

## STRUGGLE FOR SCIENTIFIC AUTHORITY: THE RECEPTION OF WATSON'S BEHAVIORISM, 1913-1920

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Supported by the Zeitgeist, Behaviorism supposedly spread quickly through American psychology after the publication of Watson's manifesto in 1913. But an extensive search of published and unpublished source material from 1913 to 1920 shows only limited support and a good deal of resistance; documentary evidence for the conversion of psychologists to radical behaviorism during these years is hard to find. Though faced with some troubling problems, the discipline was not eager to renounce its established scientific authority and expertise on the mind. Acceptance of Watson's claims for a new authority required drastic shifts in psychologists' perception of reality, and in their interests to problems of social control.

### I. FROM BEHAVIORIST "MANIFESTO" TO AMERICAN TRADITION: HOW AND WHY?

On 24 February 1913, at a meeting of the New York Branch of the American Psychological Association held at Columbia University, John B. Watson read a paper entitled "Psychology as the Behaviorist Views It." This presentation, the first in a series of eight lectures on animal psychology Watson gave at Columbia in early 1913, was published in the March issue of the *Psychological Review*. (Written in late 1912, this paper or at least a talk with the same title had been presented at the Graduate Conference at Johns Hopkins University in January.)<sup>1</sup> Another article on "Image and Affection in Behavior" followed soon after. Apparently a response to questions raised by the initial lecture, it supplemented the earlier argument with some details on the crucial issue of central versus peripheral processes.<sup>2</sup>

Some phrases from Watson's challenge to contemporary psychology have become classics: "Psychology as the behaviorist views it is a purely objective experimental branch of natural science. Its theoretical goal is the prediction and control of behavior. Introspection forms no essential part of its methods. . . ." That was the opening gun. Another broadside followed: "I do not wish unduly to criticize psychology. It has failed signally, I believe, during the fifty-odd years of its existence as an experimental discipline to make its place in the world as an undisputed natural science." And another charge: "I firmly believe that two hundred years from now, unless the introspective method is discarded, psychology will still be divided on the question as to whether auditory sensations have the quality of 'extension,' . . . whether there is a difference in 'texture' between image and sensation. . . ." Attacking both structuralism and functionalism, Watson accused them of not being interested "in a psychology which concerns itself with human

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life."<sup>3</sup> Altogether, the manifesto was a call to fellow psychologists to abandon the useless and unscientific concepts of introspection and consciousness and to join in the creation of a new, exciting, and powerful science of behavior.

Thirty years later, at the occasion of the fiftieth anniversary of the *Psychological Review*, Watson's paper headed the count of nominations of "most important" articles published by the *Review* in its first half-century, according to Herbert S. Langfeld's survey of fifty-two prominent psychologists of two generations. As Richard J. Herrnstein and E. G. Boring were to say after another twenty-five years, Watson's 1913 paper "founded 'behaviorism'. . . [which] quite rapidly became the representative school in what was soon to be the American tradition." Agreeing with the date, John C. Burnham has argued, however, in a searching study of the "Origins of Behaviorism," that the crucial event creating behaviorism was the response of the psychological community as a "self-conscious discipline."<sup>4</sup>

But what was this response—or rather, since psychologists did not speak with one voice—what were the responses? Behaviorism, while never monolithic and never without its critics, had become the dominant force in American psychology by mid-century, as well as its export model. As Edward C. Tolman commented, sooner or later practically all of us became behaviorists.<sup>5</sup> Yet the question of how and why the change occurred has received only limited attention. Psychology's historians have usually treated the problem in a doxographic manner, describing Watson's writings and perhaps the development of his ideas, and then outlining the systems proposed by his most prominent successors and revisionists: Tolman, Hull, Skinner, etc. But major historical issues, the reasons for the appeal of the behaviorist program to others and the process of the transformation of the discipline, appear to have been dealt with only at the most general level: Watson was "conspicuously American"; he expressed the *Zeitgeist* or the culture in one way or another; he established an essentially practical psychology well fitted to the pragmatic temper of the country.<sup>6</sup> Those inside the new "tradition" saw, of course, no problem—apart from the need to disavow Watson's more outrageous specifics. Some nonpsychologist historians have pursued the question of the appeals and the impact of behaviorism on American society in a more sophisticated way,<sup>7</sup> but they were only tangentially concerned with the events inside the psychological profession.<sup>8</sup>

Restricting itself to a limited time period and a relatively "internalist" perspective, the present paper outlines the varied responses of psychologists to Watson's "radical" behaviorism, through an analysis of psychological publications and available archival material up to the early twenties. The paper then inquires just what it was that Watson had to offer to his fellow professionals and what he asked them to give up in return, in order to determine which aspects of his attempt to overthrow the old authorities and to redefine their science proved attractive (or threatening), and to whom. Altogether, its aim is to contribute toward our understanding of this instance of a major change in a scientific discipline—or paradigm shift, if one wants to call it that.

## II. THE HISTORICAL RECORD

If retrospectively the appearance of Watson's manifesto was a major historical event, primary sources do not quite reflect it as such. Except for Howard C. Warren's reference to the fact that he had repeatedly urged Watson to publish his position paper, none of the autobiographies of prominent psychologists of the period have marked it as a red letter day. In fact, the dean of psychology's historians, E. G. Boring, in an extended reminiscence of his professional life history, did not find it necessary to recall any en-

counter with Watson or Watson's ideas, even though his own orientation changed from Titchnerian structuralism to a (behavioral) "physicalism."<sup>9</sup>

*Initial Responses: Three Themes and Some Hostility*

To be sure, the contemporary literature did not ignore Watson's paper completely; neither did it give his challenge singular prominence. A summary of the events of 1913 in psychology, written by Langfeld for the *American Year Book*, started out by dealing with two other "important discussions" before mentioning the "behaviorist movement"; even then it cited Maurice Parmelee's new book *The Science of Human Behavior* rather than Watson's work. The discussion of Watson's paper came only in the second section, entitled "Psychological Method," and treated it mainly as another attack on introspection. A second overview of the preceding year, the summary on "General Problems: Mind and Body" in the January 1914 *Psychological Bulletin* did open with the question whether psychology was purely a study of behavior, or of mental states and processes, or both; commenting that the behaviorists especially were attracting attention in the debate, it then quoted half a page from Watson's paper before going on, noncommittally, to other views on the issue.<sup>10</sup>

Beyond such summaries we find that, in an address on the "Study of Human Behavior" for a June 1913 Eugenics Conference, Robert M. Yerkes had begun to use the term "behaviorist" (apparently coined in late 1912 independently by both Watson and James R. Angell);<sup>11</sup> but his references were to three recent books: Parmelee's work mentioned above, Max Meyer's book on *The Fundamental Laws of Human Behavior*, and William McDougall's *Introduction to Social Psychology*, not to Watson's paper (with which he was familiar).<sup>12</sup> Apart from some footnote references added on to papers written before the appearance of Watson's article, the first direct response in print came in a short article by Mary W. Calkins, entitled "Psychology and the Behaviorist."<sup>13</sup> Critical of Watson's "vigorous" paper, she expressed her "radical disagreement with [its] main thesis" of the uselessness of introspection, questioned his supporting arguments, and insisted that certain kinds of psychological processes could be studied only by introspection. However, she also expressed much sympathy with the "important truth embedded" in Watson's criticism of the "undue abstractness" of the present psychology as the "study of mental state." Instead, psychology needed to be concerned with "problems of life." The study of behavior by objective methods was indeed important, as long as "behavior" was understood not merely as "mechanical," but meant the study of "self related to environment."<sup>14</sup>

Here we have the emergence of three themes which in one form or another came to predominate in the published reactions to Watson for some time: (1) although Calkins conceded some problems with the method of introspection and granted the legitimacy of objective procedures, she nevertheless maintained the usefulness of introspection as one of the methods of psychology (what we might call the "don't throw out the baby with the bath" argument); (2) she expressed a strong desire to expand the subject matter of psychological study to a concern with real people in the real world (the "relevance" argument, as we might call it today); and (3) accepting the notion of behavior, but questioning Watson's narrow definition of the term, she attempted to redirect Watson's thrust toward her own goal, a special "self psychology" version of a functionalist approach (the "cooptation" theme). It is tempting to argue, by the way, that, taking psychology as a whole, Mary Calkins's view was more nearly prophetic of what psychology would become half a century later than was Watson's narrower position, even though his slogan of the "study of behavior" eventually carried the day.

The other direct, and quite enthusiastic, response to Watson came from Fred L. Wells, perhaps best described as a hybrid experimentalist-clinician working at McLean Hospital for the Insane. Once in the context of a review of Parmelee's book, and again in a summary review of "Dynamic Psychology" for the *Psychological Bulletin*, he put himself into Watson's corner, lauding Watson's "well-aimed blow at the autistic method in psychology. . ."<sup>15</sup> and quoting with obvious relish some of his attacks on the "pure" psychologists and their lack of concern with human life. "Experimental psychology. . . dodges. . . the more actual and vital questions. . . [and retreats] into a burrow of trivial inquiries. . .," Wells complained.<sup>16</sup> Yet he, like Calkins (and Angell before them, in an APA address on "Behavior as a Psychological Category," delivered about the time Watson was preparing his paper for publication), argued that at least for practical purposes some use of introspection was unavoidable. Furthermore, the crucial issue to be settled was the meaning of "behavior"; in order to be useful it could not be restricted to activities describable in physical or physiological terms, but had to include "mental [!] behavior."<sup>17</sup>

A very brief comment in a review of "Criminology and Delinquency" by Jean Weidensall (who as a student had known Watson at Chicago and was, like Wells, working in a nonacademic setting) concludes the list of references to Watson in the *Psychological Bulletin* of 1913: Though Watson's paper seemed a bit radical, she felt that "in truth [it was outlining] the psychology we shall find most useful."<sup>18</sup>

There were also three brief items in the *Journal of Philosophy, Psychology, and Scientific Method*. In the last paragraph of a short paper on the definition of Comparative Psychology, Yerkes protested strongly against Watson's attempt to "throw overboard. . . the method of self-observation" and to usurp the science of psychology for the study of behavior, although he supported wholeheartedly the integration of behavior methods into psychology. Angell put in a brief demurrer against Watson's claim, that Angell's research on imagery had justified the dismissal of the image from psychology. And finally philosopher Henry R. Marshall, in a paper asking, "Is Psychology Evaporating?," briefly referred to Thorndike, Watson, and the objective science of behavior which was, in his view, legitimate; but it was not psychology.<sup>19</sup>

In late December 1913 the American Psychological Association held its annual convention at Yale University (which hosted the American Philosophical Association at the same time). APA president H. C. Warren gave an address on "The Mental and the Physical." Rejecting any solution of the metaphysical mind-body problem as premature, he went on to argue for the adoption of a double-aspect view as a working hypothesis. This position required a redefinition of psychology to embrace both inner and outer aspects of experience and made it the "science of the relations between the individual and his environment, [to] be studied either objectively as behavior, or introspectively as events of consciousness."<sup>20</sup>

A page-long summary of Warren's address in the proceedings did not refer to Watson at all. The paper itself contained a number of references to Watson and his position; yet it was clearly not a response to him, but to a problematic which had been debated by psychologists for some time. Warren agreed with Watson that the hope for the future might lie in the study of behavior, since it revealed "dynamic aspects" more than did introspection. But he could not accept an autocratic decree prohibiting introspective study; introspection had produced many results of scientific worth; Watson's critique was too "destructive."<sup>21</sup> In summary, Warren's argument, while different in the specifics, was basically the same as Calkins's: don't throw out the baby of introspection, but accept

behavior for the sake of "dynamics," and fashion a "double-aspect" compromise instead of splitting psychology into two different disciplines.

