In the past 30 years, the “new history of psychology” and its adherents have advocated a critical approach to scholarship, increased use of primary sources, a focus on sociopolitical forces, and the active inclusion of psychologists from underrepresented groups. This article argues that many scholars exaggerate the differences between old and new history of psychology, and that where the differences are indeed large, those discrepancies reveal certain limitations unique to the new history approach. These limitations, presented in the form of 5 questions posed to new historians, lead to a discussion of professional issues in the history of psychology.

In his preface to *The Great Psychologists* (1963, p. vii) wrote that “great men in psychology live on in the work for which they are an inspiration in the field. . .Their lives, occupations, motives, families. . .must be considered if we are to understand them.” Sentiments like these made Watson’s textbook a best-seller, as far as history of psychology books go, and they also made Watson the honorary father of a movement that led to professional associations and peer-reviewed journals focusing on the history of psychology. 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.

This is partly attributable to the “new history” approach in the history of psychology. New historians argue that traditional history of psychology is too celebratory and ceremonial and that it is designed to search for lessons for present-day psychologists, to socialize students into the culture of academic psychology, and to look for antecedents to present-day research in order to bolster the claim that psychology’s story is that of a constantly progressing science. The new history approach has been explicated in several cogent papers, but no published critique of this style exists. Rather, it seems to have been accepted somewhat uncritically by professional historians and textbook writers.

This article has three goals. First, I briefly review the old and new histories, focusing on representative examples of each. Specifically, claims made by new historians about the old histories are judged against relevant documents. Second, I suggest some limitations of the new history of psychology by posing several
questions to the new historians. I argue that: (a) New historians have prematurely committed themselves to normative views on historiographical issues where no clear consensus exists; (b) new history research often rests on a particular view of science that is not widely accepted by contemporary philosophers of science, nor even understood by professional philosophers in the same ways that the new historians often assume; (c) propagating the new history uncritically may impair relations between behavioral scientists and historians of psychology; (d) the normative guidelines advocated by new historians may be impossible to follow consistently; and (e) new history avoids making commitments about the proper object of history of psychology while asserting that the old historians’ preferred objects are insufficient. Finally, I discuss several intrinsic problems of historical scholarship, focusing on the possibilities for a critical history of psychology.

It is important to note that although this is a study of the new history approach, it is not a general review of historical scholarship of recent years. The central documents examined here are the proclamations and expositions of the new history approach, not the first-order research that constitutes current history of psychology. Any material of the latter type is used only as an example of general points. As such, the “new historians” are taken to be those scholars who explicitly discuss historiographical issues, and these individuals are permitted to define the new history approach themselves.1

The Old History of Psychology

New historians often begin by first describing the “old history” approach, and so it is important to consider what documents would constitute old histories. Histories of psychology covering the psychological views of philosophers were already being published before psychology’s first laboratories had been founded; Littman (1981) found that the first history of psychology was published in the early 19th century. Not long after laboratories were founded, these histories began to incorporate the lives and work of the earliest experimental psychologists. Indeed, at the end of his Psychology: Briefer Course, William James (1892) remarked how “strange” it was to see such histories of psychology already.

Edwin G. Boring

The first history of psychology to focus on empirical work was Edwin Boring’s A History of Experimental Psychology (AHEP; Boring, 1929). Boring had studied under the structuralist Edward Titchener at Cornell University, and as one of Titchener’s most loyal students, Boring desired that psychology remain a “pure” ivory tower discipline of the laboratory. His book is erudite but also quite engaging, and it was very popular as a textbook for college courses on the history of psychology (Nance, 1962). The revised second edition of the book (Boring, 1950) expanded the focus beyond traditional experimental psychology to include such topics as psychoanalysis and educational psychology.

1 The assumption undergirding this inquiry (viz., that there truly is a new approach to doing history of psychology, markedly different from an identifiable “old history” approach) is a topic worthy of fuller analysis itself, but a comprehensive treatment of this issue is beyond the scope of this article.
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. One set of critics focused on Boring’s treatment of Wundt and claimed that Boring’s slavish adherence to Titchener had colored that treatment. Blumenthal (1979), while researching Wundt, was one of the first to make these claims, and more recently he commented that Boring’s style of history “that inspired hero-and-demon legends involving Wundt may have helped keep many a student awake during history lectures but may not have encouraged aspirations to methodological precision in historiography” (Blumenthal, 1998, p. 80). Danziger (1979) similarly commented, “It is apparent that Boring took his admired teacher, E. B. Titchener as a guide in these matters, and Titchener practically made a career out of interpreting Wundt in his own highly idiosyncratic fashion” (p. 206). A glance at contemporary textbooks on the history of psychology shows that this revision has been widely incorporated into pedagogical material; Hergenhahn (2001, p. 240) and Thorne and Henley (1997, p. 4) are but two examples. Reading Boring’s (1929, chap. 15) treatment of Wundt, it is clear that he makes Wundt out to be more atomistic than current historians believe him to be, and Boring also almost entirely ignores Wundt’s *Völkerpsychologie*, which Wundt considered to be a major part of his system (Leahey, 2001).

