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American Psychology in the 1920s*

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# *James McKeen Cattell and American Psychology in the 1920s*

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## **Introduction**

As James McKeen Cattell concluded his 1925 presidential address before the American Association for the Advancement of Science, "Some Psychological Experiments,"<sup>1</sup> he casually mentioned that "the experiments used in this address as illustrations of work in psychology may seem like an autobiographical obituary notice."<sup>2</sup> And like an obituary, much of the talk focused on the past. In it, Cattell raised various issues that vitally concerned his generation of psychologists at the turn of the century—arguing, for example, that psychology could be as experimentally quantitative as any physical science.<sup>3</sup> Also, most of the experiments discussed dated from before 1900, as Cattell reviewed his own work first published in the 1890s<sup>4</sup> and that of some of his students from the same period.<sup>5</sup> The reasons for Cattell's concern with the past are clear.

Cattell was the first psychologist elected president of AAAS, but he was not elected as a student of what are now called the behavioral sciences. From at least 1900 he was best known in the American scientific community as the editor of *Science*, the official weekly of the association and probably the country's most important scientific journal. He had purchased it late in 1894, and from 4 January 1895, when he issued the first number of its "new series" with the cooperation of the editorial committee, composed of outstanding members of this community, the affairs of *Science*, and of the various other journals that he edited, took more and more time away from his psychological activities.<sup>6</sup> At the same time, the weaknesses of his early program for mental testing, to which he had devoted a good deal of himself for over a decade, gradually became clear.<sup>7</sup> As early as 1905, Cattell's day-to-day interests were not those of the typical American research



psychologist, who continued to experiment regularly. Also, in 1917, three years before he turned sixty, he was dismissed from Columbia University in what soon became a celebrated academic freedom case,<sup>8</sup> and so lost his institutional base in academia. He continued to participate in the activities of the American psychological community throughout his life, despite all the setbacks relating to his ideas and affiliation, but clearly by 1925, and probably much earlier, he did not keep up with the latest experimental work in psychology. Of course, the phenomenon of the elder statesman of a science losing touch with the work of his younger colleagues is not unknown, but in Cattell's case, as noted, there were many reasons for this beyond the usual course of events. No wonder, then, that the experiments discussed in his presidential address were at least twenty-five years old.

All of this is not to say, however, that Cattell lost touch with the science of psychology in the United States, or with the community of psychologists. He participated actively in this community throughout the 1920s. For example, he personally arranged for the American Psychological Association to be incorporated in the District of Columbia in 1924,<sup>9</sup> and was elected by the American psychologists as president of the Ninth International Congress of Psychology, at Yale in 1929, the first ever held in this country.<sup>10</sup> Many of the issues he discussed in his AAAS Presidential address, though he referred to older experiments, were of as much concern in the 1920s as they were when Cattell first was appointed professor of psychology in 1889 and, interestingly enough, greatly concern psychologists today. Furthermore, a survey of how the psychologists of the 1920s approached many of these issues reveals an interesting pattern within the structure of the psychological profession, the ideas held by the psychologists, and the actual confidence that they had in these ideas. Cattell's presidential address, therefore, provides both direct and indirect access, though not the most obvious one, to the interests and concerns of American psychology in the 1920s, which is the general topic of this essay.<sup>11</sup>

The intellectual boundaries of the decade of interest here are not, of course, those of the chronological decade, 1920–1930. American psychology received a tremendous developmental impetus as a result of World War I, and the years that followed 1918 saw the growth and decay of at least three psychological movements—testing, industrial psychology, and the certification of consulting psychologists. The Ninth International Congress of 1929 marks a natural end to the period, and the years immediately before it saw the growth of large-scale support for child psychology and the beginnings of the migration of Gestalt psychology to the United States. In all, the period from 1918 through 1929 was an exciting one for American psychology, and historians can learn much by examining just what went on within the community of psychologists.

A number of conclusions emerge readily from such a study, and some may appropriately be suggested here, to clarify the direction that the rest of this essay will take. First, the psychologists of the 1920s shared many of the attitudes that characterized the vast majority of middle-class, native-born, white Anglo-Saxon Protestants. This group—and most psychologists were members of it<sup>12</sup>—shared a great deal of self-confidence, derived in part from America's economic success during the period and the country's major role in helping to "make the world safe

for democracy."<sup>13</sup> In the same way, the psychologists typically shared the xenophobic smugness of the period, well illustrated by Sinclair Lewis in *Babbitt*.<sup>14</sup> The fact that the psychologists of the 1920s, like most people, reflected the world in which they lived should surprise nobody. But apparently it has shocked several psychologists of the 1970s. One has complained that *Even the Rat Was White*,<sup>15</sup> while others seem to believe that the earlier psychologists, with their scientific "pretensions," should have transcended their milieu.<sup>16</sup> Whatever the merits of these charges in the 1970s, to read them back into the 1920s is ahistorical and distorts the past.

A second conclusion that emerges from a study of the 1920s is that psychologists of the period shared a confidence possibly not seen at any other time in their science's history. Even graduate students could feel that they worked in "exciting times," with the "journals bristling with controversy."<sup>17</sup> One reason for this confidence was, at least in the early years of the decade, the belief that psychology had done much to win the Great War. This belief was reinforced by the attention paid in the popular press and in such journals as *Harper's* and the *New Republic* to the psychology of the period, in all of its aspects. Scientific methods of child care, philosophical implications of behaviorism, and practical applications of mental testing were all regularly discussed in articles written for the literate layman.<sup>18</sup> Humorists wrote about "the outbreak of psychology" and of ways to avoid it.<sup>19</sup> With such attention, it is no wonder that psychologists thought well of what they were doing. Another reason for their confidence was the high academic status that many of their colleagues were reaching. For example, between 1919 and 1921, Northwestern, Yale, Cornell, and the University of North Carolina all appointed psychologists to their presidencies.<sup>20</sup> For many, this confidence also reflected a belief in the validity of the "school" with which large numbers of psychologists identified themselves. The behaviorists, whose ideas have been well discussed by others,<sup>21</sup> knew that they had the answer to all, or at least most, of the world's problems, and some of them believed so strongly in their approach that they regularly read a chapter from John B. Watson's latest book before going to bed each night.<sup>22</sup> The Gestalt psychologists, who became an important force in American psychology long before Hitler drove many German intellectuals off the continent, felt to some degree that they were missionaries called to save the scientific souls of the poor benighted Americans.<sup>23</sup>

But another important conclusion of this paper is that the importance of warring "schools" in the history of American psychology has been greatly exaggerated,<sup>24</sup> and that, in many ways, American psychologists of the 1920s formed a small, fairly tightknit community whose members were usually quite friendly with each other, whatever systematic disagreements they might have had. For example, one psychologist who later stressed the differences between *Contemporary Schools of Psychology*,<sup>25</sup> concluded in an overview of the systematic approaches of the last half of the decade that:

There is a curious contrast in present-day psychology between the mutual hostility of the several schools, on the one hand, and the solidarity of the group of psychologists, on the other. From the insistence of each school on the futile and

reprehensible tendencies of the others, you would scarcely expect to find them meeting in associations and congresses on a footing of mutual respect and interest, nor see them laboring together on abstract journals and the like; yet this cooperation is just what you find.<sup>26</sup>

This "mutual respect" and "cooperation" rested on, in large measure, what has been described as an "inborn eclecticism" shared by most American psychologists of the period.<sup>27</sup> This eclecticism played a major role in the early reception of Gestalt psychology in the United States. In all, American psychologists were clearly psychologists first, and only after that behaviorists, structuralists, or whatever.

The final major conclusion that comes out of this study of the 1920s is perhaps the most significant, but at the same time cannot be illustrated readily without reference to specific examples. Conversely, this conclusion is tied to a pattern that will emerge at several points in this essay, so that to connect it to one or another example would prevent its general importance from becoming clear. One way around this dilemma is to sketch this pattern here, and then, in the detailed discussions that follow, to indicate just how this pattern appears in the specific episode under consideration.

Specifically, this pattern saw the American psychologists of the 1920s approach one or another problem with extreme confidence, buoyed by the factors that had led to their faith in psychology in the first place. This confidence led, in some cases, to important scientific, theoretical advances, and to results of genuine practical applicability. But more importantly, this confidence went further, and thus the psychologists often vastly overstated their claims for the validity of their results and the usefulness of their work. These overstatements in turn led to exaggerated and unrealizable expectations of what psychology could do, on the part of the public, educators and other professionals, and often other scientists. Even before these expectations were created, many more critical psychologists, less involved in overstating the value of their work, attacked those who did claim that they could do more than they actually could. And the unfulfilled expectations often led to attacks from outside the community of psychologists as well. These attacks—both from within and without the discipline—led usually to disclaimers and sometimes public apologies by some psychologists, and portions of the field actually underwent what might be called scientific retrenchment. In any event, this pattern will become clear in the detailed analyses of this paper.

### Mental Testing

To many historians and psychologists, the 1920s are best known as the first period of large-scale, nonmilitary psychological testing, and this aspect of the psychology of the period has been well treated by several observers.<sup>28</sup> Cattell, of course, is best known in the history of psychology as one of the pioneers of psychological testing. At the 1923 meetings of the American Association for the Advancement of Science he claimed to have originated the terms "mental tests" and "individual differences."<sup>29</sup> The failure of these early tests has been referred to above, and in his AAAS presidential address Cattell spoke of the results he

obtained as "disappointing."<sup>30</sup> But around 1910, American psychologists became familiar with other tests, derived not from Cattell's work but from that of Alfred Binet, that gave more useful, if not more precisely defined results.<sup>31</sup> These tests were used primarily in schools and colleges, and many students were examined individually, mainly to segregate from the mass of students those who could not benefit from regular instruction, and those for whom exposure to advanced material would be useful.<sup>32</sup> By 1916, so many psychologists were devoting so much of their time to the use and development of tests that a number of them attempted to standardize them for all users,<sup>33</sup> and some progress was made toward organizing a journal devoted solely to such mental tests.<sup>34</sup>

However, psychology and its tests were not really well known in the United States until World War I, and it was the role that they played during the war that first brought them national attention. When America entered the War in 1917, the governing body of the American Psychological Association met to decide how the nation's psychologists could best come to the aid of their country.<sup>35</sup> The outcome of this meeting was multifaceted,<sup>36</sup> but perhaps the best known and probably most important results of these deliberations were to be the development of two standardized group tests of "intelligence," given to most recruits taken into the army. These were the Alpha test for men literate in English, and the Beta test for those literate only in another language, or totally illiterate. The results of these tests were quite useful to the army in the limited areas of selecting men for officer training and eliminating men unfit for various types of service, and Cattell quoted an estimate of the value of the tests at one billion dollars. He also said that the tests cost about fifty cents per man to administer, thus claiming that psychology gave to the war effort much more than it received.<sup>37</sup>

As a recent historian of intelligence testing in the United States put it, "psychology was on the brink of a boom that was to prove financially profitable. . . . The work on the army tests . . . was a powerful stimulant to intelligence testing and to psychology" in general. Right after the war, many more jobs for psychologists opened up than there were men and women to fill them. As one of the leading members of the psychological community wrote to another, who was looking for a young psychologist to fill a position, "I am afraid it will be difficult to find the right person. . . . Men with any substantial training in psychology are scarce and very much in demand."<sup>38</sup>

Psychological testing also boomed. The first heavy users of the group tests developed for the army were the schools and colleges, which found them more efficient than the individual tests. Even before the armistice, various colleges and even high schools approached psychologists connected with the army testing program about getting copies of the examinations, but due to their confidential nature, all such requests were discouraged during the war. The first official announcement of the tests made after the war, however, in *Science* in March 1919, stressed "the applicability of mental measurement" to education.<sup>39</sup> Only three weeks later, John B. Watson, then at Johns Hopkins, wrote to Robert M. Yerkes, who headed the team of psychologists who developed the Army tests, about the possibility of obtaining test forms for use in a school in Baltimore.<sup>40</sup> That fall similar tests were used as part of the entrance procedure at such colleges as

Columbia and the University of Rochester, and by April 1921 at least ten institutions of higher education were using the Army Alpha Test (and many others were using other sets of tests) for similar purposes.<sup>41</sup> Such tests were also used at the high school level. Various school officials—for example, B. M. Stigall of the Kansas City, Missouri, school system—apparently contacted prominent psychologists and Army officials involved with the earlier tests to inquire just how they could use the testing material then becoming available.<sup>42</sup> A distinguished psychologist, Carl E. Seashore of the University of Iowa, used his term as resident chairman of the Division of Anthropology and Psychology of the National Research Council in 1920–21 to travel around the country and urge educators to use such tests to section their student bodies by ability, and thereby give each student the education most suited to his or her capabilities.<sup>43</sup> Helen Thompson Woolley worked with the Cincinnati Public Schools during this period to establish a Vocational Guidance Bureau, using tests to direct students to special vocational and remedial classes, and in general to augment the guidance role that many public schools were then beginning to play.<sup>44</sup>

The aim of most of the educators in using these tests was at least twofold. They wanted both to tailor the education received by each student, insofar as possible, to his or her capacities, and to increase the efficiency of the educational process.<sup>45</sup> Both of these goals reflected the ideas of the progressive education movement, which remained an important force in American education long after John Dewey wrote *The School and Society* in 1899. The first aim derived from the movement's basic assumptions, and the second, at first seemingly antithetical to its goals, came from an application of the principles of scientific management to education. Standardized group tests quickly became an objective way to measure school and teacher efficiency and were soon installed in many public schools.<sup>46</sup>

On an individual basis, the use of these tests by educators probably proved beneficial to many of those students tested, by directing some to remedial classes, or by discovering in others abilities that had previously lay hidden and thus opening various educational possibilities. For example, in 1910, only 20 percent of all freshmen entering Princeton were graduates of public high schools, as opposed to Eastern preparatory academies. By 1935, this fraction had grown tremendously, primarily due to the use of examinations given by the College Entrance Examination Board, and the Scholastic Aptitude Test, first given in 1926, was purposely introduced for scholarship examinations so as not to bias the selection in favor of prep school graduates.<sup>47</sup> Cattell himself spoke of such tests as being "of untold value to our schools and to the children who are the ultimate origin and end of all our efforts," and Seashore spoke of "the measurement of talent" as one of the "central factors in this scientific movement," leading to "giving scientific advice in regard to the treatment of precocious children."<sup>48</sup> The use of such tests by psychologists and educators playing the role of counselor reflected the assumption that "psychology knows best," or, more generally, "science knows best," explicitly seen in Seashore's comments. In this respect, the educational testing movement of the 1920s reflected the open concern of many psychologists with "social control," an important aspect of the popular program of behaviorism, discussed elsewhere.<sup>49</sup> And when psychologists generalized the results of these



tests from individuals to groups, and extrapolated on the basis of various assumptions, this concern with "social control" became explicitly important.

The results of the tests that had the greatest immediate social implications indicated, quite simply, that whites scored higher than blacks, that native-born whites scored higher than immigrant whites, and that immigrants from northern and western European countries scored higher than those from southern and eastern Europe.<sup>30</sup> These facts were not disputed by anyone. What led to a great deal of controversy were the interpretations attached to these data. For example, it has been shown that educators used these results to justify ethnic segregation and mediocre education for Chicanos in southern California during this period,<sup>31</sup> thus providing a counterexample to the argument that testing often led to increased educational opportunity. But as the largest collection of data resulted from the army testing program, it was their interpretation that was most controversial.

In preparing their final report, Robert M. Yerkes and his fellow psychologists made at least three major assumptions: that their tests measured actual innate "intelligence"; that this intelligence was hereditary and was passed in some way from parent to child; and that those individuals from various social groups—e.g., Northern blacks; Southern blacks; Northern native-born whites; English-born immigrants; Italian-born immigrants—who were recruited into the army and tested, represented an actual sample of these groups in American society at large.<sup>32</sup> These assumptions led them to interpret their data in various ways, arguing, for example, that those individuals with more years of schooling scored higher than those with fewer years of schooling because their native intelligence led them to stay in school longer, or that Southern blacks scored lower than Northern blacks because the brighter the black individual the more apt he or she would be to migrate north.<sup>33</sup> And of course the generally lower scores of the entire black group tested were used to argue that blacks were an inferior race.<sup>34</sup>

Although the official report of the army testing program did not discuss the data from foreign-born recruits, they were soon picked up by many avowed racists to argue for the superiority of the "Nordic" race over the "Alpine" and "Mediterranean" races, and for restrictions on the immigration to the United States of members of these "less-favored races." The best known of these racists, Madison Grant, never referred to test results, even in the last editions of his well-known book, *The Passing of the Great Race*.<sup>35</sup> But many others of his persuasion, such as Lothrop Stoddard and Seth K. Humphrey,<sup>36</sup> cited the test results continually. Their interpretations received support from by such psychologists as William McDougall (who answered his rhetorical question *Is American Safe for Democracy?*<sup>37</sup> in the negative, basing his opinion on the results of the tests), Carl C. Brigham (whose book *A Study of American Intelligence* neatly summarized the army test data on immigrants in accordance with views of Charles W. Gould),<sup>38</sup> and Yerkes himself, who summarized his interpretation of the tests as follows:

Whoever desires high taxes, full almshouses, a constantly increasing number of schools for defectives, of correctional institutions, penitentiaries, hospitals, and special classes in our public schools, should by all means work for unrestricted and non-selective immigration.<sup>39</sup>

Yerkes also wrote to the chairmen of both the House and Senate Immigration Committees, citing the data of the army tests. Such activities have led several historians to claim that the psychologists played a major role in the passage of immigration-restriction legislation in the 1920s, and at least one psychologist in the 1970s has been quick to shout "mea culpa."<sup>60</sup>

More probably, like the social-Darwinian arguments of fifty years earlier, the results of the tests were taken merely as scientific proof of opinions long held. Even without these data, the social climate created by the Red scare and race riots of the early 1920s, the activities of the Ku Klux Klan, the pressure of labor unions to protect the jobs of their members, and the propaganda of various eugenicists, would probably have resulted in the passage of the immigration restriction laws of 1921 and 1924.<sup>61</sup>

The confidence of the testers, and in general of most psychologists was, however, never greater than in the middle of the 1920s. Terman, for example, required letters of recommendation for graduate students to include their scores on "standard intelligence tests," and he even published an article entitled "Adventures in Stupidity: A Partial Analysis of the Intellectual Inferiority of a College Student." He also posed the rhetorical question "Were We Born That Way?" in a popular journal, and answered it with an unequivocal yes.<sup>62</sup> This arrogance spread beyond the psychologists who identified themselves as testers. Even Carl E. Seashore, a man beloved by generations of students and colleagues at the University of Iowa, made many enemies in the university's School of Religion in the mid-1920s by habitually suggesting to its best students that they change their majors to psychology and study the psychological aspects of religion, arguing that "there is no other method" of value than that of experimental psychology.<sup>63</sup>

One disconcerting attitude—an almost casual anti-Semitism—was shared by psychologists with much of American culture of the period. During a period when Jewish quotas were introduced at Columbia, Princeton, and New York Universities and talented Jewish physicists and philosophers had trouble finding employment,<sup>64</sup> some psychologists were openly anti-Semitic. One, for example, wrote to a close friend that he "did not want to return to the 'ghetto' [i.e., New York] just yet," and a distinguished biologist with close personal ties with many psychologists complained about articles in the liberal magazine he called "The Jew Republic."<sup>65</sup> Other psychologists typically did not object to Jews, as such, and were probably more open than the general milieu in which they worked. But at the same time they typically objected to those individuals who exhibited what they referred to as Jewishness. Thus, through the 1920s, graduate students were recommended for jobs as "noticeably Jewish, but not obnoxiously so," or as "vastly more agreeable as a fellow-worker than one might suspect from his [Jewish] name," or as "brilliant, and his race is not objectionable."<sup>66</sup> In the 1930s, many of these psychologists were particularly concerned that their Jewish students found it especially difficult to find employment. And most ironically, in the 1920s, Terman felt he had to tell a graduate student from a Christian family—Harry Israel—to adopt his mother's maiden name and change his name to Harry Harlow. But even with his old name, before the change became official, he won his first position at the University of Wisconsin.<sup>67</sup>

Before the end of the decade, pressure began to build that eventually forced the testers to reevaluate their assumptions, and the psychological community in general to limit its claims. Such pressure came not only from nonpsychologists, but also often from psychologists themselves, and even sometimes from those who supported testing. For example, though he urged extensive use of psychological tests, Cattell stressed that he believed the tests measured "alertness" as a specific cluster of abilities, rather than a generalized "intelligence," and felt that "when it is found that Italian children in our schools do not do so well in certain tests as native American children, this may be due simply to lack of familiarity with the language or to ease in understanding the instructions." But he still argued that such tests predicted what and how well "the individual will do in a given situation," an operational definition of the results of the test.<sup>66</sup>