At the same convention, a joint session with the philosophers on the "standpoint of psychology" heard, among others, John Dewey and Hugo Münsterberg refer favorably but briefly to behaviorism. Wishing behaviorism well, Dewey expressed both fear and hope—fear, if "behavior" meant just the mechanics of the nervous system; hope, if it included the "attitudes and responses towards others which cannot be located under the skin. . . ." Münsterberg, in an exposition of his scheme of two psychologies, one "causal" and the other "teleological," expressed the opinion that behaviorism might be successful in an applied psychology derived from the causal approach. In the discussion, Knight Dunlap raised some questions about "delimiting the behaviorist's field. . . ." Earlier that year, Dunlap had presented a talk at Johns Hopkins, in which he distanced himself sharply from Watson and protested against the latter's "extreme doctrine" likely to produce opposition to more moderate innovations.<sup>22</sup>

The earliest recorded reference to Watson's manifesto apparently occurred in a discussion of "four recent tendencies" in psychology, presented by G. Stanley Hall at a Mental Hygiene Conference in April 1913. After introspection and psychoanalysis, "a rich, rank, seething mass of new facts and new ideas, sure to revolutionize. . ." psychology, Hall mentioned behaviorism briefly and in rather neutral fashion, obviously quoting or paraphrasing Watson's major thesis. From there he proceeded to an extended discussion of the last tendency, Pavlov's "amazing" work on salivary conditioning, which had barely touched American psychology as yet.<sup>23</sup> The seventy-year old Hall still had his ear to the ground.

The only indication of a "violent reaction" and "furor"<sup>24</sup> caused by Watson's polemic is found in a short notice reporting on the meeting of the Experimental Psychologists (largely the inner, Titchnerian circle of the academic discipline), held at Wesleyan University in April 1913. It appears that a "lively discussion" on introspection and behaviorism developed in one of the sessions. Introspection had been hotly debated—without Watson—at a meeting two years earlier, with Titchener on one side and Dodge and Holt on the other.<sup>25</sup> This time, "the hostility to an identification of psychology with 'behaviorism' was surprisingly unanimous. . . ."<sup>26</sup> That is unfortunately all we know about the meeting.

Concerning the other meeting, the year-end APA convention, Melvin E. Haggerty's report remarked that "in spirit [it] had a decidedly behavioristic tendency. More than half the papers either championed the behavioristic point of view in one or another form or [used] behavioristic methods [in their experiments]. A considerable part of the time the word itself was in the air."<sup>27</sup> Here at last is an indication of an apparently broad-based and positive response to Watson. Yet when we look for specifics (beyond the comments by Dewey, Münsterberg, and Warren), we cannot find, either in the titles or in the texts of the paper abstracts for the convention, any mention of Watson or behaviorism; at least for the modern reader, it turns out to be rather difficult to see which of these papers (with one or two exceptions) were supposed by Haggerty to champion the behaviorist point of view. (Judged by subsequent comments, Haggerty himself sympathized with behaviorism, but he also called Watson's refusal to consider introspective knowledge "the merest folly.")<sup>28</sup> And a different report on the convention, by APA secretary Walter V. Bingham, failed to notice any wave of behaviorism. It only remarked, with some relief, that in spite of the presence of the philosophers at the convention the paper sessions had not produced an inordinate number of philosophical or

theoretical papers; instead, it had been a well-balanced program (and, we might add, apparently without major surprises).<sup>29</sup> We shall meet this problem again: after discovering a tantalizing reference to the popularity of behaviorism among a certain group of persons, if we ask just who was involved and how it was expressed, we find the concrete evidence to be very elusive.

#### *A Behavioristic Underground?*

On this note ends our account of the recorded responses to Watson in the first year.<sup>30</sup> They were not overwhelming either in their frequency or their intensity, and furthermore came mainly from authors already in favor of some changes before Watson's appeal. Criticism of introspection was not new; neither was the use of objective methods or the advocacy of the study of behavior, as references to other authors like Meyer, Parmelee, and Thorndike indicate. (As Wells had expressed it, Watson had produced an "unusually concrete statement of a central idea that has always claimed certain adherents among us. . . ." <sup>31</sup>)

Was there a behaviorist revolution in the year 1913? The terms "behaviorist" and "behaviorism" had been accepted into professional language; there certainly was some awareness and, on occasion, lively discussion of Watson's contribution to the ongoing debate about the methods and objects of the science. In print, a few direct but mixed reactions agreed with some aspects of Watson's challenge with some enthusiasm while firmly rejecting others. But no reminiscence has described memories of a dramatic encounter with the manifesto; we have not found any contemporary evidence for the conversion of a single individual to Watson's position. While he may have issued a call to revolution, as yet we have seen no clear signs of a mass uprising. But scientific revolutions may take a bit more time. Or perhaps there was a behaviorist "movement," though it was underground, below the printed surface.<sup>32</sup>

Unfortunately, a laborious search of various archival collections has failed to be of much help. Indeed, I have not yet turned up a single letter from the year 1913 containing reactions to Watson's Columbia presentation or its printed version. The only contemporary references came from Watson himself. Sending some reprints of his paper to Yerkes, Watson commented: "I understand that [Yale's Roswell P.] Angier thinks I am crazy. I should not be surprised if this was the general consensus of opinion." (This estimate seems not far off the mark at least in terms of the consensus among the experimentalists, meeting at Wesleyan the following month.) While unfortunately Yerkes's reply is not preserved, Watson's next letter referred to some differences of opinion. At a later date, when the rift between Watson and Yerkes was widening, Yerkes implied that he had held back sharp criticism of the manifesto at the time.<sup>33</sup> And in another place, Watson indicated that James M. Cattell had scolded him for being "too radical."<sup>34</sup>

I have located very few additional pre-war comments (there are more later on) related to behaviorism in various archives: a very positive though brief one by Gilbert V. Hamilton, and two years later a rather solemn declaration by Margaret F. Washburn that she thought "JBW an enemy to psychology." In addition, there is the exchange of critical comments between Titchener, Angell, and Yerkes, reported earlier by Cedric A. Larson and John J. Sullivan.<sup>35</sup>

There are probably three reasons for this disappointing outcome of an extensive archival search. The most obvious one is that the relevant source material may be lost. Still, some of the surviving collections might have been expected to contain references to the allegedly revolutionary events. Thus a second reason, I would suggest, is that—at least by that time—the function of academic correspondence had shifted. It was no

longer a scholarly discussion and sharing of views between colleagues about the substantive issues of their field (assuming gratuitously that it had been so in earlier times); it was rather (with some exceptions) a somewhat hurried bureaucratic exchange, dealing mainly with concrete administrative-political problems: jobs, students, technical details of research and publishing activities, arrangements for meetings, etc., topped off by a bit of gossip and brief personal news. The typewriter had come to the office, but not yet the secretary; letters were usually typed, but mostly by their authors (and therefore without copies). In short, writing letters had become a chore. The discussion of substantive psychological issues may have been displaced to oral exchanges at formal meetings and informal visits; major statements on psychological issues were put into print.

And yet, I believe there is a third reason for this lack of references to behaviorism. Watson had said some strongly provocative and offensive things; but criticism of important aspects of the discipline and/or proposals for new directions had appeared before and after 1913, as they have on and off throughout the history of psychology. Usually, they are taken notice of, if coming from authors with some visibility, and may even produce a bit of a stir; some new terms may become fashionable; but then business goes on as usual for the vast majority of psychologists. Their activities are determined by other forces than verbal appeals—as any good behaviorist would know. After all, Watson's initial statement had not contained many concrete suggestions, except for the prohibition on introspective procedures. His main point had been a call for reconceptualization. We shall return to this issue later.

Two additional events occurring at year's end must be mentioned. Watson was elected president of the Southern Society for Philosophy and Psychology; he also became editor of the new *Journal of Experimental Psychology*, started by Warren upon Watson's suggestion.<sup>36</sup> But whether these honors were bestowed on him because of his call to arms or in spite of it (i.e. were based on his reputation as an outstanding young scientist acquired before 1913) is impossible to tell. We can only note that any hostility felt by the establishment was either not intense or not powerful enough to prevent these nominations.

### *The Second Year: Science or Technology?*

If, on the assumption that publication lag or other reasons delayed the response to Watson's historic paper, we search the psychology journals for the following year in order to find evidence of the full impact, we are in for another disappointment. Apart from registering some of the events and talks of 1913 already described, the *Psychological Bulletin* mentioned Watson or behaviorism hardly at all. A report on the 1914 conference of experimental psychologists made no reference to either, nor did Karl S. Lashley (then a postgraduate student at Johns Hopkins, collaborating with Watson on the homing instinct) in a general review of "Animal Behavior," even though he cited Yerkes's protest against the application of the term psychology to "behavioral material."<sup>37</sup> Besides printing Warren's 1913 presidential address, with its references to Watson, the *Psychological Review* carried a paper on psychological methods by Christian A. Ruckmich, which just listed the "behavior method" once without any elaboration, and two papers concerning psychology, consciousness, and behavior. One of them, by Elliott P. Frost, ignored Watson altogether; the other one, by B. H. Bode, did start out from Watson's paper, then shifted to Angell, Titchener, and Dewey, generally criticized introspection, and ended with a call for objective methods and the study of behavior. Yet Bode, one of the New Realists, insisted on the need to recognize the important distinction between automatic and conscious behavior.<sup>38</sup>

Fred Wells, Watson's strongest supporter in the preceding year, was the only one to bring up Watson anew in the *Psychological Bulletin*, but in the context of the major event of the year: Titchener's reply to Watson's attack. This reply, published (strangely enough) in the *Proceedings* of the American Philosophical Society, was an attempt by the leader of the introspective establishment to reassert its authority. Writing to Yerkes, Titchener had remarked that the present "flurry in favor of behaviourism" would quiet down after the appearance of "a few critical papers. . . ." <sup>39</sup> Actually, Titchener's counterattack, sharp in thrust though moderate in tone (except in a few footnotes) ended on a conciliatory note. After a brief summary of Watson's arguments, Titchener began his critique with two general impressions: (1) their "unhistorical character. . . . Watson's behaviorism is neither so revolutionary nor so modern as a reader unversed in history might be led to imagine. . . . Psychology has weathered similar proposals in the past"; and (2) the "logical irrelevance" of Watson's program to psychology. Since Watson would ignore the phenomena of experience, yet these phenomena clearly existed, someone obviously would have to start out where Watson left off and deal with the world of introspection. Focusing on a new subject matter did not make the other one vanish, and science did not have an Index which could prohibit concern with this experiential subject matter. <sup>40</sup>

(Though it seems that later on the behaviorist movement was to come dangerously close to the creation of such an Index, Titchener's argument was well taken. The exclusion of experience from behavioral psychology created a vacuum which always attracted attempts to fill it, within and more often without academic psychology—from the analysis of phantasy products to encounter groups, consciousness raising, and transcendental meditation.)

Titchener then zeroed in on the details of Watson's argument: the alleged failure of experimental psychology; the success of applied branches which supposedly had broken loose from it; Watson's way of dealing with central processes. Returning to a more comprehensive appraisal, he introduced a new argument: Watson's concern with human life, his practical goal of the control of behavior, defined not a new *science*, but was creating a new *technology*. In fact, Watson was asking psychologists to exchange their science for a technology, an exchange Titchener certainly was not willing to make. But the "technological coloring" was not really inherent in a true, scientific behaviorism which, if it was not simply biology, had to refer somehow (and here the argument seems to get a bit fuzzy) to psychological problems. As an example, Watson's reduction of thought to subvocal speech did not end up just with movements, it obviously involved words; but words, "as Watson seems to have forgotten," have also "meanings"; that fact took behaviorism back to (Titchnerian) psychology. Thus a true behaviorism would not, as he argued in the first round, remain irrelevant to psychology; rather, it could not help but contribute to psychology. Having made his peace with Watson, even if not quite embracing him, he wished behaviorism godspeed in its initial struggles to develop a new approach, while the more mature introspective psychology would "quietly go about its task. . . declining. . . to be eliminated or to be ignored." <sup>41</sup>

This counterattack appears to be a variation on Mary Calkins's arguments. Titchener maintained that one could not throw out the introspective baby; he, too, attempted to coopt behaviorism, though at a farther remove. It was the third theme, that of "relevance," which he strongly rejected, even though he recognized that it "strengthened the emotional appeal" of Watson's attack. But to transform psychology into a technology was both impossible (as technology always relied on more than one

science) and beneath the dignity of science: "Science goes its way without regard to human interests and without aiming at any practical goal"—a clear expression of the "classical" nineteenth-century tradition.<sup>42</sup> Fred Wells, in his summary of Titchener's paper for the *Psychological Bulletin*, thought that its strongest point had been the "science vs. technology" issue. But clearly differing from Titchener both in his view of Watson and in his values, Wells added that to follow Watson's rigid notions would not be helpful to the development of immediately practical knowledge. A useful technology would have to draw from both introspection and behaviorism.<sup>43</sup>

Referring to the debate in a letter to Titchener (who was to maintain friendly relations with Watson through the coming years), James R. Angell made some highly critical comments about Watson's historical and philosophical "illiteracy," which might have deserved even stronger criticism. Yet for much of Watson's work and for Watson personally, Angell claimed to have "very high regard."<sup>44</sup> Given this general attitude, it is not too surprising that Watson was selected as the new APA president by the nominating committee, of which Angell was a member.