O’Donnell (1979) did not question the accuracy of Boring’s history but rather characterized *AHEP* as part of Boring’s war against the rise of applied psychology in the United States. In the 1920s, American applied psychology was growing in the areas of personnel testing, educational psychology, and clinical psychology. Harvard’s psychology department, of which Boring was a member, remained a bastion of “pure” laboratory psychology, but Boring’s insistence on this forced many important faculty (e.g., Robert Yerkes, Ralph Perry) to leave. O’Donnell undertook to “scrutinize the historian’s preconceptions and to elicit his or her unstated purposes” (p. 289) and claimed to have found that Boring’s unstated purposes in writing *AHEP* were to provide “not only experimentalism’s consummate advertisement but also an intellectual apologia for the professional lives of embattled experimenters” (p. 293). There is no direct evidence of this, and despite his meticulous reading of Boring’s voluminous correspondence, O’Donnell never mentions any admittance on Boring’s part of his purposes in writing *AHEP*. Furthermore, Samelson (1980) argued persuasively that many other factors played a role in motivating Boring to write *AHEP*, including his need to represent psychology as a field separate from philosophy, his distaste with the conditions of laboratory work at Harvard (including poor financial support), and Boring’s self-conscious concerns about his productivity as a professor.

These are the essential direct criticisms of Boring’s history—that it mischaracterized Wundt and that it was motivated by a desire to legitimate traditional experimental psychology. These criticisms contribute to the description of *AHEP* as representative of the old history of psychology because of Boring’s reliance on Titchener (old histories are based too much on secondary sources, it is said) and Boring’s view of history as justifying current work (especially certain types of work).
It is difficult to name other works that characterize the old history style because the new historians tend to cite only Boring (1929, 1950) as an example. The seminal works on the new history approach (e.g., Furumoto, 1989) discuss the characteristics of old history in detail but without citing specific published works, apart from vague references to “textbooks.” Three well-known texts (Flugel, 1933; Heidbreder, 1933; Watson, 1963) are far less scholarly than Boring, but they share many of the same old history features discussed below.

Aspects of the Old History of Psychology: An Attempt at Systematization

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, even when it is currently believed that those contexts had great implications for the work of certain psychologists. For example, the gestalt psychologist Köhler’s role in the First World War may be debated, with some believing that he served as a German spy (Ley, 1990) and others focusing on his antinationalistic sentiments (Henle, 1978), but most agree that the war influenced his research on the island of Tenerife. However, Boring (1950, p. 596) simply noted, without further elaboration, that “At Teneriffe he [Köhler], a German subject, was caught by the First World War, much to the advantage of psychology, as it turned out.”

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 (Kragh, 1987). Although mere reference to the present does not constitute presentist inquiry, old histories select certain psychologists because their work is relevant to current problems, and an explicit attempt is often made to interpret past work in light of present knowledge. Part of this is due, no doubt, to the pedagogical function of the writing; connecting new information to facts that the student already knows is a common didactic strategy with much to be said for it.

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. This assumption is rarely if ever explicitly stated in old histories, perhaps because it is so commonly held.

These three commonalities—internalism, presentism, and Whiggishness—are obvious, but beyond these it is unclear what features make all of the old histories so similar. 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 (1950) 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. Boring’s harsh evaluation of James Mill (Boring, 1950, p. 226) and balanced but thoughtful assessment of
the philosophical foundations of Gestalt psychology (p. 593) are but two examples. Heidbreder (1933) is also notable for her evenhanded but wise assessments of each system. Current undergraduate textbooks are certainly not without criticism, and some even enumerate criticisms of each system in explicit lists (cf. Hergenhahn, 2001; Lundin, 1991). 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.

Another common claim is that the old histories are “Great Man” histories. Both Furumoto (1989) and Leahey (1986) made this claim, and Leahey informed us that the “new history of psychology favors Zeitgeist interpretations” (p. 649). But 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. He ‘founded’ experimental psychology, but in that he was more the instrument of the times than an originator” (p. 660). Rather than claiming that it was Wundt’s independent brilliance and force of will that created experimental psychology, Boring invokes the zeitgeist.

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. It is also important to be careful in what we mean in designating certain sources as “primary.” Some new history proponents (e.g., Leahey, 1986) emphasize the new trend in archival research, but a published article by William James is obviously no less primary than a handwritten letter to his brother. Indeed, Himmelfarb (1987) prudently cautioned us against a more general trend in historiography of disparaging published writings “as if formal documents are less trustworthy than private communications, as if forethought and deliberation imply Machiavellian attempts to conceal the truth” (p. 19).

So whereas the old histories are internalist, presentist, and Whiggish, they do not appear to be devoid of criticism, nor is it clear they rely more on secondary than primary sources. We have a clear portrait of the old history of psychology, so what is the new?

