

John Broadus Watson, “psychology as the behaviorist views it” (1913).*

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

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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.¹ 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 sceptical 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 pretences. 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

¹That is, either directly upon the conscious state of the observer or indirectly upon the conscious state of the experimenter.

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 fictional 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 races, 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 disci-

pline 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 feeling 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 **Bewusstsein**, 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

¹In this connection I call attention to the controversy now on between the adherents and the opposers of imageless thought. The 'types of reactors' (sensory and motor) were also matters of bitter dispute. The complication experiment was the source of another war of words concerning the accuracy of the opponents' introspection.

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 addition 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**.¹ 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 functionalism. 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 response the stimuli 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

¹My colleague, professor H. C. Warren, by whose advice this article was offered to the REVIEW, believes that the parallelist can avoid the interaction terminology completely by exercising a little care.

everyday problem 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 scien-

tic 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. One of the earliest conditions which made me dissatisfied with psychology was the feeling that there was no realm of application for the principles which were being worked out in content terms.

What gives me hope that the behaviorist's position is a defensible one is the fact that those branches of psychology which have already partially withdrawn from the parent, experimental psychology, and which are consequently less dependent upon introspection are today in a most flourishing condition. Experimental pedagogy, the psychology of drugs, the psychology of advertising, legal psychology, the psychology of tests, and psychopathology are all vigorous growths. These are sometimes wrongly called "practical" or 'applied' psychology. **Surely there was never a worse misnomer. In the future there may grow up vocational bureaus which really apply psychology. At present these fields are truly scientific and are in search of broad generalizations which will lead to the control of human behavior. For example, we find out by experimentation whether a series of stanzas may be acquired more readily if the whole is learned at once, or whether it is more advantageous to learn each stanza separately and then pass to the succeeding. We do not attempt to apply our findings. The application of this principle is purely voluntary on the part of the teacher. In the psychology of drugs we may show the effect upon behavior of certain doses of caffeine. We may reach the conclusion that caffeine has a good effect upon the speed and accuracy of work. But these are general principles. We leave it to the individual as to whether the results of our tests shall be applied or not. Again, in legal testimony, we test the effects of recency upon the reliability of a witness's report. We test the accuracy of the report with respect to moving objects, stationary objects, color, etc. It depends upon the judicial machinery of the country to decide whether these facts are ever to be applied. For a 'pure' psychologist to say that he is not interested in the questions raised in these divisions of the science because they relate indirectly to the application of psychology shows, in the first place, that he fails to understand the scientific aim in such problems, and secondly, that he is not interested in a psychology which concerns itself with human life. The only fault I have to find with these disciplines is that much of their material is stated in terms of introspection, whereas a statement in terms of objective results would be far more valuable. There is no reason why appeal should ever be made to consciousness in any of them. Or why introspective data should ever be sought during the experimentation, or published in the results. In experimental pedagogy especially one can see the desirability of keeping all of the results on a purely objective plane. If this is done, work there on the human being will be comparable directly with the work upon animals. For example, at Hopkins, Mr. Ulrich has obtained certain results upon the distribution of effort in learning—using rats as subjects. He is prepared to give comparative**

results upon the effect of having an animal work at the problem once per day, three times per day, and five times per day. Whether it is advisable to have the animal learn only one problem at a time or to learn three abreast. We need to have similar experiments made upon man, but we care as little about his 'conscious processes' during the conduct of the experiment as we care about such processes in the rats:

I am more interested at the present moment in trying to show the necessity for maintaining uniformity in experimental procedure and in the method of stating results in both human and animal work, than in developing any ideas I may have upon the changes which are certain to come in the scope of human psychology. Let us consider for a moment the subject of the range of stimuli to which animals respond. I shall speak first of the work upon vision in animals. We put our animal in a situation where he will respond (or learn to respond) to one of two monochromatic lights. We feed him at the one (positive) and punish him at the other (negative). In a short time the animal learns to go to the light at which he is fed. At this point questions arise which I may phrase in two ways: I may choose the psychological way and say "does the animal see these two lights as I do, *i. e.*, as two distinct colors, or does he see them as two grays differing in brightness, as does the totally color blind?" Phrased by the behaviorist, it would read as follows: "Is my animal responding upon the basis of the difference in intensity between the two stimuli, or upon the difference in wave-lengths?" He nowhere thinks of the animal's response in terms of his own experiences of colors and grays. He wishes to establish the fact whether wave-length is a factor in that animal's adjustment.¹ If so, what wave-lengths are effective and what differences in wave-length must be maintained in the different regions to afford bases for differential responses? If wave-length is not a factor in adjustment he wishes to know what difference in intensity will serve as a basis for response, and whether that same difference will suffice throughout the spectrum. Furthermore, he wishes to test whether the animal can respond to wave-lengths which do not affect the human eye. He is as much interested in comparing the rat's spectrum with that of the chick as in comparing it with man's. The point of view when the various sets of comparisons are made does not change in the slightest.

However we phrase the question to ourselves, we take our animal after the association has been formed and then introduce certain control experiments which enable us to return answers to the questions just raised. But there is just as keen a desire on our part to test man under the same conditions, and to state the results in both cases in common terms.

The man and the animal should be placed as nearly as possible under the same experimental conditions. Instead of feeding or punishing the human subject, we should ask him to respond by setting a second apparatus

I He would have exactly the same attitude as if he were conducting an experiment to show whether an ant would crawl over a pencil laid across the trail or go round it.

until standard and control offered no basis for a differential response. Do I lay myself open to the charge here that I am using introspection? My reply is not at all; that while I might very well feed my human subject for a right choice and punish him for a wrong one and thus produce the response if the subject could give it, there is no need of going to extremes even on the platform I suggest. But be it understood that I am merely using this second method as an abridged behavior method.¹ We can go just as far and reach just as dependable results by the longer method as by the abridged. In many cases the direct and typically human method cannot be safely used. Suppose, for example, that I doubt the accuracy of the setting of the control instrument, in the above experiment, as I am very likely to do if I suspect a defect in vision? It is hopeless for me to get his introspective report. He will say: "There is no difference in sensation, both are reds, identical in quality." But suppose I confront him with the standard and the control and so arrange conditions that he is punished if he responds to the 'control' but not with the standard. I interchange the positions of the standard and the control at will and force him to attempt to differentiate the one from the other. If he can learn to make the adjustment even after a large number of trials it is evident that the two stimuli do afford the basis for a differential response. Such a method may sound nonsensical, but I firmly believe we will have to resort increasingly to just such method where we have reason to distrust the language method.

There is hardly a problem in human vision which is not also a problem in animal vision: I mention the limits of the spectrum, threshold values, absolute and relative, flicker, Talbot's law, Weber's law, field of vision, the Purkinje phenomenon, etc. Every one is capable of being worked out by behavior methods. Many of them are being worked out at the present time.

I feel that all the work upon the senses can be consistently carried forward along the lines I have suggested here for vision. Our results will, in the end, give an excellent picture of what each organ stands for in the way of function. The anatomist and the physiologist may take our data and show, on the one hand, the structures which are responsible for these responses, and, on the other, the physico-chemical relations which are necessarily involved (physiological chemistry of nerve and muscle) in these and other reactions.

