are these historians of psychology who think that psychology (or science) is

incapable of progress? Lovett has provided no evidence to show that they exist.

The next part of this section is taken up by a critique of the philosophy of science of T. S. Kuhn:

The new historians' criticisms of *assumptions* of progress usually rest on a certain philosophy of science that arose in the 1960s and found its first main voice in Kuhn's (1962, 1970) *Structure of Scientific Revolutions*. Kuhn's revisions to the standard view of science were many but included the idea that science cannot show objectively cumulative progress. (p. 26)

It is perfectly in order to say what Kuhn did not believe, but it is surely more interesting to find out what he did believe. Anyone who is looking for evidence in Kuhn's work to suggest that he did not believe in scientific progress will be sorely disappointed. In the second edition of *The Structure of Scientific Revolutions* (1970), he wrote:

Later scientific theories are better than earlier ones for solving puzzles in the quite different environments to which they are applied. That is not a relativist's position, and it displays the sense in which I am a convinced believer in scientific progress. (p. 206)

Anyone with any degree of familiarity with the philosophy of science (and I speak here as someone with a degree in the subject) will be aware that there are many different theories of scientific progress (Niiniluoto, 2015). Kuhn's objection to the Whig view of history was that he did not consider it to be an accurate account of how science progresses and so he tried to produce a better one. This is not to say that his views on the subject are by any means the last word. It is ironic that Lovett and those who echo his views should accuse the new historians of psychology of being philosophically naïve. By offering us a stark choice between the Whig view of history and the idea that science is incapable of progress, it is Lovett who shows himself to be philosophically naïve.

More importantly, the relationship between Kuhn's work and the new history of science is not as close as Lovett would have us believe. It is certainly true that philosophers of science ruled the roost with regard to explaining scientific knowledge prior to Kuhn. This was because scientific knowledge was thought to be based on a combination of empirical evidence and logic. Kuhn's notion of a "paradigm," which was based on neither, opened up the possibility of

explaining scientific knowledge in historical and sociological terms. This is a matter of historical interest and it does not imply that the new historians of psychology have accepted Kuhn's philosophy of science in every other respect. Their work should be assessed on its own merits and not on the merits of something else.

The final part of this section is devoted to criticism of the sociology of science and a view that Lovett calls "constructivism."

Because the scientists' decisions about what to believe are not dependent on the raw experimental facts, the scientists are said to be constructing the knowledge themselves, and this view is termed "constructivism" or "constructionism." (p. 27)

The problem with this view is that "although the constructivists are right in pointing out that any raw experimental fact can be explained by many theories, the constructivists wrongly assume that each theory is an equally plausible alternative" (p. 27). He provides no names or references in support of this claim but it is probably no accident that he chose Danziger's *Constructing the Subject* as his "exemplar" of the new history approach. Is this an accurate account of Danziger's views? Anyone who is familiar with Danziger's views will know that he is no relativist. He considers himself to be a scientific realist (Brock, 2006).

There is another complication with the issue of "progress" that Lovett seems to have missed: Progress can only occur in relation to a specific goal. If I am traveling north and you are traveling south, we will inevitably define progress in different ways. This problem is not relevant to a great deal of research. For example, we all know that cancer is an evil and that "progress" in cancer research involves coming up with better treatments or cures. But is this situation typical of the history of psychology? Danziger raises this issue in relation to a controversy between E. B. Titchener and J. B. Watson over whether psychology should use introspective or behavioral evidence. As we have already seen, the received wisdom is that Watson was right and Titchener was wrong but Danziger points out that they had different goals and the methods they advocated made perfect sense in the light of those goals.

Danziger goes on to show how the early researchers in psychology tended to use a single

subject. Ebbinghaus famously conducted his memory experiments on himself. This practice was later abandoned in favor of using many subjects and aggregating the results. One could offer a Whiggish account of how this change was “progress” and many psychologists undoubtedly would but B. F. Skinner would have disagreed. He was happy to use the results from a single rat. In his autobiography, he relates an amusing incident where he was giving a presentation at an American Psychological Association (APA) conference in which he spoke about his research using a single rat. He then said “in deference to the standards of this Association I will now talk about the other rat” (Skinner, 1983, p. 123). So who was right and who was wrong? The question is pointless. When physiologists do research on the human brain, they do not use many subjects and statistics to aggregate the results. On the other hand, opinion pollsters who want to predict the outcome of an election do. Their methods in both cases are appropriate to their goals.

What Lovett neglects to mention is that these different theories were not designed to explain the same empirical results. Part of the problem here is that by calling this diverse set of activities (introspection, gathering statistics, studying rats, examining the brain, etc.) “psychology,” we give the misleading impression that they all had the same goals. They obviously did not and their “progress” can only be assessed in relation to their goals.

Q3. Is There a Place for the New History in the Psychology Department?

