

REORGANIZING THE EXPERIMENTALISTS: The Origins of the Society of Experimental Psychologists

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The “Experimentalists,” created by E. B. Titchener in 1904, experienced growing pains and other stresses in the 1920s, raising concerns about its future. Those concerns became acute with Titchener’s death in 1927. At the 1928 meeting, several “elders,” hoping to maintain the group’s vigorous advocacy of basic research in the positivist tradition, formed a reorganization committee of 5. The committee expanded to 15, 10 of whom met the following year to debate alternative plans for a new organization. The result was an honorary society for established researchers, the Society of Experimental Psychologists. E. G. Boring’s highly personalized 1938 history of the original Experimentalists understated the value of the research contributions made by group members and, by overstating the degree of Titchener’s influence, devalued the important contributions of others (e.g., Raymond Dodge).

One of E. B. Titchener’s notable, and controversial, initiatives was his 1904 creation of a group that came to be known simply as the “Experimentalists.” The group was composed of those devoted to the new psychology of the laboratory, mostly but not entirely from eastern universities, mostly but not exclusively interested in studying human mental processes and behavior, and they met every spring during Titchener’s lifetime (except for the war year of 1918). The location of the meetings varied, rotating among the laboratories of the leading members of the group. The group would convene for 2 full days; discuss research in progress in their home laboratories; validate their mutually held attitudes about the value of a positivist, data-based approach to psychology; exchange the latest academic gossip; and, occasionally, influence academic hiring practices. Attendance was by invitation only, with the primary invitation sent from the director of the year’s host laboratory to the director of the member laboratories. It was then up to the discretion of the invitee director to invite other members of his staff and perhaps one or two of his most promising graduate students (and I write “his” here because, as is well known, women were excluded from the group). Which laboratory directors to invite was left to the experimentalist hosting the event in a particular year, but invitations were often (but not always) prejudiced by Titchener’s opinions about who ought to be present and which topics ought to be discussed.

Titchener’s initial impetus for forming the group was his dissatisfaction with

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the American Psychological Association (APA). Among other things, he disliked the topical diversity of the APA meetings, which included discussions and presentations that strayed from his narrow definition of psychology's subject matter (Goodwin, 1985). For Titchener, at least in the early years of his career, psychology was the experimental analysis of adult human conscious experience, accomplished through precise laboratory procedures and accompanied by a rigorous form of systematic experimental introspection. Other topics (e.g., child study, abnormal psychology, comparative psychology) might be interesting and worthwhile, but they were not "true" experimental psychology, in Titchener's view. One initial purpose of the Experimentalists, then, was to promote Titchener's values about the nature of experimental psychology. Titchener also disliked the format of the annual APA meetings—he preferred informal discussions of research rather than formal presentations, arguing that the description of an experiment was "hardly possible to follow intelligently. . . when method and results are thrown into lecture form and the lecture reduced to a compass of 20 min" (Titchener, 1896, pp. 448–449).

The story of the origins and early years of Titchener's group has been told before (Boring, 1938, 1967; Furumoto, 1988; Goodwin, 1985). Less well known is how the group began to change and came very close to dissolving in the 1920s, during Titchener's lifetime, and how it transformed itself into the Society of Experimental Psychologists (SEP) after Titchener's death in 1927. In what follows, I examine the problems faced by the group in the 1920s and the transformation of the informal group of the Experimentalists into the more formal and honorary organization of the SEP, and I look briefly at the early years of the new group. I also discuss a history of the group that E. G. Boring wrote in 1938 to mark the 10th anniversary of the SEP's creation. Much of the subsequent textbook description of the original Experimentalists shows the imprint of this highly personalized 1938 history (along with a similar paper Boring published in 1967), which was influenced to an important degree by Titchener's persistent effect on Boring. Examining correspondence between Boring and his peers and his writing of this history provides a glimpse into the mind of Boring-as-historian and raises some questions about the traditional story of the Experimentalists. Titchener was certainly the prime mover, but by focusing on Titchener's persona and his alleged domination of the group, Boring's account overlooked the contributions of other key experimental psychologists (e.g., Raymond Dodge) and failed to highlight the important role played by the group in promoting basic research and their ideal of a positivist science, especially during the post-World War I era, a time when experimental psychologists in America perceived that their ideals were increasingly threatened by the vigorous growth of applied psychology (Capshew, 1999).