A more explicitly operational definition of "intelligence" was offered by Edwin G. Boring, who argued that "intelligence [is] . . . the capacity to do well on an intelligence test," and that this *intelligence* is analogous to the physicist's term *power*; i.e., the ability to do a given amount of work in a given time.<sup>67</sup> Boring also attacked Brigham's book, though he was a good friend of both Yerkes and Brigham, and Yerkes had gone out of his way to help arrange for Boring to review it in the *New Republic*. Specifically, Boring attacked Brigham's assumptions that the tests measured innate intelligence, and that the foreign-born recruits tested were an accurate sample of the various European races. He did not conclude that Brigham was wrong—"he may be right"—but rather felt that there were "so many other possibilities that I think we can say little more than we do not know; or, if . . . we have to make a judgment, we may say that the chances are that he is wrong."<sup>68</sup>

In general, the reaction to Brigham's book among professional psychologists was negative,<sup>69</sup> and his contemporaries attacked all of his assumptions. Also attacked were the internal inconsistencies among parts of the Alpha and Beta tests, which, some psychologists thought, made it questionable that the tests measured any such thing as general intelligence.<sup>70</sup> But these reviews did not prevent the *Literary Digest* from using Brigham's results to argue for immigration restriction, or other professional groups from accepting his arguments as fact.<sup>71</sup>

Pressures from outside the narrowly defined psychological community also forced a reevaluation of the tests and their results. William C. Bagley, an educator at Teachers College, Columbia University, whose training in experimental psychology had involved him in the interpretation of Cattell's tests about thirty years earlier,<sup>72</sup> argued against the tests. He claimed that all they really measured was the extent of the individual's education, and that to accept their results without question would lead to a deterministic self-fulfilling prophecy, an argument that Terman of course rejected.<sup>73</sup>

On a more popular level, Walter Lippmann's long series of articles in the *New Republic* attacked all of the testers' basic assumptions before a wide audience.<sup>74</sup> And even a long reply by Lewis M. Terman, whose Stanford-Binet was the most widely used individual (as opposed to group) test,<sup>75</sup> an editorial in the *New York Times* supporting Terman's position over Lippmann's,<sup>76</sup> and a fairly extensive correspondence between Lippmann and Yerkes<sup>77</sup> could not counteract the effects



of Lippmann's attacks. To be sure, American psychologists did not stop their testing,<sup>80</sup> but by about 1925, in most cases, their claims for the tests had toned down. Tests were still used by guidance counselors and in college admissions procedures, and in 1926, the College Entrance Examination Board first gave the Scholastic Aptitude Test, developed by Brigham and others. But such was the change of climate of opinion that within a few years the CEEB dismissed any claims that the SAT measured intelligence, but rather said that it measured verbal ability.<sup>81</sup> As early as 1921, at least two of Terman's assistants began even to question his reliance on the individual Stanford-Binet tests alone to measure intelligence, and by 1927, one of his colleagues, Truman L. Kelley, published a statistical analysis of the internal consistency of intelligence tests that raised much doubt as to their validity.<sup>82</sup>

Meanwhile, many anthropologists and psychologists trained or influenced by Franz Boas at Columbia challenged the concept of European races supported by Brigham.<sup>83</sup> Boas himself wrote of "The Nordic Nonsense,"<sup>84</sup> and had his students attack problems related to Brigham's ideas. With a grant from the Columbia University Council for Research in the Social Sciences, which had itself been funded by the Laura Spelman Rockefeller Memorial, he and his students carried out a long-term project on "Hereditary and Environmental Influences upon the Development of Man."<sup>85</sup> For example, Margaret Mead measured "the effect on intelligence test scores of the language spoken in the home."<sup>86</sup> But probably the most influential of Boas's students in this area was Otto Klineberg, who had come to Columbia with an M.D. from McGill University convinced of the validity of McDougall's arguments in *Is America Safe for Democracy?* Boas had Klineberg give various tests to American Indian groups, and this experience convinced the student that cultural factors were important in any testing situation.<sup>87</sup> Klineberg later developed different versions of these tests that were among the most sensitive of the period,<sup>88</sup> and with the help of fellowships from the National Research Council and the Laura Spelman Rockefeller Memorial, he gave these tests to children in France, Germany, and Italy. He showed that there was no correlation between the results on the tests and the characteristics that Grant and others had used to distinguish the Nordic, Alpine, and Mediterranean races.<sup>89</sup> Later, he used similar tests to argue that any difference between the scores of Northern blacks and Southern blacks on earlier tests was due to the better education that the former had received.<sup>90</sup> Even Brigham began to change his mind. By 1927, work on the internal inconsistencies of the army tests had led him privately to reconsider his earlier statements, and in 1929 he told Klineberg, that he "didn't stand by a word" of his book on American intelligence. By 1930, he openly concluded that "one of the most pretentious of these comparative racial studies—the writer's own—was without foundation."<sup>91</sup>

Of course, this work did not settle the question of any relationship between race and intelligence. By 1941, Raymond B. Cattell (who is *not* related to James McKeen Cattell) published what he felt to be "A Culture-Free Intelligence Test,"<sup>92</sup> and the past fifteen years have seen the writings of Arthur Jensen, Richard Herrnstein, William Shockley, H. J. Eysenck, and others.<sup>93</sup> Clearly, the question is still not settled, despite the fact that such psychologists as Anne

Anastasi have developed approaches that allow the study of *how* heredity and environment *interact* in one individual, and that these approaches are part of a broader intellectual "climate of opinion" that has recently been described as "the triumph of evolution."<sup>94</sup>

Clearly the tests of the 1920s failed to answer important questions, and this episode is the first that illustrates the general pattern of confidence, overstatement, and failure. In some ways, this pattern can also be seen in the failure of James McKeen Cattell's testing program of the 1890s.<sup>95</sup> In both, the psychologists approached the problem of testing with confidence, buoyed by the recognition their work had earned them in the years immediately preceding. This was especially true in the 1920s, when the psychologists could point to the success of their work in the important, if restricted, area of selecting men suitable for officer training.<sup>96</sup> This confidence led in both instances to vastly overstated claims for the tests, despite the fact that, again in both cases, the testers knew that there were important aspects of what they were measuring about which they knew little.<sup>97</sup> And of course the problems to which the testers tried to apply their work were more complicated than the selection of men for officer training. The failure of the tests to deliver what the testers claimed they could readily led to attacks on their use, first by psychologists who were not involved with the testing movements, and then by related specialists, such as educators and anthropologists.<sup>98</sup> However, only in the later period did such public figures as Lippmann attack the tests, perhaps because the psychological testing of the 1890s was not well known to the public. And while by the late 1920s the general scientific climate of opinion in the United States had clearly shifted away from the testers' point of view,<sup>99</sup> the main reason for the decline of the testing movement of the 1920s was the psychologists' overstatement of what their tests could do.

### Industrial Psychology

Before the testing movement as such declined, it contributed extensively to the rise of a broadly defined "applied psychology," which used tests and similar procedures to attack problems in clinical, educational, and industrial areas.<sup>100</sup> In this work, those who called themselves "consulting psychologists" often did not simply apply the results of their more academically oriented colleagues. Instead, they developed their own applied science and behavioral technology.<sup>101</sup> Cattell was deeply involved in this movement, as will be seen. But probably the most significant point to make about the development of applied psychology in the United States during the 1920s—at least industrial psychology—was that it followed the same pattern that testing had followed: limited success, overconfidence and overstatement, and retrenchment.

Industrial psychology did not begin with the application of the earliest tests.<sup>102</sup> As early as 1895, in his psychology classes at the University of Minnesota, Harlow Gale used advertisements to illustrate the various phenomena of involuntary attention.<sup>103</sup> But the first person who identified himself as a psychologist to approach the problems of advertising directly and who attempted to improve the effectiveness of advertising copy was Walter Dill Scott.<sup>104</sup> Scott had been

educated in Germany, where he worked with Wilhelm Wundt in Leipzig, and, in 1901, while teaching at Northwestern University, he was approached by a Chicago copywriter to speak to a local advertising club on the psychology of advertising.<sup>105</sup> This talk led to others, and to the serial publication of his book *The Theory of Advertising*—carefully avoiding the word psychology in its title, to prevent offending other psychologists—in an advertising agency's house organ.<sup>106</sup> Meanwhile, other psychologists began working on psychological problems of advertising,<sup>107</sup> and Scott's second book on this topic explicitly referred to psychology in its title.<sup>108</sup> Problems of advertising led directly to problems of salesmanship, and Scott soon involved himself in the development of methods to select and train salesman,<sup>109</sup> an effort which soon expanded to attempts to use psychological techniques in the selection of individuals for other jobs and professions.<sup>110</sup> This work paralleled similar studies by other distinguished psychologists such as Hugo Münsterberg, who had lectured on this topic as early as 1912, and Edward Lee Thorndike.<sup>111</sup> Other studies of worker selection by 1915, received support on a large scale at Carnegie Institute of Technology, where the H. J. Heinz Company, the Equitable Life Assurance Society, and the Burroughs Adding Machine Corporation, among others, contributed to the Division of Applied Psychology, under Walter Van Dyke Bingham. By 1916, the two leading industrial psychologists in the United States joined forces, for in that year Scott came to Carnegie Tech as professor of applied psychology—apparently the first such chair ever created—and as the director of the Bureau of Salesmanship Research within Bingham's division.<sup>112</sup> By 1917, the *Journal of Applied Psychology* appeared, and the fact that it survived America's participation in World War I, when, for example, the *Journal of Experimental Psychology* suspended publication for two years, shows how alive the new field was.

When the United States entered the First World War, Scott and Bingham offered their services to the War Department, based on their experience in selecting salesmen, and presented a "rating scale for selecting captains," a modification of Scott's earlier scales for rating business personnel. By August 1917, a Committee on the Classification of Personnel in the Army had been established, with Scott as director, and Bingham as executive secretary.<sup>113</sup> Like the Army Alpha and Army Beta tests, the work of this Committee attracted a great deal of attention to what psychology could do, and its work was quite successful. By Armistice Day, it had classified and rated the job qualifications of more than three million men, and was lauded in such journals as the *New Republic* as an instrument to prevent politics from entering into the appointment and promotion of officers.<sup>114</sup>

After the war, psychology in all of its aspects boomed, and such was the public interest in the field that psychologists found themselves discussing "Psychology as a Life Work"<sup>115</sup> before audiences of young men interested in the science and its practice as a career. Many firms, involved in businesses as diverse as retail sales and heavy manufacturing, grew interested in psychology and began to hire psychologists to study their personnel practices.<sup>116</sup> R. H. Macy and Company, for example, hired a Columbia psychologist to study its recruitment, training, and management procedures for sales and clerical positions,<sup>117</sup> and the U.S. Civil

Service Commission, interested in Scott's work during the war, hired Beardsley Ruml, another member of the committee's staff, as a consulting examiner to study its testing procedures. One result of this study was the establishment of a Research Division for the commission in 1922.<sup>118</sup> Meanwhile, in 1919, hoping to apply much of what he learned during the war, Scott himself organized many of the men who had worked with him in the Army Committee on Classification into a private personnel management consulting firm, the Scott Company. Working closely with more than forty industrial and commercial firms by 1923, the company developed special tests for particular jobs in such fields as meat packing, machine assembly, and the like, and even worked directly with both companies and unions to stimulate efficient practices. Its "active members" included most of the men who had worked with Scott during the war, as well as many of his students from Carnegie Tech. In addition, its list of "associates" included such prominent psychologists as James R. Angell, Bingham, Thorndike, John B. Watson, and Yerkes.<sup>119</sup>

Such was the situation with respect to industrial psychology in 1919, when James McKeen Cattell first began to make plans for what was to be founded in 1921 as the Psychological Corporation, his attempt at applying psychology on a large scale.<sup>120</sup> Today the company is probably the best known commercial firm involved in applied psychology, but in the early 1920s it started slowly. Its growth was retarded by a rather strange organizational scheme that Cattell imposed on it.<sup>121</sup> He often referred appropriately to the corporation as a "holding company for psychologists." Under Cattell's plan, the Psychological Corporation was to act as a sort of publicity agent, referral service, and supply company for applied psychology, in its largest sense. For example, besides personnel studies, Cattell envisioned that the corporation would administer, at a nominal fee, tests for those persons who wished to know how intelligent they were, or how well they scored on such standardized tests as the Army Alpha. The corporation was to make the availability of its services known through judicious publicity—e.g., the issuance of press releases and the circularization of reprinted newspaper stories about its activities—and was to make available to its stockholders supplies of test forms and other such items. Those individuals and business firms who felt the need of the services of a psychologist and who approached the corporation were to be referred to one of its psychologist-stockholders, or more likely, to one of its branches, which were planned for most American cities. Each individual stockholder, or branch member, would perform the required service, and charge the client a fee. This fee was to be split evenly between the individual performing the service and the corporation, subject to two understandings. One was that the stockholder agreed to devote at least half of his or her fee to the psychological research in which he or she was currently involved. The other was that the dividends to be paid by the corporation were strictly limited by law, and that any excess of profits over expenses and dividends was to be devoted, in some unspecified way, though probably through grants, to the advancement of scientific research. In this way, Cattell hoped not only to drive psychological charlatans out of business, but also to promote psychological research, add to the financial

standing of psychologists, expand the public's familiarity with psychology, and improve the social standing of psychologists throughout the United States. His program was a confident one, and it claimed a great deal.

All of the goals of applied psychology meshed well with the typical concerns of the period for efficiency and with the "progressive" concerns for "scientific" reform that continued into the 1920s from the "Progressive Era."<sup>122</sup> This similarity of approaches suggests that there were ties between the growing profession of industrial psychology and the previously existing Scientific Management movement.<sup>123</sup> To be sure, members of the two groups knew each other, and Bingham was fairly well acquainted with Frank B. Gilbreth. He even solicited a gift of a set of Gilbreth's apparatus for Carnegie Tech, and arranged for a similar set to be exhibited at the American Psychological Association in 1917.<sup>124</sup> Similarly, as part of his investigation of extremely intelligent individuals, Terman measured the IQ's of the entire Gilbreth family, and found that while Frank's was higher than all but 1.33 percent of the white recruits tested in World War I, his wife, Lillian, had a higher IQ than all but 0.12 per cent of such recruits.<sup>125</sup> Lillian Gilbreth *did* have a Ph.D. in psychology, but always referred to herself as an industrial engineer, and kept her membership in the American Psychological Association primarily for the business contacts it gave her.<sup>126</sup> In the same way, Elsie Bregman, the psychologist who had been employed by Macy's, was once invited to take part in a study of "The Application of Scientific Management to the Home" at Teachers College, Columbia University.<sup>127</sup> But as these contacts suggest, there was no real intellectual or substantive interaction between the professions. That is, at least during this period, neither group really learned much from the other, nor did they work together to apply "science" to industrial problems. Scientific management, whether practiced by a strict disciple of F. W. Taylor or by a more eclectic follower of the Gilbreths, concerned itself with the work and the task, via time or time-and-motion studies. Industrial psychology, on the other hand, was interested in the worker and his or her selection, motivation, and training. It is true that the two movements shared the same goal—increased production and efficiency—but they approached this goal in different ways. Still, as another historian has indicated, "scientific management had accustomed industrialists to the idea that a study of the production process would pay. . . . For this, the industrial psychologists would be eternally in debt to Taylor's movement."<sup>128</sup>

Much more important to the development of industrial psychology were links the emerging profession had with both the business community and, to a lesser extent, the labor movement. The ties with the business community were particularly clear. Bingham wrote of Edward A. Woods of the Equitable Life Assurance Company as "the leading spirit" behind the organization of the Personnel Research Bureau at Carnegie Tech. Likewise, many of Scott's associates, including Robert C. Clothier, who wrote with him a book on personnel management, were by experience and training businessmen and industrialists. Parallel to this situation, John B. Watson, an academic for twenty years, became a vice-president of the J. Walter Thompson Advertising Agency after the scandal following his divorce forced him to leave Johns Hopkins.<sup>129</sup> The industrial psychologist and the businessman shared the same goals and worked well together.



These ties between business and psychology in the early 1920s have led a number of historians to suggest that one reason for the growth of industrial psychology in the early 1920s was its use by business in an attempt "to kill unionism with kindness."<sup>130</sup> That is, industrial psychology provided an alternative to scientific management, which treated the worker as part of the machinery of production. There may be something to this view, especially as the psychologists were not political radicals. Cattell, however, had once issued "A Program of Radical Democracy," proposing, for example, a confiscatory tax on all inheritances.<sup>131</sup> This interest led him to enter into a long correspondence with Samuel Gompers, of the American Federation of Labor, as to the nature of intelligence tests and industrial psychology, and how psychology could contribute to the goals of unionism.<sup>132</sup> The Scott Company worked well with unions to insure industrial peace, mainly as a factor in improving the efficiency of production. Scott himself refused to work with any company determined to fight the unions, and his company lost several important contracts when it told its prospective employers that it would not work to destroy organizations of employees.<sup>133</sup> Scott and his colleagues preferred to work through trade associations—groups combining representation of both the unions and the different firms involved in each industry—hoping to get the benefit of as many different viewpoints as possible, while having access to all labor and personnel policies practiced by both management and the unions.<sup>134</sup> The Scott Company's greatest success in following this procedure was in Chicago in the early 1920s when it worked closely with both sides to settle a longstanding major dispute in the men's clothing industry. Others have suggested that Scott's work led to the relatively long peace in the industry and, undoubtedly, it did play a role. But probably a much more important factor was the "moderation and realism" of Sidney Hillman, the head of the Amalgamated Clothing Workers of America.<sup>135</sup> In any event, industrial psychologists had subtle, and not necessarily antagonistic, relations with the unions.

But like the various programs of mental testing of the 1920s, by the middle of the decade industrial psychology showed signs that it had overextended itself. The field had developed strongly during World War I, and in the years immediately afterward. But even during the War, James R. Angell, dean at the University of Chicago and a former president of the American Psychological Association, expressed his doubts to Bingham as to the viability of the program of the Army Committee on the Classification of Personnel. And General John J. Pershing, Commander-in-Chief of the American Expeditionary Force, thought that the committee overstated the value of its work.<sup>136</sup> In 1921, a young industrial psychologist, trained outside of the Bingham-Scott tradition at Carnegie Tech, argued that the tests then used by most applied psychologists were limited in that they did not consider such factors as attention span, reaction speed, and "planfulness," and urged the development of tests for such specific abilities.<sup>137</sup>

By 1923, the Scott Company dissolved itself, having been relatively successful for at least a part of its four years of existence. But by that year the team Scott had organized during the war had disbanded, as its "active members" took advantage of opportunities elsewhere. Scott himself had become president of Northwestern University by 1921, and the company's secretary, Beardsley Ruml, had left to

become director of the Laura Spelman Rockefeller Memorial. Financial factors undoubtedly played a part in the company's closing, as the relative recession of 1922 cut deeply into its activities. Several of its officers claimed that its dissolution also reflected the development of personnel departments in many industrial firms, thus reducing the need for outside consultants.<sup>138</sup> But an important factor in the Company's demise was probably its policy of not fighting unions and, perhaps even more significantly, the subtle approach it took to personnel problems. Scott and his colleagues never claimed to be able to answer all of management's problems immediately, and its sophisticated tools, including the analysis of "the worker in his work" as a unit, were not the simple, straightforward, and easy-to-administer tests that many industrialists apparently expected. The Scott Company offered no easy answers, and its honesty apparently cost it much business.<sup>139</sup>

Only one year later, in 1924, the Division of Applied Psychology at Carnegie Tech also closed. During its existence, it had sponsored a Bureau of Salesmanship Research, a Research Bureau for Retail Training, and a Bureau of Personnel Research, all well supported by the business community of Pittsburgh. The reasons for the discontinuance of this division are many, and none are entirely clear. Certainly personal animosities were involved, as well as an effort to make the most efficient use of the scholarly resources of Pittsburgh. But except for the Research Bureau of Retail Training—i.e., a practical business school—which was transferred to the University of Pittsburgh, it appears that the industrial community of the city did not believe it was getting its money's worth from the support it gave the division.