Lovett opens this section with a discussion of the famous article by the historian of science, Stephen Brush in which he asks, “Should the history of science should be rated X?” (Brush, 1974). Here he points out that scientists usually provide courses on the history of their science in order to introduce their students to the norms and values of the field. He goes on to point out that genuine historical scholarship is not well suited to this role and that fictionalized history is a more suitable alternative. If Lovett was aware that these remarks were meant to be tongue-in-cheek, he gives no indication of it. He writes:

If proponents of the new, critical history of psychology want not only to engage in historical scholarship, but

also to prepare scientists, these scholars need to realize that both of the roles of the history of psychology identified by Ash (1983) are valid goals for a psychology department (viz., socialization into the profession of science and critical analysis of scientific work). When Furumoto (2003, p. 115) asserted that “The time has come for a radical pedagogical overhaul of the History of Psychology course,” she failed to recognize the valid functions being served by the current history of psychology course. Until new historians engage directly with these issues, their place in psychology departments will likely be confused. (p. 28)

The history of psychology has always had a precarious relationship with the discipline of psychology and it has led to a body of literature with titles such as, “Why study the history of psychology?” (Henle, 1976). This topic is discussed at the start of many textbooks as well. Interestingly, psychologists who offer courses on topics like “Perception” or “Personality” are not usually required to justify their existence in this way. Acquiring knowledge about these topics is considered to be a good-in-itself. When it comes to the history of psychology, a different standard is applied. Acquiring knowledge about the history of psychology is not considered to be a good-in-itself. It must justify its existence on other grounds.

The suggestion that the place of the new history in psychology departments is “confused” rests on the unwarranted assumption that introducing students to the discipline’s view of itself is the only role it can play or even that it is a desirable goal. It was not my goal when I used to teach courses on the history of psychology. It seemed to me that my colleagues in other areas of the discipline were already doing a good job in this regard and they needed no help from me. I therefore thought that I could make a better contribution to the students’ education by providing them with a perspective on the discipline that they were not going to get from anyone else. Although Lovett may believe that external history is “a waste of time” (p. 28), it is a useful way of making students aware of the role that psychology has played in society and that it continues to play. We might, for example, look at the way in which homosexuality was formerly listed as a mental illness in the *Diagnostic and Statistical Manual* (DSM) and the various practices that were designed to “cure” it. We could then look at the work of someone like Evelyn Hooker whose research contributed to the view that it was not an illness (Harris,

2009). Material like this can show how psychologists have helped to reinforce the status quo and also how they can bring about change. At least some of our students are going to become psychologists who will be let loose on society at some point and it hardly seems to be “a waste of time” to make them aware of the different contributions that they can make.

Another way in which the history of psychology can contribute to the education of students (and experienced psychologists for that matter) is to show them that the way that psychologists currently do things is not the only possible way. An example from Danziger’s *Constructing the Subject* (1990) is the egalitarian and collaborative way in which research was conducted in Wundt’s laboratory. The experimenter and the subject were often colleagues in the same laboratory and their roles were interchangeable. Thus someone might act as a subject in a colleague’s experiments and the colleague would then repay the favor and act as a subject in the other person’s experiments. The subject was often a person of higher social status than the experimenter since the observations of the subject were considered crucial to the outcome of the experiment, while the role of the experimenter was merely to record those observations. Danziger even found an example in Wundt’s journal where the subject and not the experimenter had authored the published report. Although I have been using the conventional term, “subject” up to this point, it was never used. There was no standard terminology but *Beobachter* [observer] and *Versuchsperson* [test person] were frequently used. The word “subject” has its origins in hypnotism and it was introduced in conjunction with a more manipulative style of experimentation where all the power lay with the experimenter. “Naïve” subjects like animals and children were often favored and deception was frequently used. As surprising as it might seem, there are psychologists who do not regard this change as “progress” and would like to return to the situation where they relate to their subjects in a more egalitarian and collaborative way. Regardless of our views on this matter, the example shows that there are lessons to be learned from history, though they might not be the kind of lessons that a Whig historian would expect.

Q4. Is Presentism Unavoidable?

Lovett begins this section by asking the question, “Is it possible to do truly non-Whiggish, nonpresentist history?” (p. 29). He then goes on to refer to two authors who say that we cannot escape our time and place. One of them is Hans-Georg Gadamer, though interestingly this information is derived from a secondary source. It seems that Lovett does not always practice what he preaches in this regard. He concludes: “Even if nonpresentist history is possible, it is still extremely difficult” (p. 30). He illustrates this point with a quotation from Pickren and Dewsbury (2002) to the effect that “Yerkes was a product of his time” (p. 30), a view that he characterizes as “crude social determinism” (p. 30). A remark to the effect that Yerkes abandoned his racist views “to his credit” is described as “a judgment more presentist than any made by Boring” (p. 30). Whether or not calling someone, “a product of his time” represents crude social determinism is questionable but what is more interesting is that Pickren and Dewsbury (2002) did not use the word, “product.” They say that “Yerkes was a person of his time” (p. 4), which is not the same thing. It seems that in his desperation to criticize the new historians, Lovett sees things in their work that are not even there.

