

THE CRUX OF THE PSYCHOLOGICAL PROBLEM

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Psychology began in the field of consciousness as 'mental philosophy.' In general it has gravitated toward the positive methods of science. To at least one school it has gravitated entirely outside consciousness into bodily behavior. Between these two poles of psychology—'mental philosophy' and 'behaviorism'—there is room for many differences of opinion and much discussion.

Amid voluminous discussion in psychology it is perhaps our first need merely to locate the central problem in a more significant manner than is usually done. It is the aim of this brief statement to give the main problem of psychology a clear and definitive shaping and attempt to point out, on the basis of success in practice so far, the direction in which a solution might be sought.

The central problem of psychology is the problem of conscious 'thinking' reflection. That man possesses this power is denied by no one. Until this power is clearly assessed with reference to its function in man's adaptive skill, there is an obvious and disturbing 'unknown quantity' in human psychology which no amount of dogmatism can either evaluate or exorcise. Moreover all human psychology comes under the spell of this 'unknown' element. All that we know about psychology is *conscious* knowledge. It is a body of fact that we 'know that we know' and until we have discovered exactly what 'knowing that we know' contributes to learning and adaptation, a mist hangs low over all the facts. In brief, there is an *X* in human psychology and it is everywhere present in the science.

Nor is this 'unknown' factor pertinent within the problems of human psychology alone, as comparative psychology

abundantly shows. Science is of course as such possessed by *conscious* minds. But sciences, other than psychology, are, it seems, not so seriously embarrassed by the fact that we do not know the function of conscious mind. They may assume consciousness with some impunity. Psychology, however, cannot do this. *Consciousness is its first datum*. Until we know how to interpret this datum with reference to psychology as a whole the science is a good deal at the mercy of the personal equation of the psychologist. According to the temper of the particular psychologist consciousness may be projected into all parts of psychology or ejected entirely as of no scientific moment. Thus we have the doctrine of 'entelechy,' 'cell-soul' and less mystic views of lower-level consciousness standing in juxtaposition with thoroughgoing mechanism which denies to consciousness any function whatever,—and each respective position unassailable except by an attack as doctrinaire as the position itself. This situation promises to continue until *we can draw a clear line marking the relation between organic types of learning and human consciousness*.

In order to pursue our aim to give this central problem a clearer statement we shall first try to evaluate the strictly practical elements in the major controversy between orthodox psychology and behaviorism by separating out those facts of present scientific importance from the large body of controversy which is for the present of theoretic import only. This procedure does not of course assume that the theoretic considerations are unimportant. Our step is assumed to be of adventitious value only,—a mere matter of good pragmatic approach.

It must be patent to anyone who has followed the controversy between behaviorism and the older views that the argument all develops within the technically rather narrow field which in common usage is called 'thinking' or 'reflection.' It is only less clear that although the issue develops at several points in the supposed facts it roots back in the assumptions of the respective positions. In other words the

differences arise in distinctively human psychology—'reflection'—and the answer which each school makes is *appropriate to its assumptions*. It is quite necessary to see this in order to take away the *semblance of objectivity* from a good deal of argument which is in the main strictly theoretic. Let us look at the major assumptions of the two positions.

The older psychology accepts consciousness as a *bona fide* datum, and accordingly reckons with the content it offers, sensation, image, concept, meaning, purpose, etc. It is not however proved that consciousness is scientifically relevant.

The behaviorist on the other hand defines stimuli and response, situation and adjustment, in purely physiological terms, and by this means makes it irrelevant whether or not the subject responds in the field of consciousness. Thought is then identical with its expression—at least for scientific purposes. This makes consciousness as objective to psychology as it is to the other sciences, and rules out its content as irrelevant. This procedure the behaviorist justifies on the grounds that it has already been standardized by other sciences. Other sciences assume an observer and his ability to make an observation.