Troubles for the Experimentalists in the 1920s

Even before Titchener's death in the summer of 1927 forced an examination of the nature of the Experimentalists, the group's future was becoming increasingly unclear. For one thing, in the early 1920s, the APA tried to duplicate the concept of the informal meetings of the Experimentalists by creating so-called Round Tables for discussion of experimental psychology at the annual meetings.

When he first learned of these sessions, an alarmed Titchener wrote to Boring about what he called a

threatening complication about the Experimentalists. Dodge has decided to try to imitate us, by inaugurating a sort of round-table experimental informal conference, at the Assn. Meetings. He asked me, very politely, whether his plan would interfere with my group; I told him . . . the Assn. had not been consulted when I started, and there was no reason now why I should be consulted by them

I think, however, that we shall presently be snuffed out. We are an arbitrary and one-sexed lot; and the Assn. will give room to anybody who is a member and wants to attend, and will let women in. . . . All the people whom we have offended will therefore work hard for the success of the venture; and we have offended a good many.

What I am now pondering is whether we had better dissolve while our reputation is high, or try to run along parallel with the Assn. and see what we can make of it. (Titchener, 1923, as quoted in Boring, 1923)

What Titchener did not know at the time of his letter was that the impetus for the APA-sponsored Round Tables came from within his inner circle of Experimentalists. The idea evolved out of informal discussions “in the corridors” (Boring, 1924) at the 1922 meeting of the APA at Harvard. Boring was a prime mover, along with fellow Experimentalists Karl Dallenbach and Samuel Fernberger, but the eminent Raymond Dodge was recruited to organize the sessions (Goodwin, 1990). At a time when applied psychology was becoming a force in American psychology, the Round Tables were part of a deliberate strategy among those in power in the APA to reestablish the primacy of basic laboratory psychology.

The APA-sponsored Round Tables appeared at the APA’s annual meetings for six consecutive years in the 1920s. The first one, held in 1923 at the APA meeting in Wisconsin, attracted 35–45 attendees and was considered a success. Titchener had little to fear, however, for the Round Tables quickly became much too large for the informality that he valued so highly. In 1924, two Round Tables for experimental psychology were held, with groups of 80 and 50; these numbers increased to 150 and 60 the following year; in 1926 the attendance information was not reported, but the Round Tables were described as being so big that they became “general meetings” rather than informal discussions (Langfeld, 1927, p. 313). The same was true in 1927 and 1928, before they collapsed of their own weight and were discontinued.

Although the Round Tables themselves did not seriously threaten the Experimentalists in the 1920s, the problem with the Round Tables—size—began to be a problem for the spring meetings. Just prior to the 1925 meeting at Princeton, for example, Titchener wrote to Herbert Langfeld of Princeton that the list of invitees looked “terribly long” and that “we continually tend to get too popular and too large” (Titchener, 1925). In his history of the group, in reference to the Princeton meeting, Boring (1938) noted that the group was becoming “unwieldy” and observed that a group photo of the meeting had 40 men in it. Although the number declined to 32 the following year at the University of Pennsylvania, the 1927 meeting at Harvard attracted 58 people, 20 from the host institution alone. This

prompted Titchener to grouse that Harvard always brought “a maximum of men and a minimum of research to the meetings” (quoted in Boring, 1938, p. 416). A large part of the increase was a tendency in the 1920s for most of the graduate students at the host institution to come to the sessions. For Titchener, bringing promising young apprentices into the guild was always an important element of the Experimentalists, but the meetings were getting out of hand.

Boring wrote to Dodge that he was “quite sure that Titchener went away depressed” from the 1927 Harvard meeting and wondered if the Experimentalists had a future (Boring, 1927). He went on to say that he had discussed with Titchener an idea that Titchener had always rejected in the past. As Boring put it, “I discussed with him the difficulties of these meetings and renewed a suggestion of some years ago that it might be best if we all went on to Cornell every year. When I had suggested this before, he emphatically rejected it. This time, however, he said it had occurred to him that, if the Experimentalists ‘broke up this year,’ it might be best to do that very thing—to have certain of us come to Ithaca every spring” (Boring, 1927).

Somewhat paradoxically, although size was beginning to change the nature of the meetings and to call their future existence into question, another problem that was becoming increasingly salient was a narrowing of the group’s unofficial membership, resulting from the cumulative ill will caused by Titchener’s personal idiosyncrasies and his attempts, persistent but not always successful, to control the invitation list. Consider this paragraph from a letter that Boring wrote to Fernberger in January of 1926, leading up to the Experimentalists meeting to be hosted by Fernberger that year at Penn.