Much more important is the issue of whether or not presentism can be avoided. The point about us being embedded in our time and place will not come as news to any historians of my acquaintance. We are aware that we ourselves are a part of history and cannot step outside it. This is why I suggested that the question in the previous section, “What is wrong with studying the past for the sake of the present?” (p. 25) is misguided. There is nothing wrong with it. The questions that we ask of history will inevitably be shaped by present-day concerns. An obvious example is all the work that has been done on women in the history of psychology in recent years. That is clearly the result of more and more women becoming psychologists and their concerns in relation to feminism and women’s rights. There is also no need for us to abandon our values. Believing that the transatlantic slave trade or the Nazi gas chambers were an abomination is no barrier to writing good history.

So where is the problem? Presentism in the usual sense of the term refers not to the ques-

tions that we ask of history but to the answers that we find. One example is the kind of approach to history that involves looking for “precursors” of modern-day views. Thus it has been claimed that Wundt “anticipated” modern cognitive psychology (Brock, 1993). There are some similarities between the two—for example the use of reaction times—but the differences between them are far more striking than the similarities. The problem here is that, if we are only looking for precursors to modern-day views, these differences will be overlooked and we will learn nothing new. We generally study history because the past is different from the present. As the opening lines of the novel, *The Go-Between* famously put it: “The past is a foreign country: they do things differently there” (Hartley, 1953, p. 3). The presentist historian is similar to the American tourist who goes to some Caribbean beach resort and insists on speaking only with other Americans, eating American food and watching American TV. Most people would say, “Why go to another country if you are not going to experience anything new?” The same question might be asked of the presentist historian. You will never learn anything from history if all that you find there is what you already know.

Thus the issue is not about avoiding presentism but avoiding certain kinds of presentism. Illustrations of this point can be found in some of the literature that Lovett cites. George Stocking Jr. (1965) in his famous editorial for the *Journal of the History of the Behavioral Sciences* refers not to nonpresentism but what he calls “an enlightened presentism” (p. 215). Allan Buss (1977) in the same journal speaks approvingly of what he calls a “critical presentism” and contrasts it with the “justificationist presentism” of which he disapproves (p. 252). Similar distinctions can be found elsewhere. For example, Roger Smith (1988) has advocated what he calls “an extremely thoughtful presentism” (p. 151). More recently, Naomi Oreskes (2013) has published an article with the provocative title, “Why I Am a Presentist”. Here it can be seen that similar distinctions are being made: “I believe it is useful to distinguish between *types of presentism* . . . While both substantive and methodological presentism are problematic for historian—and we are right to eschew them—I believe that motivational presentism is not only inescapable, it is actually

desirable” [emphases in original] (p. 600). Not all of these terms mean the same thing of course but they show that none of these authors consider “nonpresentism” to be possible or even desirable. The issue is about the kinds of presentism that we should reject or accept. The question, “Is presentism unavoidable?” presents the issue in overly simplistic terms and it is not for the first time. The view that we must choose between Whig history and being opposed to the idea of scientific progress is another case in point.

Q5. Who Should We Study?

Lovett writes:

A fifth and final question for the new history of psychology asks whom historians of psychology should study. Out of thousands of psychologists, how should we decide whom to emphasize in the history that is presented in research monographs, textbooks, and courses? New historians have made critical comments concerning the ranking of psychologists by eminence, but not everyone is equally important in the history of psychology. (pp. 30–31)

What Lovett is referring to here is the criticism of figures like Furumoto and Scarborough (1986) and Guthrie (1998) that the history of psychology has traditionally excluded women and ethnic minorities, work that he characterizes as “Great Woman” and “Great African American” history (p. 31). His objection to this kind of history is that these figures were “not especially important or influential”:

There is no indication that the psychologists profiled by Furumoto and Scarborough or by Guthrie were especially important or influential, and perhaps that is why so few of these individuals can be found listed in standard history of psychology texts. A paper on an unknown woman who worked for a year in Ivan Pavlov’s laboratory might excite new historians, but it is difficult to know whom to take out of the history of psychology textbooks so as to have room to include her. (p. 31)

He concludes

If the new historians wish to criticize the old histories’ selection of figures and topics, they should be prepared to offer a better system for deciding which people and ideas are most worth studying. (p. 32)

This last statement is a *non sequitur*. If someone points to the limitations of the current history of psychology textbooks, they are under no obligation to offer a different set of limitations. The argument rests on the unquestioned assumption

that everything worth knowing about the history of psychology should be contained in a single book. As I pointed out earlier, it tells us more about the limited tolerance that most psychologists have for the history of psychology than it does about the subject itself. When psychologists offer a course in other areas of the discipline, they do not usually expect to be able to cover everything that there is to know in that area. The basic idea is that students should come away from the course knowing more about the subject than they did before. The content of our courses will be selective whether we like it or not and we can accept that different people will have different priorities.

