The Journal of Mind and Behavior
Winter, 1982, Volume 3, Number 1
ISSN 0271-0137

Reconstructing Accounts of Psychology's Past

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Histories of psychology have, up to the present, been largely narrative, and imbued with a positivist bias. Only recently have some historians recognized that the close identification of the discipline's history with this perspective provides psychology with no more than a partial understanding of its history. Arguments are advanced in this paper to support the contention that historians of psychology must explore alternate ways of presenting historical accounts of their discipline. Reference is made to the development of a procedure which could permit historical reconstructions by formulating arguments from contextual data gleaned from documents written by professional psychologists. It is further argued that psychologists could benefit from an application of their methodological rigor to questions related to the history of their discipline.

It is generally acknowledged that the history of psychology, conceived in classical Victorian positivist terms as *the* history of psychology, bears the unmistakable imprint of E.G. Boring (Kelly, 1981). Only recently have some historians of psychology (Blumenthal, 1975; Danziger, 1979; O'Donnell, 1978, 1979; Weimer, 1974a, 1974b) recognized that such a close identification of the discipline's history with one historian provides psychology with no more than a unidimensional understanding of its history. As scientists, psychologists are attuned to a positivist account of their history, and have accepted Boring's uncritically. Consequently, his classic, *A History of Experimental Psychology* (1929; rev. ed. 1950), has been perceived as normative, and his positivist philosophy, whether expressed in terms of the Zeitgeist or the Great Man theory of historical continuity, has pervaded most considerations of psychology's past. The image of the discipline's history has been accepted as one of continuous progress, with very little, and then only cautionary attention, paid to "dysfunctional" as opposed to "functional" aspects of its development (Stocking, 1965; Young, 1966; Kelly, Note 1).

One of the purposes of this paper is to lay challenge to some currently held vital assumptions in relation to the history of psychology:

a) that the history of the discipline is absolute—i.e., that the discipline began at some definable point and has moved continuously and progressively ever since;

b) that psychology's methodological rigor can make no significant contribution to historical research (see Young, 1966); and

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This paper is partly based on the author's Ph.D. dissertation (University of Manitoba, 1981). The research was funded by a Doctoral Fellowship from the Social Sciences and Humanities Research Council of Canada. Requests for reprints should be sent to B. Hyman, Trinity College, University of Toronto, Ontario, Canada M5S 1H8.

c) that historiography—historical methodology—need only focus on laying challenge to psychology's master historian, E.G. Boring.

The most stringent and comprehensive attack on this particular image of psychology's past came from Young, a historian of science who, while acknowledging Boring's primacy in the field of the history of psychology, added:

> Nothing said here should be construed as diminishing the sense of debt which every beginner in the history of psychology owes to him [Boring]: his contribution is non-pareil. But it must be stressed that the worst way to repay intellectual debts is to repeat the findings of one's mentors rather than extending, amending, and deepening them. (Young, 1966, p. 10)

The most serious assaults by psychologists on our "manifest history" have appeared only during the last five years. They are critical of the discipline's dependence on Boring's history, but pay very little attention to the issue raised by Young in relation to historiography, that is, to the ways in which our histories are written.

O'Donnell (1979) came closer to this matter than others when he advanced the argument that Boring's *History* was influenced by his professional concerns about the lesser status of experimental, as opposed to applied, psychologists in America during the nineteen twenties. When speaking of Boring's interpretation of psychology's past, he said "the historiography of psychology has followed Boring's lead" (O'Donnell, 1979). With the exception of two recent general texts—*An Intellectual History of Psychology* (Robinson, 1976), and *The Persistent Problems of Psychology* (MacCleod, 1975)—this would appear to be the case.