As early as 1917, in response to a visit to Carnegie Tech at the invitation of Bingham, James R. Angell expressed his doubts as to the division's ability to do all that it had planned to do, just as he questioned the program of the Army Committee on Classification at about the same time. In 1920, Edward A. Woods, one of the businessmen who helped found the division, noted that "it is by no means certain that the present Intelligence Tests given to applicants for positions as salesmen are valuable," and sketched a research program for Bingham and Scott. He suggested studies to determine which qualities of good salesmen were ascertainable by physical examination, experience, and educational record, and which were measurable only by psychological tests. These studies were to be followed by the design of tests to determine these traits, without wasting time devising mental tests for "qualities that are comparatively unimportant." But nothing came of Woods's proposal, and four years later the division was disbanded.<sup>140</sup>

The Psychological Corporation, in the 1920s at least, failed even more spectacularly. Cattell's organizational scheme concentrated more on how the expected profits were to be used than on how the corporation was actually to apply psychology, and therefore there was literally no coordination between the various branches.<sup>141</sup> More than that, Cattell's own approach to industrial psychology was as unsophisticated as his approach to psychological testing had been in the 1890s. For example, he believed that simple tests could be developed that could easily pick out various traits in individuals that would qualify, or disqualify, them for specific positions. From his point of view, the role of industrial psychology was to

select the proper person for the proper job, or, analogously, to select the best position in a magazine or newspaper for an advertisement.<sup>142</sup> An example he often used in describing this approach was to classify individuals into those interested primarily in: (1) persons; (2) material objects; and (3) "abstractions such as words and figures." He claimed that a transportation company could use the services of a psychologist to select the first type of individual as conductors and pursers, the second as motormen and engineers, and the third as clerks and bookkeepers.<sup>143</sup> While carrying out his early testing program, he explicitly ignored what were known as the "higher mental processes," at least partially because he could not easily quantify them. In the 1920s, he again concentrated on simple aspects of the problem that he could measure, or thought he could measure. That an individual's performance on a given job could depend on much more than his or her aptitude for it is a question that finds no concern in his writings. His approach was the "square peg in a square hole" industrial psychology that the "worker in his work" concept of the Scott Company was designed to combat.

The Psychological Corporation, unlike the Scott Company, never did show a profit at any time during the 1920s. For ten months of 1924 and 1925, the total *gross* income of the New York office of the corporation—one of the busiest, apparently—was \$215.00, and the net income, after royalties and salaries, was \$51.75.<sup>144</sup> Meanwhile, the corporation was keeping offices in one of the prime commercial buildings in New York—Grand Central Terminal—and was paying a secretary \$2,500.00 per year.<sup>145</sup> By 1925, Cattell had to lend the corporation a total of \$5,000.00 so that it could meet its expenses,<sup>146</sup> and it is doubtful if this debt was ever paid off.

By the end of 1926, the situation had gotten so bad that a drastic reorganization of the corporation took place. Cattell resigned, either under pressure or by choice, as president, and was "kicked upstairs" as chairman of the board. The new president was Walter Van Dyke Bingham, a man who had had over a decade of experience with industrial psychology and its problems. Dean R. Brimhall, who had been appointed secretary of the corporation as Cattell's protégé, left, and was replaced by Paul S. Achilles, an industrial psychologist who had had close ties to Bingham since the beginning of the Army Committee on Classification.<sup>147</sup> The board of directors drew up detailed plans for what they hoped the corporation would accomplish over the next five years, including such subtle problems as "mental aspects of the prevention of industrial accidents" as part of its program. But this program cost much—an estimated \$540,000.00 for the five years<sup>148</sup>—and in 1929, just before the Great Depression, the corporation's gross income totaled \$1,642.33.<sup>149</sup> The Depression hit the corporation hard, of course, as most businesses cut all but their most necessary expenditures.<sup>150</sup> Throughout the 1930s, even though a full-time staff had been hired, it "had to fight for its existence," and at least one methodologically sophisticated observer despaired at what he considered the "simple-minded approach" the corporation took to most of its problems.<sup>151</sup>

In general, then, by the middle of the 1920s, the interest of businessmen and industrialists in applied psychology had decreased greatly. The reasons for this again are not totally clear, but they are at least partially related to the stabilization



of the business and employment atmosphere during this period, after the period of high labor turnover immediately after the war. Other reasons included the presence of various fraudulent and pseudoscientific consultants, and, more importantly, the fact that the reputable psychologists who conscientiously practiced their profession could offer no immediate solutions to any of the problems that business faced. The Psychological Corporation was, to be sure, an extreme example, but, in general, the psychologists again confidently overstated what they could do, and were, therefore, subject to disappointment.

### Certification of Consulting Psychologists

The same pattern of confidence, based upon success with handling a limited problem, leading to overstatement, and, in turn, to disenchantment, appeared in the movement for the certification of psychologists engaged in nonacademic, professional practice of one sort or another that emerged in the years immediately following World War I. This movement saw the establishment of an American Association of Clinical Psychologists,<sup>152</sup> the absorption of this group into the American Psychological Association,<sup>153</sup> and an abortive attempt to have the APA itself certify what it broadly called consulting psychologists.<sup>154</sup> This story is much too complex to be told in full here, but as early as 1917, a committee of the American Psychological Association was established to investigate "the qualifications for psychological examiners and psychological experts," including clinical, educational, and industrial psychologists.<sup>155</sup> However, many psychologists believed that such an investigation was unnecessary—i.e., that enough was known about what clinical psychology, at least, should be—and at the same meeting of the APA at which this committee was formed a group of nine men and women organized an American Association of Clinical Psychologists, which discussed such standards for qualification at its organizational meeting.<sup>156</sup> Many members of the APA feared that this new group would split from the American Psychological Association, and debate between the two groups continued through December 1918, when the newer group met with the older. The Association of Clinical Psychologists had planned both a business session and scientific symposium for the meeting, but interest in the relations between the two groups forced the postponement of the formal papers.<sup>157</sup> The business meeting led to the formation of a joint committee of both associations to study problems of certification, and a year later the clinical psychologists were merged into the American Psychological Association. At that time, an APA Committee on the Certification of Consulting Psychologists was formed, with a mandate broader than clinical psychology.<sup>158</sup>

Throughout this episode both groups of psychologists were trying to protect, or establish, the reputation of psychology as a service profession, able to administer individual intelligence tests, offer vocational guidance, and help industries solve their personnel problems. A related important goal was the defense of psychology—especially industrial psychology—from the claims of such pseudoscientific or even fraudulent services as "psychoanalysis" [*sic*], phrenological psychography,

the Blackford School of Character Analysis, and the like.<sup>159</sup> The *Literary Digest* even organized a "Society of Applied Psychology," whose president, Warren Hilton, was acquainted with the science of psychology only through one undergraduate course in the area taken at Harvard.<sup>160</sup> But both groups were also explicitly concerned with establishing the psychologist as an expert in a legal sense, analogous to the physician's position before the courts.<sup>161</sup> In the 1920s, this desire of the applied psychologists was opposed by academic psychologists, who were afraid of having their science "dirtied" by contact with practical problems, and who probably also realized many of the problems of applying a fledgling science.<sup>162</sup> In the same way, psychiatrists opposed the professionalization of psychology, in part because they saw their monopoly in the diagnosis and treatment of certain disorders threatened, and in part because of some real doubts that they had about the usefulness of psychology.<sup>163</sup> In any event, the issue of the certification of psychologists is still not settled, at least in part as a result of the opposition of "scientific" psychologists and psychiatrists. Still, the Committee on the Certification of Consulting Psychologists started with high hopes despite its charge to limit its expenses to the funds it raised by requiring fees for certification.<sup>164</sup> By 1922, however, it found itself with little or no role to play as less than two dozen (of more than four hundred) members of the association applied for certification.<sup>165</sup> After the agitation of the previous five years, the committee at first could not understand why more psychologists did not apply for certification, and then finally decided that this low number was a result of the relatively high fee (\$35.00) it charged for certification, and of some confusion about the meaning of certification. It therefore decided to recommend that the certification fee be reduced, and to develop a list of "distinguished" psychologists who would be invited to apply for certification.<sup>166</sup>

The issuance of a large number of invitations to certification clashed with the whole philosophy of certification itself, and many members of the APA were "perturbed" by the committee's action. Most of those circularized did not apply for certification—"puzzlement" was the typical reaction<sup>167</sup>—and the association's president, Knight Dunlap, refused to sign any diplomas of certification until the matter had been thrashed out at the annual meeting.<sup>168</sup> At that meeting, the committee sought to clarify its role, but its report, recommending fundamental changes in the concept of certification—suggesting, for example, that certification be granted in either clinical, educational, or industrial psychology—was not accepted.<sup>169</sup> From that point on, the committee was moribund, and after an attempt in 1926 to establish a definite policy with regard to unprofessional conduct on the part of APA members—stimulated by a report that an unnamed clinical psychologist engaged in sexual intercourse with his patients<sup>170</sup>—the committee was finally dissolved in 1927.<sup>171</sup> In its six-year history it had certified as consulting psychologists only twenty-five members of the APA.<sup>172</sup>

It is striking how many of the issues raised by the history of this committee still face psychology today.<sup>173</sup> But the pattern of overstatement followed by disenchantment is perhaps more interesting, and certainly more significant. But if the attempts by psychologists to apply their science were disappointing, the

confidence that the psychological community shared throughout this decade was hardly dampened. The reasons for this continued confidence and good feeling were many.

### Financial Support of Child Psychology

Perhaps the main reason for the continued good feeling and confidence among psychologists through the 1920s was the large amount of money invested in their science throughout the decade. Large sums were granted for work in many areas of psychology, especially child study, by several large philanthropic foundations. The Commonwealth Fund was especially active, supporting Truman L. Kelley's "Study of the Structure of the Intellect" at Stanford, and, from 1921 through the Depression, the Harvard Growth Study. This last project, funded heavily, saw Walter F. Dearborn and his colleagues "assess the nature of physical and mental growth by making annual measurements on the same individuals over a period of twelve years."<sup>174</sup> Even more spectacular were Terman's "Genetic Studies of Genius," which saw Stanford match the major grant from the Fund. Begun in 1921, this study identified one thousand young "geniuses" in California public schools, and, since that date, has traced its subjects' lives and careers through the present. Employing a large number of student and postdoctoral assistants, Terman's work in this area is perhaps the earliest example, in psychology, of what has come to be called "Big Science."<sup>175</sup>

Perhaps the largest amount of money given to psychologists for their work came from the Rockefeller Foundation and its associated agencies.<sup>176</sup> For example, in 1926, the Laura Spelman Rockefeller Memorial gave \$76,500 to the American Psychological Association to subsidize, for ten years, the publication of *Psychological Abstracts*. It also regularly provided funds for European psychologists to work in the United States.<sup>177</sup> And in 1929, the foundation pledged a total of \$4,500,000 to Yale over ten years to establish an Institute of Human Relations, dominated by psychologists, while including work in psychiatry, law, and the social sciences. Including funds for child study and for an "anthropoid breeding station," these grants established a center for psychological study that in later years produced some of the most exciting research to appear in America before World War II.<sup>178</sup>

But like most philanthropic agencies that supported psychology during the 1920s, the Laura Spelman Rockefeller Memorial was interested primarily in "Child Study and Parent Education."<sup>179</sup> Headed by Beardsley Ruml, a psychologist who had earlier been associated with James R. Angell at the University of Chicago and the Carnegie Corporation, with Bingham at Carnegie Tech, and with Scott at the Scott Company, the Memorial funded work in this area via grants to home economists, nursery school educators, nutritionists, and education teachers, as well as to child psychologists. Besides funding National Research Council fellowships and scholarships for child study,<sup>180</sup> and providing direct and indirect grants in support of *Parents' Magazine*,<sup>181</sup> the Memorial also gave much support for various child study research centers, following the model established

in Iowa in 1917. In that state, a well-known clubwoman, Cora B. Hillis, had argued since 1913 that the state legislature should spend as much for the study of children as it did for research on hogs and cattle. With the support of Carl E. Seashore and other psychologists at the state university, Hillis lobbied at the legislature regularly through 1917. In that year, the failure of many young men from Iowa to meet the minimum standard set for draftees convinced the state legislators to establish a Child Welfare Research Station at the university.<sup>182</sup> At first a fief of the university's Department of Psychology, the station had a strong director, Bird T. Baldwin, a psychologist.

He soon established important research programs in child health and institutional and social patterns of rural and urban families, as well as important extension programs for parent education and the training of kindergarten teachers.<sup>183</sup> To aid its work, Hillis arranged for the Women's Christian Temperance Union to grant the station \$50,000 over five years, and at her death left it \$10,000.<sup>184</sup>

In 1920, Baldwin began corresponding with officers of the memorial about the possibilities of obtaining funding for the station's work, and early in 1921 he wrote optimistically to Hillis that he expected such funding "sooner or later." In 1922, \$22,500 was granted to the station by the Memorial over a three-year period, representing its first large-scale investment in child-study research. Other grants were made in 1925 (ca. \$100,000 over five years), 1926 (an additional \$15,000 for a "study of the rural child"), and 1928 (ca. \$850,000 over ten years).<sup>185</sup> In addition, smaller grants were made to the Iowa State College of Agriculture at Ames (in 1925, \$22,500 over three years; in 1928, \$30,000 over five years), where child-study work and extension education programs were centered in the School of Home Economics under a strong dean, Anna Richardson, and to the Iowa State Teachers College at Cedar Falls (in 1925, \$22,500 over three years; in 1929, \$32,000 over six years) for nursery-school education.<sup>186</sup> By the end of the decade, then, Rockefeller money supported a unified program of child-study and parent education in many different areas, under several auspices, throughout the state of Iowa.<sup>187</sup>

Once the Iowa model was established, the Memorial followed it, and supported other centers of child research at such institutions as Teachers College of Columbia University, the University of California, and the University of Minnesota.<sup>188</sup> At Yale, Arnold Gesell received some support for his photographic studies of children's behavior quite early, but the Memorial soon—by 1924—supported this work only as part of the Yale Psycho-Clinic, and later as part of the Institute of Psychology, established by 1925, which set the pattern later to be followed by the Institute of Human Relations.<sup>189</sup> Other smaller institutions of various kinds also received grants for psychological work with children of one kind or another. Mills College in California, for example, received \$36,600 over three years for a program in nursery school education, and the University of Cincinnati was granted \$15,000 for similar work. And at the Agricultural College of the State of Georgia, a program in preschool education was established with a grant of \$12,000 over three years through the intervention of Rosa M. Walker, a woman much like Cora B. Hillis, an officer of the state Parent-Teacher Association and the wife of the

governor.<sup>190</sup> In many ways, psychologists had arrived, and the acceptance of their work did much to support the good feeling and confidence that permeated the profession.

### The Psychological Community and E. B. Titchener

Another reason for the confidence and good feeling of the 1920s was the fact that the community of psychologists was still quite small, and most of its members knew many of their colleagues well and could speak of them as friends and coworkers, even if they sometimes disagreed with one another. Robert S. Woodworth's comment on this point has already been quoted, but nothing illustrates the unity of the profession during the 1920s better than a sketch of the role played in the community of psychologists by Edward Bradford Titchener.

By 1919, Titchener's "structural" psychology stood in almost total isolation from the views of the rest of the profession and, as during the first years of the century, many American psychologists defined their theoretical positions by how far they stood from Titchener.<sup>191</sup> Yet Titchener was loved and respected even by the most ardent behaviorists. Many leading psychologists came long distances to attend the meetings of the Society of Experimental Psychologists, which he organized and over which he presided with a kindly paternalism.<sup>192</sup> He never really took part in the formal activities of the American Psychological Association and even let his membership in it lapse every few years, yet in 1921 various of his students and their contemporaries urged their colleagues to elect him president of the APA. Many agreed with Margaret Floy Washburn's opinion—"T. is the ablest living American psychologist"—but even she had to note that "that fact is irrelevant to the presidency," and in fact he was not elected. In the same year he again let his membership in the association lapse, and its members reacted with a unanimous resolution asking him to reconsider his decision. He did so, only to let his membership lapse five years later.<sup>193</sup> This personal affection was reciprocated, and Titchener went out of his way, for example, to take a stand against the A. N. Marquis Company, when it dropped Watson from *Who's Who* after his divorce in 1920 and the scandal that followed.<sup>194</sup>

When Titchener died in 1927, the reaction of his colleagues showed how much he was loved. His beard had grown extremely white a year or two earlier, and his normally perfect lectures, just before his death, were interrupted by sudden pauses while he struggled to regain his train of thought. The death was totally unexpected, although members of his family had suspected the existence of the cerebral tumor, which "fortunately" hemorrhaged before it incapacitated him completely.<sup>195</sup> Upon receiving the news, Edwin G. Boring of Harvard, who long before had adopted Titchener as a surrogate father, immediately telegraphed the leaders of the profession, reporting the death, and followed this action with almost identical letters supplying the details of the actual disease, the autopsy, and the cremation.<sup>196</sup> To "forestall" all other candidates, he also immediately sat down to write a long, analytic, and cathartic obituary of his mentor, published only a few short months later.<sup>197</sup> None of this reaction seemed overdone at the time, and



even those without a strong personal tie to him felt his loss. Cattell wrote that "Titchener's death leaves the world more empty," while Yerkes indicated that "his death . . . came as a terrible shock. I feel as though half my professional world were gone. Never before have I experienced such a sense of combined professional and personal loss."<sup>198</sup> A former student of Titchener's, Karl M. Dallenbach, who had quarreled furiously with him just before his death over the policies of the *American Journal of Psychology*, felt the death of his mentor keenly. "In spite of the estrangement between us, his death hit me pretty hard."<sup>199</sup> And even a nineteen-year-old student at Barnard College, a psychology major who had never met Titchener, reacted strongly to the news. When told by her mother, who was reading the daily newspaper, that a famous psychologist named Titchener had died, Anne Anastasi could do nothing for a few minutes but repeat aloud, "I can't believe he's dead."<sup>200</sup> One of the giants of American psychology had fallen, and his loss was felt by all.

### The Ninth International Congress of Psychology

The end of the decade saw a reaffirmation of the self-confidence of American psychology with the Ninth International Congress of Psychology, held at Yale in September 1929.<sup>201</sup> The Institute of Human Relations had just opened officially, and the stock market crash was still a month away. Chronologically, the setting could not have been better. The congress meant much to many of the older psychologists, who had been greatly embarrassed when plans for an international congress in America in 1913 fell through completely, and the invitation had to be withdrawn, because of intramural squabbling within the profession.<sup>202</sup> In 1923, Cattell and others tried to have the 1926 congress invited to the United States, and the APA even formed a Committee on an International Congress of Psychology in America. But these plans fell through as the Americans soon realized that the financial situation in Europe in 1923 was such that many Europeans probably could not afford a trip to America even three years later.<sup>203</sup> The 1929 congress, therefore, was a realization of the hopes of many Americans and Europeans, and Edouard Claparède, permanent secretary of the International Congress, expressed this feeling well by opening his address at the congress with the exclamation, "Enfin, enfin en Amérique!"<sup>204</sup>

The congress itself was an immense success, and the more than one thousand registrants interacted socially and took part in stimulating scientific discussions that were reflected in their work for many years. Even the *New Republic* editorialized as to its importance.<sup>205</sup> Cattell, of course, was the congress's president—chosen by the members of the American Psychological Association—and his address reviewed the development of American psychology in a way that impressed some of his auditors favorably and struck others as self-aggrandizement. He highlighted the talk by distributing copies of it at the meeting, printed in a large format, and illustrated with photographs of Wundt, William James, G. Stanley Hall, and other important early psychologists. In many ways, it was a bravado performance.<sup>206</sup>

The social history of the congress is also interesting, but here only Cattell's own actions can be discussed. They struck the participants so forcefully that forty-five years later those who attended the congress, and were still living, remembered them. While the various memories differ with respect to many details, all remember a public insult of some sort hurled by Cattell at William McDougall, the English psychologist who had taught at Harvard from 1920 to 1927 before moving to Duke.<sup>207</sup> McDougall had recently been studying learning in successive generations of rats, hoping to show that the learned ability to swim a water-maze was heritable, and hence a Lamarckian trait.<sup>208</sup> His early experiments appeared to show that this acquired ability was indeed inherited. Later McDougall realized that the data upon which he based these conclusions were falsified by a student hoping to please his professor. It was either at a general session, at which McDougall commented while Cattell was in the chair, or at a symposium on physiological psychology, in which McDougall presented the findings bearing on the Lamarckian theory, that the confrontation took place. Cattell stood up when McDougall finished and said, in just about so many words, that he personally would not believe any results coming from McDougall's laboratory. Paul Farnsworth remembered that he reacted by turning to the person sitting next to him and saying, "My God, the old boy was certainly crude on that, wasn't he?" Unfortunately, he found himself speaking to Cattell's daughter, Psyche, herself a prominent psychologist, and immediately apologized, but he remembered that she responded, "That's perfectly all right. He was crude, terribly crude. That was an awful thing to do." Anne Anastasi remembered a "horrified gasp" from the audience, followed immediately by a hushed silence, and eventually by comments about Cattell's lack of tact. Roswell P. Angier, a professor of psychology at Yale, the host of the Congress, felt especially bad about it.