The New History of Psychology

Definitions and Distinctions

Furumoto (1989) wrote the seminal article introducing the new history of psychology to general psychology. In an invited G. Stanley Hall Lecture in 1988, she characterized the new history succinctly:
The new history of psychology tends to be critical rather than ceremonial, contextual rather than simply the history of ideas, and more inclusive, going beyond the study of “great men.” The new history utilizes primary sources and archival documents rather than relying on secondary sources, which can lead to the passing down of anecdotes and myths from one generation of textbook writers to the next. And finally, the new history tries to get inside the thought of a period to see issues as they appeared at the time, instead of looking for antecedents of current ideas or writing history backward from the present context of the field (p. 18).

Furumoto (1989) traced the origin of new history to the year 1912, when historian James Robinson published a volume of essays entitled The New History, in which he argued that instead of focusing on “heroic persons,” historians should characterize the past in terms of “national habits,” the broadly conceived characteristics of a culture. Furumoto reviewed how the new history gradually infiltrated the history of science and only recently incorporated itself into the history of social science.

Leahey (1986), in a book review of a two-volume anthology containing chapters in the old history style, also characterized the new history of psychology by describing what it reacted against: namely, the old history. Leahey described the new histories as externalist, historicist, zeitgeist histories, aware of recent historical scholarship, and seeking the deep roots of psychology, not content to stay within the past two centuries.

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). Ash (1983) discussed this further and related this dichotomy to what he described as the two functions of the discipline of the history of psychology: providing an appreciative overview of the field of psychology for psychologists and providing a valuable research area for historians.

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, to those with specialized training in historical research methods, who are interested in studying the past not because it is relevant today, but merely because it is there.2

Typical Examples of the New History of Psychology

To assess whether the new history of psychology has met its own goals, it is imperative to find typical examples of this new genre. Until now, most of the

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2 This describes the approach explicitly called the “new history of psychology.” It is important that there have been many recent trends in the history of psychology, including feminist theory, postmodernism, and even quantitative approaches (e.g., Simonton, 2002). This article limits its focus to those aspects identified by those scholars who use the term “new history.”
documents we have examined contain calls for a new approach to the history of psychology but are not historical work themselves. Unfortunately, most of the first-order work in history of psychology does not self-identify as taking a “new history” perspective, and so “typical” examples of this approach are difficult to find. I have chosen to discuss one monograph (Danziger, 1990) and one textbook (Jones & Elcock, 2001), because the former is, by common consensus, a central work in the new history of psychology, and the latter identifies itself as part of the new history movement. My discussion is brief and aims only to illustrate the characteristics of new history of psychology described in the previous section.

In *Constructing the Subject*, Danziger (1990) examined the “socially constructed nature of psychological knowledge” (p. 2), focusing on interactions between behavioral scientists and the societal context. Kurt Danziger explored the surprising changes in research methodology between 1880 and 1940, arguing that the objectivity, quantification, and asymmetrical relationship between experimenters and participants that are now considered uncontroversial parts of the scientific method are in fact a fairly recent invention and that their ascent has more to do with societal factors than with purely scientific concerns.

The clearest new history attitude expressed by Danziger (1990) was a disavowal of Whig history; he asserted that “In studying changes in prevailing conceptions of psychological knowledge, the question of the relative truth of these conceptions is not relevant” (pp. 11–12). This anti-Whiggishness leads to a critical history in that current opinions of which psychologists and ideas had “won” and “lost” are challenged, and current practices of psychology are questioned. In addition, *Constructing the Subject* is externalist both in its basic argument and in the research contained therein; Danziger used original sources from statistics, education, and philosophy to illustrate connections between these fields and psychology as well as to provide contrasting perspectives that psychologists of that time period ignored or misinterpreted.

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. Also, although his purpose was certainly not to legitimate present practice in psychology, the present seems firmly in mind when topics were chosen; whether this should count as presentism is debatable. In sum, then, *Constructing the Subject* is squarely within the new history framework but does not fulfill all of the criteria exactly.

A similar judgment may be made of *History and Theories of Psychology: A Critical Perspective* (Jones & Elcock, 2001), which offers a distillation of the new history meant for students. It is so far one of the only introductory history of psychology textbooks to incorporate the critical perspective so strongly (see also Richards, 2002, and to a lesser extent, Leahey, 2001), and many of the features of the new history are to be found here. Dai Jones and Jonathan Elcock have written a moderately externalist text, with substantial coverage of social context in the development of professional psychology. The authors also echoed the new historians in their perspective on the old history of psychology as consisting largely

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3 One index of the importance of Danziger (1990) to the new history of psychology can be found in a new edited volume in honor of Danziger (Brock, Louw, & van Hoorn, 2004), in which *Constructing the Subject* is used for intellectual foundation by many of the contributors.
of self-justifying “origin myths.” The section on “The Myth of Wundt” (Jones & Elcock, 2001, pp. 25–27) discusses the easy confusion of “psychology” with experimental psychology and how the founding of a laboratory is taken by many to be the start of “true” psychology.