The view that “eminent” should be equated with “important” is based on what Moghaddam and Lee (2006) have called “a free-market model” in which eminence arises out of a competition in the free market of ideas:

In such a free market, different ideas are put forward and critically evaluated, and the best are adopted. The free-market model assumes that psychological research evolves independently from social, political and economic forces. (p. 163)

Anyone who looks at the history of marginalized groups, such as women and ethnic minorities, will find no evidence to support this model. They were excluded from the inner sanctum of psychology because of discrimination, not the quality of their ideas. A similar situation applies to the psychologists from outside the handful of countries that are usually covered in the history of psychology texts.⁷ Where are all the psychologists from Latin America and the Caribbean, Africa, the Middle East, and Asia? Could it be that nothing has ever happened in these places that is worth mentioning in a history of psychology text? It seems more plausible to suggest that the psychologists in these countries have been just marginalized as women and ethnic minorities in the United States. So one way in which the study of people who were “not especially important or influential” (p. 31) can contribute to our understanding of psychology is to cast doubt on the free-market model itself.

It can also contribute to our understanding of psychology by showing that psychologists shared the prejudices of their time. This is particularly telling if we move away from the biographical details of psychologists and look at the content of psychology itself. There was, for

example, a branch of the subject called, “racial psychology” that flourished in the United States in the early part of the 20th century. Among other things, it promoted the view that some ethnic groups were more intelligent than others. We can also learn a great deal from early work on gender, such as Lewis Terman’s masculinity-femininity scale, not forgetting of course the various “cures” that were devised for homosexuality (Richards, 2010; Tyson, Jones, & Elcock, 2011). Work on these subjects shows not only that psychologists shared the prejudices of their time, but that they also lent support to these prejudices by imbuing them with the authority of “science.”

This kind of research can also remind us that psychology’s “subjects” have usually been the relatively weak and powerless members of society. The situation in psychoanalysis where the person being studied is the person who pays the analyst’s fees is rare. Within academic settings, the subjects of psychology have usually been nonhuman animals and undergraduate students. Outside academic settings, the common situation is for psychologists to be employed by powerful agencies to study the people in their care (Danziger, 1987). Thus they have studied the mentally ill on behalf of hospital authorities, children on behalf of educational authorities, prisoners on behalf of prison authorities, industrial workers on behalf of their employers, and colonial peoples on behalf of their colonial rulers. The last people that psychologists were likely to study were the people who were employing them. These aspects of the history of psychology can also improve our understanding of the field.

It is not only people who have been marginalized in the history of psychology; theories and methods that have been marginalized as well. For example, it can be instructive to ask why Wundt’s experimental psychology was given so much attention while his *Völkerpsychologie* was virtually ignored. The same thing happened to other theories that took the group as their basic unit of analysis. They were often dismissively referred to as “group mind” theories in the United States (e.g., Allport, 1985). The re-

⁷ Pickren and Rutherford (2010) is a notable exception to this rule, though even here the amount of space devoted to psychologists outside Europe and North America is small.

jection of these theories did not mean that psychology was willing to examine the unique features of individuals either. There is another lost continent of literature that is based on what has traditionally been called the “idiographic” approach (Lamiell, 2003). These examples serve to remind us that we can learn a great deal about psychology not only from the theories and methods that it has included; we can learn at least as much, if not more, from the theories and methods that it has excluded as well. The same is true of the psychologists themselves and that other category of sentient beings that they have traditionally called their “subjects.”

Critical Histories

The final section of the work opens with a discussion of different kinds of critical history. Here Lovett outlines the kind of critical history that he considers acceptable and the kind of critical history that he does not. This is what he considers acceptable:

Historians who think critically will not accept anecdotes about the great psychologists unless there is good evidence supporting those anecdotes. Critically thinking historians will not base their scholarship on secondary sources and will not present information without thoughtfully evaluating it. All this seems beneficial to the field—it contributes to history that is more correct. (p. 32)

It is ironic that he should express this view because, as we saw earlier, he refuses to believe the revisionist accounts of Wundt. While he is willing to accept these points in principle, he is not willing to put them into practice when improvements in our knowledge occur. The reason is that he considers criticism of the traditional accounts of the history of psychology and of psychology itself to be beyond the pale:

The problem comes when the word critical becomes an antonym of “ceremonial,” as it has in recent years (e.g., Furumoto, 1989). At this point, “critical” history takes on a purpose beyond increased accuracy—it must actively criticize psychology and its histories, disparaging psychology’s “pretensions” to scientific objectivity and deprecating past histories for not being “critical” enough. (p. 32)

These points lead him to his conclusion:

Those historians who wish to influence psychologists, then, should recognize that if we explicitly warn historians against making their writings positive toward psychology, we are likely to lose the audience that most requires the accuracy and insights of the history

of psychology: the critically thinking psychologist. (p. 34)

Psychologists come in all shapes and sizes. They come in different genders, different generations, different ethnicities, and different nationalities. They also come with a wide range of different views. So who is this “critically thinking psychologist” who will accept criticism if it is within what he considers acceptable limits but will not accept criticism if he considers it to be beyond the pale? The answer must be obvious: it is Lovett himself.

