Edwin G. Boring: The Historian's Path in the Pages of *The American Journal of Psychology*

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Although he is best known for his classic textbook, *A History of Experimental Psychology,* Edwin Garrigues Boring published dozens of articles in *The American Journal of Psychology* and used its various formats to guide the discipline in the early 20th century. This report reviews a small sample of his publications, including obituaries, notes, and experimental articles, and presents them in historical and biographical context. A central objective is to show how Boring shared the values of his structuralist training with the emerging American schools and how time allowed him to reconsider his approach to history and the legacy of his iconic mentor, Edward Bradford Titchener.

KEYWORDS: Edwin Garrigues Boring, Edward Bradford Titchener, structuralism, *American Journal of Psychology,* history of psychology

To write about Edwin Garrigues Boring is to imagine him sitting in his cramped third-floor office in Harvard's Emerson Hall, waiting to address your 4,000-word manuscript with 8,000 words of criticism. In a passionate homage to Boring, his protégé Stanley Smith Stevens (1968) described a multifaceted Boring who valued, above all else, expressive literacy. In the 18-page obituary, Stevens offered at least four pages that described Boring's commitment to writing, editing, and criticism. The description was not disproportionate. Boring came of age when psychology seemed unmoored and fractured, and he wrote to provide cohesion and direction. For the first half of the 20th century, *The American Journal of Psychology* (*AJP*) carried his message.

This is not a comprehensive biography, and I write knowing that Boring had a specific definition of scientific psychology (Kelly, 1981), that he was a politically complex individual (Boring, 1951; Winston, 2002), and that he was an imperfect chronicler (Thomas, 2009, 2016). I will describe selected publications to show how Boring's work in *AJP* reflected his journey from structuralist student to disciplinary historian. In fact, the historian's voice was apparent in *AJP* long before Boring published any of his landmark books on the origins of experimental psychol-

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ogy (Boring, 1929, 1942, 1950). With room for not only experimental reports but also for commentary, obituaries, and book reviews, *AJP* allowed him to address his audience from different directions, always in an effort to explain *why* history matters and how it can and should direct the emerging schools. Even his biographical pieces, retrospective by definition, end with an eye to the future and a reminder that legacies are never complete. One legacy, that of his mentor, preoccupied Boring and gave his historical writing—and American psychology—a background against which it could become clear (Boring, 1961, p. 22).

An Engineer With the Psychological Point of View

In 1905, E. G. Boring was an engineering student at Cornell when he elected to take Edward B. Titchener's course in elementary psychology. He was enthralled by the Englishman who lectured in a master's gown on topics ranging from tonal beats to the English language. Indeed, it was Boring's use of language that distinguished him in the undergraduate classroom, and a written response to a test question—not a dazzling display of technical skill—drew Titchener's praise; Wilhelm Wundt's former student singled out the aspiring engineer and told him, "You have the psychological point of view!" (Boring, 1961, p. 19). Boring graduated in 1908 and carried the memory of Titchener's accolade back to his home state of Pennsylvania, where he tried his hand as an electrical engineer at the Bethlehem Steel Company and as a teacher at a Moravian parochial school. He disliked engineering because, contrary to his expectations, it was more about profit than discovery; he disliked teaching because, among other things, his students once glued him to a chair. Still dependent on his father's support, he had become the family failure. All the while, "the magic of Titchener's lectures . . . was still working" (p. 21). In 1910, the magic lured him back to Cornell, and with a $500 annual assistantship, he had the financial independence to pursue a PhD under Titchener.

An Experimental Investigation in "The German Tradition"

Working with Titchener meant publishing in *AJP*. Granville Stanley Hall founded the journal in 1887 to promote the experimental psychology of Johns Hopkins and other American universities, and when Titchener joined as a cooperating editor in 1895, he made it the "organ for himself and the Cornell Laboratory" (Boring, 1961, p. 42). The lab published work fashioned in what Boring called the "German tradition" (p. 22), dedicated to exploring the contents of consciousness with a large effort to avoid applied intent. Boring's earliest manuscripts were not exceptions. In late 1911, Titchener instructed his student to develop a thesis on visceral sensibility, and the results were published in *AJP* as "The Sensations of the Alimentary Canal" (Boring, 1915). The manuscript is a prime example of introspective psychology, and Titchener's student dutifully avoided the temptation to address the clinical significance of what may be structuralism's most physiological investigation. Boring (1915) reviewed work from those who believed that the internal organs were, in fact, sensitive and others who believed that visceral sensations arose not from the organs but from the body wall, pleura, or peritoneum. He aimed to resolve this (mostly German) debate by administering stimuli to the esophagus, stomach, rectum, and colon while noting "the dependence of sensation upon intensity of stimulus" and obtaining "a description of the psychological character of the experiences by taking full introspections upon all occasions" (p. 5).

Although one short paragraph gets tantalizingly close to addressing the functional significance of pain and hunger (p. 4), Boring had no interest in *Why?*; this was a psychophysical study that only asked *What?* and *Where?*. Most of the trials required "observers" to swallow one end of a rubber tube so the experimenters could thermally, electrically, mechanically, and chemically stimulate the esophagus and stomach. For example, the tubes and associated apparatus allowed Boring to place an inflatable bladder or deliver electric shocks at precise locations along the length of the esophagus (it seems the engineer's training was not entirely wasted).