There is little evidence to be found in the periodical literature of the last fifteen years regarding the plea advanced by Stocking (1965) concerning less presentism in historical studies. While some psychologist-historians have demonstrated a willingness to eschew the perception of Boring's *History* as definitive (e.g., Blumenthal, 1975; Kelly, Note 2), none, with the exception of Danziger (1979), O'Donnell (1979), and Weimer (1979), have paid significant attention to the central aspect of Boring's historiography—his positivism. Perhaps this can be attributed in part to the fact that, as Young (1966) pointed out, the history of psychology is written primarily by avocational, rather than professional, historians, As scientists, psychologists are not expected to explore in depth such areas as philosophy of science, sociology of knowledge, or historical research procedures. By and large, psychologists still continue to write stories rather than explore issues; in Butterfield's terms (1963, p. 16), they are interested in "the past for the sake of the present"—which explains the term "presentism." But as Young (1966, p. 17) argued, "History, like science, is controversy, not story-telling."

Recognition of this point began to take effect in philosophy and history

by the late nineteenth century. The classical Victorian positivism had repudiated metaphysics; i.e., speculative considerations regarding the nature of reality were set aside in favor of an adherence to observation and experience. All knowledge regarding matters of fact, it had been maintained, was based upon the "positive" data of experience. This philosophical bedrock led inevitably to an expository, rather than a controversial, stance in history; it was in these terms that the histories written by late-Victorians were intelligible to their contemporary academic mainstream.

This philosophy did not persist among historians, but perhaps because of their scientific operationism and their avocational interest in history, it has persisted among psychologist-historians. Ironically, one of the factors which influenced historians into moving away from historical positivism was the experimental evidence of individual differences in perception provided by early scientific psychology (e.g., see von Helmholtz, 1860; Lotze, 1852; Titchener, 1910; Wertheimer, 1912). Psychologist-historians, however, appear to have been blind to the knowledge generated by members of their own discipline.

Becker (1968, p. 116) has argued that "of all the present-day psychologists, perhaps it was E.G. Boring who scorned philosophy most." I am more inclined to argue that Boring strove to reject the idea that metaphysics, rather than philosophy, had any useful part to play in a science of human behavior, and that he was sympathetic to Mach's experiential positivism, and wholly committed to Bridgman's operational positivism. When Boring wrote the first edition of his *History* in 1929, psychology, as a discipline, was still trying very hard to demonstrate and justify its separation from the disciplines out of which it grew—philosophy, medicine, and theology.

To say that Boring "scorned philosophy" is to make a statement similar to the equally inaccurate claim that Watson (1913) denied the existence of consciousness. Watson denied consciousness a place in the scientific discipline of psychology, and he did so because at that time psychology did not have the methodological tools for dealing with consciousness empirically. Boring and Watson excluded metaphysics and consciousness from psychology's domain because, in Boring's words (1950, p. 654), "those problems are not the psychologist's." As O'Donnell (1979) argued, with a great deal of convincing evidence, Boring was anxious, when he wrote his classic *History* in 1929, to redress what he perceived as an imbalance between applied and experimental psychology in America at that time. Since, in Boring's words (1950, p. 656), operationism was then a "trend of the times," it can hardly be found surprising that his *History* both endorsed and reflected this trend. What is being challenged in this paper is the fact that present historians of psychology (with the exceptions noted above) have failed both to recognize this positivist bias in Boring's historiography, and to attempt to deal with it. As Young (1966) stated, psychologists owe their master historian more than uncritical devotion.

Blumenthal (1975), Mackenzie (1972, 1976), and Weimer (1974a, 1974b) explicitly acknowledged the desirability of multiple perceptions of an historical episode, or of a general historical account. But they illustrated their arguments by appealing to a comparison of different interpretations of the same historical episode, while pointing out that, in Weimer's (1974a) terms, "someone's account cannot be correct"—a regrettably positivist declaration. If one examines the only English-language journal in its field—*The Journal of the History of the Behavioural Sciences*—one finds that apart from an excellent article by Stocking during the *Journal*'s first year of publication (1965), only a few others (Buss, 1977; Danziger, 1979) have focused on the historiography of histories of psychology.