Like all the authors in *History of Psychology*, he provided an author note:

Benjamin J. Lovett is a doctoral student at the Department of Psychology, Syracuse University, where his interests include professional and historiographical issues in the history of psychology. Currently, he is investigating changes in the treatment of behaviorism in histories of psychology. (p. 17)

Anyone who is familiar with the situation in the United States would have known that he could not have been a doctoral student in the history of psychology since the only university in the United States that had a doctoral program in this area at the time was the University of New Hampshire. He was clearly doing his doctorate in something else. That something else was school psychology and he is now an Associate Professor in that field.⁸ There are approximately 70 publications on his CV, and only one of them is on the history of psychology, namely the article under discussion here. It is therefore disingenuous of him to refer to historians as “we” with the implication that psychologists are “them.” As far as Lovett is concerned, it is psychologists who are “we” and historians who are “them.”

The article is best seen as a reply from one of the nonspecialists that Furumoto and her colleagues were trying to reach and he is saying, “Thanks but no thanks, we are happy with the kind of history that we have.” The article reads to me like the work of a psychologist who has encountered critical history and has been offended by it. Having been offended, he has decided to write a critique. No one should be surprised by this. If you criticize someone’s family, their home town or their country, the chances are that you will get a negative re-

⁸ See <https://sites.google.com/site/benjaminjlovett/>.

sponse. The situation will be no different if you criticize their discipline or their profession.

What Furumoto and her colleagues may have overlooked is that the traditional role of the history of psychology in psychology departments is to introduce students to the discipline's view of itself and any kind of history that might question this self-image is profoundly threatening to many psychologists. In an article which Lovett cites, Bhatt and Tonks (2002) relate anecdotes concerning people who have taught the history of psychology course from a critical perspective and have suffered negative consequences as a result. I am sure that anyone who is a critical historian or knows any critical historians will be able to supplement them with some anecdotes of their own.

These points raise serious questions about the politics of teaching the history of psychology from a critical perspective. It may also explain why Lovett's article has been positively received. Most psychologist-historians have a background in psychology and it would be no more realistic to expect them to be critical of psychology than it would to expect executives in the fast food industry to be critical of that industry or church officials to be critical of their church. There will always be exceptions but they will never be anything more than that.

The current situation in the history of psychology is much more complicated than the old-new dichotomy would suggest. Old historians in the sense of practicing psychologists who have written history of psychology textbooks in their spare time are still around. Some of these textbooks are astonishingly successful. The vast majority of psychologists who teach the history of psychology are not specialists in the field and it is possible that they can more easily identify with a textbook that has been written by another nonspecialist.

With regard to specialists in the field, there are some who can unambiguously be described as "new historians" but the majority display aspects of both the old and the new. While Rappard (2008) might say that his sympathies lie with the "old, uncritical history," he is not a part-time hobbyist and he has not authored any textbooks. Among other things, he was one of the founder members of the organization that is now known as the European Society for the History of the Human Sciences (ESHHS), and he served as its first president. He also uses

primary sources in his work (e.g., Rappard, 2004).

Another historian of psychology who displays aspects of the old and the new is Daniel Robinson and, like van Rappard, he has clashed with Kurt Danziger over historiographical issues. Danziger (2013) holds the view that psychology is no older than the 18th century and that to call anyone who lived before that century a "psychologist" is an act of presentism. Robinson (2013) holds the more traditional view that psychology can be traced back to antiquity and that there is nothing wrong with describing Aristotle as a "psychologist."

A rule of thumb in deciding how close a psychologist-historian is to the new history of psychology is the degree to which they have been influenced by developments in the history and/or sociology of science. As Danziger (1990) makes clear at the start of *Constructing the Subject*, he has been profoundly influenced by the literature in these fields, whereas van Rappard and Robinson have not. Rappard (2008) has even suggested that the influence of history of science on psychologist-historians has been counterproductive and that they ought to develop a different approach that will appeal to the interests of psychologists and be more acceptable to them.

Conclusion

Debates over the advantages and disadvantages of different approaches to the history of psychology have their place but it has not been my aim to argue that the new history of psychology is better than other approaches here. Lovett's article is full of misinformation and it is this more than anything else that led to me writing this reply. Claims that the new history involves moving the history of psychology out of the psychology department and into the history department, that Boring did not write about "Great Men" or that history of psychology textbooks are based on primary sources are quite simply wrong. The same is true of claims that the new historians deny the possibility of scientific progress or that they are trying to avoid presentism in some absolute sense. The list goes on and on. That a graduate student who was not even a specialist in the history of psychology should produce an article that is full of misunderstandings and

errors is perhaps unsurprising. What is more surprising is how it managed to get past the editor and reviewers of this journal and why it has been so uncritically received. It was clearly a message that some psychologist-historians wanted to hear. That should not lead them to overlook its deficiencies. If they want to suggest, for example, that the new history is not so different from the old or that it is methodologically deficient, they need to come up with better arguments than these.