As was the custom in Titchener's lab, the observers were departmental associates and students, presumably trained in introspective techniques and all identified by name in the report. Boring's collaborators withstood the procedures to varying degrees; one frequently vomited the tube. Another was unable to talk (and therefore unable to report) with the tube in place, and another simply could not tolerate the tube without gagging. Boring, on the other hand,

spent two years practicing his technique, and in his biography he boasted that he could swallow one end of a tube and answer the telephone without removing it (Boring, 1961, pp. 27–28). He was the primary observer and the only one to undergo "experimental work" on the colon and intestine (Boring, 1915, p. 49). A photograph of his bare torso shows the coordinate system upon which all localization reports were mapped.

The 57-page article is mostly a single-subject study, and in the structuralist tradition, Boring presumed to speak for humanity when he reported his introspections. He concluded that "the esophagus is sensitive to warm and cold throughout its length," although localization is not as precise as others had reported, that "electrical stimulation of the stomach gives rise to sensations characteristic of electric shock," which may or may not be due to current spread to adjacent tissues, and that "distention of the rectum produces the call to defecation" (pp. 56–57). The article ends with a summative list of similar observations. There are no remarks about how the esophagus tolerates temperatures that are similarly tolerated by the mouth, for example, and there is no speculation about why hydrochloric acid in the stomach produces hunger pains. There is no discussion of how these results might vary from person to person. There is no speculation about how illness might affect sensations, and there is no mention of how the study might be expanded or improved. In the structuralist tradition, stimuli were presented, thresholds were documented, and introspections were reported.

Boring completed his graduate work, passed his examination, and earned his degree with very little trouble and very little direct consultation with Titchener. Indeed, he must have had the psychologist's—or at least Titchener's—point of view. He would soon find his own.

Mental Testing and the Allure of Functionalism

Boring remained at Cornell until 1917, when he took his first tortured steps away from Titchener and introspection. This split haunted him for the rest of his life. In his biography, time and again, Boring reflected on how Titchener regarded or, after his passing, would have regarded each professional decision. Titchener was Boring's great man, the one who had shaped history, and his link to psychology's past, but he did not want to follow the mentor he so admired. Therefore, Boring's writing was dedicated to reconciling past and present; American psychology, he argued, could find its way by defining itself in contrast to tradition generally and Titchener specifically. Boring elevated Titchener by describing him as American psychology's perfect opponent: "No intellectual movement can get moving without something to push against, and American psychology had Titchener" (Boring, 1961, p. 22). Boring's passion for the history of psychology was born of the need to know "why American psychology, while attempting to copy German introspective psychology in the late nineteenth century, nevertheless went functional" (p. 69). The fault was partly his own.

Titchener was meeting with his exclusive group of experimentalists at Harvard on April 6, 1917, when the United States declared war. He had established the society in 1904 and annually met with these psychologists who shared, above all, his social proprieties. Although they were an eclectic group, functionalist pursuits such as mental testing were typically off limits. However, when the group's attention turned to how American psychologists could contribute to the war effort, the Englishman tactfully recused himself, and Robert Yerkes—president of the American Psychological Association, comparative psychologist, and friend of behaviorist John Watson—took the chair. Yerkes invited Captain W. S. Bowen, Harvard instructor of military science, to advise the group on the army's problem of eliminating the "feeble-minded" from recruitment pools. "It was this small stone that began the avalanche of intelligence testing in the Unites States Army" (Boring, 1938, p. 415). After the war, Titchener continued his annual meetings, but in 1917 a large portion of American psychology had stormed away from him and taken E. G. Boring with it.

Boring followed Yerkes into military service and set to work testing recruits, scoring tests, and generating reports. Much to his own surprise, the structuralist graduate student found fulfillment as a practicing functionalist. He "saw clearly that good, honest, intelligent work in any field merits respect and that the testers closely resemble the pure experimentalists in habits of work, in enthusiasm, and in thoroughness" (Boring, 1961, p. 31). Boring had served for less than a year when the war ended, but before returning to

civilian life, he went to Washington to help Yerkes prepare and present the test results.

In 1919, Boring accepted G. Stanley Hall's invitation to join the faculty at Clark University and fill the vacancy left by the untimely death of Titchener's former student John Wallace Baird. Although he finally had his own lab, Boring postponed experiments to draft a significant historical manuscript. Through his military service, Boring saw utility in mental testing, but while studying the history of probability theory at the Library of Congress, he came to appreciate its limitations. In particular, he was troubled by the zeal with which the testers overapplied the normal law of error, and he aimed to warn them (Boring, 1920). He did not discourage mental testing, as Titchener might have, but instead dismantled the arguments of past researchers who had given the normal distribution its inappropriately exalted status. The article does not scold or dispute; it explains and warns. The mental testing "avalanche" was about to spread far beyond the military, and like a parent offering last-minute advice to departing offspring, Boring acknowledged the promise of a new frontier while he explained the dangers and the wisdom of the elders. He aimed to offer the testers "discriminating encouragement" (Boring, 1920, p. 1).

Directing Methodology With a Historical Perspective

Hall shared Titchener's appreciation for history, and by 1920 the *AJP* had an established record of publishing historical pieces such as Freud's (1910) landmark "The Origin and Development of Psychoanalysis." The context Boring (1920) provided in "The Logic of the Normal Law of Error in Mental Measurement" is an early example of how well he learned the historian's craft at Cornell and, more importantly, how well he could communicate to a broad audience. He did not write like Titchener, and comparing the two, S. S. Stevens noted, "The master wrote for himself, whereas the pupil wrote for the reader" (Stevens, 1968, p. 591). The normal law paper (Boring, 1920) invited a wide audience from "laboratories, school-systems, factories, or the army" (p. 1), and Boring wrote with the style and patience to reach them.