### *Popularity and Cooptation*

The fact of Watson's election has often been taken as proof of the popularity of behaviorism at that time.<sup>45</sup> But in order to understand how his election came about, we must first consider the organizational structure of the APA. According to the 1894 constitution the APA's governing body, the Council, nominated all officers including new Council members. Subsequent election of the single slate of nominees at the annual meeting seems to have been a rubber stamp affair. Thus for twenty years the Council had been, in Samuel W. Fernberger's words, a self-perpetuating body which in effect selected the APA presidents.<sup>46</sup>

In 1911, a move to grant the membership a bigger voice was made, although Fernberger's APA history gives no details about the precipitating events. Apparently, there had been some rumblings.<sup>47</sup> If we can believe a letter by Watson to James M. Cattell, it was Watson who triggered off the change, by "joshing" three of his friends then serving on the Council about the control of the Council and the APA by a few men.<sup>48</sup> The Council promptly appointed a committee to consider the question of greater member participation in the nominating process. At the next convention, the committee proposed a three-year trial for a new plan. In place of the Council, a nominating committee elected from the floor at the annual meeting would nominate the president and two new Council members, after canvassing the membership for suggestions.

The meeting accepted this recommendation and elected a nominating committee consisting of J. R. Angell, one of the powers in the APA, as chairman, Edward L. Thorndike, the outgoing APA president, and J. B. Watson. The date was December 1912, i.e., *before* Watson's public pronouncements on behaviorism. Why was Watson elected? It seems likely that his role of vocal critic of the old procedures led to his position on the new committee, according to rather standard procedures of voluntary organizations. But while Angell stayed on the committee for two more years, Watson was replaced in the following year by the outgoing president. The nominating committee quickly became the preserve of ex-presidents of the APA, a development later formalized by constitutional change. As Fernberger commented, the new plan did not lead to greater democracy; it merely changed the personnel of the group making the nominations. Even this was a change more in appearance than fact: Watson was the only person ever to serve on the committee without having been an APA officer. Furthermore, the instructions to the

committee were vague, charging it to canvas the membership for suggestions, without stipulating how the results of the canvas should be used.<sup>49</sup>

Thus Watson's election in December 1914 did not come about simply as the result of a majority vote. In fact, it is not quite clear whether, under the new procedure, there even was a formal vote.<sup>50</sup> In any case, a week before the annual meeting Watson already talked about accepting office in the APA (about which he claimed to have some misgivings). Whether or not the nominating committee had selected him on the basis of the membership canvas we do not know. We do know that a decade later the nominating votes were usually spread out wide and thin over a large number of names: the eventual nominees received no more than a fraction of the nominating votes.<sup>51</sup> Most likely then, Watson's nomination was determined by a decision of the committee, in that year consisting of Angell, Thorndike, and Warren (in the chair), all three of them on good terms with Watson. Altogether, Watson's selection as president is no proof of a groundswell in favor of his behaviorism. More likely, it happened because (1) he had a good deal of visibility even without his manifesto (e.g., he had been editor of the *Psychological Review* since James Mark Baldwin's resignation from Hopkins dropped the journal into his lap in 1908; Cattell had starred him in *American Men of Science of 1910*; he had been secretary for the aborted International Congress of Psychology); (2) he had close personal connections with people influential in the Association; and (3) more generally because he represented the new generation, the truly experimental psychologists, when most of the older generation had retired from the lab by that time.

One last observation from a wider perspective on the question of the APA presidency appears relevant. While election to the Council was no guarantee of subsequent election as president, throughout APA history from 1893 to 1945 the APA president had always been a member of the Council before his election. There are only three exceptions to what looks very much like an unwritten rule: Hugo Münsterberg, a foreigner; Mary Calkins, a woman; and JBW, the behaviorist. But both Münsterberg and Calkins, selected by the Council under the old procedures, were sooner or later seated on the Council after their year as president. Not so Watson, who did not serve on the Council at any time. His election is clearly an anomaly; it may very well have been an attempt to coopt a critic.

Apart from this election, the 1914 APA program was "characterized by a lack of distinct specialization and by the complete absence of any Freudian titles," according to Harry L. Hollingworth's report. Neither the paper abstracts nor observer comments mentioned behaviorism. One special event relevant to our theme was a public demonstration of the introspective method, given by John W. Baird on the suggestion of the program committee.<sup>52</sup>

Another session heard an address by Walter B. Pillsbury on the "Definition and Method in Psychology." In his 1911 textbook Pillsbury had defined psychology as the study of behavior, only—in Watson's view—to drop this idea after a few pages in favor of a conventional treatment of the subject matter.<sup>53</sup> Now he made a strong plea for tolerance, arguing that the methods and definition of psychology should emerge from the actual work of psychologists; to impose rigid a priori definitions could have only detrimental effects. Wishing the plague on both single-minded introspectionists and behaviorists who claimed a monopoly on the proper view of science, he conceded that the terms "mind," "consciousness," and "experience" had become problematical, and that the best definition of psychology might be in terms of behavior. After all, the practical man was not interested in the mental states of others, but in their behavior.

Theoretically, too, much of what psychologists had been doing involved behavior; some of the assumed mental states were irrelevant to their experimental results. Furthermore, adopting this new definition need not change in the least the treatment of the subject as ordinarily presented (exactly the point which Watson had criticized in Pillsbury's earlier position). Certainly, to give up introspection was "to abandon. . . much if not most of the body of knowledge that we have at present. . . ." Using a variety of standpoints and all methods available will "lead soonest to the end of psychology, the discovery of mental laws and their explanation."<sup>54</sup>

Once more, a prominent psychologist had come to the defense of the method of introspection and its results. The retreat from "consciousness" to "behavior" as psychology's subject matter was seen largely if not exclusively as terminological, without implying any major change; the "relevance" issue appeared in a weak form, in the reference to the interests of the practical man. And finally, Pillsbury, not having any clear systematic position of his own, could afford to argue for tolerance, for letting a thousand flowers bloom.

Given this content of the 1914 journals and meetings, Langfeld's survey of the year 1914 in the *American Year Book* noticed no major changes; he reported a continued discussion of the fundamental problem of psychology: the relation of the mental and the physical world, with references to Warren, Holt, Münsterberg, and Prince. Mention of behaviorism remained relegated to the Methods section, according to which "discussion still center[ed] about the question of introspection *versus* behaviorism. . . ."; although Watson was still maintaining his radical view, "many psychologists believe in the combination of these two methods. . . ."<sup>55</sup>

In a similar vein, the *Bulletin* article summarizing "General Problems" began with the metaphysical issue of mind-body relations in an extended discussion of Holt's book and Santayana's reply; only later did it proceed to the "less general" issue raised by the "behaviorist" and reported the mixed reactions to Watson's "extreme" position we have described.<sup>56</sup> We still have not found a single individual coming out in wholehearted support of Watson, no hard evidence for a palpable change in the way psychologists were arguing or doing their work, even though the terminology of "behavior" had obviously gained some popularity as well as ambiguity (and even Titchener was soon to give up his defense of "consciousness" in his 1915 text).<sup>57</sup> While we have discovered several references to the interest (and concern) aroused by Watson, we note that some writers still referred to the "behaviorist" in the singular<sup>58</sup> and only two or three to a behaviorist "movement" with at least Langfeld's early use not referring primarily to Watson.<sup>59</sup>

By 1915, Watson's first book, *Behavior: An Introduction to Comparative Psychology*, had been published. The introductory chapter had reprinted his 1913 papers with only minor changes; the main text had fleshed out Watson's behaviorist program a bit more in a discussion of instincts, reflexes, and habit development in animal psychology. A short description of the book's content by Langfeld and three special reviews by Carr, Thorndike and Herrick, and Haggerty were quite favorable overall; the longer ones criticized some details and all rejected Watson's more extreme theoretical statements, especially the ban on introspection.<sup>60</sup> In a 1910 APA paper and in the introduction to the 1911 edition of *Animal Intelligence*, Thorndike had argued strongly for the importance of objective studies of behavior. Now he expressed his regret that Watson had not added a chapter on *human* psychology to show that recognized psychologists had, for thirty years, carried out behavioral studies of humans. Watson should have corrected the impression that human psychology had been exclusively an introspective

affair. But even Thorndike found it unwise to ignore the special form of observation of themselves humans were capable of; it might "well play some part in science."<sup>61</sup>

Apart from the reviews, few references to Watson or his book can be found in the 1915 *Psychological Bulletin*. In the *Psychological Review* of 1915, Watson's name does not seem to have appeared even once (except on the masthead, as the journal's editor). Only one passing reference to the "behaviorist standpoint" could be located,<sup>62</sup> while five of the six issues of the journal contained at least one article dealing with imageless thought, images, or imagery of one sort or another. The 1915 volume of the *Journal of Philosophy, Psychology, and Scientific Method* included a protest by Walter Hunter against Watson's misinterpretation of Hunter's delayed reaction experiment, and a few articles on the issues of consciousness and behavior, with both positive and critical references to Watson.<sup>63</sup>

### *Some Indices of Influences—Or Lack Thereof*

This rather detailed (though not exhaustive) account of recorded reactions to Watson stands in definite contrast to some retrospective histories which claim or at least imply that Watson's behaviorism, supported by an anonymous *Zeitgeist*, quickly swept the field. It is impossible, though, to continue with such a detailed description. Fortunately, by 1915 we can find some sets of data allowing more general estimates of the situation.

One of these involves an attempt by Knight Dunlap to determine the usage of psychological terms. In 1915, Dunlap sent a questionnaire to over one hundred senior members of the APA, asking them a number of questions about their preferences concerning the terms: experience, consciousness, thought, and sensation. He published the tabulated results, together with individual comments identified by author, in the house organ of Johns Hopkins University. Dunlap's general conclusion from this survey was that the answers "mirror[ed] most strikingly the confusion which reigns in psychological discussions. . . ."<sup>64</sup> We can ignore most of this confusion and concentrate on information relevant to behaviorism. Of Dunlap's fifty-eight respondents only two rejected clearly and unequivocally the concept of consciousness and replied consistently in a behaviorist manner: the behaviorist himself, J. B. Watson, and the New Realist philosopher Walter T. Marvin. A handful of others expressed, in different ways, some sympathy but not their identification with behaviorism.<sup>65</sup> Even Edwin B. Holt, who was never a Watsonian behaviorist,<sup>66</sup> did not seem to take an extreme position; Thorndike's replies were equivocal, implying that while he himself had little use for mentalistic terminology (and for the questionnaire as well), he did not care to impose his ways on others. Altogether, the fifty-page report represents as striking a confirmation as we can hope to find for the conclusion that by 1915 psychology was yet showing little manifest influence from Watson's polemics.