Dall writes that he understands that Titchener is not going to invite him to the Experimentalists this year. I suppose this is technically all right, where you invite laboratories and the senior man invites within the laboratory But I am inwardly raging about Dall, since I do not think Titchener’s sentiment in this case represents the sentiment of the group. . . . It is getting yearly more difficult to have this meeting, because T’s personal animosities exclude so many. . . . I do not think I am entirely ready to break with T yet, and I certainly should if I withdraw from your meeting on account of Dall. Would the Experimentalists still go on with Harvard out, do you think? At any rate my patience will not last much longer. (Boring, 1926)

Boring was upset because Titchener apparently was not going to invite his Cornell colleague Karl Dallenbach to the Penn meeting. Following normal procedure, Titchener, as head of the Cornell lab, had received the main invitation from Fernberger. Dallenbach had been one of Titchener’s students and was a close colleague of both Boring and Fernberger. But Titchener was in the middle of a fight with Dallenbach over control of the *American Journal of Psychology* and evidently did not wish to be in the same room with his former student and current faculty colleague (see Evans & Cohen, 1987, pp. 339–353, for details of the controversy over the journal). Fernberger resolved this particular incident with a personal invitation to Dallenbach, but the incident was symptomatic of a larger problem having to do with the strict code of behavior that Titchener expected in others and his attempts to control the invitations to the spring meetings, even

when he was not the host. It is likely that the Experimentalists were heading for a crisis, even if Titchener had not died of a massive brain tumor in the summer after the Harvard meeting of 1927.

Titchener's demise forced the issue of what to do with the group, however. A regular meeting, scheduled prior to Titchener's death, occurred the following year (1928) at Yale, with Raymond Dodge as the host. Dodge was one of the group's veterans, a leader in the creation of the APA Round Tables mentioned earlier, and arguably one of the three or four most prominent experimental psychologists in America in the 1920s. The Yale meeting was again too large, and at the annual dinner, a group of about 20 "elders" decided that the time had come to disband the Experimentalists as it was constituted at the time and form a new organization. The group charged five senior members (Howard Warren and Langfeld of Princeton, NJ; Boring of Harvard; and Dodge and Robert Yerkes of Yale) with the responsibility to create a new organization. Two alternative plans emerged; described in an undated memo written by Boring, the plans were as follows:

- (1) Dallenbach's plan for an American experimental academy, an honor society of the seniors, small enough to hold meetings which would be mutually helpful, competent to be a stimulus to younger men. Limited to life membership. KMD suggested 50.
- (2) A homogeneous eastern society small enough for mutual aid. The modern equivalent of the original Experimentalists. (a) To get size right, it would probably be necessary to exclude the younger men, perhaps all graduate students. (b) To get homogeneity, it would be necessary to exclude personalities that do not lead to harmonious free discussion. (Boring, n.d.)

The committee of five did not feel competent to decide between the plans. What they decided instead was to form a larger committee, an "organizing nucleus," as Boring called it (Boring, n.d.). Members would be "distinguished experimentalists" and have "homogeneous personalities" (Boring, n.d.)—in short, a group that would share the scientific values of the committee of five. Starting with a list of 32 names, they selected 10 (Madison Bentley, Edward Bott, Dallenbach, Fernberger, Walter Hunter, Karl Lashley, Joseph Peterson, Walter Miles, Edward Robinson, and Robert Woodworth). Most were from the east, but Bott represented Canada, Peterson the South, and Miles the West). This produced a group of 15, charged with deciding the fate of the Experimentalists. One final point on the process of forming the organizing nucleus of 15 is worth mentioning—some of the reasons listed (by Boring) for excluding specific individuals reflect the group's desire to emphasize congeniality and shared Experimentalist values (i.e., Boring's "homogeneity") rather than an attempt at representation. Here are some of Boring's comments about people considered for the organizing nucleus, but rejected:

- [Walter] Pillsbury, no longer effective [as a scientist]
- [Knight] Dunlap, not a homogeneous personality
- [Albert] Poffenberger, infected by Columbia and biased toward applied psychology
- [Roswell] Angier, not yet recovered from being a dean. (Boring, n.d.)