Cattell was certainly outrageous. His treatment of McDougall was insufferable. . . . I looked round for McDougall in order to let him know what we all thought of Cattell's behavior, but did not find him. [Raymond] Dodge [another Yale psychologist] did speak to McDougall about it. He seemed very grateful.<sup>209</sup>

Of course, it was the scientific program on which most of the success of the congress rested, and here the hopes of its organizers were well fulfilled. Many Americans took part in formal sessions and relatively informal symposia on such topics as "Learning Theory" and "General Intelligence" and "Psychology of Industry and Personnel."<sup>210</sup> But the most exciting feature of the congress was the participation of many Europeans at all levels of the program, including informal presentations, formal papers, and invited addresses. Perhaps the most famous foreign speaker was Ivan Pavlov, who spoke, through an interpreter, on "The Highest Nervous Activity."<sup>211</sup> Despite his reliance on a translator, "he spoke with such enthusiasm and dramatic fervor that . . . his gestures and voice alone would have held the interest of the audience."<sup>212</sup> Twelve other papers were presented by Russians, and other Europeans filled the program, with twenty-six German and seventeen British papers being presented.<sup>213</sup>

### Gestalt Psychology in America

The theoretical perspective that attracted the most attention at the congress was that of Gestalt psychology. Even Karl Lashley, in his presidential address before the American Psychological Association (which met at the congress), reflected in many ways the Gestalt point of view in his discussion of the "Basic Neural Mechanisms in Behavior."<sup>214</sup> Of course, most of the American papers at the congress reflected the "normal science" of a middle-of-the-road applied, behavioristically oriented functionalism. But many of those papers which explicitly took—or challenged—a systematic point of view concerned themselves with Gestalt psychology. Many of these, of course, were by Europeans, and the American committee had explicitly tried to attract as many Germans—and hence mostly Gestalt psychologists—to the congress as possible, often by arranging summer-school positions in this country for them to cover the costs of their visits.<sup>215</sup> At the congress, these psychologists held important positions on the program. For example, the only other psychologist to speak at the same formal session as Pavlov was Wolfgang Köhler, who spoke "Über einige Gestalt probleme,"<sup>216</sup> and the accounts of the congress published in various psychological journals stressed their attendance.<sup>217</sup> Many Americans also spoke on Gestalt psychology. Some, like Harry Helson, used its approach to psychological problems, and others, like Margaret F. Washburn, attacked its point of view.<sup>218</sup> Besides Köhler, other European psychologists with a Gestalt point of view who attended the congress included Kurt Lewin, David Katz, and Edgar Rubin. The prominence of Gestalt psychology at the congress was such that one of Lewin's leading students has said that it did more to bring this movement to the United States than Hitler ever did.<sup>219</sup> If nothing else, then, the 1929 International Congress left a major legacy to American psychology through the Gestalt school.

Despite a kernel of truth in that statement, the situation was not that simple. To be sure, at least one prominent German psychologist with close ties to the Gestalt movement—Kurt Lewin—won his first American position, at Stanford University, as a result of the impression he made at the congress.<sup>220</sup> But the transfer of Gestalt psychology to the United States was a complicated episode in the social history of ideas, which began in the 1920s and even earlier, and is too vast a topic to be considered in depth here.<sup>221</sup> Recent literature on this topic has stressed the problems that Gestalt psychologists had in being accepted in the United States<sup>222</sup> and, although this emphasis is somewhat misleading, as will be shown below, many of them undoubtedly did face problems. Thus Lewin never held a long-time position in a psychology department, serving instead in such settings as a Department of Education, a School of Home Economics, a Child Welfare Research Station, and a Research Center for Group Dynamics.<sup>223</sup> Köhler and Kurt Koffka never held the chairs at the major universities that many of their followers felt they deserved. There was some opposition to Gestalt psychology in the United States in the late 1920s and early 1930s, but this opposition was in general not based on any deep antipathy to their ideas. But more importantly, a focus on this opposition obscures a good many more significant points.



In fact, probably the most interesting aspect of the transmission of Gestalt psychology from Europe to the United States was the readiness with which most of the German ideas, and psychologists, were listened to, studied, and to at least some degree accepted. The "ingrained eclecticism" of many Americans, such as that that led to their respect for Titchener, played a major role in this phenomenon. More important, probably, several Americans had studied in Europe during the early years of this century, knew the Gestalt psychologists personally, and considered them their friends. For example, Herbert S. Langfeld of Princeton had studied in Berlin from 1904 to 1909, where he met and befriended Kurt Koffka, who later became a leading member of the Gestalt school. In 1912, Langfeld (then at Harvard) sent one of his students, Edward C. Tolman, to Giessen, where Koffka was then teaching, and eleven years later, when the opportunity next presented itself, Tolman returned to Giessen and Koffka for a few months.<sup>224</sup> In the same way, Robert M. Ogden of Cornell studied at Würzburg from 1901 to 1903 with Oswald Külpe. Ogden also knew Koffka well, included some Gestalt ideas in some of his own work, and, as early as 1922, solicited Koffka's first American paper, "Perception: An Introduction to the *Gestalt-Theorie*," for the *Psychological Bulletin*.<sup>225</sup> Similarly, in the early 1920s, Gordon Allport spent a year in Germany on a Sheldon Traveling Fellowship from Harvard, and took advantage of this opportunity to acquaint himself with the various psychological movements that sprang up after the War. Though especially impressed with William Stern's personalistic psychology, then being developed at Hamburg, he also appreciated "the brilliance of the Lewinian approach" and thought much of "the high quality of experimental studies by the Gestalt school" in general.<sup>226</sup> In 1923, he reported on latest currents in German psychological thought in the *American Journal of Psychology* and the following year presented "The Standpoint of *Gestalt Psychology*" in a leading English journal at the invitation of its editor.<sup>227</sup> None of these four men—Langfeld, Tolman, Ogden, and Allport—ever specifically identified himself as a Gestalt psychologist, but they all played a major role in its transmission to the United States. In 1922, Ogden delivered a paper at the meeting of the American Psychological Association relating to Gestalt theory, and three years earlier, Tolman, carrying out standard behaviorist experiments, "was already becoming influenced by Gestalt psychology and conceived that a rat in running a maze must be learning a lay-out pattern."<sup>228</sup>

By 1924, other psychologists were studying the work of the Gestalt psychologists, and getting excited about it. Few Americans during this early period identified themselves with Gestalt psychology but many found things in the work of Köhler and Koffka to stimulate them. Even Watson in the mid-1920s "struggl[ed] with Köhler's presentation of Gestalt-psychologie [but] failed to get a kick out of" it. Boring was more impressed with their experimental work, and wrote in 1924 of "its power to stimulate the great deal of research which I call good research and which I find very interesting."<sup>229</sup> His commitment to Titchener's structuralist approach began to weaken long before his mentor's death and, unimpressed by the various attempts to apply psychology to practical problems, he was in the early 1920s open to different views of psychology, demanding only that they be "scientific" and rigorous.<sup>230</sup>

As early as 1923, his graduate student Harry Helson began a dissertation on Gestalt psychology—despite his initial opposition—and by 1925 Boring was flirting seriously with Gestalt ideas.<sup>231</sup> That year, after a summer at Harvard, a Vassar student reported to her professor, Margaret Floy Washburn, that Boring had become a “configuralist.”<sup>232</sup> Boring denied the charge—“so I am a contortionist, or whatever the word is”<sup>233</sup>—but two months later, in reporting to Koffka that “rumor is beginning to drift in that I am a Gestalt psychologist,” he seemed quite impressed with the German’s ideas; “Very well, so be it. At least what I get from Köhler, added to the little I get from you, seems to be eminently good scientific sense.”<sup>234</sup> But strong as this statement was, it was not as strong as one he had made more than six months earlier in a letter to Köhler: “I have decided that I am not a *Gestalt* psychologist but merely a scientist. *Gestalt* psychology seems to me to be nothing more than the introduction of science into psychology!”<sup>235</sup>

In the mid-1920s, also, the Gestalt psychologists began visiting the United States themselves and the Americans could hear first hand the ideas that excited them so. Max Wertheimer, the leader of the Gestalt school, did not cross the Atlantic during this period, and the fact that he published much less than his colleagues, especially Koffka and Köhler, kept him in the background as far as the Americans were concerned.<sup>236</sup> For example, in 1924, Ogden arranged a visiting appointment for Koffka at Cornell, followed by visiting professorships at Chicago and Wisconsin.<sup>237</sup> In 1925, “on the invitation of the program committee,” he spoke before the American Psychological Association, and took part in a well-attended “Round Table Conference on the ‘Gestalt-Psychologie.’”<sup>238</sup> During the next three years, he gave at least thirty lectures on Gestalt psychology before various groups around the United States, including the Philosophical Society at Harvard<sup>239</sup> and Boring tried to arrange for him to take his—Boring’s—place at Harvard while he took a sabbatical leave.<sup>240</sup> By late 1926 it became clear that Koffka would probably settle in America, especially as he was then getting a divorce from his wife, who was planning to return to Germany, and rumors began to circulate about the salary offers he was receiving from various universities.<sup>241</sup> By February 1927, Wisconsin had offered him a professorship with a salary of \$7,500, at a time when Boring’s salary, at Harvard, was \$5,500.<sup>242</sup> Within three months, Koffka accepted a professorship at Smith College, in Northampton, Massachusetts, with a salary rumored to be around \$10,000.<sup>243</sup> Several commentators have complained that Smith did not give Koffka the institutional base in American psychology that a man of his stature deserved. But he seemed happier at a smaller institution than he had been at such large schools as Cornell and Wisconsin, and if the rumors about his salaries were correct, he had financial reasons to go to Northampton.<sup>244</sup> Meanwhile, he continued his extensive lecturing, and he and his friends continued to publish in the area of Gestalt psychology. At Smith, he even developed a small graduate program and played a major role in training such distinguished psychologists as Molly Harrower and Eleanor J. Gibson.<sup>245</sup> As Boring noted much later, at Smith Koffka “had great influence.”<sup>246</sup>

Karl Bühler was not a member of the Gestalt school, but as a German in America he was often sympathetic to its cause. He was at least as talented as

Koffka, and in the eyes of many Americans, such as Terman, he was a better scientist than either Koffka or Köhler. He had been professor at the University of Vienna since 1922, and while visiting Stanford in the late 1920s he greatly impressed both the faculty and his students. But even Terman had to admit Bühler "lacked . . . culture and aristocratic bearing," factors that played, and still play, important roles in academic appointments. Worse than this, Bühler's command of the English language was "wretched," and wherever he taught he had to read his lectures "almost entirely from manuscript."<sup>247</sup> Previous accounts of Bühler's interactions with American psychologists have stressed his tenure in the 1940s at such colleges as St. Scholastica College in Duluth and the College of St. Thomas in St. Paul, and concluded that "he was certainly one man whom America did not welcome with open arms."<sup>248</sup> He returned to Europe in the early 1930s, where such Americans as Boring and Terman followed his career closely. In 1938, they and Edward C. Tolman helped him return to this country, and arranged positions for him and his wife, Charlotte Bühler, a distinguished child psychologist. For many years thereafter, Terman, at least, helped look after the Böhlers' interests.<sup>249</sup> Karl Bühler was *not* accepted as part of the leadership of the American psychological community, but that is no reason to conclude, as others have implied, that this lack of recognition was scandalous.

Kurt Lewin's case is in many ways more interesting, and more gratifying, than Koffka's or Karl Bühler's. As noted, Lewin's presentations at the 1929 International Congress were great successes, particularly the two films he showed of children, including his son, behaving in various situations. Like Karl Bühler's, his English was not good, but he was not afraid to use any slang or gesture in an attempt to communicate, and his audiences, both formal and informal, always seemed able to understand him.<sup>250</sup> Almost immediately, various American universities attempted or planned to arrange visiting appointments for him, but not until 1931 was a term at Stanford arranged for the following year. Meanwhile, psychologists described him as "the man of the hour," and "the young dynamo" of Berlin, and during the summer of 1931, when a group of six New England psychologists met for dinner, they found that the principal topic of conversation was Lewin's work.<sup>251</sup>

Despite this great reputation, and the fact that he thought quite highly of Lewin and his work, Terman still had to make various inquiries about Lewin before even a visiting appointment could be made at Stanford. For example, he wrote to Boring and asked

whether Lewin is a Jew. It would not necessarily be fatal to his appointment here if he were, but it would be best for me to know the facts if I were recommending him. The few Jews we have on our Stanford faculty have no trace whatever of the objectionable traits usually attributed to Jews, and against this kind I haven't the slightest prejudice in the world.<sup>252</sup>

Boring had to ask around to answer Terman's question, and finally a former student of his, Carroll Pratt, who had just returned from Berlin, informed him that Lewin was a Jew.<sup>253</sup> Apparently, this did not matter, and his appointment as a visiting professor at Stanford was a success from all points of view. Terman and

his students and colleagues were greatly impressed with Lewin's psychological ideas and teaching, but, more than this, grew to like him in a warm and personal way. Of all of the Gestalt psychologists, Lewin was, in Boring's words, "anything but self-important after the German manner," and, in general, was the least formal of this group. Even in Germany, while his "careful systematic building . . . [was] being carefully watched with the greatest respect by everyone," and he was having "a great many students working under him," he "seemed to be on the most friendly terms with everyone . . . a most agreeable person."<sup>254</sup> In 1933, when Hitler came to power, Lewin was visiting Japan, and cabled both Boring and Terman in hopes of getting a position in the United States. Both were concerned about the situation, and Terman's words about what Lewin meant to him stand in contrast to his earlier inquiries about his ethnic background:

I have always been intending to write to you particularly to tell you how highly we appreciated Lewin. . . . His work commanded the respect of our students, both graduate and undergraduate, and of our department faculty. . . . Faculty and students became so fond of him that it was hard to let him go. I have known few people who were so alive to everything about them, or so genial and friendly."<sup>255</sup>

Neither Boring nor Terman was able to get Lewin a position in the United States, despite Terman's personal appeal to Alvin Johnson of the New School's University in Exile and some frantic if ineffective efforts on the part of Boring and Koffka.<sup>256</sup> Ogden finally arranged for him a position in Cornell's School of Home Economics—an institutional base that previous writers sympathetic to the Gestaltists have complained about—which allowed him to continue his studies of child development at the Nursery School. This was to be only the beginning of his permanent and outstanding career in the United States, which Terman and Boring followed and assisted as friends and admiring colleagues.<sup>257</sup>

But to many people in the United States, Gestalt psychology in the 1920s meant the work of Wolfgang Köhler, and the history of his interaction with American psychology through the mid-1930s reveals much about the way in which Gestalt ideas and Gestalt psychologists were received in America. This fact, as well as the fact that many rumors have recently been circulating concerning Köhler's relations with several American psychologists, justifies a fairly extensive treatment of this topic.<sup>258</sup> Köhler first had contacts with American psychology as early as 1914, before World War I began, when he was working with chimpanzees at the Anthropoid Research Station on Tenerife, in the Canary Islands. Early that year, Yerkes, then a young assistant professor at Harvard, wrote to Köhler, expressing interest in his work, asking for further information about it, and hoping to be able to join the German off the coast of Africa at the station. The outbreak of the European War, and Köhler's internment on Tenerife, soon brought an end to Yerkes's travel plans, but the two psychologists soon began to exchange reprints, and the American even arranged for John B. Watson to send Köhler a set of his articles. Yerkes also arranged to have Köhler's motion picture films of his chimpanzee experiments processed in the United States when this became impossible on the Islands. This friendly and mutually profitable exchange was marred in 1916,

when Yerkes published *The Mental Life of Monkeys and Apes* and did not cite Köhler's work. And with the American entry into the War in 1917 the relationship came to a temporary close.<sup>259</sup>

Soon after the War, Yerkes utilized his temporary position with the National Research Council to reestablish contacts with Köhler. By 1921 the two psychologists were again exchanging books, reprints, and congratulations on each other's appointments: Köhler's at Berlin and Yerkes's at Yale. Yerkes even offered to send money to Köhler to cover the cost of the books and reprints, in view of the deterioration of the economic situation in Germany and "the unfairness of the exchange situation" but Köhler would not accept.<sup>260</sup> By 1923, the two men were again learning much from a correspondence they both apparently enjoyed.

Then, in 1924, Carl Murchison at Clark University arranged for Köhler to serve as visiting professor at the Worcester institution during 1925, and the reaction of most American psychologists was joyful. Terman, a Clark alumnus who had been bemoaning the condition of psychology at his alma mater, was especially pleased. As he wrote to a Clark official, "It was a splendid stroke to get Dr. Koehler to come over."<sup>261</sup> Both Boring and Yerkes wrote to congratulate Köhler on his appointment and were among the first to write him letters of welcome to America. Boring's welcome was particularly enthusiastic: "the psychological stock of America took a jump upward as soon as I heard you were safely on shore."<sup>262</sup> Once Köhler arrived in Worcester, Boring and Yerkes saw him regularly, and the visitor spoke at least once at both Harvard and Yale.<sup>263</sup> Boring even attended a weekly seminar led by Köhler at Clark, which he described as "great fun."<sup>264</sup> Meanwhile, Köhler met with other members of the American psychological community, and impressed most of them. For example, Köhler, with Koffka, attended a meeting of the Society of Experimental Psychologists as its guests, two of the very few ever to do so. During a visit to Stanford, Köhler impressed Terman as "an intellectually active man" with "youth and vigor."<sup>265</sup> Yerkes also recommended Köhler and Koffka to the home secretary of the National Academy of Sciences as "two of the foremost German psychologists," in an effort to get for them a place on the Academy's programs, and an official of the Rockefeller Foundation referred to Köhler's movies on apes as "interesting scientifically . . . [and] . . . highly entertaining."<sup>266</sup> By 1926 Ogden began arrangements for the translation and publication in English of several of Köhler's German books. He never completed these arrangements, but the fact that he began them indicates the importance to American psychology of Köhler's ideas at that time.<sup>267</sup>

Soon after Köhler started lecturing at Clark in January 1925, Boring and James H. Woods, the chairman of the Department of Philosophy and Psychology at Harvard, began making plans to invite Köhler to Cambridge the following fall as a Visiting Professor. Köhler himself was intrigued with the idea for a while, and wrote of being particularly interested in working with Harvard graduate students. Boring himself presented to Köhler many details about the situation at Harvard, including lists of facilities available, and concluded that "I am very anxious for you to come."<sup>268</sup> These plans fell through that spring, however, as Köhler concluded that his responsibilities to his colleagues in Germany were pressing and, as Boring wrote to Yerkes, he "did not feel he could extend his leave from Berlin."<sup>269</sup>



Köhler indicated, however, that he would welcome a visiting position at Harvard some time in the future, and Boring and Woods reacted positively to this idea, projecting such an appointment for the fall of 1926. The two Americans even began planning a curriculum into which lectures and seminars that Köhler might teach would fit, and by March 1926 were pressuring the German to accept their offer. Köhler cabled, "Sorry can not decide before May," but wrote soon afterward to explain that financial considerations made it impossible for him to teach at Harvard, as had been proposed.<sup>270</sup> Throughout this episode, all correspondence was polite and even cordial, and both Boring's hopes and Köhler's regrets were apparently honest. But it was a trying experience for all three men.

Another possibility for Köhler to come to Harvard opened up in December 1926, when McDougall resigned his position, and left Cambridge for Duke. Both Boring and Woods immediately thought of Köhler as McDougall's successor, and such members of their department as the distinguished philosopher Ralph Barton Perry urged that Köhler be offered the position immediately. But Boring was beginning to doubt Köhler's suitability for Harvard. The university's major need, Boring thought, was for an experimentalist working in the mainstream of American experimental psychology, unlike McDougall or Köhler. Sometime in 1925 Köhler had visited Harvard from Clark, and spoke at a departmental colloquium. Boring was greatly disappointed in the talk, and later described it as being full of "general theoretical analogies [and] unformulated psychological events [with] not [one] bit of experimentation."<sup>271</sup> But Harvard's philosophers, especially Woods and Perry, admired Köhler greatly and Boring, for the moment, agreed to ask the Gestalt psychologist to join the Harvard faculty.<sup>272</sup>

It was not easy, however, to invite Köhler to Harvard, as he did not respond to two of Boring's letters on the subject. A third letter from Boring, and one from Woods, did bring a reply, but it did not clarify the situation extensively. To be sure, some of the correspondence between these three men has apparently been lost, so the details of the episode are unclear, but even those who saw all the letters could not agree what Köhler, at least, intended. As Boring wrote to Terman, "my own interpretation of the correspondence is that he will not accept . . . [but] some at Harvard interpret the same letters as meaning that he is coming." And in the one letter of Köhler's that has survived, he was clearly ambivalent about the invitation: "When I think of you and my other friends at Harvard the choice looks simple. But when I look at the economic situation and the number of lectures to which an American Professor tends to be obligated, then my face drops."<sup>273</sup> In August 1927, the situation was unresolved and not until that fall did Köhler definitely notify Harvard that he would keep his Berlin professorship. He apparently wrote to Boring to apologize for keeping Harvard in suspense for as long as he had. Boring tried to be conciliatory, but he could not totally hide his annoyance at what had happened. As he wrote to Köhler, "after all what is Harvard against Berlin? . . . That you have left us in the lurch and we are still limping along is certainly not your fault, but is entirely our responsibility." It is no wonder then that he later described this period as "the summer that Köhler blew up on us."<sup>274</sup>

Harvard still had to appoint a successor to McDougall and the philosophers

continued to urge that a European be chosen for the position. Boring still wanted an American and an experimentalist and wrote critically of European psychologists who performed "the scissors and cardboard kind of experiments [that] do not reflect favorably upon Harvard's psychology in America." In fact, he was greatly concerned about the reputation of psychology at Harvard and about "criticism from the men whose opinion I respect, and in whose judgment I concur."<sup>275</sup> When Köhler's name was again mentioned for the position, he blew up. "American psychologists who felt that Harvard had made a mistake with both Münsterberg and McDougall would feel that it again erred."<sup>276</sup> But Boring's outburst got him nowhere, as his colleagues in philosophy still wanted Köhler for Harvard. As he expressed the situation in the fall of 1928.