References

- Allport, G. W. (1985). The historical background of modern social psychology. In G. Lindzey & E. Aronson (Eds.), *The handbook of social psychology* (pp. 1–46). Cambridge, MA: Addison Wesley.
- Araujo, S. de Freitas. (2016). *Wundt and the philosophical foundations of psychology*. New York, NY: Springer. <http://dx.doi.org/10.1007/978-3-319-26636-7>
- Ash, M. G. (1983). The self-presentation of a discipline: History of psychology in the United States between pedagogy and scholarship. In L. Graham, W. LePenies, & P. Weingart (Eds.), *Functions and uses of disciplinary histories* (pp. 143–189). Dordrecht, Netherlands: Reidel. http://dx.doi.org/10.1007/978-94-009-7035-9_7
- Ash, M. G. (1987). Introduction. In M. G. Ash & W. R. Woodward (Eds.), *Psychology in twentieth-century thought and society* (pp. 13–33). New York, NY: Cambridge University Press.
- Bhatt, G., & Tonks, R. G. (2002). What lies in the future of teaching the history of psychology? *History and Philosophy of Psychology Bulletin*, *14*, 2–9.
- Blumenthal, A. L. (1979). The founding father we never knew. *Contemporary Psychology*, *24*, 547–550.
- Boring, E. G. (1929). *A history of experimental psychology*. New York, NY: Century.
- Boring, E. G. (1950a). *A history of experimental psychology* (2nd ed.). New York, NY: Appleton-Century-Crofts.
- Boring, E. G. (1950b). Great men and scientific progress. *Proceedings of the American Philosophical Society*, *94*, 339–351.
- Boring, E. G. (1952). Autobiography. In E. G. Boring, H. S. Langfeld, H. Werner, & R. M. Yerkes (Eds.), *A history of psychology in autobiography* (pp. 27–52). Worcester, MA: Clark University Press.
- Brock, A. C. (1991). Imageless thought or stimulus error? The social construction of private experience. In W. R. Woodward & R. S. Cohen (Eds.), *World views and scientific discipline formation* (pp. 97–106). Dordrecht, the Netherlands: Kluwer. http://dx.doi.org/10.1007/978-94-011-3164-3_9
- Brock, A. C. (1993). Something old, something new: The “reappraisal” of Wilhelm Wundt in textbooks. *Theory & Psychology*, *3*, 235–242. <http://dx.doi.org/10.1177/0959354393032008>
- Brock, A. C. (2012). The history of introspection revisited. In J. W. Clegg (Ed.), *Scientific self-observation* (pp. 26–43). New Brunswick, NJ: Transaction.
- Brock, A. C. (2006). Rediscovering the history of psychology: Interview with Kurt Danziger. Interview by Adrian C. Brock. *History of Psychology*, *9*, 1–16.
- Brush, S. G. (1974). Should the History of Science Be Rated X?: The way scientists behave (according to historians) might not be a good model for students. *Science*, *183*, 1164–1172. <http://dx.doi.org/10.1126/science.183.4130.1164>
- Buss, A. R. (1977). In defense of a critical-presentist historiography: The fact-theory relationship and Marx’s epistemology. *Journal of the History of the Behavioral Sciences*, *13*, 252–260. [http://dx.doi.org/10.1002/1520-6696\(197707\)13:3_252::AID-JHBS2300130305_3.0.CO;2-0](http://dx.doi.org/10.1002/1520-6696(197707)13:3_252::AID-JHBS2300130305_3.0.CO;2-0)
- Butterfield, H. (1965). *The Whig interpretation of history*. New York, NY: Norton.
- Danziger, K. (1979). The positivist repudiation of Wundt. *Journal of the History of the Behavioral Sciences*, *15*, 205–230. [http://dx.doi.org/10.1002/1520-6696\(197907\)15:3_205::AID-JHBS2300150303_3.0.CO;2-P](http://dx.doi.org/10.1002/1520-6696(197907)15:3_205::AID-JHBS2300150303_3.0.CO;2-P)
- Danziger, K. (1980). The history of introspection reconsidered. *Journal of the History of the Behavioral Sciences*, *16*, 241–262. [http://dx.doi.org/10.1002/1520-6696\(198007\)16:3241::AID-JHBS23001603063.0.CO;2-O](http://dx.doi.org/10.1002/1520-6696(198007)16:3241::AID-JHBS23001603063.0.CO;2-O)
- Danziger, K. (1987). *Psychology for whom? Michael Keenan Memorial Lecture*. Saskatoon, Saskatchewan, Canada: St Thomas More College.
- Danziger, K. (1990). *Constructing the subject: Historical origins of psychological research*. New York, NY: Cambridge University Press. <http://dx.doi.org/10.1017/CBO9780511524059>
- Danziger, K. (1997). The future of psychology is not its past: A reply to Rappard. *Theory & Psychology*, *7*, 107–111. <http://dx.doi.org/10.1177/0959354397071009>
- Danziger, K. (2013). Psychology and its history. *Theory & Psychology*, *23*, 829–839. <http://dx.doi.org/10.1177/0959354313502746>
- Dewsbury, D. A. (2003). Archival adventures in the history of comparative psychology. In D. Baker (Ed.), *Thick description and fine texture: Studies in the history of psychology* (pp. 143–161). Akron, OH: University of Akron Press.
- Furumoto, L. (1989). The new history of psychology.

