By the beginning of the twentieth century, the new psychology had a firm foothold in American universities. After the founding of Hall’s laboratory at Johns Hopkins University in 1883, nearly forty more universities followed suit by 1900. James’s Principles, having been in print for ten years, had led many new converts to psychology’s promised land. That book played an important role in functionalism’s establishment as a viable alternative to the more restrictive brand of structural psychology espoused by Titchener.

Although only a decade old in 1900, the University of Chicago was a university on the move, largely due to the efforts of the founding president, William Rainey Harper, who raided the established universities (especially G. Stanley Hall’s Clark University) in search of their best faculty talent. Chicago was an exciting place to be at the turn of the century. The biology department included Jacques Loeb (see Chapter 12) and Henry H. Donaldson (1857–1938), an internationally known authority on the human brain who had studied with G. S. Hall at Johns Hopkins. John Dewey and James Rowland Angell were members of the Philosophy Department.

In 1900, John Broadus Watson (1878–1958) arrived at the University of Chicago from his home in Traveler’s Rest, South Carolina, having completed his baccalaureate and master’s degrees at a southern Baptist college, Furman University. Watson had planned to pursue his doctorate in philosophy but lost interest in Dewey’s classes. In Angell, though, he found the new psychology more to his liking. He took classes from Loeb and Donaldson as well, with Angell and Donaldson eventually directing his doctoral dissertation research. Watson’s research was a comparative psychological investigation using infant rats of vary-
ing ages to study the relationship between neurological development and behavioral complexity. By 1903 the university granted authority for a degree in psychology, separate from philosophy, and the first psychology doctorate was awarded to Watson in that year. His dissertation was entitled, "Animal Education: The Psychical Development of the White Rat." Watson had several job offers upon graduation but elected to stay at Chicago when Angell offered him a position in psychology. There he continued his animal research until he was offered a professorship and head of the department of psychology at Johns Hopkins in 1908. The offer was too good to refuse.

According to his autobiography, Watson's dissatisfaction with the prevailing psychology of his day began in 1904 when he was at Chicago (Watson, 1936). Influenced by his physiological training, he searched for a way to make his own science more objective. More and more he was troubled by the mentalism of a psychology defined as the study of consciousness. He greatly doubted the validity of the introspective method, preferring the more controlled stimulus-response conditions of his laboratory studies with rats. For Watson, those ideas were expressed in some detail in an address he delivered at Columbia University in 1913 entitled "Psychology as the Behaviorist Views It." A few months later that address was published under the same title in the Psychological Review, a journal founded by Cattell and James Mark Baldwin, and edited by Watson since his arrival at Johns Hopkins. The paper became known as the "Behaviorist Manifesto" and marked the beginning of a revolution in psychology (although not an immediate rebellion—see the Samuelson article in this chapter).

Watson's manifesto began:

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, nor is the scientific value of its data dependent upon the readiness with which they lend themselves to interpretation in terms of consciousness. (Watson, 1913, p. 158)

He rebuked not only the structuralists but also the functionalists among whom he had trained. He claimed that he could not distinguish between them, that both were mired in a mentalism that thwarted objective science. In continuing his attack he wrote:

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.... The time seems to have come when psychology must discard all reference to consciousness; when it need no longer de-lude itself into thinking that it is making mental states the object of observation. (Watson, 1913, p. 163)

Needless to say, those were harsh words from this brash young man who had received his doctorate in psychology only ten years earlier. Regarding Watson's admonition against consciousness and other mentalistic terms, some psychologists have referred to it as "the time when psychology lost its mind."
Watson was not alone in his dissatisfaction with the subjectivism of psychology, and his 1913 paper was by no means the initial appearance of such ideas. The unmistakable rumblings of behaviorism were all too apparent in the earliest days of the twentieth century. Behavioral ideas, in one form or another, were espoused by William McDougall (a British psychologist), Herbert S. Jennings, Adolf Meyer (a psychiatrist), Knight Dunlap, and others. Jennings, Meyer, and Dunlap were Watson’s colleagues at Johns Hopkins University and all exerted identifiable influences on Watson’s thinking. Indeed, in his autobiography, Dunlap (1932) lamented the fact that Watson had received the credit for behavioristic ideas that were largely Dunlap’s. Interestingly, in his own autobiography Watson (1936) acknowledged Dunlap’s priority.