The issue is out in the open. It is between A and B.

A. Breadth of interest, vision and imagination

B. Technical skill and knowledge within a given field.

We want both. We can not have them. Actually they are negatively correlated.

Hocking, Perry and Woods are for A and thus for a renewal of the offer to Köhler.

I am for B and thus can not conscientiously agree to Köhler.<sup>277</sup>

Boring was also annoyed that the philosophers were trying to tell the psychologists how to manage their affairs. His protests over the way in which the entire situation had been handled were later a major reason why philosophy and psychology at Harvard were administratively reorganized in the early 1930s.<sup>278</sup>

Meanwhile, Boring had been devoting all of the time that he could spare from Harvard matters to the study of the history of psychology, in preparation for the writing of his well-known book, published in 1929. This research, and his quarrels with his Harvard colleagues about Köhler, led him to reconsider his early enthusiasm for Gestalt psychology, which, after all, he had never expressed in print. Several of his friends had always been critical of the school—e.g., Margaret Floy Washburn<sup>279</sup>—and Boring had always been sensitive to the opinions of other psychologists. Some of them thought highly of the work of one or another Gestalt psychologist, but criticized that of others. Terman, for example, found the work of Lewin exciting, but by 1927 began to have qualms about Koffka's and Köhler's. He described a talk by Koffka at Stanford as "piffle," and, though somewhat impressed by Köhler's writings, thought little of Köhler's future as a psychologist: "I doubt . . . he will ever do much more experimenting."<sup>280</sup> Terman was also put off by what he felt was the propagandizing of the Gestalt psychologists, which he thought detracted from the merit of their school's ideas, and Boring began to be bothered by this aspect of his relationships with Koffka and Köhler.<sup>281</sup>

Of course, the Gestalt psychologists were not the only ones of the 1920s to argue that their school possessed the only valid approach to psychology, but something about their attitude especially bothered the Americans. To many, it appeared that the Gestalt psychologists had come to the United States almost as intellectual missionaries, spreading the new gospel. Wertheimer's biographer and son writes of his father as a "Gestalt Prophet" and of his views of Gestalt psychol-

ogy as "indeed an all encompassing religion." In 1967, Köhler just about admitted sharing a similar perspective by quoting a remark by Lashley about Gestalt psychology: "Excellent work—but don't you have religion up your sleeve?" And as late as 1943, Köhler still implied that Gestalt psychology helped civilize American psychology.<sup>282</sup> The term *Mandarin* has been well used to characterize the attitudes and behavior of many of the German university professors of the period,<sup>283</sup> and while in some ways the entire Gestalt movement represented a revolt against traditional German university culture, in other, deeper ways the Gestalt psychologists shared many of the traits of their colleagues who made up the faculty of most German universities.<sup>284</sup> None of the Americans of the period ever described a Gestalt psychologist as a "Mandarin," but they probably would have easily recognized the characterization.

In 1929, *A History of Experimental Psychology* was published, and throughout the ten-page discussion of Gestalt psychology, Boring's tone was critical. Apparently he believed that he tried to be fair to the school—he later noted that "it is a question as to whether I have been favorable or unfavorable"—but such friends of Gestalt psychology as R. M. Ogden had little doubt as to Boring's actual feelings.<sup>285</sup> His analysis of the school continually stressed its origins as a "psychology of protest" against the older, atomistic theories of psychology. And while he admitted that "if this negative element were all that there is to *Gestalt* psychology, it would never have become an important movement," most of his discussion revolved around its criticisms of older ideas. Furthermore, he regularly stressed the continuity of Gestalt ideas with older theories, and criticized the Gestalt psychologists because they "made little effort to show the antiquity of the[ir] objection."<sup>286</sup> One can readily see why Ogden felt attacked.

The following year, Boring published a note on "The *Gestalt* Psychology and the *Gestalt* Movement," in which he wrote well of the former, but criticized the latter.<sup>287</sup> He tried to explain the note to Köhler before it appeared, sending galley proofs of it to Berlin, and expanding a bit on his own ambivalence. As he noted:

Sometimes I seem to be so enthusiastic about it and sometimes so negative. My enthusiasm is for the research that has come out under this label. . . . On the other side I get very angry about the label of *Gestalt* psychology and its solidarity as a new movement.<sup>288</sup>

If Köhler responded to Boring's letter or to the article, his answer has been lost, but Koffka's reaction was calm, reasoned, and cordial. He argued "that the *Gestalt movement* has been created not by the Gestalt psychologists but by their opponents," stressing that he had been "overwhelmed with the intrinsic beauty and fruitfulness of Wertheimer's new approach and . . . wanted to share the gift I received with all other psychologists." He further claimed that "many misunderstandings may have been caused by the fact that Köhler and I were invited to give so many public lectures," from which "concrete details" had to be omitted.<sup>289</sup> There was some validity in Koffka's points, especially in view of the long list of critical papers on Gestalt psychology that Boring presented in his history, and Koffka's attitude towards his public lectures might certainly explain why Terman thought the one he heard was "piffle."<sup>290</sup> Boring conceded at least some validity to



Koffka's rebuttal. But at the same time he harked back to the seminar that Köhler had given in 1925 at Harvard that had impressed him so unfavorably. Boring made sure to stress that these specific criticisms did not apply to Koffka, but though he was cordial, Boring clearly was not happy with Gestalt psychology, or at least the Gestalt school.<sup>291</sup>

In the years that followed, the number of articles in American psychology journals critical of Gestalt psychology, and especially of Köhler's work, increased, perhaps as more and more psychologists became familiar with the school.<sup>292</sup> Meanwhile, Koffka continued teaching at Smith, Boring remained at Harvard, and Köhler returned to Berlin, while still carrying a heavy load of popular lecturing. These three men all continued to correspond regularly, and cordially, with each other and all continued their scientific work. Boring for example, drifted toward behaviorism and published *The Physical Dimensions of Consciousness*, an attack on dualism that he later spoke of as "immature" and which a friendly colleague later called a "silly little thing."<sup>293</sup> In Germany, the rise of Hitler did not immediately affect Köhler, as he was an "Aryan," but he clearly felt uncomfortable under the Nazis and was one of the few non-Jewish scientists in Germany to oppose the regime.<sup>294</sup> In December 1933, the Harvard Division of Philosophy and Psychology asked Köhler to come to Cambridge as the third William James Lecturer, after John Dewey and Arthur O. Lovejoy, to deliver a course of ten or twelve public lectures, and conduct a seminar for graduate students. Boring himself wrote to Köhler that *both* the philosophers and psychologists in the division wanted the Gestalt psychologist to accept the appointment, and that he himself was especially hopeful of hearing Köhler again. Köhler responded as he did to so many previous invitations from Harvard, writing that he would love to accept "this invitation, which I regard as an unusual honour," and stressing that "it should have been impossible for me to accept without knowing about your point of view." He did not accept immediately, citing the difficult problems he faced at the University of Berlin with regard to the Nazi-controlled administration.<sup>295</sup>

Köhler soon afterward accepted the invitation, and arrived in Cambridge in September 1934 to begin his lectures and seminar. Boring attended the series of formal talks and apparently hoped to see a good deal of Köhler, arranging at least one social event for the visitor and his wife. But on the whole he was greatly disappointed in that Köhler spent most of his time with Harvard's distinguished philosophers, especially Perry, and the two psychologists rarely interacted. By November, the situation became so bad that Boring felt that he had to write to Köhler, because "things are so disposed that we are not thrown together for conversation," in order to discuss some points that had been raised in the lectures.<sup>296</sup> But, worst of all from Boring's perspective, he felt strongly that Köhler's lectures were very, very poor. Other psychologists agreed with Boring, and when the lectures were published four years later as *The Place of Value in a World of Facts*,<sup>297</sup> despite some very favorable reviews in philosophy journals and in the popular press,<sup>298</sup> the reaction from professional psychologists was typically quite negative.<sup>299</sup> In 1934, at least one psychologist wrote to Boring that the single

lecture of Köhler that he heard did not impress him, and Boring's reply reveals much:

You commented on being disappointed in a lecture which you heard recently. I can say only that I heard the whole series and am terribly disappointed, and a little humiliated at the knowledge that I took the time to go to them. The content was not well informed nor related to current knowledge. The ideas were not important or clear. Most of the argument was childishly elementary, although I caught suggestions of something sinister behind the scenes once in a while—but I was never sure. The vocal presentation was dull and tiresome, although the literary exposition was, if you could grasp it, exceptionally able. This then is what we applaud so heartily!<sup>300</sup>

To be sure, Boring himself was at a critical point in his life, and was soon to undergo psychoanalysis in an attempt to free himself from the despondency that plagued him throughout the mid-1930s.<sup>301</sup> But his criticisms of Köhler were as much intellectual as personal. Apparently, soon after Köhler completed his William James lectures, the philosophers at Harvard again urged that he be appointed professor at the university and, apparently, with the situation in Germany worsening, Köhler was more open to such an appointment than he ever had been before. But Boring, as director of the Psychological Laboratory, and as head of the Department of Psychology within the Division of Philosophy and Psychology, again adamantly opposed the appointment.<sup>302</sup> This opposition was final and lost him several of his Harvard friends, which in turn contributed to his despondency, and led also to the rumors about the relations between Köhler and Boring that are still heard in the late 1970s.<sup>303</sup> In any event, by the end of 1935 Köhler had settled at Swarthmore College where he established an institutional base for his work comparable to Koffka's at Smith. There he played a major role in training such distinguished psychologists as Mary Henle, Solomon Asch, and Robert B. MacLeod. Within a year or two Boring and Köhler were again corresponding cordially, and by the mid-1940s Boring readily admitted Köhler's great influence on American psychology. In fact, as soon as he became a U.S. citizen, Köhler was immediately elected to the National Academy of Sciences.<sup>304</sup>

### Closing Comments

It is difficult, of course, to end an essay gracefully when its conclusions have already been presented and discussed in its Introduction. However, for one appearing in a volume of similar papers, a few historiographic remarks may be appropriate. If this essay does nothing else, it at least illustrates that the social history of psychology can be written. That is, psychologists do not work in a social vacuum, and a history of the people who identified themselves as psychologists, and of the community and profession in which they worked, can, perhaps, be as interesting as any discussion of the history of psychological ideas. Moreover, such a social history of psychology can reveal the ways in which psychological ideas originated, developed and changed through time, and influenced the world in which they emerged, in a way that a traditional history of

psychological ideas may miss. The point here is *not* that the history of ideas is unimportant, or that it should be abandoned, or that its practitioners write mediocre history. The other articles in this volume well illustrate the very real strengths of such an "internal" approach to the history of psychology. But there are other ways to write this history and, when this essay was first written—in 1974—few of those who wrote the history of psychology realized the possibility of other approaches. Since that date, other historians and psychologists have begun writing excellent social history of psychology, and this essay may therefore be viewed as one of a several examples of the emergence of the new historiographic approach. Therefore, if it serves as a marker in the growth of the historiography of psychology, this paper will have well served its exploratory purpose.

### ACKNOWLEDGMENTS AND NOTE ON SOURCES

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American Psychological Association (APA) Papers, Manuscript Division, Library of Congress, Washington, D.C.

James R. Angell Papers, Yale University Archives, New Haven, Conn.

Bird T. Baldwin files, Faculty Information Records, University of Iowa Archives, Iowa City, Iowa

- Walter Van Dyke Bingham Papers, Carnegie-Mellon University Archives, Pittsburgh, Penn.
- Edwin G. Boring Papers, Harvard University Archives, Cambridge, Mass.
- Elsie O. Bregman Papers, Archives of the History of American Psychology, University of Akron, Akron, Ohio
- Leonard Carmichael Papers, American Philosophical Society Library, Philadelphia, Penn.
- James McKeen Cattell Papers, Manuscript Division, Library of Congress, Washington, D.C.
- Harry L. Hollingworth Papers, Archives of the History of American Psychology, University of Akron, Akron, Ohio
- Institute of Child Welfare Papers, University of Iowa Archives, Iowa City, Iowa
- Truman L. Kelley Papers, Harvard University Archives, Cambridge, Mass.
- Kurt Koffka Papers, Archives of the History of American Psychology, University of Akron, Akron, Ohio
- Wolfgang Köhler Papers, American Philosophical Society Library, Philadelphia, Penn.
- Laura Spelman Rockefeller Memorial (LSRM) Archives, Rockefeller Archive Center, North Tarrytown, New York
- Kurt Lewin files, Faculty Information Records, University of Iowa Archives, Iowa City, Iowa
- George and Jean Mandler Papers, Archives of the History of American Psychology, University of Akron, Akron, Ohio
- Hugo Münsterberg Papers, Boston Public Library, Boston, Mass.
- Department (and Division) of Philosophy Papers, Harvard University Archives, Cambridge, Mass.
- Rockefeller Foundation (RF) Archives, Rockefeller Archive Center, North Tarrytown, New York
- Walter Dill Scott Presidential Papers, Northwestern University Archives, Evanston, Illinois
- Carl Emil Seashore files, Faculty Information Records, University of Iowa, Iowa City, Iowa
- B. M. Stigall Papers, University of Missouri Archives, Columbia, Missouri
- Lewis M. Terman Papers, Stanford University Archives, Stanford, California
- Edward B. Titchener Papers, Cornell University Archives, Ithaca, New York
- Robert M. Yerkes Papers, Yale University Archives, New Haven, Conn.

## NOTES

Details concerning manuscript collections cited will be found at the end of this essay under the heading "Acknowledgments and Note on Sources."

1. Cattell, "Some Psychological Experiments," *Science* 63 (1 January 1926): 1-8; (8 January 1926): 29-35. This paper, with many others by Cattell, is reprinted in *James McKeen Cattell, Man of Science*, 2 vols., ed. A. T. Poffenberger (Lancaster, Pa.: The Science Press, 1947), 2: 381-406. In general, the

versions of Cattell's papers cited in this essay will be those found in these volumes. (Hereafter cited as *Man of Science*.)

2. Ibid., 2: 404.

3. E.g., ibid., 2: 381. Cattell's concern with quantification runs throughout his career, and relates directly to an early exposure to the ideas of Comtean Positivism. See Michael M. Sokal, ed., *An Education in Psychology: James McKeen Cattell's Journal and Letters from Germany and England, 1880-1888* (Cambridge, Mass.: MIT Press, 1981), p. 16; idem, "The Education and Psychological Career of James McKeen Cattell, 1860-1904" (Ph.D. dissertation, Program in History of Science and Technology, Case Western Reserve University, 1972; University Microfilms order no. 73-6341).

4. E.g., Cattell, "Some Psychological Experiments," pp. 384, 389. See also Cattell, "Über die Zeit der Erkennung und Benennung von Schriftzeichen, Bildern und Farben," *Philosophische Studien* 2 (1885): 636-50; idem, "On the Time Required for Recognizing and Naming Letters and Words, Pictures and Colors," in *Man of Science* 1: 13-25; idem, "The Time Taken Up By Cerebral Operations," *Mind* 11 (1886): 220-42, 377-92, 524-38; *Man of Science*, 1: 41-94.

5. E.g., Cattell, "Some Psychological Experiments," p. 395; Edward Lee Thorndike, "Animal Intelligence," *Psychological Review Monographs* 2, no. 8 (1898).

6. Besides *Science*, Cattell edited the *Psychological Review* (1894-1904), *Popular Science Monthly* (1900-1915), the *American Naturalist* (1907-44), *Scientific Monthly* (1915-43), *School and Society* (1915-39), and the first six editions (1906, 1910, 1921, 1927, 1933, 1938) of *American Men of Science*.

7. On the failure of Cattell's testing program, see Michael M. Sokal, "James McKeen Cattell and the Failure of Anthropometric Mental Testing, 1890-1901," in *The Problematic Science: Psychology in Nineteenth-Century Thought*, ed. William R. Woodward and Mitchell G. Ash (New York: Praeger, 1982), pp. 322-45.

8. See Carol Singer Gruber, "Academic Freedom at Columbia University, 1917-1918: The Case of James McKeen Cattell," *AAUP Bulletin* 58 (1972): 297-305.

9. See "Proceedings of the Thirty-Second Annual Meeting of the American Psychological Association," *Psychological Bulletin* 21 (1924): 69-120; Cattell to Henry Cabot Lodge, 24 May 1923, 1 June 1923, 16 June 1923, Cattell Papers.

10. Edwin G. Boring to Cattell, 9 December 1927, Boring Papers; *Ninth International Congress of Psychology: Proceedings and Papers* (Princeton, N.J.: Psychological Review Co., 1930).

11. Of course, the overview of this topic presented in this essay reflects the author's own interests and perspective, and a totally different paper could be, and, in fact, has been, written on the same general topic. See the brilliant paper by John C. Burnham, "The New Psychology: From Narcissism to Social Control," in *Change and Continuity in Twentieth-Century America: The 1920's*, ed. John Braeman, Robert H. Bremner, and David Brody (Columbus, Ohio: Ohio State University Press, 1968).

12. It can be argued—with much justification—that American psychology was more open than most other academic areas during the 1920s and even in the years before the decade. After all, a Roman Catholic priest—Edward A. Pace—was a charter member of the American Psychological Association, and two women—Mary W. Calkins and Margaret F. Washburn—had served as its president. But of the other leaders of psychology who did not fit into the general pattern, most conformed in more ways than they differed. For example, among the readily identifiable Jews, one—Joseph Jastrow—was the son of a leading "Liberal" rabbi, and another—Hugo Münsterberg—had himself baptized when he came of age. See *DAB*, 13: 337-39; 3d supp., 383-84; Matthew Hale, Jr., *Human Science and Social Order: Hugo Münsterberg and the Origins of Applied Psychology* (Philadelphia: Temple University Press, 1980), pp. 19-20. In the same way, of the very few individuals who were not native born, at least two—George S. Fullerton and Charles H. Judd—were born in India of missionary parents and another two—Edward B. Titchener and Münsterberg—came to the United States as professors. See *DAB*, 7: 66-67; 18: 564-65; 4th supp., 443-46.

13. A popular history that well illustrates this aspect of the 1920s is Frederick Lewis Allen, *Only Yesterday: An Informal History of the Nineteen-Twenties* (New York: Harper and Brothers, 1931).

14. Lewis, *Babbitt* (New York: Harcourt, Brace and Company, 1922).

15. Robert V. Guthrie, *Even the Rat Was White: A Historical View of Psychology* (New York: Harper and Row, 1976).

16. Leon Kamin, *The Science and Politics of I.Q.* (Kensington, Md.: Lawrence Erlbaum Associates, 1974); Stephen Jay Gould, *The Mismeasure of Man* (New York: W. W. Norton, 1981).

17. Anne Anastasi, "Reminiscences of a Differential Psychologist," in *The Psychologists*, ed. T. S. Krawiec, vol. 1 (New York: Oxford University Press, 1972), pp. 3-37.

18. See the articles cited in Burnham, "The New Psychology"; in Lucille C. Birnbaum, "Behaviorism in the 1920's," *American Quarterly* 7 (1955): 15-30; and below.