- In I. Cohen (Ed.), *The G. Stanley Hall lecture series* (Vol. 9, pp. 9–34). Washington, DC: American Psychological Association.
- Furumoto, L. (2003). Beyond great men and great ideas: History of psychology in sociocultural context. In P. Bronstein & K. Quina (Eds.), *Teaching gender and multicultural awareness: Resources for the psychology classroom* (pp. 113–124). Washington, DC: American Psychological Association. <http://dx.doi.org/10.1037/10570-008>
- Furumoto, L., & Scarborough, E. (1986). Placing women in the history of psychology: The first American women psychologists. *American Psychologist*, *41*, 35–42. <http://dx.doi.org/10.1037/0003-066X.41.1.35>
- Guthrie, R. V. (1998). *Even the rat was white: A historical view of psychology* (2nd ed.). Boston, MA: Allyn & Bacon.
- Harris, B. (1980). Ceremonial versus critical history of psychology. *American Psychologist*, *35*, 218–219. <http://dx.doi.org/10.1037/0003-066X.35.2.218>
- Harris, B. (1997). Repoliticizing the history of psychology. In D. Fox & I. Prilleltensky (Eds.), *Critical psychology: An introduction* (pp. 21–33). Thousand Oaks, CA: Sage.
- Harris, B. (2009). What critical psychologists should know about the history of psychology. In D. Fox & I. Prilleltensky (Eds.), *Critical psychology: An introduction* (2nd ed., pp. 20–35). Thousand Oaks, CA: Sage.
- Harrison, E. (1987). Whigs, prigs, and historians of science. *Nature*, *329*, 213–214. <http://dx.doi.org/10.1038/329213a0>
- Hartley, L. P. (1953). *The go-between*. London, England: Hamish Hamilton.
- Henle, M. (1976). Why study the history of psychology? *Annals of the New York Academy of Sciences*, *270*, 14–20. <http://dx.doi.org/10.1111/j.1749-6632.1976.tb34212.x>
- Hergenbahn, B. R., & Henley, T. B. (2013). *An introduction to the history of psychology* (7th ed.). Belmont, CA: Wadsworth.
- Hull, D. L. (1979). In defense of presentism. *History and Theory*, *18*, 1–15. <http://dx.doi.org/10.2307/2504668>
- Jones, D., & Elcock, J. (2001). *History and theories of psychology: A critical perspective*. London, England: Arnold.
- King, D. B., Woody, W. D., & Viney, W. (2016). *A history of psychology: Ideas and context* (5th ed.). London, England: Routledge.
- Kuhn, T. S. (1962). *The structure of scientific revolutions*. Chicago, IL: University of Chicago Press.
- Kuhn, T. S. (1970). *The structure of scientific revolutions* (2nd ed.). Chicago, IL: University of Chicago Press.
- Lamiell, J. T. (2003). *Beyond individual and group differences: Human individuality, scientific psychology, and William Stern's critical personalism*. Thousand Oaks, CA: Sage.
- Laudan, R. (1992). The “new” history of science: Implications for philosophy of science. *Proceedings of the Biennial Meeting of the Philosophy of Science Association*, *2*, 476–481.
- Leahey, T. H. (1986). History without the past [Review of the book, *Topics in the history of psychology*]. *Contemporary Psychology*, *31*, 648–650.
- Lovett, B. J. (2006). The new history of psychology: A review and critique. *History of Psychology*, *9*, 17–37. <http://dx.doi.org/10.1037/1093-4510.9.1.17>
- Moghaddam, F. M., & Lee, N. (2006). Double reification: The process of universalizing psychology in the three worlds. In A. C. Brock (Ed.), *Internationalizing the history of psychology* (pp. 163–182). New York, NY: New York University Press.
- Niiniluoto, I. (2015). Scientific progress. In E. N. Zalta (Ed.), *Stanford Encyclopedia of Philosophy*. Retrieved from <http://plato.stanford.edu/archives/sum2015/entries/scientific-progress/>
- O'Donnell, J. M. (1979). The crisis of experimentalism in the 1920s: E. G. Boring and his uses of history. *American Psychologist*, *34*, 289–295. <http://dx.doi.org/10.1037/0003-066X.34.4.289>
- Oreskes, N. (2013). Why I am a presentist. *Science in Context*, *26*, 595–609. <http://dx.doi.org/10.1017/S026988971300029X>
- Pickren, W. E., & Dewsbury, D. A. (2002). Introduction. In W. E. Pickren & D. A. Dewsbury (Eds.), *Evolving perspectives on the history of psychology* (pp. 3–8). Washington, DC: American Psychological Association. <http://dx.doi.org/10.1037/10421-000>
- Pickren, W. E., & Rutherford, A. (2010). *A history of modern psychology in context*. Hoboken, NJ: Wiley.
- Rappard, F. J. H. van. (1997). History of psychology turned inside(r) out: A comment on Danziger. *Theory & Psychology*, *7*, 101–105. <http://dx.doi.org/10.1177/0959354397071008>
- Rappard, F. J. H. van. (2004). Wundt as an activity/process theorist. In A. Brock, J. Louw, & W. van Hoorn (Eds.), *Rediscovering the history of psychology: Essays inspired by the work of Kurt Danziger* (pp. 141–160). New York, NY: Springer.
- Rappard, F. J. H. van. (2008). The future of history of psychology is history [Web log post]. Retrieved from <http://psychozine.blogspot.ie/2008/08/future-of-history-of-psychology-is.html>
- Richards, G. (2010). *Putting psychology in its place: Critical historical perspectives* (3rd ed.). London, England: Routledge.
- Robinson, D. N. (2013). Historiography in psychology: A note on ignorance. *Theory & Psychology*,