Boring believed that the army's mental tests had served their purpose. However, he was troubled by how the Gaussian law of normal distribution was assumed to apply to almost every scale imaginable, including measures of human intelligence. The functionalists were blind to the limitations of their craft, and Boring relished the opportunity to explain why. Whereas Titchener objected to what the functionalists wanted to do, Boring objected to how they were doing it. According to Boring, this was the essence of effective criticism; it must be internal and not external. No reader would be impressed by a structuralist who criticized a functionalist for behaving like a functionalist. He told Stevens (1968), "You can criticize only by showing that the author has failed to do what he himself has set out to do. . . . I have got to show that my method will do [his] self-appointed job . . . better than his own" (p. 600).

Boring began his internal criticism by tracing probability theory to its origins in a practice that most readers could appreciate: games of chance. In the case of a coin flip, it is not unreasonable to argue from probability to frequency; that is, if a coin is a uniform, homogeneous circular disc, one should expect a roughly equal number of heads and tails from a large number of tosses. With no reason to expect a bias in favor of one outcome or the other, most investigators would consider each one equally probable. This reasoning may work perfectly well for a gambler who understands the few physical constraints of a coin flip, but it will fail almost anyone else. In most cases, to argue that two outcomes are equally probable because one has no reason to assume otherwise is to argue from ignorance, to derive probability from thin air.

In his article, Boring turned to Johannes von Kries (1886), a German physiological psychologist and student of Hermann von Helmholtz, to illustrate the dangers of what von Kries called the principle of insufficient reason. Boring explained how von Kries invited his readers to consider whether iron is in the distant star Sirius. With no evidence in favor or against, the principle asserts that the probability that Sirius has iron must be one in two (i.e., either it does or it does not). Von Kries expected his reader to understand that ignorance is complete uncertainty and that with complete uncertainty there can be no probability. Boring quoted von Kries (1886) in German: "Die Aufstellung der gleich möglichen Fälle muss eine in zwingenrer Weise und ohne jede Willkür sich ergebende sein" (p. 11). Translated, von Kries stated that the establishment of equal possible outcomes

must be compelling and not arbitrary. At the time, it was common for American psychologists to assume that their audience could read at least some German, but Boring took the opportunity to emphasize the cultural origins of this wisdom, offering no translation in an era of vicious anti-German sentiment. He added, "There is no alchemy of probabilities that will change ignorance into knowledge" (Boring, 1920, p. 3). This is not to say that probabilities should never be estimated, but they should be informed and perhaps deferred until science provides more information. Boring chose his example well: Concurrent with the publication of von Kries's book, English astronomer William Huggins was using spectroscopy to demystify the composition of stars. By 1920, a curious scholar knew that Sirius probably had a tiny bit of iron (i.e., a little science revealed that the odds were much greater than 50/50) and almost certainly had hydrogen and helium. Boring's contemporary audience could see the absurdity in estimating probability from ignorance and the benefit of waiting for data to inform estimates.

Boring then accused the mental testers of estimating probability from ignorance through the overapplication of Pierre-Simon Laplace's (1812) normal law of error. In particular, Boring blamed Belgian polymath Adolphe Quetelet (1849) for imposing the law on the measurement of human traits generally and then on Francis Galton (1869) for applying it to human intelligence specifically. Quetelet saw the law as naturally preordained:

> Everything occurs then as though there existed a type of man from which all other men differed more or less. . . . This mean varies among different people, and sometimes even within the limits of a single country, where two people of different origins may be mixed together. (p. 96)

According to Quetelet, the measurements were destined to follow the law, and when they did not, he was able to make them conform. For example, he noted that if a distribution appears skewed, it is not because the law has been violated but because there must be multiple normal distributions, with unequal means, within the larger distribution. He went on to review data from 100,000 French conscripts and noted that 28,620 had been rejected for failing to meet the minimum height requirement of 5′2″. His calculations, based on the assumption of a perfectly normal distribution, indicated than only 26,345 should have been rejected. Rather than accept that the men had been appropriately dismissed from a skewed distribution, Quetelet concluded that 2,275 had been victims of fraud (p. 98). Instead of allowing observation to describe the distribution, he demanded that the data bow to theory. Quetelet applied the normal distribution with insufficient reason, and others followed.

Francis Galton (1869) embraced Quetelet's theory, and it is a cornerstone of his landmark text *Hereditary Genius*. In the introduction Galton declared, "The range of mental power between . . . the greatest and least of English intellects, is enormous. . . . The method I shall employ for discovering all this, is an application of the very curious law of 'deviation from an average'" (p. 26). In all his mental test measurements, he claimed, "deviations from the average follow theoretical computations with remarkable accuracy" (p. 30).

Boring noted that it took Galton only 10 years to disavow his faith in the normal distribution (Galton, 1879), and he was probably pleased to report that Galton's revelation came by way of German psychophysics and Fechner's law. In its simplest form, the law states that sensation = log(stimulus intensity). An observer tasked with differentiating a standard stimulus and comparison stimuli that are either more or less intense than the standard will make larger errors for the more intense stimuli; errors in excess and errors in deficiency will *not* be equal in magnitude. Most psychologists of the time understood the law, and if perception, the gateway to the mind, did not conform to the normal law, it seemed unlikely that other mental functions would. Boring stated the obvious: A law derived from a finite set of examples in the physical world cannot be universally applied to the mental. He concluded his description of Galton's revelation with exasperation: "If the earlier writers . . . had endeavored to first classify experience in deviations from the average . . . that curve would never have obtained its present position in the theory of errors" (Boring, 1920, p. 14). They had ignored data in favor of theory.

Having told the story about how the French (Quetelet) and English (Galton) became inappropriately enchanted by the normal distribution, Boring (1920) took another opportunity to salute the cau-

tious Germans, who were "not so much interested in the applicability of the normal law as [they were] in the facts. The appeal to facts is . . . always a protest against theory which is given *a priori*" (p. 17).