In addition, Jones and Elcock (2001) were inclusive in an intellectual sense—areas of psychology that are not typically covered were given their own chapters (e.g., connectionism, social psychology), and related areas of other disciplines (e.g., the folk psychology research of cognitive science) were included. It is unclear whether, given the space limitations, the coverage is deep enough to give the projected audience of advanced undergraduates more than a superficial overview of these areas, but Jones and Elcock have obviously not ignored recent trends in psychological theory the way that some historians (or, more properly, textbook authors) seem to have done.

Whether inclusiveness is an effective weapon against presentism is a more difficult issue. Jones and Elcock (2001) are presentist in that they like to expose the “real” historical foundations of the practices that we take today for granted. This appears to be a reasonable goal in keeping with a critical perspective and raises the possibility that presentism may be a more effective way to do critical history than the pure historicist framework advocated by so many of the new historians (see also Buss, 1977). Looking at Danziger (1990) and Jones and Elcock (2001), it appears that the new history is not so different from the old as its practitioners would argue. Just as an analysis of Boring (1929, 1950) reveals many features that do not support the old history stereotype, readers of these new history sources may be led to question just how “new” these types of books are.

Five Questions for the New Historians

The aims of the new history of psychology have been accepted virtually without protest by many in the field. It is difficult to find a journal article, research monograph, or even an undergraduate textbook that defends the old history in any way. Even when authors do not practice the tenets of the new history, they are anxious to preach them. Here I consider some of the issues that the new history raises, issues that I believe the field has not considered sufficiently.

Is the New History Merely Whiggish in a New Way?

First, is the new history of psychology merely Whiggish historiography? That is, do new historians assume that older histories and historians were, at best, necessary stepping stones toward the current, better way to do history of psychology? Young (1966) showed this type of condescension when he noted that the history of the behavioral sciences “has all of the trappings of a validly scholarly discipline; like history of science in general, it has passed through the useful but limited stage of amateurism” (p. 16) and commented that “the limitations which were inevitable in pioneering work such as that of Boring should now be eliminated” (p. 16). Littman (1981) similarly saw the transition from history written by scientists to history written by historians as the sign of the maturity of the discipline.

It was, however, Samelson (1999, p. 248) who presented the clearest and most explicit (as well as Whiggish) chronology of historical work on psychology. He
described the field as emerging from the “shadow” of Boring’s AHEP and “awakened” by the critiques of Young (1966; see above) and Stocking (1965) as well as the philosophy of science of Thomas Kuhn (1962). Since then, Samelson noted, the history of psychology has become even “newer,” adding perspectives from social constructionism, deconstruction, and feminist theory. That Samelson sees these changes as desirable is obvious when he complains about the “lack of visible impact” of the new history of psychology on textbooks and college classes.

Is this new Whiggish history of the history of psychology defensible? Some of its claims do seem indisputable. 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.

More important, these proponents of the new history of psychology will have to contend with critics of the general new history program. Heralds of the new history of psychology imply that advances have been made in historiography and that historians of psychology are late in catching up, but in holding this view, they ignore the many critics of the new history of science: Hull (1979) offered a now famous defense of certain aspects of presentism, concluding that “knowledge of the present is absolutely crucial for the historian, both in reconstructing the past and in explaining it to his readers” (pp. 14–15). Laudan (1992) characterized the new history of science as based on “high-falutin and ill-formulated theories” from “continental philosophy, literary theory, cultural anthropology, and the gurus of postmodernism” (pp. 479–480) and argued that the new historians’ focus on social context makes them ignore the mental activity of the scientist. As a final example, Harrison (1987) responded to charges of Whiggishness by developing the concept of “priggishness” in the history of science, accusing the new historians of a priggish “extreme that makes a virtue of ignorance and discards from the present what contributes nothing to the past” (p. 214). Certainly, the work of Hull, Laudan, and Harrison is not itself above criticism, but 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.

Is Science Less Capable of Progress Than Historical Scholarship?

The changes in historiography lead naturally to a second question: 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 done much good for the field. Yet measured arguments against psychology’s progress are rare; direct assertions are more common. And so Samelson (1999) told us that psychology is “not a story of unilinear accumulation of scientific knowledge and straightforward progress” (p. 249). Previously, Ash (1987) had referred to “the story of a continuous upward climb from the depths of philosophical speculation to the heights of cumulative
experimentation” (p. 3), and Harris (1979) referred to the “false sense of continuity” in the history of psychology (p. 157).

Ash (1983) asserted that the desire for a sense of continuous progress led early historians of psychology to downplay disagreements between different schools of psychology or to suggest optimistically that new discoveries would end such disagreement. Ash attributed these hopeful appraisals to the pedagogical purpose of the books in which they were offered, but Ash never actually demonstrated that psychology has not progressed, nor is it clear that he thinks no progress has taken place.

