

## **Inventing Psychology's Past: E.G. Boring's Historiography in Relation to the Psychology of his Time**

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Boring's eminence as a historian of psychology has sometimes obscured the fact that he wrote his histories from a very specific historiographic view. This meant that both his interpretation of the past and his hopes for the psychology of the future were influenced by specific political and administrative and methodological purposes in the organization of the psychology of *his* time. This paper explores some of these issues by examining the relationship between Boring's selective historiographic principles and the kinds of psychology he favored as being most truly scientific.

The history of psychology, as it has typically been written, is "Whig history"; that is to say, it is an account of the specific successes and failures of psychology written by those who have a vested interest, deliberately acknowledged or not, in the exaltation of certain successes and the cautionary display of certain failures. This form of history implies that a designated pattern of events has produced desirable progress and, usually, that this progress has been due largely to the effects of particular eminent individuals. In "Whig histories," themes of progress and the prestige of personalities figure more markedly than do detailed analyses of the processes or of the context of actual historical unfoldings. "Whig history" is history with a happy ending: as such, it mutes critical analysis of the details of currently sanctioned successes and of the social, political, and cultural circumstances that made them possible. In large part, such historical perspective creates "origin myths" which depict a discipline's recent history in retrospective terms that compliment the currently dominant orthodoxies. The major function of "Whig history" is contemporary and political and not truly historical. Much extant history of psychology must be understood in these terms. An important case in point is much of the work of this century's most influential historian of psychology, E.G. Boring.

For a great many psychologists, educated in North American universities over the last few professional generations, E.G. Boring is known as *the* historian of psychology. Most frequently, they have encountered his

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masterwork, *A History of Experimental Psychology* (1929a; revised 1950b) as the major text for one of their graduate courses. For some, this was their last contact with psychological history, memorable mostly for the opportunity to make a bad pun on the author's name and, by implication, on the topic. Nevertheless, a smaller number continued to read Boring's other books and articles, were led to other historical sources and writers, and considered themselves better psychologists for the experience. For nearly all, Boring's interpretations of psychology's past comprised the principal secondary source, making any further inquiry into classic psychological writings seem unnecessary.

From the middle 1930's to the early 1960's, another group of psychologists knew Boring in a rather different capacity. In that period, he was one of the most diligent and insistent organizers and administrators of psychological activities on the American academic scene. He was the founding chairman in 1933 of the Department of Psychology at Harvard. Oddly, Harvard was one of the last major universities in the United States to establish an administratively separate psychology department. In his autobiography (1961), Boring states that he thought his "mission" in achieving formal recognition there was somewhat silly, something that contributed "about as much as a minister contributes to the success of a marriage" (p. 57).

As an example of Boring's involvement, this analogy was prophetic. Over and over again in his career Boring played this ministering role when some reorganization seemed to be required in the activities of psychologists. This formal diligence was matched by an extensive private correspondence with psychologists all over the continent. Boring's personality added much to this. He was a compulsive worker who advocated the 80 hour week (Boring, 1963) and an extrovert, frequently playing the roles of co-conspirator and wise advisor in some psychological enterprise. Over time, Boring was viewed as an elder statesman and father figure by some of the most influential and creative psychologists in North America (Jaynes, 1968; Watson & Campbell, 1963). When psychologists from various "schools" gathered among themselves or with other scientists to discuss the nature, place, and promise of scientific psychology, more often than not Boring was allotted (or could, with impunity, appropriate) the first or last word. Within the social organization of academic scientific psychology and almost independently of his work on substantive psychological problems, Boring had remarkable impact and presence. Much that is in Boring's histories, as well as the implications for psychology of his methods of *doing history*, can best be understood by recognizing his pivotal position as a powerful figure in the emergent bureaucratic structure of the psychology of his time.

For Boring, the history of psychology since the late nineteenth century

was a tale of progressive, significant achievement. It was this tale that he told to his professional colleagues in such detail and with such persuasive repetitive effect over the major portion of his academic career. Yet, at the centre of his continuing account and as a critical counterpoint to the main progressive themes of his histories, certain ambiguities and inconsistencies emerged.