The normal law of error had been overapplied and Galton himself admitted it. Boring's final task was to explain what should be obvious: There is no justification for assuming the normal law will apply to the (human-made) units of mental testing, and he cited a budding line of research that, in fact, shows that it does not (Trabue, 1916; Williams, Titchener, & Boring, 1918).

Even if a mental measurement does conform to the normal law, Boring (1920) argued that we may not be fortunate enough to guess and use the appropriate unit. He provided a simple example by considering the size distribution of common salt crystals and asking whether size should be measured by height or by weight. Because weight should be proportional to the cube of height, the normal law cannot possibly apply to both scales. Nature will not guarantee us a normal distribution in whatever we choose to measure, and even if a normal distribution is to be found, it may not apply to our chosen scale.

In closing the article, Boring (1920) did not deny the utility of the normal law of error but outlined criteria by which it should be cautiously applied. Like Titchener, who believed that psychology needed a better understanding of consciousness before venturing into applied fields, Boring believed that the functionalists needed more data and better mental scales before using the normal law to estimate probability. He did not dismiss mental testing and concluded with words of encouragement:

> There is nothing new in the contention that mental measurement is impossible, whereas now we do gain the assurance that rank-orders at least are validly demonstrable. And there is a great deal that can be done with rank-orders. . . . The serial constraints that do not presuppose a unit, yield less intricate resultants, but they present a rougher picture that represents truly the rough material which they describe. (pp. 32–33)

Although he never named his mentor in the normal law paper, Boring (1920) knew that *AJP* was Titchener's platform, and he had used it to, at least partly, endorse functionalism. The young professor had asserted himself but not without directly praising the wisdom of the German tradition and, perhaps, excusing introspection by indirectly explaining why its largely descriptive mental "measurements" may not yield to mathematical models. Boring offered Titchener all the respect he could afford.

The article cemented Boring's reputation, and he believed that it impressed philosopher R. B. Perry enough to earn him an invitation to Harvard in 1922 when Clark's political climate soured. Forty years after the article's publication, however, Boring lamented that it "had never been effective, although the changing stream of scientific opinion (had) continued to flow in . . . the right direction" (Boring, 1961, p. 32). Reflecting on the 1920s, Boring stated that he "contributed no important research" (p. 51) but "found that (he) could get (his) crucial problems worked out at other universities if (he) would discuss the problems in publication" (p. 46). This statement belies Boring's claim that his normal law paper had been ineffective. It secured his position—at Harvard and in the psychological community—and gave him the confidence to leave the lab work to his graduate students and focus on the "crucial problems." He knew he had an audience. He had become "mixed in with the stream of American psychology, [he had become] its agent" (p. 51).

Losing the Language

By the 1920s, the mental testers, Gestalt psychologists, and behaviorists were running at their own pace and in their own directions, with no regard to pedigree. The trend was not new. Decades before, when James Mark Baldwin (1895) wrote his scathing critique of introspective methodology, Titchener (1896) responded by addressing the Princeton professor's insolence when he replied, "I cannot think that his attitude to a long line of predecessors in the field is either scientifically or ethically defensible" (p. 241). Decorum and deference mattered to Titchener but not to the Americans who were disillusioned by introspection's restrictive methodology. Titchener was destined to be disappointed, and from the moment he joined the staff in 1895, had no reason to expect *AJP*'s readers to blindly revere psychology's intellectual ancestry. An editorial that bears his name declared that *AJP* was a decidedly "American" journal

and that it would not honor "discipleship to past or present leaders, or excessive deference to European thinkers" (Hall, Sandford, & Titchener, 1895, p. 4).

Nothing compromised American deference to European thinkers like the Great War. The impact on the German economy and universities was incalculable, and through the 1920s, an American psychologist would be hard pressed to keep up with the work coming from proliferating domestic laboratories, much less European ones. In a brief communication titled "Do American Psychologists Read European Psychology?" published in the *AJP* "Notes and Discussions" section, Boring (1928) presented results from a survey of 114 American libraries (31 public and 83 university) to show that, among foreign serials, German-language journals had lost favor to British ones, even though "German psychology (had) long led British psychology" (p. 674). The article's title is misleading. Boring was not surprised to find that an American psychologist has access to the English journal *Mind,* "which he will seldom want" (p. 675), nor was he surprised to see that the libraries did not favor French- or Italian-language journals, but the unpopularity of the "new" German Gestalt journal, *Psychologische Forschung,* was unforgivable. Today, a reader might assume that Boring was simply bemoaning American provincialism. He was frustrated, but his historical perspective was conspicuously absent. Boring did not mention the anti-German fervor that swept the United States in the postwar years or the fact that some American professors were concerned that a German victory could come by way of scientific, if not political, domination. George A. Miller, a mathematician at the University of Illinois, called for American universities to commit themselves to their own research and break ties with German science and the German language:

> Our students should not have to feel that the great majority of the best expository works relating to their subject are to be found only in the language of a people of low ideals imbued with a morbid desire to dominate the world at any cost. (Miller, 1918, p. 117)

Before the Supreme Court ruled such laws unconstitutional in 1923, 22 states had banned the public use and teaching of German (Gordin, 2015, pp. 181–182). American interest in German psychology was not just declining; it had been actively opposed and, for a time, legally forbidden. Boring's article is therefore a brief, cautious statement of facts. A decade after the Treaty of Versailles, Boring was gently asking his English-speaking audience to consider the consequences of losing access to German science. He closed with a note of resignation: "Perhaps there is nothing to do about it, unless the fault and remedy lie in our graduate schools" (Boring, 1928, p. 675). American interest in the German language never recovered (Gordin, 2015, pp. 181–182), and the next generation of American psychologists did not have the ability or the inclination to read Wundt in his own words. Boring's disappointment was probably compounded by the fact that his mentor, his link to the German tradition, had died the year before.