- 23, 819–828. <http://dx.doi.org/10.1177/0959354313499426>
- Rose, N. (1996). Power and subjectivity: Critical history and psychology. In C. F. Graumann & K. J. Gergen (Eds.), *Historical dimensions of psychological discourse* (pp. 103–124). New York, NY: Cambridge University Press. <http://dx.doi.org/10.1017/CBO9780511571329.006>
- Ross, D. (1969). The “Zeitgeist” and American psychology. *Journal of the History of the Behavioral Sciences*, 5, 256–262. [http://dx.doi.org/10.1002/1520-6696\(196907\)5:3_256::AID-JHBS2300050305_3.0.CO;2-5](http://dx.doi.org/10.1002/1520-6696(196907)5:3_256::AID-JHBS2300050305_3.0.CO;2-5)
- Samelson, F. (1980). E. G. Boring and his history of experimental psychology. *American Psychologist*, 35, 467–470. <http://dx.doi.org/10.1037/0003-066X.35.5.467.b>
- Samelson, F. (1999). Assessing research in the history of psychology: Past, present, and future. *Journal of the History of the Behavioral Sciences*, 35, 247–255. [http://dx.doi.org/10.1002/\(SICI\)1520-6696\(199922\)35:3_247::AID-JHBS4_3.0.CO;2-G](http://dx.doi.org/10.1002/(SICI)1520-6696(199922)35:3_247::AID-JHBS4_3.0.CO;2-G)
- Schultz, D. P., & Schultz, E. S. (1987). *A history of modern psychology* (5th ed.). San Diego, CA: Harcourt Brace Jovanovich.
- Schultz, D. P., & Schultz, E. S. (2004). *A history of modern psychology* (8th ed.). San Diego, CA: Harcourt Brace Jovanovich.
- Skinner, B. F. (1983). *A matter of consequences. (Part three of an autobiography.)*. New York, NY: Knopf.
- Smith, R. (1988). Does the history of psychology have a subject? *History of the Human Sciences*, 1, 147–177. <http://dx.doi.org/10.1177/095269518800100201>
- Stocking, G. W., Jr. (1965). On the limits of “presentism” and “historicism” in the historiography of the behavioral sciences. *Journal of the History of the Behavioral Sciences*, 1, 211–218. [http://dx.doi.org/10.1002/1520-6696\(196507\)1:3_211::AID-JHBS2300010302_3.0.CO;2-W](http://dx.doi.org/10.1002/1520-6696(196507)1:3_211::AID-JHBS2300010302_3.0.CO;2-W)
- Thomas, R. K. (2007). Recurring errors among recent history of psychology textbooks. *American Journal of Psychology*, 120, 477–495.
- Tyson, P. J., Jones, D., & Elcock, J. (2011). *Psychology in social context: Issues and debates*. Hoboken, NJ: Wiley.
- Vaughn-Blount, K., Rutherford, A., Baker, D., & Johnson, D. (2009). History’s mysteries demystified: Becoming a psychologist-historian. *The American Journal of Psychology*, 122, 117–129.
- Walsh, R. T. G., Teo, T., & Baydala, A. (2014). *A critical history and philosophy of psychology: Diversity of thought, context, and practice*. New York, NY: Cambridge University Press.
- Watson, J. B. (1913). Psychology as the behaviorist views it. *Psychological Review*, 20, 158–177. <http://dx.doi.org/10.1037/h0074428>
- Watson, R. I. (1963). *The great psychologists: From Aristotle to Freud*. Philadelphia, PA: Lippincott.
- Weinberg, S. (2015). Eye on the present: The Whig history of science. *The New York Review of Books*, 62, 82–84.
- Wundt, W. (1900–1920). *Völkerpsychologie: Eine Untersuchung der Entwicklungsgesetze von Sprache, Mythos und Sitte* [Völkerpsychologie: An examination of the developmental laws of language, myth and custom]. Leipzig, Germany: Engelmann & Kröner.
- Wundt, W. (1916). *Elements of folk psychology: Outlines of a psychological history of the development of mankind* (E. L. Schaub, Trans.). London, England: Allen & Unwin. <http://dx.doi.org/10.1037/13042-000>
- Young, R. M. (1966). Scholarship and the history of the behavioral sciences. *History of Science*, 5, 1–51. <http://dx.doi.org/10.1177/007327536600500101>
- Zenderland, L. (1998). *Measuring minds: Henry Herbert Goddard and the origins of American intelligence testing*. Cambridge, England: Cambridge University Press.

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