Now we are merely urging at this point that the significant thing about these respective views is the assumptions. If we were to accept the assumptions of either of these positions in a thoroughgoing manner we would probably not find serious fault with the details of the argument of the position chosen; for each theory gives a fairly rigorous application of its assumptions to the details of the problem. This appears to be quite clear in the fact that neither side has truth of undoubted scientific value which cannot be interpreted according to the alternative position. The real differences then are in the *theoretic approach*, and in order to get at them as such we must raise the previous question from the argument back to the assumptions.

When we get back to premises the theoretic advantage of the older psychology is too manifest to need defense or profit by it. Behaviorism in distinctively human psychology is highly doctrinaire. It appears to have no better defense for

ruling out consciousness than a superficial appeal to an associative comparison with the practice of other sciences. This will not do. By no stretch of theory could we hold that consciousness is a datum of any other science than psychology, but it is entirely natural to consider consciousness the *first* datum of psychology whether or not it finally proves to be of scientific relevancy.

But even if we pass by this serious weakness in the theoretic armor of behaviorism in the realm of 'thinking,' we have its own admissions that within this realm 'behaviorism' is *no more than a doctrine*. Watson grants that we do not possess knowledge of the relation existing between thinking and concomitant bodily activity, and admits that "the time seems far off when such a thing is possible." When we recall that the whole attack against behaviorism is made at this point, it seems clear that we should call for an armistice on the ground that there can be no worthwhile scientific issue where it is impossible to make a stronger case against the opponent than he makes against himself. If we are prepared to grant this conclusion it will release our attention to strictly practical considerations of the controversy—considerations which have received too little attention because of the preoccupation with theoretic matters.

From a practical point of view the situation is quite different. Such advantage as behaviorism has is practical advantage. The wings which its opponents have been trying to clip never got it any progress. It has been walking on its legs. Its manifest theoretic handicap has not proved serious from a practical point of view at least for the present. It is difficult to make a scientific case against a theory in a part of the field in which no theory is making progress. If behaviorism seems logically inadequate for distinctively human psychology, the older psychology has at least failed to be adequate in practice. Meanwhile in the field of animal psychology and that large part of human psychology which appears to have the same pattern as animal psychology, the methods of behaviorism have revealed a distinct advantage over older methods. We may shake our heads when told

that behaviorism, as at present defined, is the only scientific way in psychology. We cannot deny however that it has made progress where the older methods were lost in confusion. Nor can we claim that this method is reaching a margin of diminishing returns—a claim which might be made with much plausibility of the older methods provided we mean really scientific returns. Behaviorism is making progress where it is reaching its accepted data and that which it neglects in human psychology is not proved to be significant from a scientific point of view.

Briefly then the practical situation appears to be this. Neither the introspectionist nor the behaviorist is making any sure progress in distinctively human psychology—that is, human psychology as contrasted with animal psychology.¹ This is the very point at which they are at issue with each other. This issue is then for the present purely theoretic. As such the older psychology has the advantage. But this logical advantage is not yet a practical one; for the behaviorist's 'end result' view offers a type of explanation for all facts of proved value in the disputed field. Meanwhile his theory has the advantage in practice elsewhere in psychology. All this lends some weight to the bold assertion of the behaviorist to the effect that the supposed problems which this theory is neglecting will all "be sucked under never to return again" by the normal advance of science.

The writer sympathizes with the theoretic objections urged against behaviorism as applied to human psychology. We must not however let these objections blind us to practical considerations; especially is this true since it appears clear that *within the field of the psychology of thinking, known facts—or at least agreed-upon facts—are too few to make an attack at this point by the older methods lead to determinate results.* On this account the case against the behaviorist at best never

¹ Watson subdivides 'thinking' as follows: (1) The mere unwinding of vocal habits. (2) The solving of problems not new. (3) The solving of new problems. Number three represents the area in which behaviorism is attacked when the attack is discriminating. Whatever definite elements are found in one and two, they fall within the general laws of learning found at lower levels also.

gets beyond the stage of probability, and in no case does the accumulated argument become so impressive against behaviorism as does the *mere setting of respective assumptions in juxtaposition*. We shall try to illustrate the truth of this statement further by brief reference to a symposium on the psychology of thinking in the October, 1920, number of *The British Journal of Psychology*. The findings which this symposium reveals will perhaps enforce the opinion that we are shut up by the facts of the situation in this controversy to a more pragmatic attack upon the problem than that which we have been making.