19. Stephen Leacock, "A Manual of the New Mentality," *Harper's* 148 (1924): 471-80.

20. Cattell, "University Presidents Who Have Been Psychologists," *Scientific Monthly* 45 (1937): 473-77.

21. See Burnham, "The New Psychology," and Birnbaum, "Behaviorism in the 1920's."
22. Interview with Anne Anastasi, 20 November 1973.
23. Jean Matter Mandler and George Mandler, "The Diaspora of Experimental Psychology: The Gestaltists and Others," in *The Intellectual Migration: Europe and America, 1930-1960*, ed. Donald Fleming and Bernard Bailyn (Cambridge, Mass.: Belknap Press of Harvard University Press, 1969), pp. 371-419. See also Wolfgang Köhler, "The Scientists from Europe and Their New Environment," in *The Cultural Migration: The European Scholar in America*, ed. W. Rex Crawford (Philadelphia: University of Pennsylvania Press, 1953), pp. 112-37.
24. For a similar conclusion, see also some of the papers in *Schools of Psychology: A Symposium*, ed. David L. Krantz (New York: Appleton-Century-Crofts, 1969).
25. Robert S. Woodworth, *Contemporary Schools of Psychology* (New York: Ronald Press Co., 1931).
26. Woodworth, "Dynamic Psychology," in *Psychologies of 1930*, ed. Carl Murchison (Worcester, Mass.: Clark University Press, 1930), pp. 327-36.
27. Burnham, "Psychology, Anthropology and Sociology: Change in American Thought in the 1920's" (M.A. thesis, Department of History, University of Wisconsin, 1952).
28. The wide range of books, dissertations, and articles on this topic, both older and recent, makes an elaborate history of psychological testing during the 1920s unnecessary here. What follows, therefore, is a largely interpretive overview. Among the more important general discussions of testing during this period are: Hamilton Cravens, *The Triumph of Evolution: American Scientists and the Heredity-Environment Controversy, 1900-1941* (Philadelphia: University of Pennsylvania Press, 1978), chap. 7, "Mental Testing," pp. 224-65; Joseph Peterson, *Early Conceptions and Tests of Intelligence* (Yonkers, N.Y.: World Book Co., 1925); Thomas Pogue Weinland, "A History of the I.Q. in America, 1890-1941" (Ph.D. dissertation, Joint Committee on Graduate Instruction, Columbia University, 1970; University Microfilms, order no. 73-8991); Russell Marks, "Testers, Trackers and Trustees: The Ideology of the Intelligence Testing Movement in America, 1900-1954" (Ph.D. dissertation, School of Education, University of Illinois, 1972; University Microfilms, order no. 73-17,311); Andrew T. Wylie, "A Brief History of Mental Tests," *Teachers College Record* 23 (1922): 19-33. Other important specific discussions are cited below where appropriate.
29. Cattell, "The Interpretation of Intelligence Tests," *Scientific Monthly* 18 (1924), 508-16; *Man of Science*, 2: 376.
30. Cattell, "Some Psychological Experiments," p. 396.
31. E.g., Lewis M. Terman, *The Measurement of Intelligence: An Explanation of and a Complete Guide for the Use of the Stanford Revision of the Binet-Simon Intelligence Scale* (Boston: Houghton Mifflin, 1916); Robert M. Yerkes et al., *A Point Scale for Measuring Mental Ability* (Baltimore, Md.: Warwick and York, 1915).
32. Weinland, "The I.Q. in America," pp. 120-25.
33. Yerkes to Bingham, 13 January 1916, Yerkes Papers; Melvin E. Haggerty to Bingham, 22 March 1916, 1 April 1916; Bingham to Haggerty, 30 March 1916; A. H. Sutherland to Bingham, 4 November 1916; Bingham Papers.
34. See Yerkes to Bingham, 15 January 1917, Yerkes Papers; J. W. Baird to Bingham, 19 January 1917; Bingham to J. Carleton Bell, 23 January 1917; Bell to Bingham, 25 January 1917; Bingham Papers.
35. Yerkes to Bingham and Scott, 6 April 1917, 25 April 1917; Scott to Yerkes, 14 April 1917; Bingham to Yerkes, 23 April 1917, 10 May 1917; Bingham Papers.
36. See Thomas Marley Camfield, "Psychologists at War: The History of American Psychology and the First World War" (Ph.D. dissertation, Department of History, University of Texas, 1969; University Microfilms, order no. 70-10,766); Daniel J. Kevles, "Testing the Army's Intelligence: Psychologists and the Military in World War I," *Journal of American History* 55 (1968): 565-81.
37. Cattell, "Practical Psychology," *Science* 53 (14 January 1921): 30-35; *Man of Science*, 2: 359. Some psychologists, however, have argued recently that the tests in general were not very useful to the army. For example, see Franz Samelson, "World War I Intelligence Testing and the Development of Psychology," *Journal of the History of the Behavioral Sciences* 13 (1977): 274-82.
38. Weinland, "The I.Q. in America," p. 132; Bingham to Frederic L. Wells, 2 July 1919, Bingham Papers.
39. Margaret F. Washburn to Bingham, 5 October 1917; Ernest H. Koch, Jr., to Bingham, 23 October 1917; Bingham to Koch, 31 October 1917; Bingham to E. P. Frost, 6 November 1917; Bingham Papers. "The Measurement and Utilization of Brain Power in the Army," *Science* 49 (7 March 1919): 221-26; (14 March 1919), pp. 252-59. Even before the war had ended, however, a little-known pamphlet, entitled *Army Mental Tests*, was published in Washington under the editorship of Terman and Mabel R. Fernald. See Nicholas Pastore, "The Army Intelligence Tests and Walter Lippmann," *Journal of the History of the Behavioral Sciences* 14 (1978): 316-27.



40. Watson to Yerkes, 29 March 1919, Yerkes Papers.
41. *New York Times*, 17 July 1919, p. 8; 23 September 1919, p. 17; Lewis M. Terman, "Intelligence Tests in Colleges and Universities," *School and Society* 13 (1921): 481-94; Weinland, "The I.Q. in America," p. 147. See Bingham to Frederic Palmer, 20 April 1921; Pennsylvania State Educational Association to A. A. Hamerschlag, 8 July 1919; Memorandum, "Society for the Promotion of Engineering Education; Committee No. 22 on Intelligence Tests," 1 November 1919; Bingham Papers.
42. H. C. Morrison to Stigall, 29 September 1920; P. C. Harris to Stigall, 1 February 1921; J. Carlton Bell to Stigall, 23 February 1921; Stigall Papers.
43. Seashore, "Sectioning Classes on the Basis of Ability," *School and Society* 15 (1922): 353-58.
44. Weinland, "The I.Q. in America," pp. 154-57; William M. Proctor, "The Use of Psychological Tests in Educational and Vocational Guidance of High School Pupils," *Journal of Educational Research Monographs*, June 1921, no. 1. See also Roger F. Aubrey, "Historical Development of Guidance and Counseling and Implications for the Future," *Personnel and Guidance Journal* 55 (1977): 228-95.
45. Weinland, "The I.Q. in America," pp. 150-52.
46. Dewey, *School and Society* (Chicago: University of Chicago Press, 1899); Lawrence A. Cremin, *The Transformation of the School: Progressivism in American Education, 1876-1957* (New York: Random House, 1961); Raymond E. Callahan, *Education and the Cult of Efficiency: A Study of the Social Forces that Have Shaped the Administration of the Public Schools* (Chicago: University of Chicago Press, 1962); Helen L. Horowitz, "The Progressive Education Movement After World War I," *History of Education Quarterly* 11 (1971): 79-84.
47. Lee J. Cronbach, "Mental Tests and the Creation of Opportunity," *Proceedings of the American Philosophical Society* 114 (1970): 480-87; Weinland, "The I.Q. in America," pp. 154-57; Matthew T. Downey, *Carl Campbell Brigham: Scientist and Educator* (Princeton, N.J.: Educational Testing Service, 1961), p. 23. It was not until after World War II, however, when college applications rose tremendously, that the older CEEB essay examinations were generally replaced by the SAT. See Diane Ravitch, "The College Boards," *New York Times Magazine*, 4 May 1975, pp. 12ff.
48. Cattell, "Some Psychological Experiments," p. 394; Seashore to Cora B. Hillis, 14 February 1920, Institute of Child Welfare Papers.
49. Burnham, "The New Psychology," pp. 357, 373, 377; Birnbaum, "Behaviorism in the 1920's."
50. Yerkes, editor, *Psychological Examining in the Army*, National Academy of Sciences, *Memoirs* 15 (1921): 698-707.
51. Gilbert G. Gonzalez, "Racism, Education, and the Mexican Community in Los Angeles, 1920-30," *Societas—A Review of Social History* 4 (1974): 287-301.
52. Weinland, "The I.Q. in America," pp. 157-65.
53. Yerkes, *Psychological Examining*, pp. 707, 735.
54. *Ibid.*; Weinland, "The I.Q. in America," pp. 162-63.
55. Grant, *The Passing of the Great Race; or, The Racial Basis of European History* (New York: Charles Scribner's Sons, 1916, 1918, 1920, 1926).
56. Stoddard, *The Revolt Against Civilization* (New York: Charles Scribner's Sons, 1923); Humphrey, "Men and Half-Men," *Scribner's Magazine* 73 (1923): 284-87. It is interesting to note that Scribner's published much of the avowedly racist literature of this period. See also Mark Aldrich, "Progressive Economists and Scientific Racism," *Phylon* 40 (1979): 1-14.
57. McDougall, *Is America Safe for Democracy?* (New York: Charles Scribner's Sons, 1921).
58. Brigham, *A Study of American Intelligence* (Princeton, N.J.: Princeton University Press, 1923); Gould, *America: A Family Matter* (New York: Charles Scribner's Sons, 1920). The close friendship shared by Brigham, Gould, and Yerkes, which helped shape the views of each, can be traced in detail in the Yerkes Papers.
59. Yerkes, "Testing the Human Mind," *Atlantic Monthly* 121 (1923): 358-70.
60. Yerkes to Albert Johnson, 11 February 1921, Yerkes Papers; Thomas F. Gossett, *Race: The History of an Idea in America* (Dallas, Tex.: Southern Methodist University Press, 1963), p. 373; Leon J. Kamin, "The Science and Politics of I.Q.," *Social Research* 41 (1974): 387-425; *idem*, *The Science and Politics of I.Q.* For a more balanced analysis, see Franz Samelson, "On the Science and Politics of the I.Q.," *Social Research* 42 (1975): 467-88.
61. John Higham, *Strangers in the Land: Patterns of American Nativism, 1860-1925* (New Brunswick, N.J.: Rutgers University Press, 1955), pp. 306-24; Helen F. Eckerson, "Immigration and National Origins," *Annals* 367 (1966): 4-14.
62. Terman to Bingham, 27 May 1921, Bingham Papers; Terman, "Adventures in Stupidity," *Scientific Monthly* 14 (1922): 24-40; "Were We Born That Way?" *World's Work* 40 (1922): 656-60.
63. Eddie Baker to E. D. Starbuck, 1 November 1926, Seashore files.
64. Gossett, *Race*, pp. 372-73; Heywood Broun and George Britt, *Christians Only: A Study in*

*Prejudice* (New York: Vanguard Press, 1931); Daniel J. Kevles, *The Physicists: The History of a Scientific Community in Modern America* (New York: Alfred A. Knopf, 1978), pp. 211-13, 278-79; Bruce Kuklick, *The Rise of American Philosophy: Cambridge, Massachusetts, 1860-1930* (New Haven, Conn.: Yale University Press, 1977), pp. 455-58.

65. Brigham to Yerkes, 7 November 1927, Yerkes Papers; Edwin G. Conklin to Terman, 6 February 1923, Terman Papers.

66. J. W. Baird to Bingham, 17 June 1916, Bingham Papers; Boring to Leonard Carmichael, 8 January 1931, Boring Papers.

67. Boring to Carmichael, 2 June 1925, 26 September 1935; Carmichael to Boring, 28 June 1934, Carmichael Papers; Carol Travis, interview with Harry Harlow, *Psychology Today* 6, no. 11 (April 1973): 72-73.

68. Cattell, "The Interpretation of Intelligence Tests," pp. 377-80.

69. Boring, "Intelligence as the Tests Test It," *New Republic*, 6 June 1923, pp. 35-37.

70. Boring, "Facts and Fancies of Immigration," *ibid.*, 25 April 1923, pp. 245-46. See also Yerkes to Boring, 2 September 1922, Boring Papers; Yerkes to Brigham, 3 January 1923; Boring to Yerkes, 23 March 1923, 2 April 1923, Yerkes Papers; Boring to Brigham, 23 March 1923, Boring Papers.

71. Frank N. Freeman, "An Evaluation of American Intelligence," *School Review* 31 (1923): 627-28; A. J. Snow, *American Journal of Psychology* 34 (1923): 304-7; Kimball Young, *Science* (8 June 1923): 666-70.

72. Truman L. Kelly, *Interpretation of Educational Measurement* (Yorkers, N.Y.: World Book Co., 1927). This book summarizes many of the inconsistencies of these tests, and had a great effect on Brigham's later work. See Weinland, "The I.Q. in America," pp. 241-42. An earlier analysis of these tests, stimulated by Brigham's book, made many similar points, but was written by a social worker and a psychiatrist. See Maurice B. Hexter and Abraham Myerson, "13.77 versus 12.05: A Study in Probable Error," *Mental Hygiene* 8 (1924): 69-82.

73. *Literary Digest*, 9 June 1923, pp. 56-57; Charles Leonard Stone, *American Economic Review* 13 (1923): 523.

74. Bagley, "On the Correlation of Mental and Motor Abilities in School Children," *American Journal of Psychology* 12 (1901): 193-205.

75. Bagley, "Educational Determinism; or Democracy and I.Q.," *School and Society* 15 (1922): 373-84; Terman, "The Psychological Determinist; or Democracy and the I.Q.," *Journal of Educational Research* 6 (1922): 57-62; Bagley, "Professor Terman's Determinism: A Rejoinder," *ibid.*, pp. 372-85. See also Weinland, "The I.Q. in America," pp. 197-205.

76. Lippmann, "The Mental Age of Americans," *New Republic*, 25 October 1922, pp. 213-15; *idem*, "The Mystery of the 'A' Men," *ibid.*, 1 November 1922, 246-48; *idem*, "The Reliability of Intelligence Tests," *ibid.*, 8 November 1922, pp. 275-77; *idem*, "The Abuse of the Tests," *ibid.*, 15 November 1922, pp. 297-98; *idem*, "Tests of Hereditary Intelligence," *ibid.*, 22 November 1922, pp. 328-30; *idem*, "A Future for the Tests," *ibid.*, 29 November 1922, pp. 9-11. Lippmann's attacks, and the controversy that followed them, have been well analyzed by Nicholas Pastore, in "The Army Intelligence Tests and Walter Lippmann," and "In Defense of Walter Lippmann," *American Psychologist* 30 (1975): 940-42. See also Weinland, "The I.Q. in America," pp. 184-97.

77. Terman, "The Great Conspiracy, or The Impulse Imperious of Intelligence Testers, Psychoanalyzed and Exposed by Mr. Lippmann," *New Republic*, 27 December 1922, pp. 116-20; Lippmann, "The Great Confusion: A Reply to Mr. Terman," *ibid.*, 3 January 1923, pp. 145-46. (It should be noted that Terman's reference to psychoanalysis was sarcastic, since he shared with most American psychologists of this decade an extreme distaste for what they felt to be the totally unscientific work of Freud and his disciples. Of course, this general topic could well form a section of this paper, but that is now impossible. For Cattell's own view, see "Some Psychological Experiments," *Man of Science*, 2: 389.) Lippmann and Terman continued their debate for a while, and other correspondents also made their views known. See "Correspondence," *New Republic*, 17 January 1923, pp. 201-2; 7 February 1923, pp. 289-90.

78. "Another War Beginning," *New York Times*, 28 December 1922, p. 16. See Terman to Yerkes, 8 January 1923, Yerkes Papers.

79. Yerkes to Lippmann, 28 November 1922, 4 January 1923, 23 January 1923; Lippmann to Yerkes, 6 December 1922, 9 January 1923; Yerkes Papers. Another account of the Lippmann-Terman controversy is Lee J. Cronbach, "Five Decades of Public Controversy Over Mental Testing," *American Psychologist* 30 (1975): 1-14.

80. See Yerkes to Terman, 2 January 1923; Terman to Yerkes, 8 January 1923; Yerkes Papers. Boring, meanwhile, had written to Terman, telling him that he agreed with most of Lippmann's arguments. In response Terman sent an undated postcard claiming to have received "30 or 40 letters" supporting his position. See Boring Papers.

81. Weinland, "The I.Q. in America," pp. 248-51. See Brigham et al., "Second Annual Report of the Commission on Scholastic Aptitude Tests," *Twenty-Seventh Annual Report of the Secretary of the College Entrance Examination Board* (New York: CEEB, 1927).

82. Helen Marshall to Terman, 22 October 1921; Florence Fuller to Terman, 11 March 1922, 3 April 1922; Terman Papers. See also Kelley, *The Interpretation of Educational Measurement*.

83. See Cravens, *The Triumph of Evolution*, pp. 230-35; Weinland, "The I.Q. in America," pp. 210-24.

84. Boas, "This Nordic Nonsense," *Forum* 74 (1925): 502-11. See also Boas, "Fallacies of Racial Inferiority," *Current History* 25 (1927): 676-82.

85. Arthur Woods to Nicholas Murray Butler, 28 May 1925; Boas, "An Investigation of Hereditary and Environmental Influences upon the Development of Man," LSRM Archives.

86. Mead, "Group Intelligence Tests and Linguistic Disability among Italian Children," *School and Society* 25 (1927): 465-68; idem, "Autobiography," in *A History of Psychology in Autobiography*, vol. 6, ed. Gardner Lindzey (Englewood Cliffs, N.J.: Prentice-Hall, 1974), pp. 295-326.

87. Klineberg, "Autobiography," in *A History of Psychology in Autobiography*, vol. 6, pp. 161-82; idem, "Racial Differences in Speed and Accuracy," *Journal of Abnormal and Social Psychology* 22 (1927): 273-77.

88. See Quinn McNemar, "Sampling in Psychological Research," *Psychological Bulletin* 37 (1940): 331-65.

89. Klineberg, "A Study of Psychological Differences Between 'Racial' and National Groups in Europe," *Archives of Psychology*, 1931, no. 132.

90. Klineberg, *Negro Intelligence and Selective Migration* (New York: Columbia University Press, 1934).

91. Brigham to Terman, 27 December 1927, Terman Papers; Klineberg, "Autobiography," p. 167; Downey, C. C. Brigham, pp. 26-27; Brigham, "Intelligence Tests of Immigrant Groups," *Psychological Review* 37 (1930): 158-65.

92. R. B. Cattell, "A Culture-Free Intelligence Test," *Journal of Educational Psychology* 31 (1940): 161-79; 32 (1941): 81-100.

93. E.g., Jensen, "How Much Can We Boost I.Q. and Scholastic Achievement?" *Harvard Educational Review* 39 (1969): 1-123.

94. Anastasi, "Hereditry, Environment, and the Question 'How?'" *Psychological Review* 65 (1958): 197-208; Cravens, *The Triumph of Evolution*, pp. 269-74.

95. Sokal, "James McKeen Cattell and the Failure of Anthropometric Mental Testing." Much of this discussion follows this article.

96. In the 1890s, psychology was the new science, and universities were regularly establishing new laboratories—twenty-six were created between 1890 and 1899; see C. R. Garvey, "List of American Psychology Laboratories," *Psychological Bulletin* 26 (1929): 652-60—and offering high salaries to psychologists. Cattell, for example, doubled his salary in moving to Columbia University from the University of Pennsylvania in 1891.

97. In the 1890s the testers knew that by concentrating on physical measurements they were ignoring what was known as the "higher mental processes." See James Mark Baldwin, et al., "Physical and Mental Tests," *Psychological Review* 5 (1898): 172-79. Throughout the 1920s, many psychologists admitted that they did not have a precise definition for intelligence. See Cattell, "The Interpretation of Intelligence Tests"; Boring, "Intelligence as the Tests Test It."

98. In the 1890s, one of the first attacks on the testing movement came from a student of Edward Bradford Titchener (Stella Sharp, "Individual Psychology: A Study in Psychological Method," *American Journal of Psychology* 10 (1898): 329-91), and the statistical analyses of Cattell's data that showed them to be useless were performed by Clark Wissler ("The Correlation of Mental and Physical Tests," *Psychological Review Monographs*, 1901, no. 6), who had been trained by Boas. In the 1920s, the attacks by Boring were soon followed by those of Bagley, an educator, and Boas, an anthropologist.

99. This point, of course, is the major thrust of Cravens, *The Triumph of Evolution*.

100. See Donald S. Napoli, *Architects of Adjustment: The History of the Psychological Profession in the United States* (Port Washington, N.Y.: Kennikat Press, 1981).

101. For a brilliant analysis of the parallel situation regarding the physical sciences, technology, and the engineering sciences, see Edwin T. Layton, Jr., "Mirror-Image Twins: The Communities of Science and Technology in Nineteenth Century America," *Technology and Culture* 12 (1971): 562-80.

102. In addition to Napoli, *Architects of Adjustment*, two general histories of industrial psychology in the United States are Leonard W. Ferguson, *The Heritage of Industrial Psychology* (Hartford, Conn.: Finlay Press, 1963-68), and Loren Baritz's interesting if one-sided analysis, *The Servants of Power: A History of the Use of Social Science in American Industry* (Middletown, Conn.: Wesleyan University Press, 1960).

103. Frank G. Coolsen, "Pioneers in the Development of Advertising," *Journal of Marketing* 12 (1947): 80-86; Gale, "On the Psychology of Advertising," *Psychological Studies* (Minneapolis: by the author, 1900).

104. See Jacob Z. Jacobson, *Scott of Northwestern* (Chicago: Louis Mariano, 1951); Edmund C. Lynch, "Walter Dill Scott: Pioneer Industrial Psychologist," *Business History Review* 42 (1966): 149-70; David P. Kuna, "The Concept of Suggestion in the Early History of Advertising Psychology," *Journal of the History of the Behavioral Sciences* 12 (1976): 347-53; Kuna, "The Psychology of Advertising, 1896-1916" (Ph.D. dissertation, Department of Psychology, University of New Hampshire, 1976; University Microfilms, order no. 76-26.875).