Other early behaviorists included Max F. Meyer (1873–1967), a psychologist at the University of Missouri, whose 1911 book, The Fundamental Laws of Human Behavior, attacked introspection and a psychology of subjective states, especially the notion of consciousness. Historian John O’Donnell (1985) has said of Meyer: “Had he been an American working in a more prominent eastern institution and capable of communicating his notions in the evangelical language of a southern Baptist, the name John B. Watson might be less prominent today” (p. 216).

The intellectual roots of behaviorism obviously predate Watson and they extend beyond the boundaries of psychology into the allied fields of physiology, medicine, and sociology (see Parmelec, 1912). But behaviorism as a movement in psychology belongs to Watson. Some historians, for example John Burnham (1968), have argued against labeling Watson as the founder of behaviorism. Burnham prefers to view Watson’s role in behaviorism as that of “charismatic leader.” Yet that label does not seem to do justice to Watson’s role, if it implies that in 1913 the philosophical tenets of behaviorism were already in place in any centralized formulation. Watson’s contribution was that he crystallized those scattered ideas into a systematic formulation that was new. In Burnham’s (1968) words, “Watson combined these elements into a synthesis, the whole of which was greater than its parts” (p. 145). If anyone deserves the label of founder of behavioral psychology, it is John B. Watson. The impact of his words, while not immediate, has been profound in the history of modern psychology.

For Watson, psychology was to be the science of behavior, not mental states. Processes that were not directly observable would have no place in a behavioral psychology. Not only was the subject matter of psychology to be changed, but so were its methods. Introspection was rejected; it was a method that only pretended to produce accurate observations. Watson did recognize the possible value of verbal report from subjects, but only in conjunction with other corroborating observations. Instead Watson called for more objective forms of observation, with and without the use of scientific instruments. He approved of the reaction time studies and the experimental research of Hermann Ebbinghaus on learning and memory. Further, he approved of some psychological tests, so long as they were not mental tests. Somewhat later, Watson called for extended use of the conditioned reflex method of Pavlov. He noted
how it could be used to answer questions that heretofore seemed answerable only by the method of introspection, for example in determining the range of the visible spectrum to which the human eye is sensitive.

We start with any intermediate wave length and by the use of the electric shock establish a conditioned reflex. Each time the light appears the reflex occurs. We then increase the length of the wave rather sharply and if the reflex appears we again increase the wave length. We finally reach a point where the reflex breaks down, even when punishment is used to restore it—approximately 760 millimicrons. This wave length represents the human being's spectral range at the red end. We then follow the same procedure with respect to the violet end (397 millimicrons). In this way we determine the individual's range just as surely as if we had stimulated the subject with monochromatic lights varying in wave lengths and asked him if he saw them. (Watson, 1919, pp. 35-36)

Watson followed the publication of his manifesto with his first book, Behavior: An Introduction to Comparative Psychology (1914), in which he lauded the value of animal research in psychology and expressed his views about a wholly behavioral psychology. By the time his next book appeared, Psychology from the Standpoint of a Behaviorist (1919), he had shifted his work to human infants. In 1919 he was engaged in the most famous research study of his career. Indeed, it is one of the most famous studies in the history of American psychology. With the assistance of a graduate student, Rosalie Rayner (whom he would later marry), Watson sought to demonstrate that fear could be acquired in humans as the result of conditioning, an idea he had proposed in an earlier article (Watson & Morgan, 1917). Using an 11-month-old infant, Albert B., they presented first a white rat (which the infant did not fear) and then a loud noise (which the infant did fear). After repeated pairings, Watson and Rayner (1920) reported that fear had been conditioned to the rat. It was an important claim for Watson because it supported his largely environmentalistic theory of emotion in humans. The success of the conditioning and the generalization of the fear demonstrated that conditioning was a fact, not just in the lives of laboratory dogs but in humans as well.

That classic study was Watson's last as an academic psychologist. Scandal over his affair with Rayner forced him to leave Johns Hopkins at the age of 42. He went to New York City, where he pursued a successful career in advertising and wrote a number of books and articles on psychology for the popular press.