The subject of the symposium is "Is Thinking Merely the Action of Language Mechanism." Brief articles are contributed by T. C. Bartlett and E. M. Smith, Godfrey H. Thomson, T. H. Pear, Arthur Robinson, and a rejoinder by John B. Watson whose views, as expressed in Chapter 9 of his book "Psychology from the Standpoint of a Behaviorist," are especially combated by the other writers.

We shall not attempt to do justice to the suggestiveness of the various articles nor the full position of the writers. It will be sufficient for our purposes to set down the three most fundamental questions which arise in the controversy between the behaviorists and their opponents and note how far the different writers—especially those who are colleagues—agree among themselves as to the facts. The following questions appear to be primary.

1. How far does 'substitution' account for thinking?
2. What is thinking?
3. What is the relation of thinking as response to its expression?

Viewing the symposium as a whole these questions stand out as fundamental to the controversy. Let us take the questions up in order and note briefly the position of each writer upon each question. For the sake of brevity we shall refer to the questions by number and the authors by the initials of their surnames.

I. B. and S. find the behaviorists' notion of 'substitution' inadequate. We need 'substitute-sign' to satisfy the facts.

The use of the latter is not however peculiar to thinking. T. objects to the criticisms of substitution offered by B. and S. and defends substitution as adequate. P. expresses no view on this point. R. also finds substitution inadequate and holds that nothing beyond this is possible under the assumptions of behaviorism. W. as a behaviorist defends substitution as an explanation of thinking. Upon question number one, which has to do with the mechanical technic of thinking, two papers favor substitution, two deem it inadequate, and one offers no opinion.

2. B. and S. hold that the unique characteristic in the thinking response is that it remains constant in many settings. Thinking is a response to abstract qualities. T. objects to defining thinking as response to abstract qualities. This is not peculiar to thinking. Animals also respond to abstract qualities. Thinking is a search for the relevant point in a situation and is in this sense response to abstract qualities. False starts are the very essence of thinking. It is trial and error by proxy in which substitute signs are used instead of actual bodily trials. P. believes thinking is a process beyond definition perhaps. It is to be contrasted with the mere rehearsal of experience and with habits. R. expresses no opinion. W. says, "The whole man thinks in his whole body in each and every part." This bodily activity is at a maximum in the laryngeal region and minimum in the larger musculature. Thinking is never a response to abstract qualities, but always a response to "definite and particular things." Thinking may be divided into, "(1) the mere rehearsal of vocal habits, (2) solving problems not new, (3) solving new problems." Number 3 has alone become identified with 'thinking,' but without defense. A man's behavior in solving a new problem is just like that of the rat in the maze.

Upon this question, number 2, there is no clear agreement. There is at best, approach to agreement in minor matters. The behaviorist, in accordance with his assumptions, makes thinking more inclusive than the others. While the latter all limit thinking to that activity which Watson calls "solving new problems," they differ among themselves as to

what are the significant points of the process and even contradict each other in this matter.

3. B. and S. believe the behaviorist not justified in identifying thought and its expression. They grant however that *thought can be studied in only a very meager way apart from its expression*. It is perhaps always a response to qualities and relations as such. This Watson denies and apparently Thomson also. T. implies that thought is not to be identified with its expression. P. says that thought and its expression are to be sharply contrasted, as much so as skating and figure skating. R. expresses no definite opinion but his whole position is against the identification of thought and its expression, it seems. W. declares that only the expression is significant.