Two qualifications are in order. First, Dunlap had received answers from only half his sample. Another fifteen percent had declined to answer, most of them claiming to be out of touch with the problems involved. As for the rest, a check of the APA membership list shows very few if any individuals among those failing to reply who could have been expected to take a radical position. The second question concerns Dunlap's selection of only the senior psychologists (defined in terms of a professional degree received by 1903, the year of Dunlap's doctorate) made in order to obtain stable, considered opinions developed over years of professional activity. Unfortunately, this criterion eliminated the younger men who, according to some views, had been more responsive to Watson's call. Yet the APA *Yearbook* for 1917 contains only about a dozen names of younger psy-

chologists,<sup>67</sup> whose inclusion in the survey *might* conceivably have produced more support for Watson's position. Dunlap's results may not have been very biased after all.

In the same year (1915), L. W. Sackett conducted a survey on the question of the preferred order of topics for a general psychology course. Disturbed by the lack of uniformity in the chapter sequence of different texts, and by what he took to be its implication for the scientific status of psychology, he asked his thirty-five respondents for the "most fundamental definition of psychology." As he reported, "those who define in terms of mental processes are about as numerous and as insistent as the behaviorists. . . ." This gives us, for the first time, a specific reference to a set of behaviorists. But Sackett unfortunately failed to give us their number and names; furthermore, in the remainder of the article, we find again that the definitions classified as "behaviorist" were not necessarily in Watsonian terms. Nor was the selection and organization of topics strongly affected by these definitions; according to Sackett, "there was as much conformity among those not holding the same point of view as among those who did."<sup>68</sup> Finally, according to his tabulation of topic orders, almost all respondents were willing to include such topics as self, will, imagination, apperception (if in varying order), which surely a true Watsonian would have protested against—or so it seems to me. As Harvey A. Carr had said in his review of Watson's book: "Behavior in the human can be studied by the subjective method. . . . [in the opinion of] some human psychologists who call themselves behaviorists."<sup>69</sup>

Another general comment, in the *American Year Book*, claimed that courses in behavioristic psychology were "increasing in the large universities." Yet when in 1919 John A. McGeogh tabulated all psychology courses offered nationwide, he found only one out of hundreds carrying the title "Human Behavior" (probably Richard M. Elliott's course at Minnesota) and twenty-five classes in "Animal Behavior"; how many of the general or experimental psychology courses were behavioristic in orientation we cannot tell.<sup>70</sup> Again, we have been unable to pin down some comments about the popularity of behaviorism. Let us look at a different approach to the question.

In 1916, Christian Ruckmich reviewed the "Last Decade in Psychology" as represented in journal publications. A good Titchnerian, he had been somewhat annoyed by the attacks on traditional psychology. For his analysis, he classified about 500 articles published during the decade in the area of general human psychology (since specialties might require their own special methods) as using either introspective or nonintrospective, of late called "behavioristic," methods. According to his count, the data showed introspective methods to have produced two and a half times more experimental studies than the behavioristic ones, "with a slump in 1914 and a definite recovery from the critical attacks. . . ." With obvious satisfaction he concluded that "introspection has contributed more generously to normal, human, adult psychology. . . than has any other method."<sup>71</sup> While we are surely entitled to some reservations about Ruckmich's procedures and biases, this is the most specific estimate of the effect of the Watsonian revolution on the research activities of psychologists we have up to this time, and it is not impressive. Still, it is noteworthy that Ruckmich seems to have seen no difficulty in applying the category of "behavioristic" *method* to studies reported long before 1913.

At about the same time, Albert T. Poffenberger produced a laboratory manual for psychology, in which "special attention [was] devoted to an encouragement of introspective analysis on the part of the student."<sup>72</sup> As Edward C. Tolman recalled much later, in this period he had been exposed to Watson's book in Yerkes's class on Comparative Psychology, and as a result he was "sold" on behaviorism.<sup>73</sup> But in another place Tolman

added that the "behavioristic point of view had not yet really got into [his] blood."<sup>74</sup> In fact, two years later he published a largely introspective study on "Meaning and Imagery."<sup>75</sup> Watson had not yet wiped introspection off the map.

At year's end of 1916, the twenty-fifth anniversary of the APA and of the *Philosophical Review* elicited a number of papers by renowned psychologists discussing the past, present, and future of their science. In general, these papers treated behaviorism as only one trend among many and dealt with it briefly. Margaret Washburn defended introspection against Watson's attacks. Joseph Jastrow mentioned behaviorism in passing. Pillsbury pointed to the disagreement between Watson and Yerkes regarding animal consciousness. Cattell, while strongly urging the replacement of introspective studies of the mind by experiments on "behavior and conduct," was more concerned with other issues, especially the economics of research support. Dewey's address on the future of social psychology applauded behaviorism as a promising trend, which could—in a twist surprising to modern readers—in combination with McDougall's work on instincts lead to an understanding of the social emergence of mind—not strictly a Watsonian position. Finally G. Stanley Hall, little concerned with theoretical quibbles, speculated in the grand manner about the role of psychology in the cataclysm looming on the horizon: the war, which was soon to disrupt the lives of many psychologists.<sup>76</sup>

The events of the war years did not silence the behaviorism debate completely. And even before that time Watson had expanded his position in his presidential address on the conditioned reflex, begun his observational studies of human infants, and written an early version of the first chapter for his new book on behaviorism.<sup>77</sup> However, the narration of events will conclude with three more indications of Watson's influence, or lack thereof. In 1915, Dunlap's efforts had initiated the formation of an APA Committee on Terminology, charged with producing some agreed-upon definitions of crucial psychological terms. The first installment of this work was published in the 1918 *Psychological Bulletin*. But Watson's position was not represented in these definitions. With the exception of one subcategory, which accepted "behavior" as the "reaction of an organism to the environment" but expressly restricted it to biological usage, all relevant definitions, e.g., of "psychobiological," led back to others containing the words "mental" or "conscious."<sup>78</sup>

This omission of behavioristic views was apparently no accident. The papers of Mary Calkins, one of the committee members, contain a preprint of the committee report, dated September 1917, and bearing some handwritten corrections. Instead of the twenty-eight definitions published in the *Psychological Bulletin*, this document listed twenty-nine items. Number 29 was: "Behaviorism. Identification of *psychology* with the science of *behavior*." But this definition had been crossed out in ink.<sup>79</sup> The subsequently published version did not include the term behaviorism.

Unfortunately, no correspondence is attached to this preprint. Thus it remains uncertain whether the elimination of Watson's slogan was a bit of skulduggery on the part of one or more committee members, or whether it reflected the result of a mail survey of sixty psychologists in the fall of 1917. Still, in either case this "smoking gun" supports the argument that, five years after his manifesto, any inroads Watson had made in psychology did not lead very far into its center. Even an updated version of the committee's work published in 1922, defining eight varieties of psychology, did not include behaviorism among them. The one closest to it, "Objective Psychology," described in an added note as a "synonym for *Behavior Psychology*," was defined as "concerned with *mental* [!] *phenomena* expressed in the *behavior* of the organism to the exclusion of *introspective data*."<sup>80</sup>

Another formal map of psychology, the classification system of the *Psychological Index* (the forerunner of the *Psychological Abstracts*) had introduced the term "behavior" into a major heading in 1911, as "Behavior in Other Species." "Behaviorism" did not make its first appearance until 1924, and even then only as a minor category under a subtitle, together with "Vitalism."<sup>81</sup>

While this eventual inclusion of the label reflects the fact that after the war, and with Watson's new publications, the debate over behaviorism had heated up again, these further developments will not be treated here. One final item from this period must suffice. In 1922, Walter S. Hunter addressed his "Fellow Workers" in an "Open Letter to the Anti-Behaviorists," in which he speculated about the reasons for the frequent attacks on behaviorism. "If here, there, and yonder, psychologists were joining Watson's banner, you might be actuated by the menace of opposing numbers."<sup>82</sup> But if one rejected several attempts to appropriate the label for other, improper purposes, the literature failed to reveal the spreading of true (Watsonian) behaviorism. In fact, Hunter—who had earlier<sup>83</sup> described his own view as semi-behaviorist—could find only *two* behaviorists: John B. Watson and Albert P. Weiss.<sup>84</sup> Twenty-three names "and others" made up Hunter's list of the antibehaviorists. Did Watson really gain only one adherent in nine years? Watson himself had, three years earlier, acknowledged the fact that "my type of psychology is not popular. . ."; replying to Paul T. Young who had asked for help in finding a job he had added: "[therefore] I rarely hear of positions. . ."—at a time when many jobs were opening up after the war.<sup>85</sup>

### III. IN SEARCH OF EXPLANATIONS

Obviously, this is not the whole story. For instance, although the Terminology Committee of the APA had failed to print a definition of Behaviorism in 1918, the *Encyclopedia Americana* carried a two-page article on "Behavior and Behaviorism" in the same year.<sup>86</sup> Although Watson's 1914 book was never reviewed by *Science*, in spite of Watson's anxious inquiries, Edwin B. Holt had recommended it to his readers as a "valiant and clear-headed volume."<sup>87</sup> And though it turned out to be difficult to identify many probehaviorists in the contemporary records, later sources do indicate that behaviorism had, in the teens, an impact on a number of mainly younger people besides Weiss and Hunter: Karl S. Lashley, Harold C. Bingham, Melvin E. Haggerty, John F. Dashiell, and a group of Harvard students, among them Floyd H. and Gordon W. Allport, Richard M. Elliott, and Edward C. Tolman. (However, at Harvard the influence had come less from Watson than from Holt, who was teaching a "red-hot behaviorism" at the time,<sup>88</sup> and from Ralph B. Rerry.)

Neither is this the end of the story of the behaviorist revolution, only of its first phase. But it is high time to ask what all the details reported so far add up to. Perhaps the general drift of this account has not really come as a surprise to the reader. Though I had initially expected a rather different course of events, once I started to think about it I found the emerging story not too surprising either. Nevertheless, it may present difficulties for some traditional explanations: If there was a *Zeitgeist*, it seems that so far he (or she) communicated mainly like God to Moses, on a one-to-one basis. If the fact that Watson's program was a strictly American product had any influence on its acceptance, so far we have not seen any direct or even indirect reference to it. Fred Wells, Watson's first vocal supporter, was anything but parochial; his writings were sprinkled generously with German, French, and Latin quotes.

Another popular explanation has to do with the acceptance of behaviorism because it was so practical. Although this argument touches on what I believe to be a crucial

aspect (and though we have found mostly favorable responses to what was called the "relevance" theme), it puts some complex issues too simply. For instance, the (American) *Journal of Applied Psychology* did not begin publication until 1917; the similarly titled German *Zeitschrift für Angewandte Psychologie* had first appeared in 1907. An "*Institut für Angewandte Psychologie*" had been established in Berlin in 1906, almost a decade before the start of an applied psychology program at Carnegie Institute of Technology. (And the *Journal of Educational Psychology*, appearing in 1910, had been preceded by a decade by the *Zeitschrift für Pädagogische Psychologie und Experimentelle Pädagogik*.) When Titchener had warned, in 1909, against the undesirable developments toward applied psychology, his specific references were to five German psychologists (and one Frenchman: Binet).<sup>89</sup> While such a list may in part reflect Titchener's European orientation, it should also help to scuttle the myth that applied psychology was "ganz amerikanisch," and that the impractical German professors were preoccupied with nothing but abstruse and esoteric speculations of a philosophical nature. Applied psychology had its roots at least as much in Europe as in America. Furthermore, as the European example shows clearly, an applied psychology does not have to be behavioristic at all (unless, of course, we view it through behaviorist eyes).

Another myth should also be laid to rest: that behaviorism developed out of animal psychology because the situation there forced the researcher into a behavioristic stance. As others<sup>90</sup> have pointed out before, this does not seem altogether true. At least some of the major figures in the small group of American animal psychologists did not feel at all compelled by their subject matter to adopt this position. Washburn and Yerkes both rejected Watsonian behaviorism (though Yerkes claimed that in his early days around 1900 he had been a pre-Watsonian Watsonian behaviorist).<sup>91</sup> Carr belongs in this category, too. In fact, in the early twenties we find more philosophers than animal psychologists among those taking a behaviorist stance; the psychologists in this group (Holt, Tolman, Edwin R. Guthrie, and a bit later Clark L. Hull) were more likely to turn to animal work after their conversion than to move in the reverse direction.