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. On one view, he argued that progress in science is apparent rather than real, because science is based on broad, high-level theoretical frameworks (paradigms), and when paradigms change, all the knowledge from the old paradigm is, in a very real sense, lost. That is, when basic foundational assumptions change, all the “progress” becomes meaningless, because even the words that scientists use change meaning. The results of experiments are always tainted by the paradigm that gives the results their interpretation, and so even at a given time, what is progress to one scientist may not be progress to another. It should be pointed out that scholars vary tremendously in how radical they take Kuhn to be. Some (e.g., Reisch, 1991) see Kuhn’s thought as the expected outcome of logical empiricism, whereas others (e.g., Klee, 1997, chap. 7) see Kuhn as replacing rationality with extreme relativism. An in-depth analysis and assessment of Kuhn’s ideas is beyond the scope of this article, but such an evaluation is not necessary to judge the propriety of the influence of this single thinker on the new historians.

Despite Kuhn’s tremendous influence on psychology (see Driver-Linn, 2003), his views are by no means taken as valid by philosophers of science. Criticism against Kuhn began as soon as the first edition of The Structure of Scientific Revolutions was published (e.g., Lakatos & Musgrave, 1970). Many aspects of Kuhn’s thought are hotly debated issues among the scholars whose training allows them to understand those views best—that is, to philosophers of science and not historians of psychology. Moreover, Kuhn’s writings are no longer necessarily taken as the best statements of those fundamental issues; one recent chapter-length presentation on trends in the philosophy of social science (Kincaid, 2002) made no reference to Kuhn at all. Perhaps most important, those historians who make claims against the possibility of progress in science and support their claims by citing Kuhn do not realize that in philosophy, citing a work of scholarship does not provide an argument for its validity.4

If Kuhn is both radical and right, there are significant implications for histories of science. Whig history, which treats the past as a series of progressive

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4 The application of Kuhn to the history of psychology has been treated by Gholson and Barker (1985), who used the development of learning theory in the middle 20th century to argue that Kuhnian ideas do not account for scientific changes during this period as well as do other contemporary philosophical perspectives on science (specifically, Lakatos and Laudan).
steps toward the present, assumes that such progressive steps are possible. However, enough philosophers of science believe that progress in science is real and measurable that basing a history of a science on this assumption is hardly absurd. For instance, Mayr (1990) argued that “in a succession of theories dealing with the same scientific problem each step benefits from the new insights acquired by the preceding step and builds on it” (p. 302). Proponents of the new history or new philosophy of science might be correct in calling Mayr naïve, but this charge must be argued out in detail, rather than asserted as an equally bald assertion, followed by several citations that are thought to settle the matter.

An issue very close to the possibility of progress in science concerns the basis for changes in science—specifically, the role of rationality in the interpretation of experiments and the motivation to accept new theories. The new historians of science appear to endorse a model of scientific change pioneered by post-Kuhnian sociologists of science whose scholarship is summarized by Golinski (1998, p. 27) thus:

Scientific practice was shown as open-ended and underdetermined, scientists not being compelled either by logical deduction from existing beliefs or by unambiguous evidence to develop their ideas in a particular direction. Instead, they were found to be making practical judgments that could be related to the local subculture in which their resources and skills were invested and their specific aims pursued. In controversies, the texture of these (normally hidden) commitments was brought to the surface and exposed to view.

New historians are more likely than their predecessors to emphasize the role of social and political factors in theory choice, rather than presuming that a pure faculty of reason decides what each experiment means and which theory is supported. 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.” The evidence for motivated reasoning (which even a weak form of constructivism depends on) is certainly abundant (ironically, the most compelling demonstrations come from scientific research on reasoning; see Kunda, 1990), but sociologists of science have been challenged as having overstated their case. For instance, Klee (1997, pp. 177–179) argued 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; Klee would suggest that certain outlandish explanations are ruled out because of concerns of rationality. Sociologists of science might claim that the choice of which theories to label “outlandish” is determined by the social and political expediencies of the situation, but this is a topic widely debated in the philosophy of science (McGuire, 1992), and so new historians of science who assume the validity of constructivism may beg questions that they could be answering with evidence from their own scholarship.

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

Even if we ignore the debates about progress and assume for the moment that the new historians are correct in their skepticism concerning the rationality and...
objectivity of scientists (including psychologists), a third question to the new historians of psychology arises: Given such a situation, what is the place of the new history in the field of psychology?

This issue was applied to science instruction by Brush (1974), who noted the incompatibility between the historical accounts of seminal scientists given in science textbooks and the most recent and accurate accounts given by trained historians. Brush also cited embarrassing statements of important physicists that go against the most basic rules of the scientific method and eventually concluded that “the science teacher may be justified in following his instincts to ignore history, especially if his purpose is to train scientists who will follow the currently approved research methods” (p. 1166).