By the 1930s, historians had moved into a post-positivist phase of perceptual relativity (Buss, 1977; Young, 1966). They were no longer convinced that any historical investigation dealt with, or could develop into, a "true" story. In Hughes' words (1958, p. 16), they were "striving to comprehend the newly recognized disparity between external reality and the internal appreciation of that reality." The historical relativist excluded the possibility of any historical account being judged as "right" or "wrong." An historical reconstruction, the relativist argued, may be found convincing or unconvincing, and typically the focus of critical attention is directed to its components, and to the nature and persuasiveness of the arguments developed, rather than the perspective adopted by the historian. When this perspective, or bias, is critically evaluated, or when the existence of multiple perceptions of any historical situation is ignored (as in Weimer, 1974a), the logic of the evaluation leads inexorably to a verdict of "someone's wrong."

I am suggesting that historians of psychology have yet to free themselves from *all* the shackles of their historiographical mentors. It is not Boring's historical account of psychology's past that is "wrong," but historians' evaluations of his account on the basis of a right-wrong dichotomy. Such critics have not yet shaken off the pervasive positivism of psychology in general, and of the historians of psychology in particular (Robinson, 1976, pp. 402-411; Wyatt, 1961; Young, 1966). I do, however, agree with the arguments advanced by Weimer, O'Donnell, and others that historians of psychology must explore and justify alternate ways of presenting historical accounts of their discipline.

Alternate Procedures in Histories of Psychology

As was indicated earlier, the systematic development of alternate procedures for reconstructing historical accounts of psychology's past has been limited. Blumenthal (1975) has done some interesting and much-needed re-evaluations of Wundt, striving to rescue him from the unidimensional position of "founder of experimental psychology" assigned to him rather

repeatedly by Boring. The procedure adopted by Blumenthal was to compare selected statements about Wundt made by different historians, one of whom was Boring. For the purpose of illustrating the key issue of multiple perceptions of an historical episode, such a procedure is impeccable. It is, however, only a first step in the attempt to reexamine the nature of Wundt's contribution to psychology; while Blumenthal was able to demonstrate that the historians presented significantly differing accounts, he concluded by evaluating Boring, in particular, as Weimer did, on the basis of a right-wrong dichotomy. The logical corollary of relativist historicism, that no such positivist conclusion can be attempted, was ignored. Similar criticisms can be advanced with regard to articles by Mackenzie (1974, 1977), Weimer (1974a, 1974b), and, to some extent, Danziger (1979).

Without the enormous body of empirical research psychology has been so careful to encourage and record, the discipline would not have been able to develop as extensively as it has. Yet, few psychologists have been able to move away from taking for granted the didactic, expository histories of their discipline; with even less excuse, the same criticism can be advanced in relation to most psychologist-historians. What they have generally failed to do is regard the history of psychology as "a research discipline whose standards must be as high as those practised in the laboratory" (Young, 1966, p. 16). Consequently, psychologists still have an image of psychology as a continuously progressing discipline; that whatever research was conducted in a certain field during, for example, the 1960s, must by definition be an advance to that conducted in the same area forty years earlier.

Such may very well be the case, but not *ipso facto.* Methodological sophistication appears to be increasing all the time, and new research instruments are being developed constantly. But, perhaps in the process, psychologists have tended to lose sight of the fact that they are still grappling with what MacLeod (1972) has called "the persistent problems of psychology." Robinson (1976) addressed this issue when he wrote of methodology as the metaphysic of psychology. By placing emphasis on the methodology of a study, he argued, to the exclusion of an equally rigorous consideration of the issue being investigated, psychologists have sometimes allowed themselves to be enraptured by their procedure rather than challenged by their questions. And, in order to respond to that challenge, a more sophisticated understanding of the fundamental problems on which these questions are based, and some of the ways in which earlier attempts were made to deal with them, would appear to be necessary.