105. Jacobson, *Scott*, pp. 70-74; Coolsen, "Pioneers," pp. 83-84.

106. Scott, *Theory of Advertising* (Boston: Small, Maynard, 1903), first appeared serially in John Lee Mahin's *Mahin's Magazine*. See Coolsen, "Pioneers," pp. 83-84.

107. See Harry L. Hollingworth, "Prospectus for the Ad League Study Courses in Advertising, 1911-1912," Hollingworth Papers; Hollingworth, *Advertising and Selling* (New York: D. Appleton, 1913). See also Ludy T. Benjamin, Jr., "Harry Levi Hollingworth: Reluctant Pioneer in Applied Psychology" (unpublished paper, American Psychological Association, Chicago, Ill., August 1975).

108. Scott, *The Psychology of Advertising* (Boston: Small, Maynard, 1908); A. Michal McMahon, "An American Courtship: Psychologists and Advertising Theory in the Progressive Era," *American Studies* 13, no. 2 (1972): 5-18.

109. Lynch, "Scott," p. 155; Scott, "Selection of Employees by Means of Quantitative Determinations," *Annals* 65 (1916): 182-93; idem, "The Scientific Selection of Salesmen," *Advertising and Selling* 25, no. 5 (October 1915): 5-6, 94-96; 25, no. 6 (November 1915); 25, no. 7 (December 1915): 11, 69-70.

110. E.g., Elsie Oschrein Bregman, "Vocational Tests for Retail Saleswomen," *Journal of Applied Psychology* 2 (1918): 148-55.

111. Münsterberg, *Psychology and Industrial Efficiency* (Boston: Houghton Mifflin, 1913); Thorndike, "Fundamental Theorems in Judging Men," *Journal of Applied Psychology* 2 (1918): 67-76. See also Baritz, *The Servants of Power*, pp. 35-40.

112. Ferguson, "Industrial Psychology and Labor," in *Walter Van Dyke Bingham Memorial Program*, ed. B. von Haller Gilmer (Pittsburgh, Pa.: Carnegie Institute of Technology, 1962), pp. 7-22; Bingham, "Autobiography," in *A History of Psychology in Autobiography*, vol. 4, ed. Edwin G. Boring et al. (Worcester, Mass.: Clark University Press, 1952), pp. 1-26; Jacobson, *Scott*, pp. 88-97; Bingham to Guy M. Whipple, 5 October 1915, Bingham Papers.

113. Lynch, "Scott," pp. 160-65; Bingham, "Autobiography," pp. 14-15. See also Memorandum, "Committee on Classification of Personnel in the Army," 5 May 1917; Thorndike to Bingham, 1 August 1917, Bingham Papers.

114. *The Personnel System of the United States Army*, vol. 1, *History of the Personnel System Developed by the Committee on Classification of Personnel in the Army (Subsequently the Classification Division, Adjutant General's Department)* (Washington, D.C., 1919); Scott to Newton D. Baker, 4 August 1917; undated memorandum on "The Work of the Committee," "Memorandum on Duties and Personnel of the Committee on Classification of Personnel," 28 September 1918; Scott Papers. See also William Hard, "Captain Smith-77," *New Republic*, 6 July 1918, pp. 283-85.

115. Raymond Dodge, Edward L. Thorndike, Shepherd I. Franz, and Walter V. Bingham, "Psychology as a Life Work," *Science* 57 (13 April 1923): 429-31.

116. Camfield, "Psychologists at War," p. 283, writes of the application of psychology becoming "something of a fad in the nation's business and industrial world in the period following the war." But see also Baritz, *The Servants of Power*, pp. 44-76.

117. Bregman, "The Development and Application of Tests for the Indication of Special Abilities," unpublished report, R. H. Macy and Company, March 1920, Bregman Papers; idem, "Studies in Industrial Psychology," *Archives of Psychology*, no. 54 (September 1922).

118. Samuel Kavruck, "Thirty-Three Years of Test Research: A Short History of Test Development in the U.S. Civil Service Commission," *American Psychologist* 11 (1956): 329-33; Ruml to Bingham, 12 April 1921, Bingham Papers.

119. Leonard W. Ferguson, "The Scott Company: Chapter 15 of *The Heritage of Industrial Psychology*," *JSAS Catalog of Selected Documents in Psychology*, vol. 6, no. 4, (1976), p. 128, MS number 1397.

120. See Sokal, "The Origins of The Psychological Corporation," *Journal of the History of the Behavioral Sciences*, 17 (1981): 54-67.

121. One major problem with Cattell's program for the Psychological Corporation was that it was apparently never spelled out clearly, perhaps because its details were continually in flux, and perhaps

because Cattell never really understood many of the problems involved in the application of psychology, as will be shown below. The closest Cattell ever came to a clear statement of this program was in a special issue of the *Annals of the American Academy of Political and Social Science* on "Psychology in Business." See Cattell, "The Psychological Corporation," *Annals* 110 (1923): 165-71. The discussion of the program presented here is derived from this article and from various unpublished and undated memoranda—e.g., "A Corporation for the Advancement of Psychology," "Memorandum for the Directors of the Psychological Corporation," and "Confidential for Psychologists—The Psychological Corporation"—in the Cattell Papers.

122. Burnham, "Psychiatry, Psychology, and the Progressive Movement," *American Quarterly* 12 (1960): 457-65.

123. Samuel Haber, *Efficiency and Uplift: Scientific Management in the Progressive Era, 1890-1920* (Chicago: University of Chicago Press, 1964).

124. Bingham to Gilbreth, 17 October 1916, 20 November 1916; Gilbreth to Bingham, 25 October 1916, 19 January 1917; J. B. Miner to Gilbreth, 10 November 1917, 17 November 1917, 15 December 1917, 3 January 1918; Miner to Bingham, 15 November 1917; Herbert S. Langfeld to Miner, 17 November 1917; Bingham Papers.

125. "Collection of Data on Child Prodigies—Gilbreth," Terman Papers.

126. Edna Yost, *Frank and Lillian Gilbreth: Partners for Life* (New Brunswick, N.J.: Rutgers University Press, 1949).

127. Emma H. Guntler to Bregman, 14 February 1927, Bregman Papers.

128. Baritz, *The Servants of Power*, pp. 30-31.

129. Bingham to J. J. Apatow, 20 May 1916; Ruml to Bingham, 7 April 1917; Watson to Bingham, 16 April 1921; Bingham papers.

130. See Burnham, "The New Psychology," p. 390; Baritz, *The Servants of Power*, pp. 11, 59-60.

131. Cattell, "A Program of Radical Democracy," *Popular Science Monthly* 80 (1922): 606-15. See also Sokal, "The Unpublished Autobiography of James McKeen Cattell," *American Psychologist* 26 (1971): 626-35.

132. Cattell and Gompers exchanged at least ten letters between 20 December 1922 and 9 January 1923. See Cattell Papers.

133. Ferguson, "The Scott Company," pp. 361-62.

134. *Ibid.*, p. 363.

135. Ferguson, "Industrial Psychology and Labor," pp. 7-22. See also *DAB*, 4th supp., pp. 364-77.

136. Angell to Bingham, 8 June 1917; Bingham to Angell, 13 June 1917; Memorandum, Commander-in-Chief to Adjutant General, 2 January 1918; Committee on the Classification of Personnel to "All Personnel Supervisors," 5 April 1918; Bingham Papers.

137. Morris S. Viteles, "Tests in Industry," *Journal of Applied Psychology* 5 (1921): 57-63; *idem*, "Industrial Psychology: Reminiscences of an Academic Moonlighter," in *The Psychologists*, ed. T. S. Krawiec, vol. 2 (New York: Oxford University Press, 1974), pp. 440-500.

138. Clothier to Yerkes, 5 June 1923, Yerkes Papers; "Development of the Scott Company," undated memorandum, Scott Papers. See also Ferguson, "The Scott Company," pp. 373-75.

139. Ferguson, "The Scott Company," pp. 339-43.

140. Bingham, "Autobiography," pp. 17-18; Ferguson, "Industrial Psychology and Labor," pp. 7-9. See also Angell to Bingham, 25 March 1917, 30 July 1917, 2 August 1917; Woods, "Suggestion in Relation to Salesmanship Tests," 5 October 1920; Bingham Papers.

141. Sokal, "Origins of the Psychological Corporation," pp. 59-60.

142. Untitled and undated interview about the Psychological Corporation, Cattell Papers.

143. Cattell, "The Psychological Corporation," p. 169.

144. A. M. Johnson, "Report of Examinations Given in this Office," 31 August 1925, Cattell Papers.

145. Cattell to Dean R. Brimhall, 11 December 1920, Cattell Papers.

146. Untitled Memorandum, 15 January 1926, Cattell Papers.

147. Yerkes to Bingham, 3 December 1926; Bingham to Yerkes, 8 December 1926; Yerkes Papers; Cattell to Bregman, 4 December 1926, Bregman Papers; Achilles to Bingham, 9 July 1917, Bingham Papers; Achilles, "A Statement of the Present Status and Possible Future Development of the Psychological Corporation and the Personnel Research Foundation," 9 December 1926; Bingham to "the Members of the Board of the Psychological Corporation," 20 December 1926; Minutes, Meeting of the Board of Directors, 29 December 1926; Cattell Papers. See also "Notes and News," *Journal of Applied Psychology* 11 (1927): 81.

148. "Report of the Committee on Programs, Ways and Means," Psychological Corporation, 29 December 1926, Cattell Papers.

149. Treasurer's Report, Psychological Corporation, 2 November 1929, Cattell Papers.



150. Achilles, "Commemorative Address on the Twentieth Anniversary of the Psychological Corporation and to Honor Its Founder, James McKeen Cattell," *Journal of Applied Psychology* 25 (1941): 609-18; "The Role of the Psychological Corporation in Applied Psychology," *American Journal of Psychology* 50 (1937): 229-47.

151. Paul F. Lazarsfeld, "An Episode in the History of Social Research: A Memoir," in *The Intellectual Migration: Europe and America, 1920-1960*, pp. 270-337.

152. It must be stressed that in the American psychological community of the 1920s, clinical psychology referred *not* to psychotherapy, but to various testing and diagnostic procedures. See Virginia Staudt Sexton, "Clinical Psychology: An Historical Survey," *Genetic Psychology Monographs* 72 (1965): 401-34, and Robert I. Watson, "A Brief History of Clinical Psychology," *Psychological Bulletin* 50 (1953): 321-46.

153. The history of this group has been sketched in various articles by one of its leaders, J. E. Wallace Wallin. For example, see "A Note on the Origin of the APA Clinical Section," *American Psychologist* 16 (1961): 256-58; "A Red-Letter Day in APA History," *Journal of General Psychology* 75 (1966): 107-14; and *The Odyssey of a Psychologist* (Wilmington, Del.: by the author, 1955), pp. 119-22.

154. The history of the APA's efforts in this direction may be traced in summary in Samuel W. Fernberger, "The American Psychological Association: A Historical Summary, 1892-1930," *Psychological Bulletin* 29 (1932): 1-89; in some detail in "Proceedings" of the Annual Meetings of the Association, published in the *Psychological Bulletin*; and in great detail in the file headed "Committee on the Certification of Consulting Psychologists," APA Papers; and in the Terman Papers. Another recent secondary account, totally independent of the one presented here, is Napoli, *Architects of Adjustment*, pp. 57-61.

155. "Proceedings . . . 1917," *Psychological Bulletin* 15 (1918): 25-56.

156. Wallin, *Odyssey*, pp. 191-221. See especially Leta S. Hollingworth to Terman, 6 January 1918, Terman Papers.

157. Wallin, *Odyssey*, pp. 121-22; "The Field of Clinical Psychology as an Applied Psychology: A Symposium," *Journal of Applied Psychology* 3 (1919): 81-95.

158. "Proceedings . . . 1919," *Psychological Bulletin* 17 (1920): 37-38.

159. These services were used extensively throughout the 1920s and afterwards, despite the attempts by psychologists to discredit them. See A. W. Kornhauser and A. W. Jackson, "A Note on the Extent to which Systems of Character Analysis are Used in the Business World," *Journal of Applied Psychology* 6 (1922): 302; Guenter B. Risse, "Vocational Guidance during the Depression: Phrenology versus Applied Psychology," *Journal of the History of the Behavioral Sciences* 12 (1976): 130-40. See also Napoli, *Architects of Adjustment*, pp. 31, 58, 67, 82.

160. B. O. Taylor to Bingham, 29 October 1917; "The Literary Guild" and "The Society of Applied Psychology" to Bingham, 26 January 1920; Bingham to I. B. Davies, 25 February 1920; Bingham Papers.

161. "Report of the Committee on the Qualifications of Psychological Experts," December 1919; Minutes, Meeting of the Committee on Qualifications and Certification of Consulting Psychologists, August 1920; Leta S. Hollingworth to Frederic L. Wells, 15 April 1920; Committee file, APA Papers.

162. See Napoli, *Architects of Adjustment*, pp. 46-47.

163. See *ibid.*, pp. 54-55.

164. "Proceedings . . . 1920," *Psychological Bulletin* 18 (1921): 119-22.

165. In 1920, at least sixty-two psychologists indicated that they were in favor of certification. "Memorandum on Letters from Psychologists Concerning Accrediting of Consulting Psychologists," 13 September 1920, Committee file, APA Papers.

166. Frederic L. Wells, circular letter to the other members of the committee, 9 March 1922; "Memorandum on Meeting of the Committee in New York," 9 May 1922; Committee file, APA Papers.

167. On 5 October 1922, as executive officer of the committee, F. L. Wells wrote to at least twenty-two distinguished psychologists asking them to apply for certification. Many of these—for example, Raymond Dodge, Knight Dunlap, G. Stanley Hall, William McDougall, and Margaret Floy Washburn (whose reply is quoted)—could not under any circumstances be considered consulting psychologists. See Washburn to Wells, 15 October 1922, Committee file, APA Papers.

168. Dunlap to Wells, 10 October 1922, 17 October 1922, 25 October 1922, Committee file, APA Papers.

169. "Proceedings . . . 1922," *Psychological Bulletin* 20 (1923): 61-108. See Wells to Dunlap, 24 October 1922, Committee file, APA Papers; Boring to Terman, 25 January 1923, Boring Papers.

170. "Confidential Memorandum to the Council," 11 December 1926, Committee file, APA Papers; "Proceedings . . . 1926," *Psychological Bulletin* 24 (1927): 137-201.



171. "Proceedings . . . 1927," *ibid.* 25 (1928): 125-98.

172. Fernberger, "The American Psychological Association, 1892-1942," *Psychological Review* 50 (1943): 33-60.

173. See Cheryl M. Fields, "What Kind of Training Should be Required for a Psychologist?," *Chronicle of Higher Education*, 23 October 1978, pp. 9-10.

174. Financial Statement, Commonwealth Fund Grant, Expenditures to December 31, 1924, Kelley Papers; Leonard Carmichael, *Walter Fenno Dearborn and the Scientific Study of Reading* (Cambridge, Mass.: Harvard Graduate School of Education, 1957), p. 7.

175. Terman to S. P. Capen, 23 February 1921; Terman to Max Farrand, 18 May 1921; E. P. Cubberly to Terman, 22 May 1921; Terman Papers. See Terman et al., *Genetic Studies of Genius*, vol. 1, *Mental and Physical Traits of a Thousand Gifted Children* (Palo Alto, Calif.: Stanford University Press, 1925); vol. 5, *The Gifted Group at Mid-Life: Thirty-Five Years' Follow-up of the Superior Child* (Stanford, Calif.: Stanford University Press, 1959).

176. Of course, a history of the Rockefeller Foundation is outside the scope of this essay, but clarity requires the following notes. In the early 1920s, three agencies deriving their funds from the Rockefeller family made grants to psychologists for their work: the Rockefeller Foundation, the Laura Spelman Rockefeller Memorial, and the General Education Board. These groups and several others underwent a number of permutations in the years that followed, to emerge in the 1940s as the unified Rockefeller Foundation. See Robert Shaplen, *Toward the Well-Being of Mankind: Fifty Years of the Rockefeller Foundation* (London: Hutchinson, 1964); Raymond B. Fosdick, with Henry F. Pringle and Katharine Douglas Pringle, *Adventure in Giving: The Story of the General Education Board, A Foundation Established by John D. Rockefeller* (New York: Harper and Row, 1962).

177. George M. Stratton and Walter S. Hunter to Laura Spelman Rockefeller Memorial, 29 March 1926; Lawrence K. Frank, memorandum of interview with Stratton and Hunter, 29 March 1926; Beardsley Ruml to Stratton, 10 May 1926; "Foreign Fellowship 'Yellow Sheets'"; LSRM Archives. See also The Rockefeller Foundation, *Directory of Fellowships and Scholarships, 1917-1970* (New York: Rockefeller Foundation, 1972).

178. Minutes of the Rockefeller Foundation, 3 January 1929, 22 January 1929, 22 May 1929, RF Archives; John A. Dollard, "Yale's Institute of Human Relations: What Was It?," *Ventures* (The Magazine of the Yale Graduate School), 1964, pp. 32-40. Among the products of the Institute's first ten years were: Dollard, Neal E. Miller, Leonard W. Doob, O. H. Mowrer, and Robert R. Sears, *Frustration and Aggression* (New Haven, Conn.: Yale University Press, 1939); Arnold L. Gesell, *An Atlas of Infant Behavior: A Systematic Delineation of the Forms and Early Growth of Human Behavior Patterns . . . Illustrated with 3200 Action Photographs*, 2 vols. (New Haven, Conn.: Yale University Press, 1934); and Yerkes, *The Great Apes: A Study of Anthropoid Life* (New Haven, Conn.: Yale University Press, 1929).

179. An excellent overview of the Memorial's activities in this area is Elizabeth Lomax, "The Laura Spelman Rockefeller Memorial: Some of Its Contributions to Early Research in Child Development," *Journal of the History of the Behavioral Sciences* 13 (1977): 283-93.

180. Lawrence K. Frank to Stratton, 9 November 1925; Stratton to Frank, 20 January 1926; Ruml to Stratton, 3 March 1926; Minutes of the Meeting of the Committee on Child Development . . . National Research Council, 21 March 1926; LSRM Archives.

181. Grants were made directly to George J. Hecht, the founder of the Parents Publishing Association, in 1925, and to Teachers College of Columbia University, Yale University, and the University of Iowa, among others, so that the institutions could purchase stock in the association. Frank, memorandum of interview with Hecht, 6 November 1924; Hecht to Ruml, 7 November 1925; Angell to Frank, 8 October 1929; Hecht to Ruml, 18 October 1927; Hecht to Walter A. Jessup, 7 June 1929; LSRM Archives. See Marilyn Bender, "A Dr. Spock of Magazines," *New York Times*, 21 September 1975.

182. Hillis to Seashore, 2 December 1913; Seashore to Hillis, 21 November 1914; Minutes of the Child Welfare Committee, 22 December 1914; Hillis to Seashore, 27 May 1916; Hillis, Child Welfare Circular, 6 April 1917; Institute of Child Welfare Papers. See also *Pioneering in Child Welfare: A History of the Iowa Child Welfare Research Station, 1917-1933* (Iowa City, Iowa: State University of Iowa, 1933) and *The Institute of Child Behavior and Development: Fifty Years of Research, 1917-1967* (Iowa City, Iowa: University of Iowa, 1967), both of which will soon be superseded by the definitive and critical history of the station now being prepared by Hamilton Cravens.

183. Walter A. Jessup to Baldwin, 17 July 1917; Baldwin to Jessup, 6 August 1917; Seashore to Baldwin, 13 August 1917; Seashore to Hillis, 13 August 1917; Institute of Child Welfare Papers; Lawrence K. Frank, memorandum of interview with Baldwin, 25 October 1927; Amy L. Daniels to Jessup, 24 December 1928, LSRM Archives.

184. Baldwin to Ruml, 13 December 1922, LSRM Archives; Margaret C. Munns to Baldwin, 7 October 1924, Institute of Child Welfare Papers.

185. Baldwin to Hillis, 18 January 1921, Institute of Child Welfare Papers; Baldwin to W. S. Richardson, 8 December 1920; Ruml to Jessup, 22 November 1922; Baldwin and Jessup to LSRM, 2 April 1925; Arthur Woods to Jessup, 14 April 1925; Ruml to Baldwin, 7 January 1926; Frank, memorandum of interview with Baldwin, 25 October 1927; Baldwin to Frank, 1 November 1927; Ruml to Jessup, 2 March 1928, 27 April 1928; LSRM Archives. For the work of the station during these years, see *Pioneering in Child Welfare*, and *The Institute of Child Behavior and Development*.

186. Frank, memorandum of interview with Richardson, 2 March 1925; Arthur Woods to R. A. Pearson, 14 April 1925; Ruml to R. M. Hughes, 2 March 1928; Ruml to Homer H. Seerley, 4 February 1926; Revell MacCallum to G. R. Latham, 30 April 1929; LSRM Archives.