Abandoning such obviously post hoc explanations as, at the very least, overstatements, we should look at a different version of explanation, which is not new but in our days has been formulated in Kuhnian terms.<sup>92</sup> It goes like this: Around 1912 the "imageless thought" controversy laid bare an "anomaly" which the existing science could not deal with; this produced a "crisis" which led to the abandonment of the old "paradigm" and the acceptance of a better one, which could account for the anomaly. But this version, too, is at least a gross oversimplification; it seems to fit neither the facts nor Kuhn's theory. The imageless thought controversy was indeed a problem, but one among at least several; only retrospective historians and polemicists have made it into a "crisis." In his original paper, Watson referred to it only in one sentence in a footnote, in which he listed other problems of introspective psychology.<sup>93</sup> Robert S. Woodworth, not a bad scientist, was trying to solve the problem two years later; he did not see it as an anomaly creating a crisis.<sup>94</sup> And Titchener, in my view quite properly, replied to Watson's claims about the failure of introspection that in many scientific areas the results of observations did not always agree; it was reasonable to allow some time to work out the apparent contradictions. After all, his kind of introspection had been introduced less than ten years before, and not fifty, as Watson had asserted.<sup>95</sup> (We might add that after a turn to behavioral methods, the results obtained by different experimenters have not always agreed either). And when we look carefully at Kuhn's argument, we find that anomalies are always around in science. Only rarely do they touch off crises and revolutions.

*A (Slow) Perceptual Shift and a Missing Paradigm-Exemplar*

I am impressed by the applicability of one of Kuhn's ideas: the change in the way of seeing things involved in paradigm change. Such a shift did occur in, I believe, a fundamental way. It is most visible in the manner psychologists described their methods of observation. In the earlier phase we find again and again the statement that the introspective method constitutes direct and immediate contact with the subject matter, while what we now mean by objective observation was then only an indirect or mediate one.<sup>96</sup> After the revolution, the meanings are reversed: objective observation is the direct contact, while information obtained through introspection, if not altogether impossible or irrelevant, is at best indirect, a tenuous base for fragile inferences from questionable verbal reports. I think this is more than a manner of speaking; it reflects a real change in the way psychologists experienced, or had been trained to experience, their reality. For most psychologists, however, this shift did not seem to occur suddenly, as an "aha" experience with a reversible figure; it took a long time to develop—even if for us, immersed as we are in post-Watson "behavior" language, it is hard to look upon the earlier construction as anything but patently contrived and transparent. But this shift is what Watson, having made it himself, demanded from others. To accept the addition of objective observations and performance measures was not so difficult for many psychologists (as we have heard), because they had said or done so even before Watson.<sup>97</sup> But he rejected such a mixture of methods, such a compromise; he was asking for the reversal in the definition of what was real—this made him appear so radical, and made it difficult for others to follow him.

Besides the crisis-inducing anomaly, another element of Kuhn's theory seems to be missing: the new paradigm. Many people have, in my view, misread Kuhn (helped along by his ambiguities) and assimilated his concept of paradigm to other, more familiar ideas: theories, conceptual systems, viewpoints. But such an understanding turns Kuhn's argument into an old story. What may be novel in Kuhn was his emphasis on the role of the paradigm-exemplar, the specific case of the successful solution of a (crucial) problem, which becomes a relatively concrete model-example for the solution of other problems.<sup>98</sup> But where was Watson's paradigm-exemplar? It was not there.

Should one not cite the conditioned reflex and Pavlov's salivating dogs? Our textbooks often seem to portray the development of modern psychology as an historical chain, from Darwin to Pavlov to Watson, and on to Hull and Skinner. But this compact story is not entirely true. While eventually coming to play the role of paradigm-exemplar (a count of the textbooks reprinting the original line drawing of Pavlov's dog is overdue), the conditioned reflex entered only slowly and in stages into Watson's thinking and did not gain its dominant role until the mid-twenties. Even then, a close look shows the surprising fact that the actual experimental data underlying the diagram, the concrete observations made, were almost nonexistent, as far as Watson and American psychology in general were concerned. After all, Pavlov's dogs lived in a far-away country. Knowledge of them came only through indirect channels, in translations and third-hand reports; some of these reports were imprecise, obscure, or clearly wrong.<sup>99</sup> Did nobody try to replicate the work?

Watson's APA address describing his own and Lashley's observations on motor conditioning was actually based only on pilot studies, which had raised at least as many questions as they had answered. The literature contains no final report of Lashley's elaborate studies of salivary conditioning; a close reading of his progress reports seems to indicate that he gave up the effort because it had failed. (Hilgard and Marquis's classic

on conditioning drew a similar conclusion.) As for Watson, he once mentioned briefly an attempt to develop an experimental analogue to reactions to lightning and thunder, by exposing infants to a strong light followed by a loud sound.<sup>100</sup> Subsequently, Watson never referred to this experiment again—had it been a failure too?

The only concrete observation Watson produced (in 1920) was the famous case of "Albert and the rat." But while this case did come to serve as a powerful exemplar, it was not a very solid data-base which could carry a whole theory. It was, after all, an experiment with a sample of one; it also involved some fairly problematic procedures.<sup>101</sup> Some years later, Elsie O. Bregman tried to replicate Watson's experiment in a more systematic manner. As Hilgard and Marquis summed up: "Later experiments have been unsuccessful in duplicating it. . . . The process is not as simple as the story of Albert suggests."<sup>102</sup>

But surely, there must have been other American conditioning studies. Not really. The first bona fide American conditioning experiment with humans was not reported until 1922, by Hulsey Cason; and he did not feel compelled to accept a Watsonian interpretation. The mass of conditioning experiments did not appear until after the translation of Pavlov's work had become available to American psychologists in 1927 and 1928. All Watson had was Little Albert. Yet while he presented a beautiful example of an idea, if one had already accepted this idea, he did not provide solid scientific evidence to a skeptical observer. The actual paradigm-exemplar, as a way of doing things, did not produce the paradigm shift at all; the exemplar came after the formula had been developed, and even then it was more like a diagram than a way of actually doing things.<sup>103</sup>

Here we may have put a finger on one of the places where Watson was hurting, on one of the facts at least partly responsible for the slow rate of conversion of his fellow scientists. What was it, after all, that Watson had to offer them? He had used some strong words in attacking their psychology and had exploited some of their troubles; he had proposed some intriguing ideas. But in spite of his insistence on a new, harder science, objective observations, etc., when it came to experimental data he had very few (apart from his animal studies) to justify his attempt to usurp scientific authority.

Watson's 1913 research program, loose as it was, seems to have been plagued by false leads or experimental failures. The two concrete proposals of 1913, the identification of thinking with subvocal movements and his explanation of affection, in good Freudian fashion, in terms of activity of the sex glands, had been proffered without any empirical evidence. (The two major specifics radical behaviorism eventually became identified with, environmentalism and the conditioned response, did not become central to Watson's system until ten years later.)

Apparently, Watson spent some time trying to collect data on laryngeal movements, but eventually gave up.<sup>104</sup> His first attack on conditioning (still within a limited theoretical context) also seems to have ended with an impasse, and with a shift to observational work on infants. By 1920, not one concrete experimental problem of human psychology had been solved convincingly by Watson and had provided him with a Kuhnian paradigm.

Yet he was addressing professionals who had been trained in the use of introspective methods, and were so training others; who had believed all along that what they were doing was indeed real science, since it involved laboratories, observations, measurements, controlled conditions, etc. Watson was asking these professionals to throw their tools overboard as not scientific, to declare all the hard-won generalizations that filled their

textbooks and their lectures to be artifacts of bad methods. This was too much to ask, as we heard one psychologist after another assert in their reactions to Watson. Though obviously they had not yet solved all the difficult problems of mental phenomena, nevertheless they were the professional experts on the mind, on the inner experience of man. All of a sudden they should forswear their claim to this expertise, surrender their scientific authority?

In recent years we have heard some calls for radical changes in psychology or in its specialities. Their reception, with responses ranging from hostility to indifference—even though there are at least *some* anomalies around in our science—should let us empathize with the feelings of the established psychologists of Watson's time. What did Watson have to offer them in return for their renunciation? He promoted a different version of science which, so it seemed to them, would make them lose their professional identity and turn them into either biologists or physiologists. Why should they risk such an exchange?

#### *A New Goal for Psychology*

After all, Watson's call for a revolution in psychology had been largely programmatic. His main thrust had aimed at a redefinition of scientific standards and a redirection of psychology. Put simply, this redefinition proceeded on three different levels: First was the change in *method*: the call for objective procedures and the elimination of "unscientific" introspection. This argument, having the most direct impact on the work-day of psychologists, drew the largest share of public responses. While the emphasis on objective methods, already widely used and advocated, met with a good deal of sympathy, the total proscription of introspection ran into strong resistance, if only for the intolerant tone of its imposition (even from those not using introspection in their own work, like Thorndike and Yerkes).

The second level concerned the *subject matter* of psychology, changing it from mental contents and/or processes to movement and behavior, with its attendant peripheralism, rejection of central processes, and associated metaphysical connotations. This issue, too, met with considerable debate. Its acceptance required a fundamental figure-ground reversal which was not easy to accomplish and took its time in coming about, although the expansion of the field to problems of "real life" had widespread support in the growing discipline.

I would like to propose, however, that the crucial argument occurred at a third level and dealt with the *goal* for psychology. According to Watson, this goal was to be the "prediction and control of behavior." Here Watson proposed something radical and new for psychology. All textbooks before him had defined psychology's aim in a different way, as description and/or explanation of mental phenomena, their understanding (on occasion including self-understanding, even self-improvement), etc.: the traditional goals of academic science.

Where Watson obtained his formula about prediction and control is not quite clear. Initially I assumed that he had taken a cliché from the natural sciences which he was trying to emulate, but a somewhat cursory search complicated this answer. Most sources I found (discussions of philosophy of science and encyclopedia definitions of "science")<sup>105</sup> did not define science in terms of prediction and control, mentioned prediction only in passing, and were more concerned with the problem raised by positivism: the banishment of causes, description versus explanation. However, the biologist Jacques Loeb had on several occasions described the goal of modern biology as the "control of life-phenomena" and in 1912 even referred to two outcomes, control or quantitative predic-

tion. Watson, who had studied with Loeb at Chicago, may well have derived his novel definition of the goal of psychology from Loeb's ideas.<sup>106</sup>

Of course some psychologists had, if only in passing, spoken of control before Watson: William James had once talked about "practical prediction and control" as the aim of all sciences, and about the demand on psychologists from all kinds of managers for "practical rules" for the "control of states of mind."<sup>107</sup> Cattell's famous St. Louis address had eagerly anticipated the "application of systematized knowledge to the control of human nature," to the "control of ourselves and our fellow men."<sup>108</sup> Thorndike had mentioned "control [of man's] acts" in a 1911 essay defining psychology as the study of behavior.<sup>109</sup> Yerkes's 1911 textbook contained, as sixth and final part, a rather abstract discussion of foresight and the control of mental events.<sup>110</sup> And finally, in England William McDougall had published a little book, in which he stated as psychology's aim: "to increase our understanding of, and our power of guidance and control over, the behaviour of men and animals."<sup>111</sup> (Watson knew McDougall's earlier books.)

Still I believe that Watson's treatment of the issue constituted a quantum jump. Only with him did control become a fundamental idea, part of the textbook definition; and it came right at the start, appearing in the second sentence of his 1913 paper (and at least four more times in fourteen pages): The "theoretical goal [of psychology] is the prediction and control of behavior." Why did Watson use this phrase? Why "theoretical goal," why not "practical" goal, or just "the" goal? Did theoretical mean hypothetical, ideal—a goal unreachable in practice? I do not think that this is what Watson tried to say.