Watson's impact on psychology was substantial, perhaps more so than any other figure in the history of American psychology. Yet the value of his legacy is debated today. In arguing for an objective science of behavior he eliminated a number of topics that have only begun to reappear in American psychology in the last twenty years, for example, consciousness, thinking, dreaming, and emotion. Some psychologists believe that Watson's philosophy was too radical, that in throwing out what he saw as bad, he also contributed to the elimination of much that was good. They would argue that in the long run he inhibited psychology's progress.
Others would argue that psychology's progress as a science was largely because of Watson, that he was the one figure who demanded a complete break with philosophy and the mentalistic baggage attached to it. Watsonian behaviorism strengthened the role of physiological processes in psychological explanations, expanded psychological methods, and made apparent the ties between animal and human psychology.

Whatever the value of the legacy, Watson's ideas dominated American psychology for more than fifty years through many varieties of behaviorism (see the next chapter). Although the cognitive psychology movement has weakened the stronghold of the behaviorists, they continue to be a major force in contemporary psychology.

The first selection in this chapter is Watson's famous 1913 paper that started the behaviorist revolution. Histories of psychology have generally reported that the rebellion was immediate, that psychologists were instantly upon Watson's bandwagon, waving the banners of objectivism. But recent historical research (see the second selection in this chapter) suggests that the acceptance of Watson's ideas was much slower in coming. This second selection, by the historian of psychology Franz Samelson, an authority on behaviorism, represents extensive research in the published and unpublished sources of psychology in search of the impact of Watson's ideas and the timetable for their acceptance.

The final selection is by another historian of psychology, Ben Harris. Its subject matter is the famous infant conditioning study of Watson and Rayner, a study that has perhaps been more misquoted and misunderstood than any other in the history of psychology. Harris discusses the famous experiment and its numerous misrepresentations in contemporary psychology. He concludes (as does Samelson, 1980) that the study is interesting but so poorly designed and controlled that its data are uninterpretable. Both the Harris and the Samelson papers in this chapter are good examples of the reevaluation of the "truths" in the history of psychology, in what is being called critical history. They challenge some of the many legends built up around the figure of John Watson and behaviorism and give us new ways to think about these subjects.

REFERENCES


Psychology as the Behaviorist Views It

John B. Watson

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, nor is the scientific value of its data dependent upon the readiness with which they lend themselves to interpretation in terms of consciousness. The behaviorist, in his efforts to get a unitary scheme of animal response, recognizes no dividing line between man and brute. The behavior of man, with all of its refinement and complexity, forms only a part of the behaviorist's total scheme of investigation.

It has been maintained by its followers generally that psychology is a study of the science of the phenomena of consciousness. It has taken as its problem, on the one hand, the analysis of complex mental states (or processes) into simple elementary constituents, and on the other the construction of complex states when the elementary constituents are given. The world of physical objects (stimuli, including here anything which may excite activity in a receptor), which forms the total phenomena of the natural scientist, is looked upon merely as means to an end. That end is the production of mental states that may be 'inspected' or 'observed.' The psychological object of observation in the case of an emotion, for example, is the mental state itself. The problem in emotion is the determination of the number and kind of elementary constituents present, their loci, intensity, order of appearance, etc. It is agreed that introspection is the method par excellence by means of which mental states may be manipulated for purposes of psychology. On this assumption, behavior data (including under this term everything which goes under the name of comparative psychology) have no value per se. They possess significance only in so far as they may throw light upon conscious states. Such data must have at least an analogical or indirect reference to belong to the realm of psychology.