Brush noted that the science teacher aims to be a presentist historian, presenting the past only insofar as it has important influence on present problems, and Brush would not find it pedagogically useful to waste time reviewing experiments that seemed important only at that time or to discuss historical figures who worked on problems that are not now considered to be part of the subject. External history would seem even more of a waste of time. Even though history of psychology is taught to psychology majors (and graduate students) as a separate course, one purpose of such a course is to help the student make connections between different psychologists, contradictory theories, and disparate areas by showing that many of these differences are reduced in historical perspective. One contemporary textbook (Thorne & Henley, 1997) even uses “concept maps” to trace the historical lineage of various psychologists and their theories.

I am certainly not suggesting that current historians should distort their work to motivate students. Even if empirical research found that Whig histories of psychology did this, more accurate new history should hardly be abandoned. The problem is not with the new history per se but with the mismatch between its own perfectly valid goal (accurate, contextual historical scholarship) and one of the goals of undergraduate education in the history of psychology (i.e., presentist searches for the origins of today’s ideas). Whether or not the new history will serve to demoralize potential psychologists, a recent article on the status of the history of psychology course (Bhatt & Tonks, 2002) suggested that many historians of psychology (as well as psychologists familiar with the new history) would like to be of service to psychology departments and students. Bhatt and Tonks complained about how they “noticed that the undergraduate courses on the history of psychology were ‘cut’ or ‘reduced’ or no longer ‘required’” as well as how they “talked to some esteemed historians of psychology who were retiring and learned about their lost ‘ideological warfares’ within their psychology departments” (paragraph 3). 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.
A more recent article by Brush (1995) suggested that the argument over pedagogical practice relates to a larger issue of the relationship between scientists and historians. Psychologists who fear the new history of psychology might not only want students to avoid it, but also detect a bias against science in the new history. The critical, contextual nature of new history of science is what led Kuhn to say that “insofar as possible... the historian should set aside the science that he knows” (as cited in Brush, p. 218); too much knowledge of present-day science leads to presentist inquiry, and besides, having knowledge that your long-dead subjects lacked constitutes “cheating.” But as Brush pointed out, it is necessary to know the details of science to be able to properly interpret the past: “Recently it has been suggested that Robert Brown did not actually observe the kind of particle-movement now named after him. Again, the technical expertise of the scientist is needed to test this historical hypothesis” (p. 229). None of this argues in favor of Whig histories or in favor of inaccurate histories designed to attract new students to the field; here Brush is simply making the point that presentism and internalism are not necessarily deficits in historical scholarship.

Is Presentism Unavoidable?

Even if the new history seeks to become nothing more than an adjunct in psychology departments, it faces a fourth question. Is it possible to do truly non-Whiggish, nonpresentist history? That is, can the historian look at the past without the thick lens of present concerns over his or her eyes?

Baltas (1994) argued persuasively that presentism can never be eliminated from the history of science. His argument starts out by assuming some very Kuhnian premises—namely, that scientists’ cultural environments (e.g., where they live, which lab group they are in, how they were raised) place significant cognitive “constraints” that constitute a framework for understanding the world. Furthermore, because of our “biological, psychological, and social constitution” (p. 112), our reasoning relies heavily on assumptions and premises that are in large part determined by that framework. Because these assumptions and premises are largely unconscious, we cannot escape our framework. If this is the case, how could we evaluate any other framework (or anything else) from outside our own framework? How could we look at the past and not be guided by the framework that we have acquired in present times?

Baltas (1994) allowed one solution to the problem: We could claim that frameworks steadily widen over time, and so while our perspective is different from that of, say, Wundt, it includes his perspective. Our cultural framework has become steadily “wider,” and so science is now “richer” than it was before. The problem with this, of course, is that it leads directly to a Whig history. The new historian tempted to find a way to make his or her own historicist inquiries possible ends up admitting that the sciences he or she studies can make progressive, cumulative knowledge.

A very different approach to history that also denies the possibility of true historicism is informed by hermeneutics, the discipline of text interpretation. The hermeneutic approach, especially in the works of Hans-Georg Gadamer (see Dostal, 2002), points to the embedded nature of the individual in time and place and also to the necessity of “prejudgments” contributed by that time and place.
For Gadamer, when we approach any text (such as a historical document), we arrive with prejudgments in hand—based on, for instance, what we have heard of the author, what others have told us about the book, what the title makes us think about, and so forth. The act of interpreting the document is an interactive process between the interpreter and the text in which (optimally) the prejudgments become explicit, analyzed, and then revised by referring to the text, and so in some sense, the interpreter constructs meaning out of the text. My purpose here is not to defend the hermeneutic approach, but to mention it; the contribution of hermeneutics to historiography is substantial and seems to continue to grow (see Stanford, 1998, pp. 187–200), and it represents an important school of thought that calls into question the possibility, as well as the desirability, of historical scholarship that seeks to erase the influence of the present.