Before Watson, the aims of psychology had been seen in terms of the category of pure science, as contrasted to either applied science or art. Of course, most psychologists have had their dreams of glory, in which their science would affect the real world and solve some of its problems. Even defenders of an ascetic science, like Titchener, believed that scientific knowledge would eventually produce its practical fruit and thus justify science to the impure, though true scientists ignored the question of application. But James's brief remark concerned the pressures from the *outside* for practical rules, presumably *derived* from theoretical knowledge. The quote from Cattell referred to the *application* of systematized knowledge. And Yerkes ended his discussion by saying: "Control is the outcome, albeit not the avowed goal, of scientific research. . . . Psychology is *not* the science of mental control."<sup>112</sup> It merely would make it possible. In other words, traditionally the issue was seen as involving two steps: first, the acquisition of knowledge as the task of science, and then its application to practical affairs. What was debatable, and debated, was the desirability, the timing, and the division of labor in such application. Watson saw the issue differently. His phrase "theoretical goal" shows him reshuffling the traditional categories<sup>113</sup>; prediction and control were no longer indirect or second-stage outcomes, but had become the direct focus and criterion of theory development. I think this notion was radically new (for psychology) and provided the fulcrum for the reorientation of psychology in subsequent decades, so that today any psychology major will state what is self-evident to him: that the goal of (behavioristic as well as cognitive) psychology is the prediction and control of behavior.

It is interesting, and somewhat puzzling, that the early reactions to Watson, the more intensive debate over behaviorism in the early twenties, and more recent analyses of Watson's contribution were largely silent on this point.<sup>114</sup> Only Titchener's rebuttal focused on the behaviorist's goal, in his accusation that Watson was trying to create a technology rather than a science. Thorndike's and Carr's reviews of Watson's *Com-*

*parative* book, which reprinted the 1913 papers, reacted in passing to this point; yet both seem to have misunderstood it. In part, I believe, the Janus-face of the term *control* is responsible for the lack of discussion. Control could mean control of conditions, precision in experimentation, elimination of unwanted influence; but that was a commonplace. Or it could mean what Watson clearly intended, at least much of the time (he also used a more abstract formula about predicting stimuli from responses and responses from stimuli), and spelled out later: *social control*, i.e., manipulation of human beings for the benefit of society.<sup>116</sup> But the experimental psychologists failed to confront this aspect of behaviorism in their theoretical debate and eventually defined the issue away.

Yet others did get the message. The first text in applied psychology—while not strictly Watsonian—opened on a distinctly behavioristic note. It introduced the ideas of prediction and control, and explained that the change in emphasis from consciousness to behavior may have been due in part to theoretical difficulties (as with imageless thought); but it was also due to the demands of practical life.<sup>116</sup> About the same time, John Dewey's address on the need for social psychology linked behaviorism with the development of a social psychology in the service of social control. Reviewing the applications of psychology to industry in 1920, Henry Link cited the Gilbreths, involved in time and motion work in industry, as the "ideal behaviorists" and concluded: "Watson's work is, in fact, the conscious methodology which practically all recent literature in industrial psychology has more or less explicitly *implied*."<sup>117</sup> Soon after, W. V. Bingham, head of the applied psychology unit at Carnegie Tech, was to complain about this accidental (and to him unfortunate) identification of behaviorism with applied psychology, which made his attempt to separate an applied science from the pure science of psychology more difficult. And Floyd Allport described social psychology as becoming "the study of the social behavior of the individual. . . [needed] for study and control of the socially significant aspects of individual response." He also wrote in his lecture notes: "Responsibilities incident to human control. Practical psychology is essentially behavioristic in method."<sup>118</sup>

#### IV. CONCLUSION

Such beginnings are part of a larger and complex pattern of developments in the twenties, which is discussed elsewhere.<sup>119</sup> So far, it appears that a less than monolithic mainstream of experimental psychology, debating issues of method and concepts, resisted Watson's advances for a long time, assimilating them gradually in the form of the more abstract S-R formula. Yet in the meantime others, inside and outside psychology, more immediately concerned with problems of social control and helped along by the exigencies and opportunities of World War I, were finding Watson's arguments a convenient or inspiring rationale. Even if they may not have accepted all of his theoretical ideas, Watson had given the discipline a strong push in the direction of technological science.

Certainly, Watson had not singlehandedly transformed psychology. Too many of the specifics of his argument had not been original with him—although the common practice of briefly quoting one or another author's use of "behavioral" definitions of psychology before 1913,<sup>120</sup> in order to demolish Watson's claim to priority, misses the mark. It overlooks the fact that Watson had already in 1907 declared that the "science of behavior" was "thoroughly established."<sup>121</sup> It is true enough that at this time he did not yet apply it to all of psychology; nonetheless, the phrase had been abroad long before 1913. What counted were its corollaries.

But while using ideas from others, as well as appealing to their dissatisfactions with the status quo, Watson had sharpened the arguments into a revolutionary weapon.

Provoking a good deal of resistance with his rhetoric, he also discovered the price to be paid for his shift, in 1913, from a strategy of succession to, in Pierre Bourdieu's terms, a high-risk strategy of subversion of established scientific authority.<sup>122</sup> When the shift finally paid off, others reaped the benefits. Watson was no longer a part of the professional community, when eventually the reestablished monopoly of scientific authority had accepted prediction and control as the criterion of positive science and declared only outward manifestations, "behavior," to be legitimate scientific data. Anything mental had become unobservable, an at best problematic inference if not a superstition pure and simple.

In a sense, the present research effort turned out to be a failure. Looking for the sources of behaviorism's powerful appeal to American psychologists, we found more often criticisms or partial acceptance. Did we look in the wrong place? What I had not realized at the outset was that the victory of behaviorism took so much longer in coming about. And at least this scientific revolution did not involve simply conceptual transformations and conversions, but something Kuhn has not talked about—a power struggle in a discipline, affected by events without. Like the other social sciences,<sup>123</sup> the young profession of psychology grew up facing a predicament, in its dependence on a larger clientele, on the one hand, and its desire for autonomy and academic status, on the other—as reflected in the rhetorics of relevance and purity. Eventually, psychology adopted Watson's ingenious solution combining the appeals of hardheaded science, pragmatic usefulness, and ideological liberation.

#### NOTES

1. John B. Watson, "Psychology as the Behaviorist Views It," *Psychological Review* 20 (1913): 158-177. For the date of his presentation see "Notes," *Psychological Bulletin* 10 (1913): 124; *New York Times*, 23 February 1913, Section 3, p. 4; "Notes," *Columbia University Quarterly* 15 (1913): 303; and "Annual Report," *Johns Hopkins University Circular* 33 (1914): 64. Watson, and others following him, later predated his lectures as given in the winter (or fall) of 1912. But no documentary evidence supports these earlier dates.
2. John B. Watson, "Image and Affection in Behavior," *Journal of Philosophy, Psychology, and Scientific Method* 10 (1913): 421-428.
3. Watson, "Psychology," pp. 158, 163, 164, 170.
4. Herbert S. Langfeld, "Jubilee of the Psychological Review: Fifty Volumes of the Psychological Review," *Psychological Review* 50 (1943): 143-155; Richard J. Herrnstein and Edwin G. Boring, *A Sourcebook in the History of Psychology* (Cambridge, Mass.: Harvard University Press, 1966), p. 507; John C. Burnham, "On the Origins of Behaviorism," *Journal of the History of the Behavioral Sciences* 4 (1968): 143-151.
5. Edward C. Tolman, (Autobiography), in *A History of Psychology in Autobiography*, Vol. 4, ed. Edwin G. Boring, Herbert S. Langfeld, Heinz Werner, and Robert M. Yerkes (Worcester, Mass.: Clark University Press, 1952), p. 331.
6. David Bakan, "Behaviorism and American Urbanization," *Journal of the History of the Behavioral Sciences* 2 (1966): 5-28, p. 5; E. G. Boring, *A History of Experimental Psychology*, 2nd ed. (New York: Appleton-Century-Crofts, 1950), p. 641; R. J. Herrnstein, "Introduction" to John B. Watson, *Behavior: An Introduction to Comparative Psychology* (New York: Holt, Rinehart, and Winston, 1967); Brian D. Mackenzie, *Behaviourism and the Limits of Scientific Method* (London: Routledge & Kegan Paul, 1977) presents a challenging analysis of the course of behaviorism, proceeding more on an epistemological than historical level, however.
7. Lucille C. Birnbaum, "Behaviorism in the 1920's," *American Quarterly* 7 (1955): 15-30; Birnbaum, "Behaviorism: John Broadus Watson and American Social Thought," unpublished Ph.D. dissertation (Berkeley: University of California, 1965); John C. Burnham, "Psychiatry, Psychology, and the Progressive Movement," *American Quarterly* 12 (1960): 457-465; John C. Burnham, "The New Psychology: From Narcissism to Social Control," in *Change and Continuity in Twentieth-Century America: The 1920's*, ed. J. Braeman, R. H. Bremner, and D. Brody (Columbus, Ohio: Ohio State University Press, 1968), pp. 351-398.
8. Since this paper was written, a major study of early American psychology by historian John O'Donnell has become available. It would be impossible to do justice to this fine and comprehensive work by adding a few comments here. The reader is urged to consult it in the original: John M. O'Donnell, "The Origins of

Behaviorism: American Psychology, 1870-1920" (Ph.D. dissertation, University of Pennsylvania, 1979 [University Microfilms, 1979, No. 7928159]). While the greater part of this work deals with an earlier period, it is only fair to point out that O'Donnell's interpretation diverges from and is quite critical of my major thesis. Another recent work, David Cohen's *J. B. Watson: The Founder of Behaviourism* (London, Boston, and Henley: Routledge & Kegan Paul, 1979), is mainly biographical; it is also unfortunately marred by a number of errors.

9. Howard C. Warren, (Autobiography), in *A History of Psychology in Autobiography*, vol. 1, ed. Carl Murchison (Worcester, Mass.: Clark University Press, 1930), p. 462. See also Watson, "Psychology," p. 166n. Walter B. Pillsbury recalled, in his autobiography, that he had read Watson's paper while in Germany. However, his only concern was Watson's misinterpretation of a comment Pillsbury had made about Watson's animal lab. See Pillsbury, *A History of Psychology in Autobiography*, vol. 2 (1932), p. 285. Finally, John F. Dashiell's autobiography mentions Watson's "prompt appeal" without giving any specifics (although Dashiell attended Columbia University at the time of Watson's presentation); nor do his early writings show much of a behavioristic influence. Dashiell, (Autobiography), in *A History of Psychology in Autobiography*, vol. 5 (1967), pp. 117-118; and "Spirit and Matter: A Philosophical Tradition," *Journal of Philosophy, Psychology, and Scientific Method* 14 (1917): 66-74. E. G. Boring, *Psychologist at Large* (New York: Basic Books, 1961).

10. H. S. Langfeld, "Psychology," in *The American Year Book, 1913*, ed. Francis G. Wickware (New York: Appleton, 1914), p. 704; Maurice Parmelee, *The Science of Human Behavior* (New York: Macmillan, 1913); Walter T. Marvin, "General Problems; Mind and Body," *Psychological Bulletin* 11 (1914): 1-7.

11. Robert M. Yerkes, "The Study of Human Behavior," *Science* 39 (1914): 625-633. James R. Angell, "Behavior as a Category of Psychology," *Psychological Review* 20 (1913): 225-270, p. 261, 264; for the origin of the term see also Howard C. Warren, "Terminology," *Psychological Bulletin* 11 (1914): 10-11.

12. Max F. Meyer, *The Fundamental Laws of Human Behavior* (Boston: Badger, 1911); William McDougall, *An Introduction to Social Psychology* (London: Methuen, 1908).

13. Angell, "Behavior as a Category"; Frederick J. E. Woodbridge, "The Belief in Sensations," *Journal of Philosophy, Psychology, and Scientific Method* 10 (1913): 599-608; Mary W. Calkins, "Psychology and the Behaviorist," *Psychological Bulletin* 10 (1913): 288-291.

14. Calkins, "Psychology," p. 289.

15. Frederick L. Wells, "Special Reviews" and "Dynamic Psychology," *Psychological Bulletin* 10 (1913): 280-281 and 434-440, p. 434.