Upon this question all agree in opposing the behaviorists' position on theoretic grounds. B. and S. venture to look at the practical side of the problem and in doing so all but agree with Watson. The other papers do not ask the practical question *as to how we are to get at that part of thought which is not in its expression*.

Of course matters outside of the scope of these three questions are touched upon in the symposium. These questions however cover the points most common to the papers and express fairly well the major issues of the problem. It should be noted that with respect to the questions there is almost no clear agreement of a definitive sort. Such agreement as appears lacks pointedness. Moreover question 2, which is central and put in positive form, reveals no notable agreement even among colleagues. This absence of agreement on the fundamental question as to the significant characteristics of thinking tends to show that the positions taken are on the whole *projected by the psychologist* rather than *objectified from the facts*. If the latter, we would surely expect repetitions (agreements). There seems to be something quite arbitrary about the point of approach chosen by the writers and quite arbitrary in what is held up as significant.¹

¹ Thomson appears to the writer to have come far closest to the correct view of thinking in his notion of "search for the relevant point in a situation" and his "trial

The overweening influence of the theoretic appears also in the answers given to question three. All four papers oppose Watson's identification of 'thinking' with its 'expression.' Only Bartlett and Smith however face the practical problem which this opposition brings to the fore,—that is, "How are we to get at in an objective manner that 'thinking' which is not in its 'expression'?" Bartlett and Smith ask how far we can study thought "apart from its expression," and conclude that it is possible only in a very meager way. They suggest that thought is perhaps always a response to abstract qualities and relations as such. This is in part denied by Thomson and categorically as a whole by Watson. It seems to me that Watson is correct.

Now this question raised by Bartlett and Smith is in some form crucial and should have been raised by each writer who denies the adequacy of identifying thought with its expression. The behaviorist surely does not deny that hypothetical objections can be raised against his doctrine of 'end results.' He however answers all such objections by neglect on the grounds that the concept 'expression' exhausts all other concepts for scientific purposes. An attack upon the 'end-result' claims of the behaviorist must therefore show that it is possible to get at that which the behaviorist is declared to be neglecting. No other type of attack really meets his position. If the behaviorist in fact accepts all that can be studied in a scientific manner he accepts all that is significant for science and his position is sound.

It develops then that the behaviorists' position is very weak theoretically in that it arbitrarily rules out consciousness, but that *apart from behaviorism we lack a clear view of any sort as to what thinking really is.* One course is open to us. Since the behaviorist is more successful in practice in psychology outside the phenomena of human thinking, we may leave theoretic considerations out of account for the present and give this practical fact right of way. We propose to do this. We shall get our orientation to the problem from and error by proxy." He does not however offer more than a statement. He neither shows that such 'thinking' is *accomplished* nor suggests any technic whereby it *theoretically might be accomplished.*

the practical advantage of behaviorism rather than from theoretic considerations. Let us accordingly accept the behaviorists' position first, and then from *within it* ask at what point in this system we would need to provide for distinctively human psychology in case this part of psychology is not adequately provided for in behaviorism as at present defined.

The background of this line of attack is somewhat as follows. Psychology is a unit. The controversy over behaviorism does not develop over its whole area but only within the field of 'thinking' as understood by the older psychology—or what the behaviorist calls "solving new problems" of thought. Since this problem of 'thinking' has at best only an indefinite statement by the school which appears to have the better theoretic position with reference to it, we are shut up to the step of blocking out this problem within the general field of psychology according to the theory which is more successful in practice elsewhere in psychology. By this means we may hope to discover at just what point this theory of behaviorism would need to adjust itself if the older theory be correct. In other words, we might be able to locate the gap in the line of behaviorist psychology which would be supplied by the materials which the older psychology claims behaviorism is leaving out, provided the older school is really correct. Merely locating the problem in this clear way should prove an advantage. Let us attempt it.