Boring acknowledged in his writings, more bluntly than most, the human biases that affected science. Personality characteristics, passionate commitment to a particular theory or method, and group membership all were revealed as influences on the apparently dispassionate conduct of scientific inquiry. Throughout his historical writings, Boring conducted a discussion with his readers and with himself to examine the underlying principles on which his historical accounts had been based. This body of critical commentary comprises Boring's historiography, in contrast to his substantive historical chronicles. In some ways, Boring's historiographic principles and arguments are more important to us than his specific historical tales if we are to achieve an intelligible contemporary perspective on scientific psychology's recent past.

It is not part of the conventional wisdom governing the design of careers in psychology to assert that those who write psychological history affect psychology's progress. By definition, historians would seem to be "after the fact" and therefore not causally important. Yet, in ways that are just now being recognized and in ways that Boring might not have willingly acknowledged, just such an influence can be found in his histories. Boring's histories reflect an energetic commitment to what he believed was progressive in psychology and an equally persistent disapproval of what he thought was regressive. Most significantly, he affected the development of psychology by inventing a past for psychology that made legitimate the major activities and concerns of selected academic psychologists in the first half of this century. Put more critically, he helped create and foster some of the most significant "origin myths" affecting modern scientific psychology. In doing so, Boring became a more important and influential psychologist than his principles of historical significance would have allowed him to recognize. It is ironic that his real importance to modern psychology can be identified only through an intensive and fundamental criticism of his methods and his aims in creating psychological history.

An accurate recognition of this influence is dependent on two background factors: first, a general understanding of the historiographic options available to the historian of psychology; second, some understanding of Boring's early personal and intellectual history. Within this technical-biographical context, the particular and sometimes peculiar nature of Boring's historiographic commitments can be ex-

amed and evaluated.

### **Historiographic Principles and Origin Myths in Psychology**

Historical beliefs can very often be treated as matters of received wisdom, revealing in a condensed version not only actual historical events but the needs of the times. Accordingly, historians of psychology have recently come to speak of origin myths from which they consider their disciplines to have emerged (Samelson, 1974). In part, the identification of such myths is an exercise in historiography with the newer generations of psychological historians criticizing the older. In part, the argument is being made that the old myths are no longer playing their supportive and regenerative roles. If modern psychology is not to become totally detached from its past, it must reinterpret that past in terms that speak to present problems. The question is: how does one go about this task—and under what historiographic rules?

An origin myth implies that some widely believed interpretation of historical events is demonstrably wrong and that a return to primary sources will identify the error. The implication is that such distortions are created and maintained because they are highly functional (perhaps even necessary) to the science of some particular time. Origin myths may be thought of as part of the intellectual machinery that maintains any scientific hegemony. Their detection (and displacement) is part of the incessant process of revolution that Kuhn (1970) argues is basic to effective scientific thinking.

To say this much is already to participate in an important contemporary debate concerning science and its relationship to cultural and sociopolitical events of a particular time span. In turn, such debate is related to arguments about how science makes legitimate both its methods and its knowledge. A number of writers have recently stressed the difference between interpretations that emphasized some specific logical progression underlying scientific evidence and theory and those views that argue that scientific achievements must be interpreted and justified in terms of the actual historical circumstances in which they were generated. This distinction correlates with the historiographic distinction between the Whig version of incessantly progressive history and some form of historical revisionism. Thus, the uncovering and analysis of scientific origin myths requires three distinct levels of analysis: the re-examination of primary sources as they apply to specific "truths," an articulated view of the epistemological and methodological nature of scientific activity (or, at least, some understanding of alternative interpretations), and an explicit awareness of the historiographic techniques being used to interpret the primary sources in epistemo-

logically and methodologically adequate ways. Any examination of the origin myths of modern psychology should then focus on specific issues, since only in specific context can all these necessary threads of valid historical reconstruction be brought together.

Within early academic psychology, two very general propositions may be seen as twin supports of a mythological statement of origin. One is the speedy consensus among psychologists that psychology was a science, similar in certain ways to prior science. The second was the slower consensus that, in order to consolidate such status, psychology had to define its subject matter in terms of behavior rather than mental events or constructs. The progressive adoption of these propositions depended upon certain interrelated commitments. Psychologists had to come to agree that specific prescribed methodologies were central to their endeavors, that certain problems were more basic than others, and that there were criteria by which results could be established as "true" (in positivistic epistemology). An additional theme was that such "true" results could be quickly converted to socially useful applications.