16. Wells, "Special Reviews," p. 281.

17. James R. Angell, "Behavior as a Psychological Category" (abstract), *Psychological Bulletin* 10 (1913): 48-49. Wells, "Special Reviews," p. 281..

18. Jean Weidensall, "Criminology and Delinquency," *Psychological Bulletin* 10 (1913): 229-237, p. 232.

19. Robert M. Yerkes, "Comparative Psychology: A Question of Definitions," *Journal of Philosophy, Psychology, and Scientific Method* 10 (1913): 580-582, p. 581. James R. Angell, "Professor Watson and the Image," *Journal of Philosophy, Psychology, and Scientific Method* 10 (1913): 609; Henry R. Marshall, "Is Psychology Evaporating?" *Journal of Philosophy, Psychology, and Scientific Method* 10 (1913): 710-716.

20. Howard C. Warren, "The Mental and the Physical," *Psychological Bulletin* 11 (1914): 35-36 (abstract), and *Psychological Review* 21 (1914): 79-100, p. 100.

21. *Ibid.*, p. 97, 95.

22. John Dewey, "Psychological Doctrine and Philosophical Teaching," *Journal of Philosophy, Psychology, and Scientific Method* 11 (1914): 505-511, p. 511; Harold C. Brown, "The Thirteenth Annual Meeting of the American Philosophical Association," *Journal of Philosophy, Psychology, and Scientific Method* 11 (1914): 57-67, p. 65; Knight Dunlap, "Images and Ideas," *Johns Hopkins University Circular* 33 (1914): 25-41.

23. G. Stanley Hall, "Food and Mind," Mental Hygiene Conference, Boston, 4 April 1913 (typed ms.), p. 3, Box 29, G. Stanley Hall Papers, Clark University Archives. (I am indebted to David E. Leary for making this item available to me.)

24. Burnham, "On the Origins of Behaviorism," p. 150.

25. E. G. Boring, "The Society of Experimental Psychologists; 1904-1938," *American Journal of Psychology* 51 (1938): 410-423.

26. "Notes and News," *Psychological Bulletin* 10 (1913): 211-212; see also Samuel W. Fernberger, "Convention of Experimental Psychologists," *American Journal of Psychology* 24 (1913): 445; and for a retrospective account in almost the same words, Boring, "The Society," p. 414.

27. Melvin E. Haggerty, "The Twenty-second Annual Meeting of the American Psychological Association," *Journal of Philosophy, Psychology, and Scientific Method* 11 (1914): 85-109, p. 86.

28. Melvin E. Haggerty, "The Relation of Psychology and Pedagogy," *Psychological Bulletin* 13 (1916): 55-56, and "Reviews and Abstracts of the Literature," *Journal of Philosophy, Psychology, and Scientific Method* 13 (1916): 470-472, p. 472.

29. Walter V. Bingham, "Proceedings of the Twenty-second Annual Meeting of the American Psychological Association," *Psychological Bulletin* 11 (1914): 29-35, p. 29.
30. This account is not exhaustive. For instance, a summary of Watson's manifesto by J. R. Tuttle appeared in the *Philosophical Review* 22 (1913): 674; "Notes and News," *Journal of Educational Psychology* 4 (1913): 180, reported briefly on Watson's Columbia address.
31. F. L. Wells, "Dynamic Psychology," p. 434.
32. Howard C. Warren's autobiography (p. 462) recounted two decades later that, although he could not accept Watson's position, "the younger psychologists hailed Watson as a second Moses." Yet specifics supporting and detailing this dramatic image are hard to find in the contemporary record. In "The Origins of Behaviorism," John O'Donnell discusses at length what he calls Watson's "silent majority" which, however, was not a group converted by the manifesto (as Warren had it), but which had been interested in applied psychology, and thus been behavioristic, before 1913. But even if there was such a majority for behaviorism, the very fact that its members are hard to track down in the record (even O'Donnell gives us only a few names) indicates their marginal role in the development of the academic discipline, its publications, and its training of students. Finally, O'Donnell's argument, in which applied interests are equated with behaviorism seems problematical to me; at the very least it proceeds at a more global level of analysis than does the present paper.
33. J. B. Watson to R. M. Yerkes, 26 March 1913; R. M. Yerkes to J. B. Watson, 16 May 1916, Robert M. Yerkes Papers, Historical Library, Yale Medical Library, New Haven, Conn.
34. Watson, "Image and Affection," p. 421.
35. Gilbert V. Hamilton to R. M. Yerkes, 22 December 1914. (Hamilton was a former Yerkes student with an M.D. degree, involved in animal research at the time); Margaret F. Washburn to R. M. Yerkes, 26 May 1916, Yerkes Papers. Cedric A. Larson and John J. Sullivan, "Watson's Relation to Titchener," *Journal of the History of the Behavioral Sciences* 1 (1965): 338-354.
36. "Notes and News," *Psychological Bulletin* 11 (1914): 28, 79.
37. Karl S. Lashley, "Recent Literature of a General Nature on Animal Behavior," *Psychological Bulletin* 11 (1914): 269-277, p. 277.
38. Christian A. Ruckmich, "A Schema of Method," *Psychological Review* 21 (1914): 393-401; Elliott P. Frost, "Cannot Psychology Dispense with Consciousness?" *Psychological Review* 21 (1914): 204-211; B. H. Bode, "Psychology as a Science of Behavior," *Psychological Review* 21 (1914): 46-61.
39. Larson and Sullivan, "Watson's Relation to Titchener," p. 342.
40. Edward B. Titchener, "On 'Psychology as the Behaviorist Views it,'" *Proceedings of the American Philosophical Society* 53 (1914): 1-17, pp. 4, 5.
41. *Ibid.*, pp. 16, 17.
42. *Ibid.*, p. 14.
43. Frederick L. Wells, "Dynamic Psychology," *Psychological Bulletin* 11 (1914): 404-409.
44. Larson and Sullivan, "Watson's Relation to Titchener," p. 342.
45. Kurt Danziger, "Social Origins of Modern Psychology," in *Psychology in Social Context*, ed. Allan R. Buss (New York: Irvington, 1979), pp. 27-46, p. 38.
46. Samuel W. Fernberger, "The American Psychological Association: A Historical Survey," *Psychological Bulletin* 29 (1932): 1-89, p. 35.
47. Warren's autobiography, p. 463, mentions that the policies of the APA had been dominated by a few of the older and more prominent members, in the manner of a closed corporation, and that Lightner Witmer once made a vigorous protest against this "ring rule."
48. J. B. Watson to J. M. Cattell, 10 January 1913, James McKeen Cattell Papers, Library of Congress. The three council members mentioned were Yerkes, Edgar Pierce, and Walter V. Bingham.
49. W. V. Bingham, "Proceedings," *Psychological Bulletin* 10 (1913), p. 44; Fernberger "The American Psychological Association," p. 35.
50. Robert M. Ogden, "Proceedings of the Twenty-third Annual Meeting of the American Psychological Association," *Psychological Bulletin* 12 (1915): 45-54, p. 47.
51. J. B. Watson to J. M. Cattell, 22 December 1914, J. M. Cattell Papers. E. G. Boring, "Proceedings of the Thirty-first Annual Meeting of the American Psychological Association," *Psychological Bulletin* 20 (1923): 61-69, p. 65.
52. Harry L. Hollingworth, "The Twenty-third Annual Meeting of the American Psychological Association," *Journal of Philosophy, Psychology, and Scientific Method* 12 (1915): 71-79, p. 73. The demonstration was followed by a discussion in which Watson participated. Twenty years later, Fernberger recalled that the demonstration, given under rather awkward conditions, essentially "fell flat." It received the "coup de grâce" from Watson's caustic comments about the lack of meaningful results obtained from the experts; in his judgment it had been a "waste of the spectator's time." S. W. Fernberger, "A Psychological Cycle," *American Journal of Psychology* 50 (1937): 207-217, p. 211. A demonstration of the "behavior

method," planned for the following year on Watson's urging, was eventually cancelled, apparently because the time was not "ripe" for it. See R. M. Ogden to R. M. Yerkes, 20 October 1915, and J. B. Watson to R. M. Yerkes, 9 November 1915, Yerkes Papers.

53. Walter B. Pillsbury, *The Essentials of Psychology* (New York: Macmillan, 1911); Watson, "Psychology," p. 166.
54. Walter B. Pillsbury, "The Function and Test of Definition and Method in Psychology," *Science* 41 (1915): 371-389, pp. 378, 380.
55. H. S. Langfeld, "Psychology," in *American Year Book, 1914* (New York: Appleton, 1915), 674.
56. W. T. Marvin, "General Problems: Mind and Body," *Psychological Bulletin* 12 (1915): 12-17.
57. H. S. Langfeld, "A Beginner's Psychology: E. B. Titchener," *Psychological Bulletin* 13 (1916): 438-441, p. 439.
58. Calkins, "Psychology"; Warren, "The Mental and the Physical"; Marvin, "General Problems," p. 13.
59. Langfeld, "Psychology"; Dewey, "Psychological Doctrine"; Marvin, "General Problems."
60. John B. Watson, *Behavior: An Introduction to Comparative Psychology* (New York: Holt, 1914); H. S. Langfeld, "Text-books and General Treatises," *Psychological Bulletin* 12 (1915): 30-37; Harvey A. Carr, "Special Reviews," *Psychological Bulletin* 12 (1915): 308-312; Edward L. Thorndike and C. Judson Herrick, "Watson's 'Behavior'," *Journal of Animal Behavior* 5 (1915): 462-470; M. E. Haggerty, "Reviews and Abstracts of Literature," *Journal of Philosophy, Psychology, and Scientific Method* 13 (1916): 470-472.
61. E. L. Thorndike, "The Study of Consciousness and the Study of Behavior" (abstract), *Psychological Bulletin* 8 (1911): 39; Thorndike, *Animal Intelligence* (New York: Macmillan, 1911); Thorndike and Herrick, "Watson's 'Behavior,'" p. 464.
62. George A. Coe, "A Proposed Classification of Mental Functions," *Psychological Review* 22 (1915): 87-98, p. 91.
63. Walter S. Hunter, "A Reply to Some Criticisms of the Delayed Reaction," *Journal of Philosophy, Psychology, and Scientific Method* 21 (1915): 38-41; Edwin B. Holt, "Response and Cognition," *Journal of Philosophy, Psychology, and Scientific Method* 12 (1915): 365-373 and 393-409; C. Judson Herrick, "Introspection as a Biological Method," *Journal of Philosophy, Psychology, and Scientific Method* 12 (1915): 543-551.
64. Knight Dunlap, "The Results of a Questionary of Psychological Terminology," *Johns Hopkins University Circular*, Whole Number 285 (May 1915): 3-55, p. 53.
65. H. Heath Bawden, George A. Coe, Steven S. Colvin, Max Meyer, and Margaret K. Smith.
66. See Holt's unflattering remarks about Watson to Titchener; 16 November 1918 and 23 January 1919, Edward B. Titchener Papers, Cornell University Archives, Ithaca, NY.
67. Harold C. Bingham, John F. Dashiell, Richard M. Elliott, Melvin E. Haggerty, Gilbert V. Hamilton, Walter S. Hunter, Karl S. Lashley, S. Stevenson Smith, John L. Ulrich, Jean Weidensall, Fred L. Wells, and Albert P. Weiss; but contemporary evidence about their position is insufficient or conflicting.
68. L. W. Sackett, "The Sequence of Topics in Beginners' Psychology," *Psychological Bulletin* 12 (1915): 89-99, pp. 93-95.
69. H. A. Carr, "Special Reviews," p. 309.
70. H. S. Langfeld, "Psychology," *American Year Book, 1915* (New York: Appleton, 1916) p. 669; John A. McGeogh, *The Present Status of Psychology* (Colorado Springs: Colorado College Publications, General Series No. 103, 1919).
71. C. R. Ruckmich, "The Last Decade of Psychology in Review," *American Journal of Psychology* 27 (1916): 109-120, p. 120. Twenty-five years later, Jerry S. Bruner and Gordon W. Allport sampled psychology journals at ten-year intervals and reported that introspection had never been used in more than five percent of the articles published in the years selected. This massive difference from Ruckmich's conclusions is in large part due to differences in the data base. Ruckmich had selected material on the "normal adult." Articles in this class make up only one quarter of the articles classified by Bruner and Allport for the year 1908. In addition, the latter study used more, and presumably narrower, analytic categories. See Bruner and Allport, "Fifty Years of Change in American Psychology," *Psychological Bulletin* 37 (1940): 757-776.
72. H. S. Langfeld, "Textbooks and General Treatises," *Psychological Bulletin* 14 (1917): 17-22, p. 18.
73. Edward C. Tolman, *Principles of Purposive Behavior* (mimeo, 92 pp., 1957), p. 2.
74. Tolman, (Autobiography), p. 329.
75. Edward C. Tolman, "More Concerning the Temporal Relations of Meaning and Imagery," *Psychological Review* 24 (1917): 114-138. It is intriguing to discover in Yerkes's unpublished lecture notes an indication that he apparently failed to assign for class reading the book's first chapter—the 1913 polemics against psychology—while using the other chapters. See R. M. Yerkes Lecture Notes, Frank A. Beach Papers, Box M272, Archives of the History of American Psychology, University of Akron, Ohio. Carr's review, too, had criticized the inclusion of this first chapter in the book as inappropriate for a text; H. A. Carr, "Special Reviews," p. 310.