And with reference to the possibility of adding Margaret Washburn, prominent enough at that point in her career to have been APA president (1921), to the organizing nucleus, Boring wrote: "It was decided to omit women as of doubtful homogeneity" (Boring, n.d.). Boring also objected to but failed to effect the exclusion of Bott, Peterson, and Miles. Because he wanted an eastern group, Boring thought inclusion of these three in the organizing nucleus would de facto eliminate Plan 2, which he clearly preferred.

The Reorganization Meeting

The organizing group met the following year (1929) at Princeton to decide between Plan 1, Dallenbach's academy plan, and Plan 2, Boring's idea of continuing the traditional Experimentalists concept. Ten of the 15 made it to the meeting, held in April of 1929. One of the important absentees was Boring, who was ill but nonetheless able to write a highly detailed four-page letter to Warren, the host, outlining his preferences for the new group. He opposed the idea of an honorary "Academy" (Plan 1), arguing that the fundamental goal should be to advance research, not create artificial tributes. That meant informal discussions of work in progress, as in the old Experimentalists meetings. To deal with the critical problem of group size, Boring suggested keeping the meetings on the east coast and limiting membership to senior researchers who had to be elected to the group. He also recommended a limitation on subject matter that reflected the lingering effect of Titchener's influence on him and his well-documented campaign in the 1920s to promote traditional laboratory experimental psychology and protect it from being diluted by what he considered extraneous interests (O'Donnell, 1979). As Boring wrote to Warren:

The question arises as to how "experimental psychology" should be defined in 1929. The term is perhaps unfortunate, since its use irritates experimentalists of other kinds. It seems to me, however, that we are still in a position to eliminate all mental tests, all applied psychology, all abnormal psychology, all the psychology of personality, all social psychology—no matter how experimental the technique in these subject-matters may be. The fact is that they do not yield sufficient community. (Boring, 1929)

By "community," Boring was referring again to the concept of homogeneity—social and abnormal psychologists might use experimental methodology, but their topics of interest were so far removed from traditional experimental psychology that fruitful discussions could not occur in a small community of members.

In a break with Titchenerian tradition, however, Boring recommended that membership be extended to those interested in animal research, but only if the research was limited to traditional experimental topics, such as perception and learning. He admitted that he was not too sure about this particular recommendation, but he also was aware that Yerkes' prominence in the reorganization group practically guaranteed the inclusion of the animal researchers.

It cannot be known how Boring's absence influenced the dynamics of the 1929 reorganization meeting. The final plan, outlined in a simple two-page set of bylaws, included some elements of the vision that he outlined in his letter to

Warren (e.g., meeting informality), but its basic structure was that of an honorary group—the Dallenbach plan. The group named itself the Society of Experimental Psychologists, with a maximum of 50 members and new members to be elected by unanimous vote of those attending the annual spring meetings. The traditional nature of the meetings was maintained in Article 2, which defined the object of the society: It would “advance psychology by arranging informal conferences on experimental methodology” (Boring, 1938, p. 422). It is not clear why the term “methodology” was used, but a clue comes from a letter from Warren to Woodworth, describing the meeting (like Boring, Woodworth did not attend). Warren wrote that the “scope is to be broadened so as to include animal and child psychology and anything which can be regarded as methodological material” (Warren, 1929). This suggests a range of topics broader than Boring would have liked and the restriction placed on methodology (i.e., experimental) rather than subject matter. The meaning might have remained somewhat ambiguous to SEP members, however, because the phrase “experimental methodology” was eventually replaced in the bylaws with “experimental psychology.”

In addition to the 15 members of the reorganization committee, 11 other experimental psychologists were elected to membership, making a total of 26 members of the new SEP. After some debate, it was decided to refer to all 26 as “charter” members. Walter Miles, known for keeping detailed notes of everything that happened at the meetings he attended, recorded the tally of votes for all those considered for membership at the pivotal 1929 meeting (Miles, 1929). Table 1 lists the 15 people of the organizing nucleus, with the superscript *a* identifying those 10 who made it to the 1929 meeting. The table also lists the 11 people elected to membership at that meeting and 14 other people who were actively considered but not elected (along with the vote tallies, as recorded by Miles).¹ In a marginal notation to his list, Miles wrote down four items that were informal criteria for inclusion (it is not clear if this was part of a general discussion or Miles’ personal criteria, but the connection with Boring’s concept of homogeneity is clear). The criteria noted by Miles were as follows:

1. How frequently would they come
2. How active are they
3. Those who would always come and talk too much
4. How harmonious is the man. (Miles, 1929)

The Society of Experimental Psychologists

The new SEP was similar to the old Experimentalists in some ways but markedly different in others. The concept of an informal spring meeting that

¹ The list of those considered or “suggested” for membership included 87 names. Voting occurred only for those 25 individuals who were either elected ($n = 11$) or not elected ($n = 14$). Miles did not explain why the remaining 62 were not voted on, but time was probably a factor. Miles noted on the list of names, just above his handwritten table of votes for those not elected, “This was as far as the voting proceeded” (Miles, 1929).

Table 1
The Society of Experimental Psychologists (SEP) Reorganization Meeting of April 1929 at Princeton University

The 15-person organizing committee, all charter members of SEP ^a		
Madison Bentley ^a	Samuel Fernberger ^a	Joseph Peterson ^a
E. G. Boring	Walter Hunter	Edward Robinson ^a
Edward Bott	Herbert Langfeld ^a	Howard Warren ^a
Karl Dallenbach ^a	Karl Lashley	Robert Woodworth
Raymond Dodge ^a	Walter Miles ^a	Robert Yerkes ^a
The 11 people elected in 1929, making 26 charter members of SEP		
Warner Brown	Knight Dunlap	Calvin Stone
Harvey Carr	Harry Johnson	Margaret Washburn
Percy Cobb	Kurt Koffka	Harry Weld
June Downey	Carl Seashore	
The 14 additional people considered and voted on in 1929 but not elected to membership ^b		
John Coover (3)	Walter Pillsbury (8) ^b	Edward Tolman (6) ^b
Elmer Culler (3) ^b	Albert Poffenberger (9) ^b	Carl Warden (2)
S. I. Franz (7) ^b	Christian Ruckmick (3)	Albert Weiss (7)
Harry Hollingworth (2) ^b	John Shepard (5) ^b	Herbert Woodrow (3) ^b
Clark Hull (7) ^b	Edward Thorndike (5)	

^a Those of the organizing committee ($N = 10$) who were present at the 1929 reorganization meeting. These 10 then voted in the additional 11 "charter" members and rejected the 14 psychologists in the bottom section of the table.

^b These individuals, although not elected as charter members of SEP, were eventually elected to membership. The number in parentheses indicates the number of votes they received at the 1929 meeting. To be elected required a unanimous vote, in this case the 10 members at the Princeton meeting.

would focus on research in progress and free-flowing discussion was maintained, and the group was kept relatively small. Membership requirements became formalized, however, and the practice of bringing promising graduate students to the meetings was dropped. This latter change would have deeply troubled Titchener, of course, who considered the inclusion of graduate students to be an essential indoctrinating device. There was no mention of a minimum age for inclusion, but the election process had the effect of limiting membership to those who had already established themselves. Incidentally, this de facto elimination of younger experimental psychologists did not sit well with them, and by 1936 they had formed their own group, which deliberately excluded *older* experimental psychologists. This Psychological Round Table, or PRT, has been described in detail by Benjamin (1977) and Hardcastle (2000). Interestingly enough, the idea of a youthful rebellion was considered by Boring during the SEP reorganization process. When the committee of 15 was just beginning its work, word about it got around to others and Boring wrote to Dallenbach that

Our younger men were regretful, . . . and they were also somewhat resentful because they sensed something mysterious in the air that they were not told about.

I still think, however, that there are too many young men for this thing to revolve around their interests; modern youth can look after itself, and if the younger men want a meeting they had better have it and keep the old fogies like you and me out of it. (Boring, 1928)

Perhaps the biggest change from the Experimentalists to the SEP was the inclusion of women into the group, at least in principle. I have been unable to find any correspondence that would shed light on the reason for the decision about women, but the commanding presence of Margaret Washburn must have made her impossible to exclude, and if she was in, then women were in. By 1929, Washburn was already one of American psychology's most prominent figures—the author of comparative psychology's most visible textbook, a past president of the APA, and one of the four principle editors of the *American Journal of Psychology* (Dallenbach, 1940). So Washburn and a second woman, June Downey of Wyoming, were included among the 26 charter members of the SEP, elected at the reorganization meeting of 1929. Three other women, Eleanor Gamble of Wellesley (PhD, 1898, Cornell, student of Titchener), Kate Gordon of UCLA (PhD, 1903, Chicago, student of John Dewey), and Gertrude Rand of Johns Hopkins School of Medicine and the Wilmer Ophthalmological Institute (PhD, Bryn Mawr, 1911, student of James Leuba) were on the list that Miles compiled of 87 possible SEP members. They were among the 62 research psychologists who were on the list but not voted upon at the 1929 meeting. None of the three were subsequently elected to membership.²