Indeed, at times, one finds psychologists who are skeptical of even this analogical reference. Such skepticism is often shown by the question which is put to the student of behavior, "what is the bearing of animal work upon human psychology?" I used to have to study over this question. Indeed it always embarrassed me somewhat. I was interested in my own work and felt that it was important, and yet I could not trace any close connection between it and psychology as my questioner understood psychology. I hope that such a confession will clear the atmosphere to such an extent that we will no longer have to work under false pretenses. We must frankly admit that the facts so important to us which we have been able to glean from extended work upon the senses of animals by the behavior method have contributed only in a fragmentary way to the general theory of human sense organ processes, nor have they suggested new points of experimental attack. The enormous number of experiments which we have carried out upon learning have likewise contributed little to human psychology. It seems reasonably clear that some kind of compromise must be effected; either psychology must change its viewpoint so as to take in facts of behavior, whether or not they have bearings upon the problems of 'consciousness'; or else behavior must stand alone as a wholly separate and independent science. Should human psychologists fail to look with favor upon our overtures and refuse to modify their position, the behaviorists will be driven to using human beings as subjects and to employ methods of investigation which are exactly comparable to those now employed in the animal work.

Any other hypothesis than that which admits the independent value of behavior material, regardless of any bearing such material may have upon consciousness, will inevitably force us to the absurd position of attempting to construct the conscious content of the animal whose behavior we have been studying. On this view, after having determined our animal’s ability to learn, the simplicity or complexity of its methods of learning, the effect of past habit upon present response, the range of stimuli to which it ordinarily responds, the widened range to which it can respond under experimental conditions,—in more general terms, its various problems and its various ways of solving them,—we should still feel that the task is unfinished and that the results are worthless, until we can interpret them by analogy in the light of consciousness. Although we have solved our problem we feel uneasy and unrestful because of our definition of psychology: we feel forced to say something about the possible mental processes of our animal. We say that, having no eyes, its stream of consciousness cannot contain brightness and color sensations as we know them,—having no taste buds this stream can contain no sensations of sweet, sour, salt and bitter. But on the other hand, since it does respond to thermal, tactual and organic stimuli, its conscious content must be made up largely of these sensations; and we usually add, to protect ourselves against the reproach of being anthropomorphic, “if it has any consciousness.” Surely this doctrine which calls for an analogical interpretation of all behavior data may be shown to be false: the position that the standing of an observation upon behavior is determined by its fruitfulness in yielding results which are interpretable only in the narrow realm of (really human) consciousness.

This emphasis upon analogy in psychology has led the behaviorist somewhat afield. Not being willing to throw off the yoke of consciousness he feels impelled to make a place in the scheme of behavior where the rise of consciousness can be determined. This point has been a shifting one. A few years ago certain animals were supposed to possess ‘associative memory,’ while certain others were supposed to lack it. One meets this search for the origin of consciousness under a good many disguises. Some of our texts state that consciousness arises at the moment when reflex and instinctive activities fail properly to conserve the organism. A perfectly adjusted organism would be lacking in consciousness. On the other hand whenever we find the presence of diffuse activity which results in habit formation, we are justified in assuming consciousness. I must confess that these arguments had weight with me when I began the study of behavior. I fear that a good many of us are still viewing behavior problems with something like this in mind. More than one student in behavior has attempted to frame criteria of the psychic—to devise a set of objective, structural and functional criteria which, when applied in the particular instance, will enable us to decide whether such and such responses are positively conscious, merely indicative of consciousness, or whether they are purely ‘physiological.’ Such problems as these can no longer satisfy behavior men. It would be better to give up the province altogether and admit frankly that the study of the behavior of animals has no justification, than to admit that our search is of such a ‘will o’ the wisp’ character. One can assume either the presence or the absence of consciousness anywhere in the phylogenetic scale without affecting the problems of behavior by one jot or one tittle; and without influencing in any way the mode of experimental attack upon them. On the other hand, I cannot for one moment assume that the paramecium responds to light; that the rat learns a problem more quickly by working at the task five times a day than once a day, or that the human child exhibits plateaux in his learning curves. These are questions which vitally concern behavior and which must be decided by direct observation under experimental conditions.