Three very interesting attempts to use quantitative methods in the history of psychology have been published by Cardno (1962a, 1962b, 1963). Young (1966, p. 31) referred to them, rather patronizingly, as "odd, but curiously interesting." The fact that Young saw the application of empirical rigor to historical investigation as having only very limited usefulness no doubt

accounts for his dismissiveness. But as Cardno (1963, p. 141) pointed out, "The history of psychology abounds in succinct judgments . . . they are impressions, which though backed by more or less evidence are arrived at by steps not always explicit." In each of his three studies, Cardno attempts to make explicit and systematic some of the grounds upon which these judgments are based.

In one of his studies (1963), Cardno attempted to make systematic, and therefore, replicable, the development of what he called a "network of reference" among eight books published in England, France, and Germany between 1842 and 1862. There was a tendency, he argued, for historical judgments (e.g., who influenced whom) to be "imprecise" and "inexplicit" (Cardno, 1963, p. 155) in the history of psychology, particularly in relation to time and nationality comparisons. He consequently developed a procedure for establishing *units* of comparison which were defined precisely, and which allowed him to make orderly and systematic comparisons. There is no evidence to indicate that other researchers have followed Cardno's lead. By and large, perhaps because they have paid more attention to their own history (or perhaps because they have never had a major figure like Boring whose history they were willing to accept as definitive), it is the sociologists who have produced some very interesting studies in the field (e.g., Ben-David & Collins, 1966; Merton, 1957, 1961). Psychologists have not been eager to apply their methodological principles—or as Robinson (1976) says so acutely, their own metaphysic—to the impressionistic inferences which historians draw. And one area in which these inferences abound is in relation to the movement of common parlance words into positions where they become formal psychological concepts.

To explore this phenomenon further, Hyman and Shephard (1980) selected four common parlance words also used as psychological concepts: behavior, personality, environment, and heredity. By systematically examining the contextual associates of these words when they appeared in popular and psychological sources, an attempt was made to develop a methodology which could prove useful as an operational definition of *Zeitgeist*. The development of this procedure appears to have taken the first step towards providing a useful definition of the Zeitgeist concept—thereby allowing its use in historical explanations to be critically analysed. The authors were only implicitly critical of the historiography of the history of psychology—but the Hyman and Shephard study was the first such report to follow Cardno's lead in attempting to develop replicable and systematic historical procedures. Although this earlier study did not explore the movement of these four words from their original position as common parlance terms to their establishment as formal psychological concepts, such an investigation of word movement might prove to be particularly revealing, as it could clarify modifications and refinements of meaning, as well as illustrating the ways in which different

scholars used a particular word, with variations of associated ideas. One such word, for example, which might be particularly valuable for such an exploration is the use of the word *intelligence* in America. IQ tests were fundamental to the development and acceptance of psychology as a discipline in the United States. Yet, in relation to efforts by psychologists to explain the nature of intelligence, and to define it, "there has been," according to Tuddenham (1968, p. 517), "a more or less continuous Donnybrook of rival views in which any number of theorists could and did join in."

"Intelligence" in Early American Psychology

Clearly, the word *intelligence* is not the only common parlance term adopted for professional use by psychologists. One could, in fact, argue that the preponderance of such terms has been, and is, a significant handicap to the ease of unambiguous intra- and inter-disciplinary communication. The degree of attention paid in America to the nature and measurement of intelligence, and the variety of attempts made to reconcile its assumed nature with its measurement, is considerable. The acceptance by Congress of the intelligence testing of the armed forces during World War I testifies to the importance accorded this procedure by powerful bodies in the United States.