Washburn was active in the SEP in the early 1930s, before suffering a series of strokes that eventually took her life in 1939, and she even hosted a meeting at Vassar in 1931. Downey, known for her work on handwriting analysis and the development of a temperament test (Anderson, 1933), died in 1932 at the age of 57 and was never able to attend a meeting. Despite the addition of Washburn and Downey to the list of charter members, women remained on the edge of the organization, at least through the 1970s (Furumoto, 1988). For instance, the next woman to be elected, after Washburn and Downey, was Eleanor Gibson in 1958.³

The first meeting of the new SEP was held, fittingly, at Cornell in 1930 and was hosted (or “chaired” as they began to say) by Bentley. Washburn chaired the second one at Vassar. For the first half dozen meetings the number attending never exceeded 20, thus allowing for the informality and easy exchange of progress reports on research that Boring hoped would continue. An insider look at some of these meetings comes from the diaries of Walter Miles, who attended several during these years. At the 1931 meeting at Vassar, for instance, his diary entries resemble those that he recorded of some of the meetings during the Titchener years—a sequence of informal discussions of the research underway in various

² Two other prominent women, Mary Calkins and Christine Ladd-Franklin, were not considered for membership in the new SEP. Calkins had long since shifted her interests to philosophy, and Ladd-Franklin (in 1914, the only woman ever to make a presentation to the original Experimentalist group during Titchener's lifetime; see Furumoto, 1988) had not been active in experimental psychology for some time. Both died in 1930.

³ There were just under 30 women in the SEP in 2001, out of a total membership of about 200. Of the current women members, half were elected in the decade of the 1990s.

labs, accompanied by some rough sketches of apparatus. Topics at the Vassar meeting ranged from Dallenbach's work on paradoxical cold to J. F. Dashiell's on maze learning to Miles' study on age differences in reaction time (Miles, 1931).⁴

The preserved tradition of informality in discussing research was accompanied by a new formality in other respects, however. Minutes of the meetings were circulated to members by Fernberger, the group's new secretary. Fernberger also sent a series of notices to members about the new membership nomination process. After an initial call for nominations, he would distribute a list of potential new members, along with the names of current members who made the specific nominations. The suitability of nominees was debated or, in Boring's words, "hotly contested" (Boring, 1938, p. 419). A nominee had to receive the unanimous vote of those present at a meeting in order to join the group. And growth was slow in the early years—three new members in 1930, five the following year, and three for each of the next 3 years (Boring, 1938). It took 10 years to get close to their maximum membership of 50. Another formal process was added in 1935 with the creation of an annual award for outstanding work in experimental psychology—this "Warren Medal" was named for Princeton's Howard Warren, one of the original reorganizing group of five and a regular contributor to the experimentalists during Titchener's years.⁵

The Titchenerian Eponym: Boring's 1938 History of the Group

At the 1937 meeting, the group decided that the upcoming 10th anniversary of the reorganization would be a good time to prepare a history of the SEP, so it formed a history committee for that purpose. Fernberger chaired the committee, which also included Dallenbach, Miles, Langfeld, and Boring. After his colleagues deferred to Boring as the leader of the project, he set out to organize and write the article that eventually appeared in the *American Journal of Psychology* in 1938. As mentioned earlier, this article, with its highly personalized recollections of the meetings during Titchener's time, became the basis for the standard portrait of the original Experimentalists. In this last section of the article, I examine briefly the writing of this history and how Boring's strong beliefs and his unique relationship with Titchener created a portrayal that to some extent distorted our subsequent understanding of the original Experimentalists.