This attempt to reason by analogy from human conscious processes to the conscious
processes in animals, and vice versa: to make consciousness, as the human being knows it, the center of reference of all behavior, forces us into a situation similar to that which existed in biology in Darwin’s time. The whole Darwinian movement was judged by the bearing it had upon the origin and development of the human race. Expeditions were undertaken to collect material which would establish the position that the rise of the human race was a perfectly natural phenomenon and not an act of special creation. Variations were carefully sought along with the evidence for the heaping up effect and the weeding out effect of selection; for in these and the other Darwinian mechanisms were to be found factors sufficiently complex to account for the origin and race differentiation of man. The wealth of material collected at this time was considered valuable largely in so far as it tended to develop the concept of evolution in man. It is strange that this situation should have remained the dominant one in biology for so many years. The moment Zoology undertook the experimental study of evolution and descent, the situation immediately changed. Man ceased to be the center of reference. I doubt if any experimental biologist today, unless actually engaged in the problem of race differentiation in man, tries to interpret his findings in terms of human evolution, or ever refers to it in his thinking. He gathers his data from the study of many species of plants and animals and tries to work out the laws of inheritance in the particular type upon which he is conducting experiments. Naturally, he follows, the progress of the work upon race differentiation in man and in the descent of man, but he looks upon these as special topics, equal in importance with his own yet ones in which his interests will never be vitally engaged. It is not fair to say that all of his work is directed toward human evolution or that it must be interpreted in terms of human evolution. He does not have to dismiss certain of his facts on the inheritance of coat color in mice because, forsooth, they have little bearing upon the differentiation of the genus homo into separate rac-
es, or upon the descent of the genus homo from some more primitive stock.

In psychology we are still in that stage of development where we feel that we must select our material. We have a general place of discard for processes, which we anathematize so far as their value for psychology is concerned by saying, “this is a reflex”; “that is a purely physiological fact which has nothing to do with psychology.” We are not interested (as psychologists) in getting all of the processes of adjustment which the animal as a whole employs, and in finding how these various responses are associated, and how they fall apart, thus working out a systematic scheme for the prediction and control of response in general. Unless our observed facts are indicative of consciousness, we have no use for them, and unless our apparatus and method are designed to throw such facts into relief, they are thought of in just as disparaging a way. I shall always remember the remark one distinguished psychologist made as he looked over the color apparatus designed for testing the responses of animals to monochromatic light in the attic at Johns Hopkins. It was this: “And they call this psychology!”

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. Psychology, as it is generally thought of, has something esoteric in its methods. If you fail to reproduce my findings, it is not due to some fault in your apparatus or in the control of your stimulus, but it is due to the fact that your introspection is untrained. The attack is made upon the observer and not upon the experimental setting. In physics and in chemistry the attack is made upon the experimental conditions. The apparatus was not sensitive enough, impure chemicals were used, etc. In these sciences a better technique will give reproducible results. Psychology is otherwise. If you can’t observe 3-9 states of clearness in attention, your introspection is poor. If, on the other hand, a feel-
ing seems reasonably clear to you, your introspection is again faulty. You are seeing too much. Feelings are never clear.

The time seems to have come when psychology must discard all reference to consciousness; when it need no longer delude itself into thinking that it is making mental states the object of observation. We have become so enmeshed in speculative questions concerning the elements of mind, the nature of conscious content (for example, imageless thought, attitudes, and Bewusseinslage, etc.) that I, as an experimental student, feel that something is wrong with our premises and the types of problems which develop from them. There is no longer any guarantee that we all mean the same thing when we use the terms now current in psychology. Take the case of sensation. A sensation is defined in terms of its attributes. One psychologist will state with readiness that the attributes of a visual sensation are quality, extension, duration, and intensity. Another will add clearness. Still another that of order. I doubt if any one psychologist can draw up a set of statements describing what he means by sensation which will be agreed to by three other psychologists of different training. Turn for a moment to the question of the number of isolable sensations. Is there an extremely large number of color sensations—or only four, red, green, yellow and blue? Again, yellow, while psychologically simple, can be obtained by superimposing red and green spectral rays upon the same diffusing surface! If, on the other hand, we say that every just noticeable difference in the spectrum is a simple sensation, and that every just noticeable increase in the white value of a given color gives simple sensations, we are forced to admit that the number is so large and the conditions for obtaining them so complex that the concept of sensation is unusable, either for the purpose of analysis or that of synthesis. Titchener, who has fought the most valiant fight in this country for a psychology based upon introspection, feels that these differences of opinion as to the number of sensations and their attributes, as to whether there are relations (in the sense of elements) and on the many others which seem to be fundamental in every attempt at analysis, are perfectly natural in the present undeveloped state of psychology. While it is admitted that every growing science is full of unanswered questions, surely only those who are wedded to the system as we now have it, who have fought and suffered for it, can confidently believe that there will ever be any greater uniformity than there is now in the answers we have to such questions. 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 intensity is an attribute which can be applied to color, whether there is a difference in ‘texture’ between image and sensation and upon many hundreds of others of like character.