Louch (1966, pp. 54-59) has nicely illustrated one of the factors contributing to the confusion that has frequently surrounded the use of the word *intelligence*. Like many words which have the joint function of being common parlance terms as well as psychological concepts, *intelligence* reflects both description of behaviour, and its appraisal. 'A question about intelligence could not arise," Louch suggested, "unless some performances were prized; it thus becomes pointless to try to set aside our preferences in order to decide what intelligence really is" (p. 57; see also Tuddenham, 1968, p. 517). Early American psychologists did not set aside their preferences at all. The environmentalists, behaviourists, educators, and learning theorists strove to establish empirically the importance of external factors as determinants of intellectual capacity. The geneticists and eugenicists were often very seriously committed—not only to verifying the importance of nature over nurture, but also to purifying the basic American population stock.

Could this mean, then, that experimental as opposed to applied psychologists framed their questions involving intelligence differently? The unquestioned assumptions and firmly committed beliefs of an experimenter have been shown to influence that person's empirically derived results (Rosenthal, 1966). An experimental psychologist, interested primarily in specific mental functions, may have worked only with subjects of average intelligence. An applied psychologist, on the other hand, may have been motivated to separate the less-than-averagely intelligent individuals from the majority, for the purposes of obtaining their social and legal protection, *or* social education,

or the protection of the majority—or all three. Experimental and applied psychologists may therefore have delineated only partially overlapping experimental arenas, and may have appraised the significance of their results from a somewhat less than mutually inclusive perspective.

Two of the major figures in America involved in this area were Goddard and Healy. Both worked primarily with the retarded, the delinquent, the orphaned, and other disadvantaged groups. Yet generally, Goddard could be considered a eugenicist, and Healy a social progressivist who challenged the current hereditary conceptions (Sarbit, Note 2). Here, two psychologists' firm convictions were in significant conflict. Goddard was, by and large, a geneticist who saw heredity as the primary causal influence in relation to intelligence. Healy did not accept the arguments advanced by eugenicists and others sympathetic to the "nature *over* nurture" resolution of this debate. Would Healy and Goddad, then, have used the word *intelligence* somewhat differently, and surrounded its appearance in their documents with different concepts?

There is no evidence in the literature that these, and a host of other similar questions, have been explored in a way that strives to make more systematic the procedures employed by traditional historians. Nor has significant attention been paid to the major point raised by Kuhn—that historians must respond to the need to understand the past in its own terms before comparing it with the present:

> Gradually, and often without entirely realizing they are doing so, historians of science have begun to ask new sorts of questions and to trace different, and often less than cumulative, developmental lines for the sciences. Rather than seeking the permanent contributions of an older science to our present vantage, they attempt to display the historical integrity of that science in its own time. (Kuhn, 1973, p. 3)

An Alternative Historical Procedure

Procedures traditionally employed by historians have not been accessible to empirical verification. Certainly, such procedures have been detailed, careful, and well-constructed, but until recently, they have not involved the application of systematic and replicable procedures to a body of data. However, the use of principles of research design and quantitative methodologies by historians has been given impetus by the development of a number of content analysis instruments (Berelson & Janowitz, 1967; Carney, 1972; Gerbner et al., 1969; Holsti, 1969; Pool, 1959). In particular, the use of quantitative methodologies when considering historical questions (Mosteller & Wallace, 1978) and word meanings (Cliff, 1978; Kruskal, 1978), encourages the development of procedures suitable to the conduct of historical research. Any of these instruments could be used in an exploration of the movement of a word such as *intelligence* from its position in the 1890s as a

common parlance term to that of a formal psychological concept by 1920. Such a procedure could enable psychologist-historians to reconstruct segments of their discipline's history by formulating arguments from contextual data, not necessarily explicit, yet gleaned from documents written by professional psychologists.

As Cardno's studies (1962a, 1962b, 1963) demonstrated, the research procedures used by historians can be employed more systematically. For example, without introducing any modifications in procedural principles, a particular methodology can be designed utilizing the following qualifiers quite successfully:

Selection of sources. Like most other disciplines, psychology (at least in America) has kept a very careful record of its publications. It should therefore be possible to develop a list of potentially relevant sources relating to any study under consideration. Similarly, it should be possible to define objectively the grounds for the selection of the sources. By employing a sampling procedure, data could be collected from defined groups of sources, so that the first stage of the procedure would be replicable.