Losing "The Great Man"

"Seldom did (Titchener) distinguish between his wisdom and his convictions, and he never hid either" (Boring, 1961, p. 23). From the beginning, most could have guessed that Titchener and *AJP* would part ways. When his former student Karl Dallenbach purchased the journal from Hall in 1920, Titchener assumed that he had a philosophical and professional ally, and for a while he did. After all, like Boring, Dallenbach joined the military's mental testers, but unlike Boring, he returned to Titchener and Cornell. However, Titchener resigned from *AJP* abruptly in 1925 amid circumstances that may have been as much about personalities as philosophies. Titchener assumed he had full editorial control, but, unknown to most, Dallenbach had taken a significant financial risk to purchase *AJP,* and "when fiscal wisdom came into conflict with [Titchener's] editorial policy, fiscal realism won" (Boring, 1961, p. 42). Nonetheless, Titchener's departure left a tremendous editorial void that Dallenbach scrambled to fill with Titchener's former students Madison Bentley, Margaret Floy Washburn, and Boring. Titchener responded by founding a competing journal and turning his back on *AJP,* "predict[ing] shipwreck for a vessel with four rudders" (Boring, 1958, p. 15). Titchener died 2 years later.

Dallenbach asked Boring (1927) to prepare Titchener's obituary, the first of many that he wrote for *AJP*. Both men must have understood that, to some degree, their reputations developed at Titchener's

expense, and in 1927 the protest against structural psychology was still fresh and *AJP*'s future was still uncertain. Titchener's obituary is an appropriate record of life events and accomplishments that concludes with a note of uncertainty, stating that "the evaluation of Titchener's psychology can be left to posterity. . . . A century hence it will be possible to say just where his psychology belongs in the history of science" (Boring, 1927, pp. 505–506).

Boring withheld judgment, and at the time he was equally uncertain about psychology's future. If one were to remove the references to Titchener from the previous quote, it would capture the sentiment that closes the first edition of *A History of Experimental Psychology* (Boring, 1929). It would read like this: "The evaluation of . . . psychology can be left to posterity. . . . A century hence it will be possible to say just where . . . psychology belongs in the history of science." Two years after Titchener's death, Boring was disappointed with psychology for not having produced a great man to unify the discipline in the way, for example, Charles Darwin had unified biology. He declared, "Psychology has never had a great man to itself. . . . There are signs that psychologists are ready for a great man or a great event . . . but the great event has not yet occurred" (Boring, 1929, p. 660). Perhaps Titchener was supposed to be psychology's great man, but he had inspired division rather than unification. In 1927, Boring could not place Titchener in the history of psychology, and in 1929 he could not place psychology in the history of science. He was disappointed with the man and the discipline.

By 1950, he was more optimistic. When Boring (1950) published the second edition of *A History of Experimental Psychology,* he had the benefit of hindsight and had retreated from the great man theory of history. Psychology had matured and established itself as a science. Gestalt psychology, behaviorism, and the study of motivation had all borne fruit, and the proliferation of departments, journals, and textbooks stood as testimony to the discipline's strength. Boring's book increased by a third from the first edition to the second. Most importantly, psychology had not needed, and would not need, a single, galvanizing leader. It had progressed by integrating the accomplishments of many people, past and present; it had "matured, not like a person who never picks up new ancestors but like a family which, when a scion marries, acquires suddenly all the ancestors of the new spouse" (Boring, 1950, p. xiii). Two decades had given Boring the opportunity to reconsider psychology and Titchener; he did not *need* to be the great man, but the obituary had already been written. "Can history be revised? Yes" (Boring, 1950, p. xiii).

Revising History Through Biography

The *AJP* thrived under Dallenbach's direction, and he and Boring cemented a partnership and friendship that lasted for decades. The bond was so strong that each agreed to write the other's necrology. Instead, Boring got the opportunity to write Dallenbach's *vita* (Boring, 1958) on the 50th anniversary of his career. In it, he painted a picture of another student who, like him, was lured into psychology by Titchener, but he had some explaining to do.

Nobody knew the Dallenbach–Titchener relationship better than Boring, and Dallenbach's biography gave him an unusual opportunity to address two legacies and to reconcile the two men he admired most. He began with portraits of character. The 40-page biography of Dallenbach names Titchener more than one hundred times and includes a page-width photograph of the Englishman seated next to a table piled high with papers. To his former students, Titchener was an intellectual giant and the praise that Boring offers Dallenbach is measured first in how well he matched Titchener's personal attributes:

> No one . . . re-presented Titchener to the psychological world more nearly than has Dallenbach: . . . his basic social values, his sense of propriety, his initial formalism . . . his dominance and also its complement, his kindliness, his intense interest in the personal welfare of his students; his capacity for indefatigable meticulous hard work . . . he still carries with him many of these Titchenerian values after all these years. (Boring, 1958, p. 11)

"Titchener," the mortal noun, had become "Titchenerian," the immortal adjective, manifested in Dallenbach. But if these men were so similar, and if Dallenbach so admired Titchener, what could explain the split? It is with an odd sense of relief that Boring describes Titchener's deadly affliction; a

brain tumor gave the former students a resolution to the 1925 crisis that most other illnesses could not:

> In 1927, two years after [he left] the JOURNAL, Titchener died of a brain tumor, which provided ample explanation for his capricious forgetfulness and his inconstant judgment in the year or two immediately preceding. . . . As soon as (Dallenbach) knew of the brain lesion, his loyalty to Titchener's memory and ideals surged back and it has never diminished, though changing times have altered the form of its expression. (p. 15)

It is not clear that Boring's loyalty surged back as quickly as Dallenbach's supposedly did. In the original obituary, Boring (1927) did not address the *AJP* controversy or Titchener's cause of death, although he surely knew about the former and probably knew about the latter. Titchener was Boring's link to psychology's experimental origins, but by the time he died, the sage had become isolated. Of course, the isolation was partly self-imposed. Titchener had left the American Psychological Association decades earlier and, after 1911, he seldom traveled except to meet with the Experimentalists (Boring, 1927). From 1912 to 1917, he toiled over but ultimately abandoned a systematic text; Boring supposed that he was unable to nurture it beyond a state of incubation (p. 501). The uncertainty was reflected in the Cornell lab, which had an ever-changing language; "a graduate three years absent had on returning to learn psychology again" (Boring, 1927, p. 500). The Titchener that Boring and Dallenbach knew as students had faded through the 1920s and certainly was not there when he stormed away from *AJP* in 1925. When Boring wrote the obituary in 1927, Titchener's legacy was incomplete and uncertain. After all, to write a conclusive obituary within a year, or even within a decade, of the subject's death would have violated Boring's own rule for historical writing: "I speak with confidence up to twenty years ago; I speak, but with less assurance, of the next decade; whatever I say for the most recent decade is based on gratuitous courage" (Boring, 1950, xv). Thirty years allowed Boring to speak, confidently and finally, about Titchener's science.

"On the record Titchener died in 1927, yet he was still enough alive for Dallenbach to take him along when he left Cornell in 1948" (Boring, 1958, p. 11). Titchener was not just a man; he was an idea that could manifest itself in his disciples. However, Dallenbach's work, although always linked to sensation and perception, seldom adhered to the structuralist approach of introspecting the generalized human mind; he studied the perceptual skills of those with visual and auditory impairments, designed experiments that did not demand trained observers, and conducted groundbreaking research on sleep and memory (Jenkins & Dallenbach, 1924). Boring needed to be thorough if he expected his readers to see Titchener in Dallenbach and to understand that Titchener's psychology could be found in the methods, if not the hypotheses. He started with the training.

Dallenbach's psychological journey began when, as an undergraduate at the University of Illinois, he drew the attention of Titchener's former student John Wallace Baird. When Titchener visited, Baird showed him Dallenbach's meticulous notebooks, and although the undergraduate had intended to pursue law, "Baird and Titchener had now become Karl's two idols in psychology, there was no place for him to go but to Titchener at Cornell" (Boring, 1958, p. 9). He completed a PhD in 1913.

Having defended his thesis, Dallenbach intended to obtain an MD at the University of Pittsburgh, but again, Titchener redirected him, and he took a faculty position at the University of Oregon. "It is hard to explain this 'magnetic' power that Titchener had over (his) disciples" (p. 12). Dallenbach enjoyed the University of Oregon, but it "had no money for 'personal research,' and that, it turned out, meant self-initiated research, research that the investigator had thought up on his own and wanted to do" (p. 13). It seemed the University of Oregon wanted only practical research, but Titchenerian Dallenbach was no functionalist. Titchener brought him back to Cornell in 1916 (p. 13).

Boring (1958, 1961) portrayed Dallenbach as Titchener's loyal and like-minded disciple up until the *AJP* crisis of 1925. Titchener, "always self-confident, regarded himself as ruler through natural right even though one of his subjects had provided the cash" (Boring, 1961, p. 42). The arrangement was doomed to fail, but Dallenbach's loyalty somehow sustained it for 5 years. Eventually, fiscal pragmatism

trumped editorial dogma, so Titchener walked away from Dallenbach, leaving only his science behind.

To Boring, Titchener was gone long before 1927, but his pre-illness ideals had already been transferred to Dallenbach. Nonetheless, the student needed to construct his own environment and shake the last remnants of the material Titchener. He needed to leave Cornell, where, despite being Sage Professor of Psychology, "Dallenbach could not see himself as having attained the goal that the Titchener-image demanded" (Boring, 1958, p. 17). An offer to chair the psychology department at the University of Texas allowed him to establish Titchenerian psychology anew.

When Dallenbach arrived in Texas in 1948, he found a university flush with oil money and ready to expand its psychology department. He found "clinical psychology predominant [at the University of Texas]," but "he was determined that experimental psychology should be the core of the department" (Boring, 1958, p. 18). When Boring described the layout of Dallenbach's lab, he presented it as an expansion of the Cornell lab:

> Dallenbach interpreted experimental psychology in Titchenerian terms, with more stress on sensation and perception than on motivation and personality. . . . The special rooms on the ground floor of the new laboratory clearly reflected the influence of Titchener, who had divided the Cornell Laboratory by sense-departments. (Boring, 1958, p. 19)

However, Dallenbach expanded outward from his Titchenerian foundation, but Boring justified each transgression:

> There was . . . one un-Titchenerian feature (in the new lab), a suite of rooms for comparative psychology, but the father image would scarcely even have scowled, for mammals were housed in another building and the apes eight miles away. (p. 19)

Boring continued with more un-Titchenerian features:

> Titchener held that experimental psychology deals with the human, adult, normal, generalized mind, and this view affected Dallenbach's conception of the proper laboratory policy and also his policy for *The American Journal of Psychology*. There are exceptions. The rules are not rigid. Dallenbach used cockroaches as subjects in one of his best experiments. More than once he studied the capacities of children, and by no means did he confine his research to introspection, though his predilection for the investigation of sensation kept him well within the Titchenerian framework. (p. 21)