These related concerns constitute very powerful tools for the intellectual and professional socialization of psychologists. They produce the image of a discipline which is both coherent and progressive. Whether justifiably or not, the multiplicity of psychological topics came to be subordinate to this image which emphasized consistency in psychological method and had a universal and easily understood aim—the prediction and control of behavior. This goal allowed psychologists to feel a community of common interest and also permitted them to communicate effectively with influential non-scientists who were unfamiliar with the substance and problematics of psychological analyses.

In any historical period, people wish to be able to make effective predictions in order to improve individual lives and social conditions. Certainly, in the nineteenth and early twentieth centuries, science was animated by a Utopian impulse to better mankind's lot. If a science promises to make applicable contributions, it can very quickly gain social recognition and support. If it can contrive to have its promises accepted over time, such support will be maintained.

Part of the basic origin myth of American psychology is that it quickly gained wide social acceptance because, in addition to its scientific success, it intelligibly examined many important social concerns. This presumed successful application was interpreted as a direct consequence of psychology's revolutionary internal development as a science. The emergence of an independent and potent psychology was seen as the direct result of the work and insight of its scientific pioneers. These innovators were seen as people who had discovered psychology's basic laws and, knowing such principles, could best determine where and how to

apply them effectively.

It is undeniable that, since the turn of the century, psychology has prospered in a very dramatic manner. Nevertheless, it is decidedly questionable whether its prosperity and its intellectual history have been casually related to any great degree. The essence of psychology's *conventional* history is that its growth was caused by its success; moreover, this success was linear and relatively continuous—as long as a sound psychological science was available to confront and defeat methodologically weak or philosophically inappropriate challenges. The record of that progress is best understood, it is claimed, by juxtaposing the special interpretation given to German and English psychology by the first few generations of American psychologists (James, Ladd, Hall, Baldwin, Cattell, etc.) with certain progressive, pragmatic themes in the American character (Boring, 1950b; Roback, 1952). The success of American scientific psychology is interpreted, then, as a kind of evolutionary triumph, with the soundest, most functional ideas and methods emerging after a period of intellectual struggle.

It is natural to write such a history by emphasizing the interpretations and actions of certain eminent individuals, even while acknowledging that their efforts are very much conditioned by the *Zeitgeist* and that they are probably in some basic sense replaceable (e.g., Boring, 1927; 1929b; 1950a). Writing the history of psychology becomes a matter of tracing lines of influence in the work of eminent psychologists. But judging what is of value requires a model of scientific excellence and that is something that is often provided in terms of the historian's own time. Unfortunately, this is one of the better ways of establishing selective distortions.

The development of a discipline is influenced by its histories. They impose sanctions and mold attitudes or opinions as to how the work of that discipline should be carried out. They both encourage and censor the choice of research topics and the appropriate methods with which to pursue them. A history of a discipline becomes very much a philosophy guiding psychologists in the conduct of their intellectual and professional lives, if only in the indirect way of providing eminent role models. The substantive and methodological achievements a history describes may soon be superseded in the discipline (Hudson, 1972). However, the origin myth of how science is done and of what a scientist is that is portrayed in that history may continue to have dramatic impact on the subsequent generations that read it.

The assertion of an origin myth also usually entails the rejection of alternate strategies and priorities. The identification and critical scrutiny of an origin myth becomes an attempt to capture the style and importance of prevailing choices that time has obscured. These forgotten options are still relevant and important to the extent that the history of a

discipline may, in fact, be cyclical. Before this possibility can be established with reference to psychology, it is necessary to determine more precisely just how mythological accounts are created, to identify both the reasonable historical interpretations with which they are associated and the historiographic biases with which they are written.

### **Origin Myths in Recent Psychological History**

Many psychologists have found the history of their discipline to be of decreasing relevance to their work. Although, at the turn of the century, such major figures as James, Hall, Baldwin, and Titchener stressed the heuristic value of putting research interests in historical perspective, by mid-century "history and systems" courses had become very minor parts of the typical psychologist's education and interest. A post-World War II survey asked individuals who had done psychological work for the American military to indicate their preferences for graduate education; two percent suggested that a course in "history and systems" should be on the curriculum (Britt & Morgan, 1945). It is evident that, as the number of psychologists and the competitive pressure for journal space increased, the size of the "historical introductions" to research reports decreased.