Even if nonpresentist history is possible, it is still extremely difficult. Pickeren and Dewsbury (2002) unwittingly showed just how difficult it is while trying to be good new historians. While pointing out the need for histories of psychology to discuss failures of the discipline as well as successes, they considered the case of Robert Yerkes:

But Yerkes was a person of his times, as his interpretation of the psychological test data gathered from the mammoth testing of military personnel during World War I demonstrates. Results of the test data indicated significant differences among various ethnic and racial groups and an overall low average intelligence of American military recruits. As Yerkes saw it, the data were best understood as supporting a notion that intelligence was mainly due to inheritance (we would now say genetics). Overall, his interpretations reflected his belief in the heredity of intelligence and the superiority of some racial and ethnic groups over others. (To his credit, Yerkes did later recant some of these views.) (Pickeren & Dewsbury, 2002, p. 4)

In examining the Yerkes passage carefully, several problems are obvious. To say “But Yerkes was a product of his time” does not so much note the influence of the social environment as it reduces Yerkes’s views to a crude social determinism. Furthermore, if Yerkes believed that some of the variability in intelligence is due to genetic variation, he is in good company with intelligence researchers of both his time and ours (cf. Brody, 1992; Mackintosh, 1998). Finally, to say that Yerkes’s recanting of his views is “to his credit” is a judgment more presentist than any made by Boring. If truly historicist history of psychology is possible, the new historians will have to make better attempts to prevent politically motivated revisionist histories (Harris, 1997).

This is no minor problem for those who both accept the new philosophy of science and attempt to do historical work themselves. The new historian must criticize older histories for presentism and legitimation biases while simultaneously advocating the new philosophy of science that implies that research is always conducted under a set of guided assumptions and that these sets of assumptions cannot be meaningfully compared.

Who Should We Study?

Until now, I have been discussing issues concerning historical figures who are well known: Wundt, Titchener, Yerkes. 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.

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 (rather than “Great Man” history), demonstrating the same hagiographical tendencies that the new historians claim to react against. In addition, the history is sometimes presented with a political agenda rather than a scholarly backdrop. In this connection, it is useful to compare Furumoto and Scarborough’s article with a paper by Bernstein and Russo (1974); the former article, although in the old history style, presents information on early female psychologists with an objective tone, whereas the latter paper, entitled “The History of Psychology Revisited: Or, Up with Our Foremothers,” begins with an anecdote about male chauvinist graduate students, infers that “Male bias pervades the very essence of the profession [of psychology]” (p. 130), and concludes with a call to use the history of psychology for a specific pedagogical function—to change students’ views about female psychologists’ competence. Bernstein and Russo do not explicitly place themselves in the new history framework, but Furumoto’s (1989) introduction to the new history of psychology cites their article, with apparent approval.

It is difficult to see how studies such as these, and that of Guthrie (1998), are different from descriptions of a random sample of psychology professors in 1910, except that the subjects come from underrepresented social groups. 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. Furthermore, if historians defend scholarship on otherwise unexceptional individuals by claiming that the presence of members of underrepresented groups changed the very nature of psychology as a profession (a reasonable claim), mere summaries of the life and works of these individuals will not be sufficient.

In any case, it is the new historians who first raised this question when they criticized the presentism evident in old histories of psychology. Not only do old histories study those historical ideas that bear a resemblance to today’s problems, they also study psychologists who had influential careers and important graduate

Certainly, historical scholarship in psychology need not be centered on individual psychologists; much interesting work has focused on a particular method, location, or school of thought. I phrase the question in terms of “who” largely because it is new historians who have criticized the choice of people that have traditionally been included in histories of psychology.
students. 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. At least Flugel (1933) was honest when he said of his own early text that “Such a book as this is almost inevitably bad. . . . things in which he [the reader] is little interested will be treated at apparently unnecessary length and with unreasonable emphasis; while other aspects of the subject, on which he would willingly linger, are summarily dismissed or omitted altogether” (p. 7).

Conclusions

Two Critical Histories

In designing these queries for the new history of psychology, it was easy to see that the new historians have struggled with some of the same questions. However, their conclusions on the value of the new history of psychology have rarely been especially balanced. Perhaps the reason for this lack of self-criticism is the meaning of the word “critical” itself, or more precisely, the different meanings that “critical” has at different times to the new historians.

One sense in which proponents of the new history use the word critical appears to be borrowed from “critical theory,” a movement in philosophy, sociology, and textual studies originating at the Institute for Social Research at the University of Frankfurt (cf. Turner, 2003, chaps. 12–13). Max Horkheimer, one of the “Frankfurt School” theorists, defined criticism as follows:

that intellectual and eventually practical effort which is not satisfied to accept the prevailing ideas, actions, and social conditions unthinkingly and from mere habit; . . . [the effort] to distinguish appearance from the essence, to examine the foundations of things, in short, really to know them. (cited in Bernstein, 1976, p. 180)

This definition is not so different from critical thinking, something that can hardly be argued against. 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.