But what was the "Titchenerian framework"? Titchener resented labels, and although others called him "structural" and "introspectional," Boring "never heard him refer to his school by any other word than 'we'" (Boring, 1927, p. 497). In Dallenbach's biography, Boring deemphasized the word *introspection* in favor of *attention*. After all, "Titchener sought to settle the problem of attention by declaring its status as an attribute of sensation" (Boring, 1927, p. 499), and Dallenbach continued this work while, for the most part, shedding Titchenerian terms such as *quality, intensity, duration, extent,* and *clearness*. Boring saw no contradiction. "You could, of course, perform experiments that dealt with the conditions of attention and the effectiveness of intended distractors upon attention without yourself taking up a position on whether sensations really do possess an attribute of clearness" (Boring, 1958, p. 24). The study of attention may not have been a functionalist pursuit, but in 1958, it certainly was not anachronistic, and it certainly fit, at least as Boring saw it, the Titchenerian model. Dallenbach was to be commended for remaining true to pure, objective science in the age of applied research.

> When Dallenbach, the Titchener image before him, stuck to sensory psychology, he was swimming against the current of the times. It was a handicap, but he never faltered in the face of difficulty. He stuck to his principles. (p. 40)

In Dallenbach's biography, Boring used *AJP*'s format to reconsider and artfully shape Titchener's legacy by showing how it was alive and well in Dallenbach. More importantly, the article showcases the historian at the peak of his career. When he wrote Dallenbach's *vita* in 1958, Boring was on his way to becoming the "unofficial biographer of psychology's

great"; having just completed memoirs for Robert Yerkes and Lewis Terman, he did one more for Karl Lashley before refusing additional requests (Boring, 1961, pp. 77–78). In his own way, Boring had emulated Titchener, not as an investigator but as a "historian *par excellence*" (Boring, 1927, p. x).

In closing the preface to the first edition of his history book, Boring apologetically stated, "[Titchener] should have written this book, and it is with great diffidence that I offer a poor substitute" (Boring, 1929, p. x). The second edition (Boring, 1950) is also dedicated to Titchener but without apology. By the time he wrote Dallenbach's biography, Boring was a seasoned craftsman. He closed the article by describing Dallenbach's greatest achievement, the steady 37-year stewardship of *AJP,* and in lauding this tremendous feat, he lamented that "the pity is that it does not show up clearly on history's pages" (Boring, 1958, p. 36). The words are their own clever contradiction.

But anyone can write History. Can he not do Research?

That was Boring's "pet paranoia." He feared that those who admired his historical work quietly assumed he was incompetent in the lab (Boring, 1961, p. 15). Indeed, he was content to delegate and let his students do the work, and receive the credit, but Boring was a tireless supervisor and a ruthless editor. Any literature-based estimation of Boring's scientific output would certainly fall short because, although he contributed to student work, he "would put his name on a paper only if he was the major contributor" (Stevens, 1968, p. 600). This stubborn position led to heated arguments between Boring and Stevens (p. 601), but it also placed a series of experimental reports, published in *AJP* between January 1940 and January 1941, in sharp contrast against the rest of his writing. These four articles, coauthored with lab assistant Alfred H. Holway, present Boring as an active experimentalist, focused on a single phenomenon and surprisingly willing to claim authorship (Holway & Boring, 1940a, 1940b, 1940c, 1941). At the midpoint of his career, the established historian designed and executed a number of elegant experiments that remain staples of undergraduate textbooks and laboratory exercises (Gallagher & Hoefling, 2013; Goldstein, 2014). A simple psychophysical function, Emmert's Law, was at the heart of this work, and in "Size Constancy and Emmert's Law," published in *AJP*'s "Notes and Discussions" section, Boring (1940) provided a concise foundation for the four experimental articles.

To Boring, size constancy and Emmert's Law were two sides of the same coin. Perceptual psychologists understood that an object's apparent size remains constant when viewed from different distances, regardless of how much the image's angular size changes. This is presumably because the observer somehow processes the inverse relationship between angular size and distance. Emmert's Law explained, among other things, why a visual afterimage appears larger on a distant surface than it does on a close one. That is, an image with a stable angular size will vary in apparent size depending on the perceived distance. True to his psychophysicist roots, Boring presented Emmert's Law as an equation that linked perception and the physical environment: $s = krd$, where s = apparent size of the stimulus, k = the constant of proportionality, r = size of the retinal image (extrapolated from the image's angular size), and d = distance between observer and stimulus. The equation was so simple, and the link to size constancy so clear, that experiment seemed almost redundant. Why, then, would this particular problem motivate Boring to experimentation and authorship when, in most cases, he "could get [his] crucial problems worked out at other universities [by discussing them] in publication"? (Boring, 1961, p. 46). The answer was simple: The Gestaltists were skeptical.

Kurt Koffka (1935) addressed size constancy in his classic *Principles of Gestalt Psychology*, but he complained, "We have to the present day no complete knowledge of the quantitative relations" between perceived size and distance (p. 91). Although all of the Holway and Boring articles could be interpreted as responses to Koffka, one article, "Determinants of Apparent Visual Size with Distance Variant" (Holway & Boring, 1941) explicitly targeted the Gestaltists' challenge.