In part, one might attribute this lack of interest in historical analyses to the proliferation of special topics in psychology and, especially, to the development of many applied and clinical specialties. However, such disinterest signals a basic change in the manner in which psychologists viewed the utility of historical perspectives in the solution of substantive problems. One can sketch this change by placing Boring in his appropriate place with respect to, first of all, Wilhelm Wundt (1832-1920) and E.B. Titchener (1807-1927) and, secondly, with respect to the subsequent generation of psychologists who came to minimize historical analysis.

Wundt made a distinction between two quite different forms of psychology—an experimental psychology focusing on the less complex psychological processes of the individual and a social psychology that required non-experimental methods for the analysis of social events in historical context (Danziger, 1979). The historical analysis of a psychological problem could be quite useful in the first form of psychology by producing a thorough description of the dimensions and complexities of a problem. In the investigation of social problems historical analysis was absolutely essential since social events could not be appropriately conceptualized independently of their historical aspects.

Titchener recognized these distinctions. He acknowledged that Wundt was insistent that these distinctions be made in the context of a revised

scientific metaphysics (Boring, 1927; Danziger, 1979). Moreover, he "...was most emphatic that these interests constitute not three programs of work, but a single integrated program" (Boring, 1927, p. 263).

Yet it is doubtful that Titchener truly absorbed the implications of the Wundtian program. Despite his reputation of being Wundt's representative in America, Titchener studied with Wundt for only two years. There is little evidence that Titchener had much direct intellectual access to the inner circle of Wundt's disciples; the typical structure of German doctoral programs at the time would make this unlikely.

When Titchener stressed the importance of a historical treatment of problems, he meant an erudite analysis of what others had said and thought about the topic at hand. Historical reviews, like specialized introspective techniques, were part of the methodology of the responsible psychologist. Such tools were very much in the service of systematic psychological inquiry. Titchener thought it very important for psychologists to engage in a long-term and integrated plan of work and considered it the responsibility of both the critic and the historian to evaluate psychological work in this extended perspective. However, Titchener's conception of the priorities of psychological research leaned in the direction of an experimental, not a social, psychology. In effect, though not in intention, this meant that historical analysis became an *optional* tool in the elucidation and investigation of psychological phenomena—a tool that was completely overshadowed in Titchener's system by his emphasis on his particular brand of introspection. In 1929, Boring dedicated the first edition of his *History of Experimental Psychology* to Titchener as the "historian par excellence." However, by that time or certainly shortly thereafter, the perceived significance of historical skills had been subtly modified. Titchener had become increasingly isolated from the emergent mainstream efforts of American philosophy. This produced a problem for Boring, both at the beginning of his career and as long as Titchener was alive. Boring was simultaneously a psychologist, a historian of psychology, and an individual with a strongly dependent ambivalence toward his mentor. How could he remain true to Titchener's structuralist and elitist interpretation of psychology's future while gathering sufficient power and influence to implement that vision in the American context? American psychology, under the impact of the positivistic ideology which fueled various forms of functionalism and behaviorism, was rejecting Titchenerian problems. No one was more publicly committed to that positivistic ideology than Boring.

Throughout his career, Boring attempted to escape his dilemma in three related ways. First, and most importantly, he adopted as his main historiographic principle the notion of a dialectical tension in scientific



progress between the contributions of Great Men and the pervasive force of the dominant cultural and intellectual themes of any given time (that is, of the *Zeitgeist*). In the short run, this allowed him to confer on Titchener the status of a Great Man (certainly he was a great personality) while leaving open the question of the substantive impact of Titchener's work on the science of his time. In the end, Boring came to the rather sad conclusion that much in Titchener's work and impact that had "...seemed so great and marvellous at the time turned out to be small and petty and personal" (Boring, 1961, p. 111). In a slightly longer perspective, the inherent obscurity of the causal relations between potent individuals and their impact on the scientific environment allowed Boring to avoid too careful a scrutiny of the social forces and the methodological problems inherent in his preferred forms of experimental and scientific psychology.