Careful researcher that he was, Boring first set out to gather as much information about the early meetings as he could. This was a difficult task, of

⁴ Miles also included a revealing description of Washburn's feelings about Titchener. According to the diary entry, Washburn reported this about her dissertation director:

He never discovered anything, he did not create any apparatus, or methods, he was simply not an experimenter . . . His great points were his learning and his ability to give a lecture. The theses which came from Cornell were doing over the things of Hering and others. Miss W. was his first grad student; he gave her but little according to her; they were too near the same age. (Miles, 1931)

⁵ The naming of the award had little to do with Warren's status as a scientist; although he was a loyal member of the Experimentalists and respected by his peers as a competent researcher, his research contributions alone would not have won him the award. It became the "Warren" Medal upon the urging of Dallenbach, and when Warren's wife donated money for the award shortly after Warren's death in 1934.

course, because there was virtually no written record of the group. Descriptions of the contents of the meetings had been published, but only sporadically, and they usually consisted of no more than a brief paragraph in the *American Journal of Psychology* (Goodwin, 1985). Furthermore, the absence of programs or lists of papers made it impossible for Boring to reconstruct the exact topics discussed at particular meetings.

Boring had attended each of the meetings since 1911, so he had a vast store of personal recollection but no first-hand knowledge of the first meetings, those between 1904 and 1910. To accumulate information, he launched a prolific (even by his standards) letter-writing campaign to solicit information from experimentalists who might have been to at least one meeting. When examining this correspondence, it is hard not to appreciate Boring's persistence in trying to track down reliable information. The major difficulty, of course, was that his correspondents were relying on their memories and perhaps some sketchy notes.⁶ For someone who had been to 10 meetings, for instance, and these meetings tended to be similar to each other, it would be virtually impossible to be certain about the details for any particular meeting unless something out of the ordinary happened. One example of this memory problem came from Fernberger, who recounted in great detail a dinner conversation with Hugo Münsterberg at the 1917 meeting at Harvard. Boring wrote back, "It is odd how memory slips" (Boring, 1937) and gently reminded his friend that Münsterberg had died several months before the 1917 meeting.

From what we now know about the vicissitudes of memory, it is not surprising that Boring had to deal with a number of these slips of memories and with numerous contradictions among his correspondents. He was never completely certain about who attended the first meeting in 1904, for instance. Nonetheless, he exercised great care in trying to verify details and omitted information or anecdotes if he could not get independent verification. For example, unable to verify a rumor that Münsterberg had brought a woman to the 1908 meeting, he left it out of the final draft. In short, he did as well as can be expected with the information he was able to collect. More interesting than the details, however, was the overall tone of the history, and it is here that Boring's relationship with Titchener undoubtedly influenced the narrative. A letter from Fernberger reminded Boring that "our picture is colored by our intimacy with T (yours more than mine, of course because of the greater intimacy)" (Fernberger, 1937). Boring was certainly aware of the problem and tried to guard against it, but influence operates at many levels, not all of them conscious.

Boring completed a draft of the history in June of 1937 and sent it to the history committee and several other veterans of the Experimentalists' meetings. There was universal praise and some minor corrections of fact and some

⁶ At one point in the process, Fernberger wrote to Boring, apologizing for not being able to contribute any hard data to the project ("I have nothing in my files") but suggesting that the notoriously compulsive Miles might have kept notes in his "famous little black notebook" (Fernberger, 1937). Miles was well known for taking copious notes at meetings. He did indeed have some entries for Experimentalists meetings during the Titchener era, but they were not very elaborate, perhaps a consequence of the culture of informality (Goodwin, 2003).

disagreement about who should count as a charter member of the reorganized SEP, but two correspondents had a more substantive concern. Both Dallenbach and Woodworth thought that Boring had placed too great an emphasis on Titchener's personal influence on the meetings, and both argued for a revision that placed greater weight on the scientific value of the meetings, the contributions and importance of others in shaping the group (especially Dodge), and the manner in which the meetings helped to develop specific research programs at various universities. Boring agreed that untold numbers of ideas for research came from the meetings but argued that the absence of any permanent record made it impossible to show exactly which important research had its roots in the sessions. Boring also believed that both Dallenbach (especially) and Woodworth had somewhat self-serving reasons for suggesting that Titchener's influence be downplayed, and Boring made few changes to the final draft. What changes he did make had to do more with streamlining the document than altering the centrality of Titchener to it, however. For instance, he removed a long discussion over the uncertainty of who went to the first meeting and which laboratories were represented. In the final version, however, it was very clear that Boring was describing a group whose dominating presence was Titchener.