The condition in regard to other mental processes is just as chaotic. Can image type be experimentally tested and verified? Are recondite thought processes dependent mechanically upon imagery at all? Are psychologists agreed upon what feeling is? One states that feelings are attitudes. Another finds them to be groups of organic sensations possessing a certain solidarity. Still another and larger group finds them to be new elements correlative with and ranking equally with sensations.

My psychological quarrel is not with the systematic and structural psychologist alone. The last fifteen years have seen the growth of what is called functional psychology. This type of psychology decries the use of elements in the static sense of the structuralists. It throws emphasis upon the biological significance of conscious processes instead of upon the analysis of conscious states into introspectively isolable elements. I have done my best to understand the difference between functional psychology and structural psychology. Instead of clarity, confusion grows upon me. The terms sensation, perception, affection, emotion, volition are used as much by the functionalist as by the structuralist. The ad-
dition of the word 'process' ('mental act as a whole,' and like terms are frequently met) after each serves in some way to remove the corpse of 'content' and to leave 'function' in its stead. Surely if these concepts are elusive when looked at from a content standpoint, they are still more deceptive when viewed from the angle of function, and especially so when function is obtained by the introspection method. It is rather interesting that no functional psychologist has carefully distinguished between 'perception' (and this is true of the other psychological terms as well) as employed by the systematist, and 'perceptual process' as used in functional psychology. It seems illogical and hardly fair to criticize the psychology which the systematist gives us, and then to utilize his terms without carefully showing the changes in meaning which are to be attached to them. I was greatly surprised some time ago when I opened Pillsbury's book and saw psychology defined as the 'science of behavior.' A still more recent text states that psychology is the 'science of mental behavior.' When I saw these promising statements I thought, now surely we will have texts based upon different lines. After a few pages the science of behavior is dropped and one finds the conventional treatment of sensation, perception, imagery, etc., along with certain shifts in emphasis and additional facts which serve to give the author's personal imprint.

One of the difficulties in the way of a consistent functional psychology is the parallelistic hypothesis. If the functionalist attempts to express his formulations in terms which make mental states really appear to function, to play some active rôle in the world of adjustment, he almost inevitably lapses into terms which are connotative of interaction. When taxed with this he replies that it is more convenient to do so and that he does it to avoid the circumlocution and clumsiness which are inherent in any thoroughgoing parallelism. As a matter of fact I believe the functionalist actually thinks in terms of interaction and resorts to parallelism only when forced to give expression to his views. I feel that behaviorism is the only consistent and logical formalism. In it one avoids both the Scylla of parallelism and the Charybdis of interaction. Those time-honored relics of philosophical speculation need trouble the student of behavior as little as they trouble the student of physics. The consideration of the mind-body problem affects neither the type of problem selected nor the formulation of the solution of that problem. I can state my position here no better than by saying that I should like to bring my students up in the same ignorance of such hypotheses as one finds among the students of other branches of science.

This leads me to the point where I should like to make the argument constructive. I believe we can write a psychology, define it as Pillsbury, and never go back upon our definition: never use the terms consciousness, mental states, mind, content, introspectively verifiable, imagery, and the like. I believe that we can do it in a few years without running into the absurd terminology of Beer, Bethe, Von Uexküll, Nuel, and that of the so-called objective schools generally. It can be done in terms of stimulus and response, in terms of habit formation, habit integrations and the like. Furthermore, I believe that it is really worth while to make this attempt now.