Boring's second tactic was to stress psychology's *busy-ness* as if sheer activity could make psychology a legitimate science (Boring, 1950c, 1961). Becker (1968) has provided a scathing critique of this argument as a justification of the intellectual credentials of psychology as a science. To some degree, however, that criticism misses the point. In Boring's view, the busy-ness of modern psychology was an enormously important achievement, for it provided an apparent justification for the allocation of enormous social and financial resources to psychology. A society that was sufficiently eager for techniques of psychological improvement would accept them without excessive scrutiny (Meyer, 1955). Boring recognized, as have other more contemporary analysts of scientific development (e.g., Merton, 1957; Mullins, 1974), that individuals who are apparently successful in science receive a disproportionate amount of the subsequent resources allocated to their discipline. As Boring's career developed, he increasingly intertwined his historic analyses of psychology's achievements with his political lobbying for the enhancement of particular forms of psychological busy-ness. O'Donnell (1979) has documented this point in detail.

One consequence of writing a history that uses busy-ness as a principle of justification is that history is no longer seen as one of the tools for the analysis of substantive problems in a discipline. It is seen as a story of achievements. It is probably no accident that the emerging dominance of "Boring-style" histories of psychology coincided with an increasing disinterest on the part of psychologists in the history of their discipline (e.g., Britt & Morgan, 1946). History came to be viewed as having no instrumental value in the solution of psychological problems—which, of course, is quite wrong.

Boring's third tactic for controlling the contradictions in his view of psychological history is best seen by looking at the pattern of his personal

commitment as a psychologist to different methodological systems. He began by arguing that introspection and the particular forms of experimentation associated with it were central to a scientific psychology (Boring, 1929a). As his commitment to Titchenerian introspectionism crumbled, Boring was forced to find some stronger sanction for the exclusiveness of his psychological vision than experimental methodology. After all, *everybody* was doing experiments; how then to distinguish the good from the bad? For a brief while, he flirted with eclecticism (Boring, 1930), but that, of course, was no solution, largely because eclecticism provided no mechanism of exclusion. Boring had to find a principle that was decidedly definitive of *good* psychological science. He attempted to do so in the first major work which he wrote after his *History*. This work was *The Physical Dimensions of Consciousness* (1933).

In this book, as he later reported, Boring espoused the doctrine of physicalism as "...the view that consciousness, (is)...an object of observation by science, reduced to the operations by which consciousness becomes known to science" (Boring, 1961, p. 52). This was, of course, an explicit repudiation of much of the content of Titchener's psychology. However, it was a necessary step in order for Boring to move into the mainstream of American psychological thinking without abandoning the notion of some form of unifying system. Throughout his career, Boring argued that only the general acceptance of some guiding conceptual principle could guarantee psychology's further productivity. Such a principle had to be easy to teach simply because the transmission of a particular perspective to emerging professional generations was as important as actually doing psychological research (Boring, 1929a; 1961; Boring & Boring, 1948).

In particular, Boring agreed with the operationist views of his favorite student, S.S. Stevens. From the perspective of Boring's historiography, it was ideal that the guiding principle of scientific progress be the objectively determined judgment of the *individual* scientist.

### Some Details of Boring's Historiography

Boring's *individualism* is a crucial simplifying theme throughout his writings. This is clearly expressed in his concern with the relative role of the Great Man and the cultural Zeitgeist.

Perhaps I should say also why there is so much biographical material in this book, why I have centered the exposition more upon the personalities of men than upon the genesis of the traditional chapters of psychology. My reason is that the history of experimental psychology seems to me to have been so intensely personal. Men have mattered much. Authority has again carried the day...quite independently of the weight of experimental evidence...Moreover, personalities have been reflected in schools and the systematic traditions of the schools have colored the research...there

is always the further question: If personalities lie, in part, back of psychology, what lies back of the personalities? I trust that I have been cautious in drawing such inferences; however, I have never been able to get this question out of my mind. (Boring, 1929a, pp. viii-ix)