Thus, when students learn about the Experimentalists, they typically learn about them as "Titchener's Experimentalists," they learn that Titchener controlled the lists of invitees and limited the topics of discussion, and they learn that this group had the effect of perpetuating Titchener's vision of experimental psychology. This description derives largely from Boring's 1938 history, and there is some truth to it, but it is only a part of the story. Although a good argument can be made that Titchener was the group's central force, others had similar degrees of influence, with Raymond Dodge being the most obvious example. Yes, Titchener often tried to influence the invitations, but his advice was also ignored on some occasions (by Dodge, for instance). Yes, Titchener tried to limit the discussion topics, but he sometimes failed to do so. Animal research kept creeping into the discussions, for example, and the 1913 meeting (hosted by Dodge) included a discussion of mental tests and a talk by Münsterberg on mind reading. Correspondence among Boring, Dodge, Fernberger, and Dallenbach often make reference to the importance of Dodge to the group. For example, after the 1927 meeting at Harvard, Boring wrote to Dodge, effusively praising him as "the tower of strength of the meetings, . . . [one who] breathed the smoke of competence upon the meeting" (Boring, 1927). In the early 1920s, when Boring, Fernberger, and Dallenbach organized the Round Table discussions on experimental psychology for APA, they deferred to Dodge as the organizer on the grounds that he had enough stature as a regular member of the Experimentalists that Titchener would not feel threatened (as we have seen, they were wrong about Titchener's reaction, but their rationale derived from the importance of Dodge to the Experimentalists' spring meetings). Hence, there is an element of truth to the standard description of the original Experimentalists, as there is truth in all myth, but the extent of Titchener's influence was rather exaggerated by Boring's personalistic accounts of the pre-1929 meetings.

The larger effect of Boring's decision to personalize his 1938 history was that the perceived impact of the Experimentalists, and the SEP in its early years, has been somewhat diminished in modern textbook accounts of the group. American psychology in the 1920s and 1930s was becoming increasingly diverse, and the Experimentalists and the SEP, while small in number, played a disproportionately large role as the "protectors" of a research tradition that was grounded in positivism and an unshakable faith in the value of basic laboratory research. At a time when American psychologists were becoming increasingly interested in applying psychology's principles, and increasingly enmeshed in theoretical squabbles among advocates of various "systems" of psychology (e.g., Woodworth, 1931), the Experimentalists and the SEP "kept the faith" and served as a constant reminder that psychology ought to be a discipline whose principles derived from laboratory-based evidence

Postscript

The SEP grew slowly but steadily after its creation in 1929 and has a current membership of about 200. The annual meeting remains fairly small (around 30–40), and it is mostly an honorary society. In 1985, concerns over the future of the group (its membership was aging rapidly) prompted a survey of the membership, but no consensus emerged, except to no longer differentiate between a member of the SEP and a fellow of the group (Boynton, 2001). As befits the 21st century, the society maintains a Web site (www.sepsych.org).⁷

In the years after the 1929 reorganization of the Experimentalists into the SEP, experimental psychologists have occasionally been moved by the same spirit that initially motivated Titchener in 1904—feelings of unease and a desire to revolt whenever it seemed to experimentalists that the APA was drifting away from its focus on psychological science. After World War II, for example, experimental psychologists acted on two separate occasions to form new organizations to redress perceived imbalances and promote the values of psychological science—the Psychonomic Society in 1959 and the American Psychological Society in 1988. On both occasions, members of SEP played leading roles in forming the new groups.

⁷ The site includes a brief history that adds a bit more longevity to the official organization than is perhaps warranted. The history begins: "The first meeting of the *Society of Experimental Psychologists*—then called '*The Experimental Psychologists*,' or '*The Experimentalists*' for short—was held at Cornell University in Ithaca, NY, on April 4-5, 1904" (www.sepsych.org/history.htm). The group has not always had a good sense of its history. Boring was at the 1954 meeting at Cornell and went away concerned that, as he wrote in a letter to Leo Postman (well known memory researcher and secretary/treasurer of the SEP at the time), "The SEP has not been much interested in its history The fiftieth meeting . . . [was] at Cornell where the first meeting had been held. I attended . . . and heard no mention of it being the semi-centennial and no mention of Titchener" (quoted in Boynton, 2001, p. 2). More recently, however, the 2004 centennial meeting, also at Cornell, opened with a historical talk by James Cutting of Cornell, ended with a talk on the group's Warren Medal by David Baker of the Archives of the History of American Psychology, and included a reception featuring memorabilia about Titchener and the Experimentalists.

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