187. This unified program was not achieved without a power struggle, as the psychologists at the State University argued forcefully that all child study work in the State should be under their control. It was only the powerful position of Richardson, and the Memorial's explicit desire to use all available resources in the state, that enabled the program to emerge as it did. See Baldwin, "Memorandum to the Laura Spelman Rockefeller Memorial," 11 December 1924, Institute of Child Welfare papers. Frank to Ruml [May 1924]; Frank, memorandum of interview with Richardson, 2 March 1925; Baldwin to Frank, 28 May 1927; Frank, memorandum of interview with Baldwin, 25 October 1927; LSRM Archives.

188. Frank, "Child Study Program," memorandum, 22 March 1924; Edward L. Thorndike to Ruml, 14 June 1927; Frank, memorandum of interview with "Faculty and Officers, University of California," 6-12 March 1925; Frank to Warner Brown, 27 May 1925; Ruml to W. W. Campbell, 31 May 1927; Frank, "University of Minnesota," memorandum, 6 October 1924; L. D. Coffman to Frank, 29 January 1925; Arthur Woods to Coffman, 14 April 1925; LSRM Archives.

189. Angell to Ruml, 13 June 1923, 23 June 1923, 29 June 1923; Gesell to Ruml, 9 February 1924; Frank to Gesell, 18 February 1924; Frank, memorandum of interview with Gesell, 20 January 1925; Arthur Woods to Angell, 19 May 1925; Ruml to Angell, 28 May 1925; Gesell, "Memorandum for Mr. Frank," 2 April 1927; LSRM Archives.

190. Ruml to Aurelia H. Reinhardt, 2 December 1927; Ruml to Herman Schneider, 3 March 1928; Walker to Laura Spelman Rockefeller Memorial, 26 March 1924; Synder Walker, memorandum of interview with Mary Cresswell, Department of Home Economics, Georgia Agricultural College, 27 February 1925; Ruml to Andrew Soule, 28 March 1925; LSRM Archives.

191. E.g., Angell, "The Relations of Structural and Functional Psychology to Philosophy," *Philosophical Review* 12 (1903): 243-71; "The Province of Functional Psychology," *Psychological Review* 14 (1907): 61-91.

192. Boring, "The Society of Experimental Psychologists, 1904-1938," *American Journal of Psychology* 51 (1938): 410-21.

193. Bingham to Yerkes, 12 December 1913, Yerkes Papers; Yerkes to Terman, 21 July 1921, Terman Papers; Boring to Yerkes, 18 January 1921; Washburn to Boring, 17 September 1921; Boring to Bingham, 24 November 1922; Bingham to Boring, 27 November 1922; Boring Papers. "Proceedings . . . 1921," *Psychological Bulletin* 19 (1922): 65-115; "Proceedings . . . 1926," *ibid.* 24 (1927): 137-201.

194. See Cedric A. Larson and John J. Sullivan, "Watson's Relation to Titchener," *Journal of the History of the Behavioral Sciences* 1 (1965): 338-54.

195. Boring, "The Society of Experimental Psychologists," p. 416; Karl M. Dallenbach to Boring, 3 August 1927, Boring Papers.

196. Boring, Telegrams to Cattell, Yerkes, Raymond Dodge, et al., 3 August 1927; Boring to Cattell, Yerkes, Dodge, 11 August 1927; Boring Papers. The two letters to Yerkes and Dodge were actually produced by use of carbon paper.

197. Boring to Yerkes, 11 August 1927; Yerkes Papers; Boring, "Edward Bradford Titchener, 1867-1927," *American Journal of Psychology* 38 (1927): 489-506. In his autobiography, Boring explicitly compared the effect of Titchener's death on him to the release that John Stuart Mill felt on the death of his father. See Boring, *Psychologist at Large: An Autobiography and Selected Essays* (New York: Basic Books, 1961), p. 49.

198. Cattell to Boring, 6 August 1927; Yerkes to Boring, 10 August 1927; Boring Papers.

199. Dallenbach to Boring, 3 August 1927, Boring Papers. See also Boring to Terman, 9 August 1927, Terman Papers.

200. Interview with Anne Anastasi, 20 November 1973.

201. See also Sokal, "Enfin in Amerique: First U.S. International Congress," *APA Monitor*, July/August 1979, p. 3; Carl P. Duncan, "A Note on the 1929 International Congress of Psychology," *Journal of the History of the Behavioral Sciences* 16 (1980): 1-5.

202. See Rand B. Evans and Frederick J. Down Scott, "The 1913 International Congress of Psychology: The American Congress that Wasn't," *American Psychologist* 33 (1978): 711-23.

203. John E. Anderson to Council of the American Psychological Association, 27 April 1923, 12 May 1923, 22 June 1923, Terman Papers. "Proceedings of the . . . American Psychological Association . . . 1923," *Psychological Bulletin* 21 (1924): 69-120.
204. Claparède, "Address by the Secretary of the International Congress," *Ninth International Congress of Psychology*, pp. 33-47.
205. *New Republic*, 18 September 1929, pp. 112-13.
206. Cattell, "Psychology in America," *Science* 70 (11 October 1929): 335-47; *Scientific Monthly* 30 (1930): 114-26. That Cattell printed this address in two of his journals, besides publishing it separately, shows how much he thought of it. See also *Man of Science*, 2: 441-84, and Paul Farnsworth and Ernest Hilgard, unpublished informal memoirs, July-August 1967, Archives of the History of American Psychology.
207. This discussion is based primarily on the following interviews and oral histories: A. T. Poffenberger, interview, 4 November 1972; Hudson Hoagland, interview, 7 November 1972; David Shakow, interview, 29 October 1973; Anne Anastasi, interview, 20 November 1973; Farnsworth and Hilgard, informal memoir. See also Duncan, "A Note on the 1929 International Congress."
208. McDougall, "An Experiment for Testing the Hypothesis of Lamarck," *British Journal of Psychology* 17 (1927): 267-304; "Second Report on a Lamarckian Experiment," *Ninth International Congress*, pp. 302-3; *British Journal of Psychology* 20 (1930): 201-18. See also Harry Helson, "E. G. B.: The Early Years and Change of Course," *American Psychologist* 25 (1970): 625-29.
209. Angier to James R. Angell, 18 September 1929, Angell papers.
210. *Ninth International Congress*, pp. xvi-xxi, xxxiii-xxxiv.
211. *Ibid.*, 331-33.
212. Herbert S. Langfeld, "The Ninth International Congress of Psychology," *Science*, 70 (18 October 1929): 364-68.
213. Katharine Adams Williams, "Psychology in 1929 at the International Congress," *Psychological Bulletin* 27 (1930): 658-63.
214. *Ninth International Congress*, p. xxviii; *Psychological Review* 37 (1930): 1-24.
215. E. g., Langfeld to Kurt Koffka, 16 December 1927, 20 January 1928, 27 January 1928, 15 February 1928, 4 June 1928, 7 November 1928, 15 December 1928, 8 March 1929, Koffka Papers. Boring to Seashore, 25 June 1928, 21 August 1928, Boring Papers. See also "Subjects of Morning Lectures and Afternoon Discussions by Six Foreign Psychologists, First Summer Session, 1929, State University of Iowa," Institute of Child Welfare Papers.
216. *Ninth International Congress*, pp. 270-72.
217. E. g., Langfeld, "The Ninth International Congress," pp. 364-68.
218. Helson, "The Effects Obtained from Rotation of Irregularly Formed Regions," *Ninth International Congress*, pp. 219-20; Tolman, "Maze-Performance as a Function of Motivation and of Reward as well as of a Knowledge of the Maze-Paths," *ibid.*, pp. 439-40; Washburn, "Köhler's Discussion of Association," *ibid.*, p. 470.
219. Interview with Tamara Dembo, 14 November 1973.
220. *Ibid.* See also below.
221. This topic is treated in greater detail, in a slightly different perspective, in Sokal, "The Gestalt Psychologists in Behaviorist America," *American Historical Review*, forthcoming.
222. Mandler and Mandler, "The Diaspora of Experimental Psychology." The material collected by these authors in the preparation of their paper is available at the Archives of the History of American Psychology, and has provided some of the information, and much of the background, for this discussion.
223. *Ibid.*, pp. 409-11.
224. Tolman, "Autobiography," *A History of Psychology in Autobiography*, vol. 4, p. 327.
225. Koffka, "Perception: An Introduction to the Gestalt-Theorie," *Psychological Bulletin* 19 (1922): 531-85; Boring to the Mandlers, 7 June 1967, 14 February 1968, Mandler Papers. See also Frank S. Freeman, "The Beginnings of Gestalt Psychology in the United States," *Journal of the History of the Behavioral Sciences* 13 (1977): 352-53.
226. Allport, "Autobiography," *A History of Psychology in Autobiography*, vol. 5, pp. 7-11.
227. Allport, "Leipzig Congress for Psychology," *American Journal of Psychology* 34 (1923): 612-15; "The Standpoint of Gestalt Psychology," *Psyche* 4 (1924): 354-61.
228. Ogden, "The Phenomena of Meaning," in "Proceedings of the American Psychological Association . . . 1922," *Psychological Bulletin* 20 (1923): 61-108; Tolman, "Autobiography," p. 329.
229. Watson to Boring, 15 September 1924; Boring to Watson, 20 September 1924; Boring Papers.
230. Helson, "E. G. B.: The Early Years and Change of Course." According to the excellent paper by John O'Donnell, "The Crisis of Experimentalism in the 1920s: E. G. Boring and His Uses of History," *American Psychologist* 34 (1979): 289-95, Boring's concern led him to write his *History of*

*Experimental Psychology* (New York: Century, 1929), which ignored most branches of applied psychology.

231. Helson, "E. G. B.," pp. 627-28; idem, "Some Highlights of an Intellectual Journey," *The Psychologists*, pp. 94-95; idem, "The Psychology of the Gestalt," *American Journal of Psychology* 36 (1925): 342-70, 494-526; 37 (1926): 25-62, 189-223.

232. Washburn to Boring, 16 September 1925, Boring Papers.

233. Boring to Washburn, 19 September 1925, Boring Papers.

234. Boring to Koffka, 17 November 1925, Boring Papers.

235. Boring to Köhler, 30 April 1925, Boring Papers.

236. See Michael Wertheimer, "Max Wertheimer: Gestalt Prophet," *Gestalt Theory* 2 (1980): 3-17.

237. Boring to the Mandlers, 7 June 1967, 14 February 1968, Mandler Papers.

238. "Proceedings . . . 1924," *Psychological Bulletin* 22 (1925): 69-138.

239. Register, Koffka Papers; Boring to James H. Woods, 12 November 1924, 24 November 1924, Boring Papers.

240. Boring to Woods, 8 November 1926, 11 November 1926, 16 November 1926, 7 February 1927, 28 February 1927; Boring to Koffka, 2 March 1927, 9 March 1927; Koffka to Boring, 13 November 1926; Boring Papers.

241. Boring to Woods, 8 November 1926; Boring to Koffka, 22 November 1926; Boring Papers.

242. Koffka to Boring, 23 February 1927; Boring to Woods, 28 February 1927, Boring Papers.

243. Koffka to Boring, 1 April 1927, Boring Papers.

244. Seth Wakeman to Koffka, 5 October 1926, 12 October 1926, 16 November 1926, 1 January 1927, 11 January 1927, 28 February 1927, 2 March 1927, 19 March 1927, 1 June 1927, Koffka Papers.

245. Ogden, "The Gestalt-Hypothesis," *Psychological Review* 35 (1928): 136-41; Ogden to Koffka, 17 June 1927, 2 January 1928, 30 March 1928, 4 April 1928, 15 October 1928, 11 November 1928, 22 November 1928, Koffka Papers.

246. Boring to the Mandlers, 7 June 1967, Mandler Papers.

247. Terman to Boring, 3 August 1927; E. C. Tolman to Terman, 5 April 1938; Boring to Terman, 14 December 1927, 22 December 1941; Terman Papers.

248. Mandler and Mandler, "The Diaspora of Experimental Psychology," pp. 409-11; A. Hunter Dupree, "The Coming of the Refugees," *Science* 166 (19 December 1969): 1495-97.

249. Bühler to Terman, 24 June 1941, 8 December 1941; Terman to Bühler, 11 December 1941, 8 May 1951; Terman Papers. See also Charlotte Bühler to the Mandlers, 9 September 1967, Mandler Papers.

250. Interview with Tamara Dembo; Boring to Terman, 31 July 1931, 6 October 1931, Terman Papers. See also Grace Heider, "News and Notes," *Newsletter of the Division of the History of Psychology* (American Psychological Association) 7 (1974): 10.

251. Mandler and Mandler, "The Diaspora of Experimental Psychology," pp. 400-405; Boring to Terman, 31 July 1931, Boring Papers.

252. Terman to Boring, 13 August 1931, Boring Papers.

253. Boring to Terman, 18 August 1931, 6 October 1931, Terman Papers.

254. Terman to Boring, 19 November 1934; Boring to Terman, 31 July 1931, 6 October 1931; Terman papers.

255. Boring to Terman, 7 April 1933; Terman to Boring, 11 April 1933; Terman Papers.

256. Terman to Johnson, Terman Papers; Boring to Koffka, 5 April 1933, 7 April 1933, 2 May 1933, 15 May 1933, 18 May 1933; Koffka to Boring, 6 April 1933, 1 May 1933, 16 May 1933; Boring Papers.

257. Terman to Lewin, 4 February 1942; Lewin to Terman, 28 March 1942, 8 February 1944; Terman to E. C. Tolman, 14 February 1944; Terman Papers. See also Mandler and Mandler, "The Diaspora of Experimental Psychology," p. 401; Boring to George D. Stoddard, 7 October 1941, Lewin files.

258. A more detailed and extensive discussion of this same topic is found in Sokal, "The Gestalt Psychologists in Behaviorist America."

259. Yerkes to Köhler, 27 March 1914, 20 May 1914, 17 July 1916, 10 October 1916, 21 December 1916, 10 January 1917; Köhler to Yerkes, 17 April 1914, 15 February 1916, 19 May 1916, 10 September 1916, 13 December 1916, 2 March 1917; Yerkes Papers.

260. Yerkes to U.S. Department of State, 14 February 1921; Yerkes to Köhler, 13 April 1921, 8 October 1921, 6 March 1922, 6 November 1923; Köhler to Yerkes, 19 May 1921, 23 October 1921, 29 January 1922; Yerkes Papers.

261. Terman to C. H. Thurber, 26 May 1924, Terman Papers.

262. Yerkes to Köhler, 22 October 1924, 26 January 1925, 7 May 1925; Köhler to Yerkes, 8 February 1924, 15 February 1925; Yerkes Papers; Boring to Köhler, 9 June 1924, 6 February 1925, Boring Papers.

263. Yerkes to Köhler, 7 May 1925, Yerkes Papers; Boring to Köhler, 31 March 1925; Köhler to Boring, 2 April 1925, 20 April 1925; Boring Papers.
264. Boring to Yerkes, 28 October 1925, Yerkes Papers.
265. Boring, "The Society of Experimental Psychologists," p. 415; Terman to Boring, 26 January 1927, Terman Papers.
266. Yerkes to David White, 5 March 1925, Yerkes Papers. Lawrence K. Frank to Terman, 7 April 1925, LSRM Archives.
267. Kegan Paul Trench Trubner and Company to Köhler, 16 November 1926, 19 January 1927, 15 February 1927, 14 June 1927, 22 July 1927, 11 October 1927, Köhler Papers.
268. Boring to Woods, 15 April 1925, 16 April 1925; Boring to Köhler, 25 March 1925, 31 March 1925; Köhler to Boring, 2 April 1925; Boring Papers; Boring to Köhler, 4 June 1925, Köhler Papers.
269. Köhler to Boring, 12 July 1925, Boring Papers; Köhler to Woods, undated draft (ca. 10 July 1925), Köhler Papers; Boring to Yerkes, 28 October 1925, Yerkes Papers.
270. Boring to Woods, 22 July 1925, 6 January 1926, 21 January 1926, 18 March 1926; Woods to Boring, 18 July 1925, 15 March 1925; Boring to Köhler, 17 March 1925; Boring Papers; Köhler to Woods, 15 March 1926, undated draft (Spring 1926), Köhler Papers.
271. Boring to Koffka, 23 April 1930, Boring Papers.
272. Woods to Boring, 17 December 1926; Boring to Woods, 27 December 1926; Boring Papers; Boring to Terman, 17 January 1927, Terman Papers.
273. Boring to Woods, 13 January 1927; Köhler to Boring, 13 May 1927 (translated from the German); Boring Papers; Boring to Terman, 11 July 1927, Terman Papers.
274. Boring to Terman, 9 August 1927, 14 December 1927, Terman Papers; Boring to Köhler, 6 January 1928, Boring Papers. See also Boring to Clarence I. Lewis, 2 August 1927, 14 August 1927, Department of Philosophy Papers.
275. Boring to Lewis, 4 December 1927, 4 April 1928, 30 October 1928, Department of Philosophy Papers.
276. Boring to Lewis, 7 December 1927, Department of Philosophy Papers. See also Kuklick, *The Rise of American Philosophy*, p. 460.
277. Boring to Lewis, 30 October 1928, Department of Philosophy Papers.
278. Boring to Perry, 15 November 1928, Department of Philosophy Papers.
279. Washburn, "Gestalt Psychology and Motor Psychology," *American Journal of Psychology* 37 (1926): 516-20; Washburn to Boring, 16 September 1925, Boring Papers.
280. Terman to Boring, 3 August 1927, 27 January 1927, Terman Papers.
281. Boring to Seashore, 25 June 1928, 21 August 1928, Boring Papers.
282. Michael Wertheimer, "Max Wertheimer, Gestalt Prophet"; Köhler, "Gestalt Psychology," *Psychologische Forschung* 31 (1967): xviii-xxx; "A Perspective on American Psychology," *Psychological Review* 50 (1943): 77-79.
283. Fritz K. Ringer, *The Decline of the German Mandarins: The German Academic Community, 1890-1933* (Cambridge, Mass.: Harvard University Press, 1969).
284. Ash, *Gestalt Psychology in Two Cultures*, makes this point well.
285. Boring, *A History of Experimental Psychology*, pp. 570-80, 591-93; Boring to Köhler, 27 February 1930, Boring Papers.
286. Boring, *A History of Experimental Psychology*, pp. 570-71, 576-78, 593.
287. Boring, "The Gestalt Psychology and the Gestalt Movement," *American Journal of Psychology* 42 (1930): 308-15.
288. Boring to Köhler, 27 February 1930, Boring Papers.
289. Koffka to Boring, 22 April 1930, Boring Papers.
290. Boring, *A History of Experimental Psychology*, p. 593.
291. Boring to Koffka, 23 April 1930, Boring Papers.
292. E.g., F. H. Lund, "The Phantom of the Gestalt," *Journal of General Psychology* 2 (1929): 307-23; William McDougall, "Dynamics of Gestalt Psychology," *Character and Personality* 4 (1930): 232-44, 319-34; S. C. Fisher, "A Critique of Insight in Köhler's Gestalt Psychology," *American Journal of Psychology*, 43 (1931): 131-36; F. M. Greg, "Materializing the Ghost of Köhler's Gestalt Psychology," *Psychological Review* 39 (1932): 257-70.
293. Boring, *The Physical Dimensions of Consciousness* (New York: Century, 1933); Julian Jaynes, "Edwin Garrigues Boring: 1886-1968," *Journal of the History of the Behavioral Sciences* 5 (1969): 99-112.
294. Mary Henle, "One Man Against the Nazis—Wolfgang Köhler," *American Psychologist* 33 (1978): 939-44.
295. Boring to Köhler, 7 December 1933; Boring to Koffka, 13 December 1933; Köhler to Boring, 22 January 1934; Boring Papers.

296. Boring to Köhler, 19 March 1934, 2 October 1934, 7 November 1934, Boring Papers; Boring to the Mandlers, 14 February 1968, Mandler Papers.
297. New York: Liveright Publishing Co., 1938.
298. *Journal of Philosophy* 36 (1939): 107-8: "Keen, wide-ranging, and original," *New York Times*, 5 February 1939, p. 6.
299. Harry L. Hollingworth, *American Journal of Psychology* 53 (1940): 146-52; J. R. Kantor, *Psychological Bulletin* 36 (1939): 292-96.
300. Carmichael to Boring, 15 December 1934, 22 December 1934; Boring to Carmichael, undated card (ca. 20 December 1934); Carmichael papers.
301. Boring, *Psychologist at Large*, pp. 53-54; Jaynes, "Boring," p. 107. See also Boring and Hanns Sachs, "Was This Analysis a Success?" *Journal of Abnormal and Social Psychology* 35 (1940): 4-16.
302. Helson to the Mandlers, 27 February 1968, Mandler Papers.
303. Jaynes, "Boring," p. 107.
304. Boring to Terman, 7 March 1947, Terman Papers; Boring to the Mandlers, 14 February 1968, Mandler Papers.