The psychology which I should attempt to build up would take as a starting point, first, the observable fact that organisms, man and animal alike, do adjust themselves to their environment by means of hereditary and habit equipments. These adjustments may be very adequate or they may be so inadequate that the organism barely maintains its existence; secondly, that certain stimuli lead the organisms to make the responses. In a system of psychology completely worked out, given the stimulus the response can be predicted; given the stimuli the response can be predicted. Such a set of statements is crass and raw in the extreme, as all such generalizations must be. Yet they are hardly more raw and less realizable than the ones which appear in the psychology texts of the day. I possibly might illustrate my point better by choosing an everyday prob-
lem which anyone is likely to meet in the course of his work. Some time ago I was called upon to make a study of certain species of birds. Until I went to Tortugas I had never seen these birds alive. When I reached there I found the animals doing certain things: some of the acts seemed to work peculiarly well in such an environment, while others seemed to be unsuited to their type of life. I first studied the responses of the group as a whole and later those of individuals. In order to understand more thoroughly the relation between what was habit and what was hereditary in these responses, I took the young birds and reared them. In this way I was able to study the order of appearance of hereditary adjustments and their complexity, and later the beginnings of habit formation. My efforts in determining the stimuli which called forth such adjustments were crude indeed. Consequently my attempts to control behavior and to produce responses at will did not meet with much success. Their food and water, sex and other social relations, light and temperature conditions were all beyond control in a field study. I did find it possible to control their reactions in a measure by using the nest and egg (or young) as stimuli. It is not necessary in this paper to develop further how such a study should be carried out and how work of this kind must be supplemented by carefully controlled laboratory experiments. Had I been called upon to examine the natives of some of the Australian tribes, I should have gone about my task in the same way. I should have found the problem more difficult: the types of responses called forth by physical stimuli would have been more varied, and the number of effective stimuli larger. I should have had to determine the social setting of their lives in a far more careful way. These savages would be more influenced by the responses of each other than was the case with the birds. Furthermore, habits would have been more complex and the influences of past habits upon the present responses would have appeared more clearly. Finally, if I had been called upon to work out the psychology of the educated European, my problem would have required several lifetimes. But in the one I have at my disposal I should have followed the same general line of attack. In the main, my desire in all such work is to gain an accurate knowledge of adjustments and the stimuli calling them forth. My final reason for this is to learn general and particular methods by which I may control behavior. My goal is not "the description and explanation of states of consciousness as such," nor that of obtaining such proficiency in mental gymnastics that I can immediately lay hold of a state of consciousness and say, "this, as a whole, consists of gray sensation number 350, of such and such extent, occurring in conjunction with the sensation of cold of a certain intensity; one of pressure of a certain intensity and extent," and so on ad infinitum. If psychology would follow the plan I suggest, the educator, the physician, the jurist and the business man could utilize our data in a practical way, as soon as we are able, experimentally, to obtain them. Those who have occasion to apply psychological principles practically would find no need to complain as they do at the present time. Ask any physician or jurist today whether scientific psychology plays a practical part in his daily routine and you will hear him deny that the psychology of the laboratories finds a place in his scheme of work. I think the criticism is extremely just....

In concluding, I suppose I must confess to a deep bias on these questions. I have devoted nearly twelve years to experimentation on animals. It is natural that such a one should drift into a theoretical position which is in harmony with his experimental work. Possibly I have put up a straw man and have been fighting that. There may be no absolute lack of harmony between the position outlined here and that of functional psychology. I am inclined to think, however, that the two positions cannot be easily harmonized. Certainly the position I advocate is weak enough at present and can be attacked from many standpoints. Yet when all this is admitted I still feel that the considerations which I have urged should have a wide influence upon the type of psychology which is to be developed in the future. What we need to do is to start work upon
psychology, making behavior, not consciousness, the objective point of our attack. Certainly there are enough problems in the control of behavior to keep us all working many lifetimes without ever allowing us time to think of consciousness an sich. Once launched in the undertaking, we will find ourselves in a short time as far divorced from an introspective psychology as the psychology of the present time is divorced from faculty psychology.

Struggle for Scientific Authority: The Reception of Watson’s Behaviorism, 1913–1920
Franz Samelson

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 encounter with Watson or Watson’s ideas, even though his own orientation changed from Titchenerian structuralism to a (behavioral) "physicalism."(1)

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.(2)

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),(3) 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).(4) 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.”(5) Critical of Watson’s “vigorously" paper, she expressed her “radical disagreement with [its] main thesis” of the uselessness of...