76. Margaret F. Washburn, "Some Thoughts on the Last Quarter Century in Psychology," *Philosophical Review* 26 (1917): 46-55; Joseph Jastrow, "Varieties of Psychological Experience," *Psychological Review* 24 (1917): 249-265; Walter B. Pillsbury, "The New Developments in Psychology in the Past Quarter Century," *Philosophical Review* 26 (1917): 56-59; James M. Cattell, "Our Psychological Association and Research," *Science* 45 (1917): 275-284; John Dewey, "The Need for Social Psychology," *Psychological Review* 24 (1917): 266-277; G. Stanley Hall, "Practical Relations between Psychology and the War," *Journal of Applied Psychology* 1 (1917): 9-16.
77. John B. Watson, "The Place of the Conditioned-Reflex in Psychology," *Psychological Bulletin* 23 (1916): 89-117. Apparently this address did not produce much of a reaction. See H. S. Langfeld to H. Münsterberg, 1 January 1916, Hugo Münsterberg Papers, Boston Public Library; Watson, "An Attempted Formulation of the Scope of Behavior Psychology," *Psychological Review* 24 (1917): 329-352; Watson and John J. B. Morgan, "Emotional Reactions and Psychological Experimentation," *American Journal of Psychology* 28 (1917): 163-174.
78. Howard C. Warren, Mary W. Calkins, Knight Dunlap, H. N. Gardiner, and C. A. Ruckmich, "Definitions and Delimitations of Psychological Terms," *Psychological Bulletin* 15 (1918): 89-95, p. 94.
79. Preprint located in 3P, Mary Whiton Calkins Unprocessed Papers, Wellesley College Archives, Wellesley, Mass.
80. Warren et al., "Definitions, II," *Psychological Bulletin* 19 (1922): 230-235, p. 231.
81. *Psychological Index*, Year 1923, No. 30 (Issued June 1924), p. VII.
82. Walter S. Hunter, "An Open Letter to the Anti-Behaviorists," *Journal of Philosophy, Psychology, and Scientific Method* 19 (1922): 307-308, p. 308.
83. Walter S. Hunter, "A Reformulation of the Law of Association," *Psychological Review* 24 (1917): 188-196, p. 196.
84. On Weiss and his relation to Watson, see Franz Samelson, "Early Behaviorism, Part 3: The Stalemate of the Twenties," p. 2. Paper presented at the twelfth annual meeting of Cheiron, Bowdoin College, Brunswick, Me., June 1980.
85. J. B. Watson to Paul T. Young, 7 March 1919, Paul T. Young Papers, M 613, Archives of the History of American Psychology, Akron, Ohio. Cf. E. G. Boring, *History*, p. 575.
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87. See J. B. Watson to J. M. Cattell, 15 January 1915, Cattell Papers, and J. B. Watson to R. M. Yerkes, 27 March 1916, Yerkes Papers; Holt, "Response and Cognition," p. 409n.
88. Gardner Murphy to Robert S. Woodworth, n.d. (in reply to Woodworth's letter dated 27 October 1932), Robert S. Woodworth Papers, Library of Congress.
89. E. B. Titchener, "The Past Decade in Experimental Psychology," *American Journal of Psychology* 21 (1910): 404-422.
90. D. Bakan, "Behaviorism," p. 16.
91. R. M. Yerkes, "Behaviorism and Genetic Psychology," *Journal of Philosophy, Psychology, and Scientific Method* 14 (1917): 154-161, p. 151.
92. Thomas S. Kuhn, "The Structure of Scientific Revolutions (Chicago: University of Chicago Press, 1962).
93. Watson, "Psychology," p. 163n.
94. Robert S. Woodworth, "A Revision of Imageless Thought," *Psychological Review* 22 (1915): 1-27.
95. Titchener, "On 'Psychology,'" p. 8. See also Kurt Danziger, "The History of Introspection Reconsidered," *Journal of the History of the Behavioral Sciences* 16 (1980): 241-262.
96. E.g., James R. Angell, *Psychology*, 3rd ed. (New York: Holt, 1906), p. 4; Harvey A. Carr, *Psychology* (New York: Longman Greens, 1926), p. 7.
97. See Danziger, "History of Introspection," pp. 257-258.
98. Franz Samelson, "Paradigms, Labels, and Historical Analysis," *American Psychologist* 28 (1973): 1141-1143. See also Mackenzie, *Behaviourism*.
99. E.g., Lashley, "Recent Literature," p. 272.
100. Watson, "The Place of the Conditioned-Reflex"; Karl S. Lashley, "The Human Salivary Reflex and its Use in Psychology," *Psychological Review* 23 (1916): 445-464; Lashley, "Reflex Secretions of the Human Parotid Gland," *Journal of Experimental Psychology*, 1 (1916): 461-495; Ernest R. Hilgard and Donald G. Marquis, *Conditioning and Learning* (New York: Appleton-Century, 1940), p. 13. Watson and Morgan, "Emotional Reactions," p. 171.
101. J. B. Watson and Rosalie Rayner, "Conditioned Emotional Reactions," *Journal of Experimental Psychology* 3 (1920): 1-14; J. B. Watson and R. R. Watson, "Studies in Infant Psychology," *Scientific*

- Monthly* 13 (1921): 493-515. For a more detailed discussion, see Franz Samelson, "John B. Watson's Little Albert, Cyril Burt's Twins, and the Need for a Critical Science," *American Psychologist* 35 (1980): 619-625.
102. Elsie O. Bregman, "An Attempt to Modify the Emotional Attitudes of Infants by the Conditioned Response Technique," *Journal of Genetic Psychology* 45 (1934): 169-198; Hilgard and Marquis, *Conditioning and Learning*, pp. 293, 294.
103. Hulsey Cason, "The Conditioned Pupillary Reaction," *Journal of Experimental Psychology* 5 (1922): 108-146; Ivan P. Pavlov, *Conditioned Reflexes*, trans. G. V. Anrep (London: Oxford University Press, 1927); Pavlov, *Lectures on Conditioned Reflexes*, trans. W. Horsley Gantt (New York: International Publishers, 1928). Two earlier American conditioning studies do not qualify for inclusion, for different technical reasons: Ignatius A. Hamel, "A Study and Analysis of the Conditioned Reflex," *Psychological Monographs* 27 (1919): No. 1; Florence Mateer, *Child Behavior* (Boston: Badger, 1918). Of course, others had started to talk about conditioning (F. L. Wells, "Von Bechterew and Uebertragung," *Journal of Philosophy, Psychology, and Scientific Method* 13 [1916]: 354-356; William H. Burnham, "Mental Hygiene and the Conditioned Reflex," *Pedagogical Seminary* 24 [1917]: 449-488), but that only proves my point. Cf. Hilgard and Marquis, *Conditioning and Learning*, on this issue, although their emphasis is different.
104. J. B. Watson to R. M. Yerkes, 22 October 1915 and 17 February 1916, Yerkes Papers.
105. For instance, Karl Pearson, *The Grammar of Science*, 2nd ed. (London: Black, 1900).
106. Jacques Loeb, *Comparative Physiology of the Brain and Comparative Psychology* (New York: Putnam, 1907), p. 287; Loeb, *The Mechanistic Conception of Life* (Chicago: University of Chicago Press, 1912), pp. 3, 196. Philip J. Pauly's recent work on Loeb comes independently to similar conclusions; see his "Jacques Loeb and the Control of Life," unpublished Ph.D. dissertation, Johns Hopkins University, 1980. Of course, in at least a loose sense these ideas go back to Auguste Comte and beyond.
107. William James, "A Plea for Psychology as a 'Natural Science,'" *Philosophical Review* 1 (1892): 146-153, p. 148 (I am indebted to John O'Donnell for this reference).
108. James M. Cattell, "The Concepts and Methods of Psychology," *Popular Science Monthly* 66 (1904): 176-186, pp. 185, 186.
109. Thorndike, *Animal Intelligence*, p. 15.
110. Robert M. Yerkes, *Introduction to Psychology* (New York: Holt, 1911).
111. William McDougall, *Psychology: The Study of Behaviour* (London: Butterworth, 1912), p. 21.
112. Yerkes, *Introduction to Psychology*, p. 416 (italics added). Thorndike had said: "Science seeks to know the world; the arts, to control it." *The Elements of Psychology* (New York: Seiler, 1905), p. 324.
113. See Watson's argument that "applied psychology" was a misnomer; "Psychology," p. 169.
114. Gustav Bergman, "The Contribution of John B. Watson," *Psychological Review* 63 (1956): 265-276; Herrnstein, Introduction to Watson's *Behavior*; Mackenzie, *Behaviourism*; not so John C. Burnham, whose repeated references to the "social control" theme helped to direct my attention to this issue. See also L. Birnbaum, *Behaviorism*.
115. Watson, "An Attempted Formulation," and *Psychology from the Standpoint of a Behaviorist* (Philadelphia: Lippincott, 1919), p. 2. See also Paul T. Young to J. B. Watson, 27 May 1917, P. T. Young Papers.
116. Harry L. Hollingworth and Albert T. Poffenberger, *Applied Psychology* (New York: Appleton, 1917), pp. 5, 6.
117. Dewey, "The Need for Social Psychology"; Henry C. Link, "The Application of Psychology to Industry," *Psychological Bulletin* 17 (1920): 335-346, pp. 341, 345 (italics added).
118. Walter V. Bingham, "On the Possibility of an Applied Psychology," *Psychological Review* 30 (1923): 289-305; Floyd H. Allport, "Social Psychology," *Psychological Bulletin* 17 (1920): 85-94, p. 85; F. H. Allport, Lecture Notes, "Psychology 35; Industrial and Vocational Psychology," (typed, 1923?), Box 3, Walter V. Bingham Papers, University Archives, Carnegie-Mellon University, Pittsburgh, Penn.
119. Franz Samelson, "Early Behaviorism, Pt. 3." See also F. Samelson, "Putting Psychology on the Map," *Psychology in Social Context*, pp. 101-168.
120. For instance, O'Donnell, "Origins," p. 537.
121. John B. Watson, "Comparative Psychology," *Psychological Bulletin* 4 (1907): 208.
122. Pierre Bourdieu, "The Specificity of the Scientific Field and the Social Conditions of the Progress of Reason," *Social Science Information* 14, no. 6 (1975): 19-47.
123. Cf. Dorothy Ross, "The Development of the Social Sciences," in *The Organization of Knowledge in Modern America, 1860-1920*, ed. Alexandra Oleson and John Voss (Baltimore: Johns Hopkins University Press, 1979), pp. 107-138.