The situation in regard to the study of memory is hardly different. Nearly all of the memory methods in actual use in the laboratory today yield

¹I should prefer to look upon this abbreviated method, where the human subject is told in words, for example, to equate two stimuli; or to state in words whether a given stimulus is present or absent, etc., as the *language method* in behavior. It in no way changes the status of experimentation. The method becomes possible merely by virtue of the fact that in the particular case the experimenter and his animal have systems of abbreviations or shorthand behavior signs (language), any one of which may stand for a habit belonging to the repertoire both of the experimenter and his subject. To make the data obtained by the language method virtually the whole of behavior--or to attempt to mould all of the data obtained by other methods in terms of the one which has by all odds the most limited range—is putting the cart before the horse with a vengeance.

the type of results I am arguing for. A certain series of nonsense syllables or other material is presented to the human subject. What should receive the emphasis are the rapidity of the habit formation, the errors, peculiarities in the form of the curve, the persistence of the habit so formed, the relation of such habits to those formed when more complex material is used, etc. Now such results are taken down with the subject's introspection. The experiments are made for the purpose of discussing the mental machinery involved in learning, in recall, recollection and forgetting, and not for the purpose of seeking the human being's way of shaping his responses to meet the problems in the terribly complex environment into which he is thrown, nor for that of showing the similarities and differences between man's methods and those of other animals.

The situation is somewhat different when we come to a study of the more complex forms of behavior, such as imagination, judgment, reasoning, and Conception= At present the only statements we have of them are in content terms.² Our minds have been so warped by the fifty-odd years which have been devoted to the study of states of consciousness that we can envisage these problems only in one way. We should meet the situation squarely and say that we are not able to carry forward investigations along all of these lines by the behavior methods which are in use at the present time. In extenuation I

¹They are often undertaken apparently for the purpose of making crude pictures of what must or must not go on in the nervous system.

²There is need of questioning more and more the existence of what psychology calls imagery. Until a few years ago I thought that centrally aroused visual sensations were as clear as those peripherally aroused. I had never accredited myself with any other kind. However, closer examination leads me to deny in my own case the presence of imagery in the Galtonian sense. The whole doctrine of the centrally aroused image is, I believe, at present, on a very insecure foundation. Angell as well as Fernald reach the conclusion that an objective determination of image type is impossible. It would be an interesting confirmation of their experimental work if we should find by degrees that we have been mistaken in building up this enormous structure of the centrally aroused sensation (or image).

The hypothesis that all of the so-called 'higher thought' processes go on in terms of hint reinstatements of the original muscular act (including speech here) and that these are integrated into systems which respond in serial order (associative mechanisms) is, I believe, a tenable one. It makes reflective processes as mechanical as habit. The scheme of habit which James long ago described—where each return or afferent current releases the next appropriate motor discharge—is as true for 'thought processes' as for overt muscular acts. Paucity of 'imagery' would be the rule. In other words, wherever there are thought processes there are faint contractions of the systems of musculature involved in the overt exercise of the customary act, and especially in the still finer systems of musculature involved in speech. If this is true, and I do not see how it can be gainsaid, imagery becomes a mental luxury (even if it really exists) without any functional significance whatever. If experimental procedure justifies this hypothesis, we shall have at hand tangible phenomena which maybe studied as behavior material. I should say that the day when we can study reflective processes by such methods is about as far off as the day when we can tell by physico-chemical methods the difference in the structure and arrangement of molecules between living protoplasm and inorganic substances. The solutions of both problems await the advent of methods and apparatus.

~~should like to call a paragraph above where I made the point that the introspective method itself has reached a *cul-de-sac* with respect to them. The topics have become so threadbare from much handling that they may well be put away for a time. As our methods become better developed it will be possible to undertake investigations of more and more complex forms of behavior. Problems which are now laid aside will again become imperative, but they can be viewed as they arise from a new angle and in more concrete settings.~~

Will there be left over in psychology a world of pure psychics, to use Yerkes' term? I confess I do not know. The plans which I most favor for psychology lead practically to the ignoring of consciousness in the sense that that term is used by psychologists today. I have virtually denied that this realm of psychics is open to experimental investigation. I don't wish to go further into the problem at present because it leads inevitably over into metaphysics. If you will grant the behaviorist the right to use consciousness in the same way that other natural scientists employ it—that is, without making consciousness a special object of observation—you have granted all that my thesis requires.