The problem of personality is central to Boring's early historiography because it bears upon his own initial judgment that, for all its energy and success with detail, experimental (i.e., general) psychology as of 1929 lacked "...any great idea or discovery that...revitalized the science..." (Boring, 1929a, p. 659). His tentative explanation of psychology's deficiency emphasized personality. Psychology lacked great achievements because it lacked great psychologists; all the eminent individuals in his history were seen as doing derivative work that went with the times. The most highly influential originators in early experimental psychology were from outside the discipline. At the same time, the conflict and debate between psychology and philosophy was seen as draining psychology of its primary vigor. The early generation of psychologists were seen as flawed for insisting upon amateur philosophizing within the context of psychological issues. These twin problems concerning the quality of experimental psychology's early history and its tendency to engage in internal polemics were central issues for Boring early in his career (1927, 1929b). He did not manage to get much beyond the realization that the sources of intellectual originality are problematic and that controversy in science is apt to be emotional or personal. Yet, individualism was seen by him as a vital expression of the egotism that is necessary for the advancement of the individual scientist and, in aggregate, of the discipline.

In his *Sensation and Perception in Experimental Psychology* (1942) Boring's concern with individualism in scientific productivity merged with his operationism. "Science is thought. It exists in the minds of scientists. At any moment it consists of what scientists believe to be true and the best established facts are, in general, the oldest ones" (Boring, 1942, p. 608). Boring saw arising from this personalistic character a number of "...important reasons as to why science progresses no faster than it does" (1942, p. 609). These reasons include having appropriate instrumentation and techniques on hand, a postulated serial logic in the generation of progressive scientific theory, the overriding importance of the Zeitgeist (especially when related to the mental inertia of individuals) and the many personal and social attitudes of scientists which tend to foster polemic debate and support conventional analyses. It is interesting that Boring saw all these factors as hampering scientific progress. One can just as easily interpret them as expressions of inescapable intellectual and social conditions under which any scientist must work. If this view is taken, it becomes critical that the social dynamics of scientific investigation be studied. However, Boring repeatedly rejected such an emphasis in

favour of a scientific individualism. The motivations, emphases, and difficulties of the field of psychology were held to be analogous to those of an individual thinker within that field. Reflecting much of the psychology of his time, Boring's sense of social dynamics reduced itself to an aggregate of the psychological dynamics of individuals until the factors inhibiting psychology's progress seemed almost like those of individual neuroticism.

This focus on individuals very quickly leads to historical ambiguity. Boring and Boring's 1948 article on master-pupil relationships in American psychology illustrates this problem. They devised charts of presumptive relationships based primarily on who served as the doctoral advisor for whom. Two points are worth noting. One was that they expressed surprise at the apparent clarity of their charts "...in view of the uncertainty of the master-pupil relationship in democratic America" (p. 139); in other words, they appreciated the social complexity of the America of this time that suggested that these master-pupil relationships, as shown, were just too pat. Secondly, although master-pupil relationships can be identified, this does not necessarily say much about the historical forces affecting the development of psychology. Boring's own relationship with Titchener suggests that this relationship can be one of repudiation just as easily as that of discipleship. More recent autobiographical writings suggest that it is very common for eminent psychologists to show great disparities between their earlier work and the work for which they subsequently became known (Krawiec, 1972, 1974). The direct influence of individual on individual seems, at best, a weak and misleading indicator of historical patterns in psychology's development. It is striking that Boring made very little comment on the patterns of clustering and discontinuity on his charts since he typically went to great lengths to provide some causal explanation. The argument for continuous individual influence as a major dynamic for scientific progress was made by implication. The enormously rich possibilities for some rudimentary structural model of scientific association and organization (e.g., Mullins, 1974) were neither explored nor implied.

This is, incidently, only one example of Boring's tendency to resolve complex problems of historical change by resorting to a simplistic dichotomy. Not only did he reduce the interrelationships between scientific generations to the distinction between master and pupil, he spoke of the complex relationships between biological and social variables in terms of biotropic or sociotropic approaches. Indeed, the notion of polarized conflict, whether between individuals or systems of thought, pervaded many of his detailed analyses of the dynamics of historical change (e.g., Boring, 1929b).