Gathering impressions/data. When historians begin to gather impressions from their exploration of the sources, then the use of an appropriate instrument could allow data to be collected empirically. If the movement of the word *intelligence,* as it was used by professional psychologists in America from 1890 to 1920, was being explored, historians, instead of gathering information by employing filtering processes to sort and organize impressions, could collect data empirically.

Drawing inferences/conclusions derived from the data. If data were collected empirically, the use of such a procedure could make it possible to conduct a number of statistical analyses upon which conclusions would be based. The results obtained by following such a procedure could permit a number of statements to be made, and give rise to questions whose focus might be considerably sharpened.

Returning to our previous example, if the use of the word *intelligence* between 1890 and 1920 was being investigated, the fact that the enormously influential work of Binet appeared between 1900 and 1910 could not be ignored. It would be interesting, and possibly revealing, to compare the linguistic context of the use of this word in translations of Binet's works with the use of the same word in original works by American psychologists. Could any of the semantic field characteristics surrounding *intelligence* in his studies have been duplicated by those found in works written later by American psychologists, and if so, when? Yet, since Binet was translated by Americans, it would not be unreasonable to assume that any translated work could reflect, at least in part, some of the translator's linguistic features. In terms of linguistic *content,* however, there may well be detectable differences.

If one were interested in comparing theories dealing with *intelligence* that

were operative in early American psychology, the use of appropriate semantic field characteristics to explore hypothesized similarities and differences could be effective. Tuddenham (1968) has suggested that Spearman's *g* factor theory was incorporated into considerations of Binet's intelligence tests in early American psychology. Was this incorporation linguistically consistent between Binet, Spearman, and American psychologists? Binet strove constantly to develop a testing procedure that would permit him to explore *intelligence* as a global, rather than a factorized, concept. Yet Spearman, and almost immediately, American psychologists, were eager to retain their understanding of *intelligence* as being composed of different mental abilities. It would appear that a study whose procedure was developed in accordance with the suggestions advanced in this paper could deal successfully with attempts to map the details of this presumed conflict, as well as with the ways in which psychologists may have tried to resolve it.

The fact that *intelligence* has always been used in common parlance gives rise to another interesting question. How was the word used in popular sources—novels, magazine articles, etc.—not written by psychologists? In a study which compared words used both in common parlance *and* as psychological concepts, Hyman and Shephard (1980) found that although the two contexts may have differed, the words in question were used in approximately the same way. However, an examination of the raw data did suggest that there may have been some differences in the critical features of the semantic fields. There was some indication that the words under investigation were used loosely in popular sources, while their appearance in professional documents was surrounded by a smaller range of concepts. This impression appears to be logical; it would not be surprising to find that when writing for professional communication, psychologists used these words more precisely. An exploration of this question in relation to the word *intelligence* could prove interesting.

Conclusion

It is possible that some of Young's (1966) skepticism about the usefulness (in principle) of empirical procedures in historical research could relate to the restrictions necessary for the effective design and conduct of empirical research. At present, historians of psychology do not have an extensive body of scholarly research upon which to base their texts, particularly in areas of interest to psychologist-historians. Since Young does feel that such studies are desperately needed, his implied objection to the "smallness" of empirically-based historical investigations is questionable. The necessary precision and apparent restrictiveness involved in such empirical histories may conflict with the broader sweep of traditional historical procedures, but the precision gained, it is felt, permits the development of more precise, and defensible,

historical arguments. "The study of the history of psychology," Young (1966, p. 14) has maintained, "has suffered mightily from those who have taken it literally . . . a scholarly tradition which is based primarily on textbooks has severe inherent limitations."

The development by psychologists of alternative procedures for conducting historical research could, in fact, be found indispensable. Psychologists could benefit immensely from a careful application of their methodological rigor to questions related to the history of their discipline.

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