To conduct their experiments, the authors and three associates worked through the night, blissfully "free of interruptions" (Boring, 1961, p. 58), to create circumstances in which observers could make size judgments in the absence of distance cues. With an observer seated at the intersection of two dark corridors (Figure 1), the experimenters projected one

luminous circle in one hallway, on an 8′ × 8′ screen, located between 10′ and 120′ from the observer; this was the standard stimulus that, in all trials and regardless of distance, subtended a visual angle of 1°. In the second hallway, another screen displayed a comparison circle, always 10′ from the observer. The observers then needed to adjust the size of the comparison circle until it matched the standard. They viewed the circles with both eyes, and, although in the dark, stray light still illuminated surfaces of the corridor, providing "a sensory ground for the perception of the stimulus" (p. 30). With the features of the hallways visible, and the benefit of binocular disparity, observers accurately assessed distances and, as Emmert's Law predicted, easily matched the comparison stimulus to the true physical size of the standard stimulus. On subsequent trials, the experimenters removed environmental depth cues, and as they did, the size estimates were no longer good indicators of the standard stimulus' physical size. First, they removed binocular cues by occluding one eye. Then, they diminished the remaining monocular cues by having the participants view the standard circle through an artificial pupil and, finally, through an extendable reduction tunnel (a tube, actually) that blocked the corridor surfaces. In this final condition, observers were unable to judge distance to the standard stimulus and consequently made poor estimates of physical size but accurately matched the circles according to angular size. In the absence of depth cues, Emmert's Law held but was reduced to $s = kr$. The authors were triumphant:

> For all that has been said by Gestalt psychologists against the validity of [Emmert's Law], it would nevertheless appear that, when no relevant datum other than retinal size is available, then the perception of size will after all vary solely with the visual angle. (Holway & Boring, 1941, p. 36)

Without depth cues, perceived size matched the relative size of the proximal stimulus. Titchener would have been pleased. The historian had returned to the lab to answer a challenge from the Gestalt school, but there was more. To Boring, the Titchenerian disciple, positivism was a hidden pillar of Gestalt psychology. In his 10-page review of Koffka's (1935) text, Boring expressed frustration in the writer's willingness to discuss immediate experience without naming it. "Has he not perhaps repressed immediate experience because he hates positivism, therefore admitting it only in disguise?" (Boring, 1936, p. 66). The apparent size experiments were conceived shortly after the publication of this passionate review; for Boring, an exploration of immediate experience, especially as it pertained to size constancy, proved irresistible.

Boring and Holway expanded their work by developing elaborate contraptions to study the "moon illusion," the perception that the moon is large on the horizon and small in culmination (Boring & Holway, 1940; Holway & Boring, 1940c). They found that the illusion is reversed when the observer is supine and must therefore depend on the angle of regard. This conclusion was subsequently challenged (Kaufman & Rock, 1962) and then reaffirmed (Suzuki, 2007), but with the exception of a few summative reports, Boring had little more to add to the story. Although one article, "The Moon Illusion and the Angle of Regard" (Holway & Boring, 1940c), concludes with a promising description of how the authors planned to test the illusion while observers viewed the sun, Boring never published the findings or returned to the lab with so much determination. World war, once again, pulled him toward more practical matters, and he assumed more teaching responsibilities as Harvard's junior faculty answered the call to service. Warfare favored applied research, and December 6, 1941, was "the last day when pure scholarship could be undertaken with a clear conscience" (Boring, 1961, p. 60).

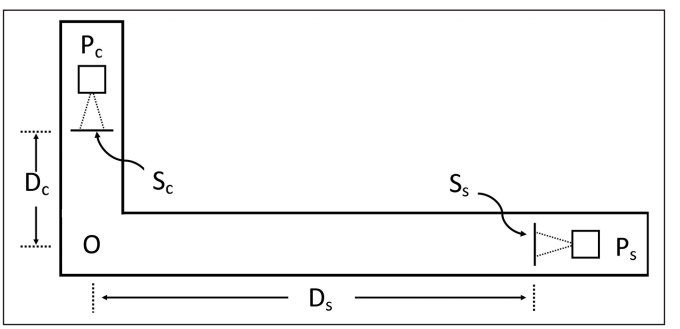

FIGURE 1. Illustration from Holway and Boring (1941) showing the corridor plan. Observer, O, is located at the intersection of two corridors. S_C indicates the position of the screen displaying the comparison stimulus located at a constant distance (D_C = 10′) from O. S_S, at a distance D_S from O, indicates one of the positions occupied by the standard stimulus, which always subtended a visual angle of 1°. Distance from O to the standard was varied from 10′ to 120′. P_C and P_S indicate the positions of the projectors

Conclusion

Throughout his career, Edwin Garrigues Boring struggled to redefine his commitment to a figure and a school of psychology that, in the eyes of many, had been abandoned. True to *AJP*'s editorial mandate of 1895 (Hall et al., 1895), Boring's manuscripts avoid deference without being indifferent. My intent has been to show how "the historical approach to the understanding of scientific fact is what differentiates the scholar in science from the mere experimenter" (Boring, 1961, p. 3). Boring was no mere experimenter, and *AJP*'s various publication formats allowed him to display the "many polished facets [that] make [the] gem" (Stevens, 1968, p. 606). In 1917, the student was fascinated and pleased with his mastery of unusual apparatus and operating against a background of German physiology; in 1920, the recently decommissioned methodologist understood the excitement of the testing movement but exposed its theoretical flaws; in 1927 and, again, in 1958, the biographer, struggling to understand a beloved mentor, wrote and then rewrote an obituary; in 1940, the positivist experimenter seemed to understand Gestalt theory better than its practitioners and used data to challenge the school from within. Boring presented many facets, but each one reflected the historian, providing perspective and working to unify the discipline.

NOTES

Dedicated to David P. M. Northmore, who once asked, "Why don't we try to replicate the Boring experiment?"

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