The 1950 revision of Boring's *History of Experimental Psychology*

added to and modified much of the original. His first revised chapter concerned the nature of science and his revised last chapter provided an updated assessment of psychology at mid-Century—both chapters represented extensive modifications of previous work. With this edition, Boring emphasized the positive features of the *Zeitgeist* and the complexities of historical causation. Although the individual scientist's thinking was still crucial,

...the truth seems to be that the thinking goes on within the culture, that the cultural forces are tremendously complex, that multiple causation is the rule, that a given decision is often a necessary, even if insufficient cause of an historical event, but that the man who made the decision may not have been necessary. Someone else could have made the decision...(Boring, 1950b, p.23)

Boring appears to be anticipating recent debates in the history of science concerning the relative merits of internalist histories which stress the logical and empirical progression within a science and externalist accounts which emphasize general social and intellectual efforts (the *Zeitgeist*). Predictably, Boring's emphasis on individuals versus the influence of the *Zeitgeist* fluctuated. Here he retreated from his earlier concern with individuals, partly because his writing since the first edition of his *History* only served to emphasize the lack of great men in psychology—at least insofar as the production of fundamental intellectual revisions was concerned. Boring's perception of this continuing deficiency, when contrasted with psychology's obvious prosperity, produced a revision of his fundamental historiographic principle.

The author has in twenty years changed his view...What is the function of the great men in science...? Are these great men the *causes* of progress or are they merely its *symptoms*? The answer is: they are neither; they are the *agents* of progress. The tiniest element of scientific progress... is a human event in a man's thought and brain.... That man is counted great whose insights are crucial and lead to long continued important progress in new directions. With proper advertising, the new development becomes identified with the name of the man in whose brain the crucial initiating insight occurred. (1950b, p. 744)

This rather ambiguous insight is further developed in one of Boring's final articles (1963), written in reference to Kuhn's newly-published thesis on the nature of scientific revolutions. Here Boring expressed the thought that the Great Man is often simply a sign of more broadly based intellectual change, "an eponym... who is said to give his name to... an important new paradigm..." or, rather, who has "...posterity wrest it from him and apply it to that period or event in which it sees his image more plainly than it sees others" (1963, p. 20). The historical eponym becomes a disciplinary placebo, satisfying the psychosocial requirements that some individuals within a science be honored and held up as models of appropriate conduct and achievement (Merton, 1957; Mullins, 1973). Thus, the discipline necessarily exaggerates their effect and influence,

diminishing the contributions of their contemporaries and of more impersonal forces.

In some ways, this revision of Boring's historiographic principles undermines much that he had written about psychology's progressive history. His major writings are predicated on the ability to identify the significant contributors to psychology's cumulative development. Such judgments must be based upon some standards of what constitutes progress. Boring had argued that this was best seen by identifying those great men in psychology who were struggling with or against the dominant *Zeitgeist*. The notion of "eponym as placebo" refers to structural features within the social organization of modern science regardless of the particular *Zeitgeist* or the special individuals that seem to be dominant. Moreover, it suggests that too great a reliance on these latter factors may serve to mask the changes of social context within which psychology develops.

What might a psychologist wishing to profit from Boring's example emphasize when studying the history of psychology? First of all, it is relevant to study the contributions of eminent individuals, partly for their intrinsic value and possibly as useful role models. Biographies and autobiographies provide much intriguing though unsystematic information. The value of the concept of *Zeitgeist* is more problematic. It has been demonstrated that an objective definition of a *Zeitgeist* can be generated using content analytic techniques (Hyman & Shephard, 1980). Unfortunately, such procedures are very time-consuming and, to date, have not been used much in psychological histories. Thus, the concept of *Zeitgeist* degenerates typically into an intuitive, excessively broad judgment of social context; more than anything else it comes to serve as a dialectic balance to the concept of the Great Man.

More specifically, this analysis suggests that psychology needs an administrative and political history that is quasi-independent of its intellectual one. Boring suggested that psychology's success had outstripped its achievements. To understand the extent to which this might be true, we need to know much more about the facts and interrelations of such events as the formation and changing character of major professional groups (such as the American Psychological Association), the emergence of clinical psychology, the homogenization of graduate programmes, the development of licensing procedures, the growth and reorganization of the study of child development, the impact of the computer, and the relationship between granting systems and the direction of psychological research. Many other examples could be added. Boring touched on many matters similar to these and certainly recognized their importance. However, his dominant model of the form of scientific psychology and of its appropriate historiography precluded emphasis of such matters in

his major works. This is a pity because his major involvement in the administrative and political affairs of psychology in the first half of this century probably equipped him to do the job better than anyone else in the psychology of his time.

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