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. If we are not careful, “critical history” becomes a thoughtless inversion of all of our current thought structures and bases its claim to validity on the simple fact that it is the opposite of what used to be thought. This may be critical in a certain sense, but it does not involve any critical thinking, and it can become almost algorithmic in its distrust of the old and preference for the new and trendy forms of sociology of science, historiography, and epistemology.

There is a tension in the proponents of new history between being critical in Horkheimer’s sense, and in the sense of merely criticizing. Rose (1996) displayed this tension, first describing a critical history as “critical not in the sense of
pronouncing guilty verdicts, but in the sense of opening a space for careful
analytical judgment” (p. 106). But by the end of his essay, Rose admitted that a
critical history “disturbs and fragments, it reveals the fragility of that which seems
solid, the contingency of that which seemed necessary, the mundane and quotidi-
ian roots of that which claims lofty nobility” (p. 122). Why could “careful
analytical judgment” not happen to reveal sturdy foundations and necessary
structures? In critical histories like those that Rose advocates, it is difficult to
distinguish careful judgment from careful prejudgment.

Some of the contributions of the “critical” approach are complementary to
“old history” in the sense that both are required for a full account of the past. A
completely external history (i.e., an explanation of change based solely on factors
external to psychological research) is just as problematic as a completely internal
history. Similarly, a historical review on a topic based solely on published
documents is simply not as full as an account that includes unpublished work, but
no complete history could be based solely on the latter. The issue of historicism
versus presentism is similar; to try to understand what historical events were like
for those who participated in those events is reasonable and desirable, but to
conduct historical research—from the selection of projects to the evaluation of
sources to the interpretation of findings—without any regard for present knowl-
dge is counterproductive. The present trend of “historical contextualization”
supports this assertion as soon as we ask what object we are historically contex-
tualizing: psychological research and practice. If we ever hope to know where
progress has happened and where it has not happened, even if we only want to
observe change, some level of presentism is necessary; without the present, the
very concept of “history” would be meaningless (cf. Hull, 1979; Kragh, 1987,
chap. 4).

The Appreciation of Psychology and History

When Gergen and Graumann (1996) claim that “psychological science has
treated historical inquiry with little more than tolerant civility” (p. 1), some
skepticism is warranted, especially when one considers the historical articles
available in such general venues as American Psychologist and the American
Journal of Psychology. However, if there is any truth in what they say, perhaps
it is because Gergen and Graumann conceive of historical inquiry as the new
history. Psychologists must suffer from low professional morale to enjoy reading
history that makes out their intellectual forebears to be, at best, irrational and mere
products of their times, and at worst, dangerous individuals who are responsible
for America’s social problems. When Samelson (1999, p. 254) asks “What have
we [psychologists] wrought?” and can only come up with “the compelling
pseudoexactness of intelligence testing and all the problems this generated,”
“modern opinion research technology that has now overwhelmed our political
life,” and “repressed memory syndrome,” it is unclear whether the new histories
of psychology are any less selective in content or less motivated in style than the
old “ceremonial” histories.

Any lack of interest in history among psychologists might, then, be due in part
to perceived lack of respect for psychology by historians. The new historians, on
one hand, dispute psychology’s claim to being an impartial, objective science, but
they also simultaneously endorse a more rigorous and disinterested type of historical scholarship. These two positions occasionally occur close enough to each other to allow for seeming contradiction. In his textbook, Leahey (2001) is generally sympathetic to the new history approach (he describes his book as being “in the spirit of the new history of science” [p. 26]), which he characterizes as “analytic rather than narrative, often incorporating statistics and analytic techniques borrowed from sociology, psychology, and other social sciences” (p. 28). But later on, Leahey tells us, “I believe that history is a humanity, not a social science, and that when historians lean on the social sciences they are leaning on weak reeds” (p. 29). Similarly, van Drunen and Jansz (2004) first disparage old histories as making historical evaluations that are “implicitly or explicitly colored by current notions” (p. 2) but then tell us that their own “book emanated from the conviction that history is not just about the past: it also reflects on discussions about the current status of psychology and its social and cultural impact” (p. 10). It may be possible to evaluate the past without mentioning the present, but is it possible to use such an evaluation to inform the present? At least in the case of Leahey, it appears to be skepticism about psychology’s scientific status (expressed throughout the book) that causes this tension and that might also cause psychological scientists to be wary of historical scholarship.

The key to mutual respect between the new history and psychology may be clarity regarding the goals of each. When new historians “put questions of truth on one side” (Danziger, 1990, p. 12), they cannot simultaneously use information from history to pass judgment on the theories and practices of psychology today. The positive features of the new history consist in its detachment from contemporary work in psychology, and so new historians who ask us to be charitable and nonjudgmental toward the past are betraying their principles if they expect to treat the present any differently. Of course, this is a limitation that the old history of psychology, Whiggish and presentist, does not share, and so psychologists would be more willing to take lessons from old history. 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.

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