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

[After writing this paper I heard the addresses of Professors Thorndike and Angell, at the Cleveland meeting of the American Psychological Association. I hope to have the opportunity to discuss them at another time. I must even here attempt to answer one question raised by Thorndike.

Thorndike (see this issue) casts suspicions upon ideo-motor action. If by ideo-motor action he means just that and would not include sensori-motor action in his general denunciation, I heartily agree with him. I should throw out imagery altogether and attempt to show that practically all natural thought goes on in terms of sensori-motor processes in the larynx (but not in terms of 'imageless thought') which rarely come to consciousness in any person who has not groped for imagery in the psychological laboratory. This easily explains why so many of the well-educated laity know nothing of imagery. I doubt if Thorndike conceives of the matter in this way. He and Woodworth seem to have neglected the speech mechanisms.

It has been shown that improvement in habit comes unconsciously. The first we know of it is when it is achieved—when it becomes an object. I believe that 'consciousness' has just as little to do with *improvement* in thought processes. Since, according to my view, thought processes are really motor habits in the larynx, improvements, short cuts, changes, etc., in these habits are brought about in the same way that such changes are produced in other motor habits. This view carries with it the implication that there are no reflective processes (centrally initiated processes): The individual is always *examining objects*, in the one case objects in the now accepted sense, in the other their substitutes, viz., the movements in the speech musculature. From this it follows that there is no theoretical limitation of the behavior method. There remains, to be sure, the practical difficulty, which may never be overcome, of examining speech movements in the way that general bodily behavior may be examined.]

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 as *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.

SUMMARY

1. Human psychology has failed to make good its claim as a natural science. Due to a mistaken notion that its fields of facts are conscious phenomena and that introspection is the only direct method of ascertaining these facts, it has enmeshed itself in a series of speculative questions which, while fundamental to its present tenets, are not open to experimental treatment. In the pursuit of answers to these questions, it has become further and further divorced from contact with problems which vitally concern human interest.
2. Psychology, as the behaviorist views it, is a purely objective, experimental branch of natural science which needs introspection as little as do the sciences of chemistry and physics. It is granted that the behavior of animals can be investigated without appeal to consciousness. Heretofore the viewpoint has been that such data have value only in so far as they can be interpreted by analogy in terms of consciousness. The position is taken here that the behavior of man and the behavior of animals must be considered on the same plane; as being equally essential to a general understanding of behavior. It can dispense with consciousness in a psychological sense. The separate observation of 'states of consciousness' is, on this assumption, no more a part of the task of the psychologist than of the physicist. We might call this the return to a non-reflective and naive use of consciousness. In this sense consciousness maybe said to be the instrument or tool with which all scientists work. Whether or not the tool is properly used at present by scientists is a problem for philosophy and not for psychology.
3. From the viewpoint here suggested the facts on the behavior of amoebae have value in and for themselves without reference to the behavior of man. In biology studies on race differentiation and inheritance in amoebae form a separate division of study which must be evaluated in terms of the laws found there. The conclusions so reached may not hold in any other form. Regardless of the possible lack of generality, such studies must be made if evolution as a whole is ever to be regulated and controlled. Similarly the laws of behavior in amoebae, the range of

responses, and the determination of effective stimuli, of habit formation, persistency of habits, interference and reinforcement of habits, must be determined and evaluated in and for themselves, regardless of their generality, or of their bearing upon such laws in other forms, if the phenomena of behavior are ever to be brought within the sphere of scientific control.

4. This suggested elimination of states of consciousness as proper objects of investigation in themselves will remove the barrier from psychology which exists between it and the other sciences. The findings of psychology become the functional correlates of structure and lend themselves to explanation in physico-chemical terms.
5. Psychology as behavior will, after all, have to neglect but few of the really essential problems with which psychology as an introspective science now concerns itself. In all probability even this residue of problems maybe phrased in such a way that refined methods in behavior (which certainly must come) will lead to their solution.