Behaviorism appears to limit psychology to the finding of the 'conditions' of responses and their 'adjustments.' This is a view of psychology which all will accept without reservation for the field of animal psychology perhaps. Likewise doubtless all will grant that human psychology is limited to the determination of conditions and their adjustments, provided we do not restrict the word 'adjustment' so as to identify it with 'expression.' This reservation roughly locates the problem. Our question is at this point, "Is there a significant factor between 'conditions' and 'expression' which the behaviorist leaves out of account?" In brief "Is there in unique human psychology an *X* between the behaviorist's

'situation' and his 'expression' which, left out of account, makes his thoroughgoing 'end-result' doctrine a *non sequitur* at this point?"

This question appears to locate the problem. If there is nothing of scientific significance between 'situation' and 'expression,' then the behaviorist is correct. His method, successful elsewhere in psychology, needs but to keep on its normal course, approaching nearer and nearer to its goal of completely objectifying the central problem of psychology also. If there be an *X* here, then behaviorism is inadequate at this point, and this *X* stands for a technic which when assessed will harmonize the two views by correcting the theory of behaviorism at this point and by giving to the older psychology the objectivity needed for scientific clearness.

This shaping of the problem forces the question which is asked by Bartlett and Smith—the practical question as to how we are to approach this possible *X* behind expression. Bartlett and Smith ask how far we can study thought apart from its expression, and decide that it is possible in only a meager way. They suggest rather timidly that perhaps thought is always a response to abstract qualities and relations as such. As we have said, this is denied. It is very difficult to see how thought could be studied apart from its expression, and yet on the other hand if there be an *X* there must be some way to get at it if it be significant. The question raised is perhaps in unhappy form but the inquiry is central in importance.

The situation in which this question places us requires that we challenge the term 'expression' as used. We should not ask, as do Bartlett and Smith, "How far thought can be studied *apart* from its expression,"—a really impossible thing it seems to me,—but, "Can we make a distinction in the term expression which the behaviorist does not make, which will show the way to a more discriminating study of thought *through its expression*?" So long as we limit the meaning of 'expression' to muscular and glandular reaction no such distinction can be made, it seems. For it is quite clear that the *motor* behavior of a man in solving a new problem is on the general pattern of that of the rat in the maze. If we are to

limit expression to muscular activity the behaviorists have won their case, it appears.

But why limit the term in this way? Is it not an abstract and arbitrary limitation—a bit of ‘rationalizing’ which does not fit the facts? Before we turn with this query to the matter of ‘thinking’ and ‘language habits’ in which we are especially interested, it might be well to raise it with reference to confessedly motor behavior—the rat in the maze.

Are the reactions of muscles and glands as such really the only or even the most significant facts in studying the rat in the maze? I think not. The *integrated* behavior of the rat is the most significant. By integrated behavior we mean the integrated reactions which come out of earlier broken and random trials, and which become an ever better adaptation to the motor problem which is being worked out. This is the behavior toward which experience moves through learning, and no amount of purely random muscular reaction would be important to science were it not for the fact that the ‘set’ of the organism with respect to its environmental problem makes for adaptation by *eliminating* some reactions and *integrating* others. This process of abbreviation and integration is not conceded to be wholly muscular and glandular.

Nor is the final learned reaction the result of muscular reaction alone, even if we pass by the problem raised above concerning the nature of the processes of elimination and integration. The adaptive reaction is shaped to the problem of the environment. It comes to be what it is, not alone by muscular reaction but because of structural elements in both the animal itself and its environment. In other words the reactions of the animal in so far as they have a pattern at all are quite as much conditioned by the articulated bony structures of the animal which are not active but responsive, as by its muscles, and the reaction as a whole comes to be as a response to the structure of the particular environment. Reactions do not of themselves have an interest for science. Before they can have such an interest they must show repetitions (patterns). The reactions therefore in which science is interested are both acts and *results*. To treat reactions as

muscular and glandular without further reference is to neglect problems, overlook facts, and after all to seize upon that which in itself does not appear to be significant for science. It seems fair to say that abstracting reactions from the structural facts which provoke them and determine their shaping is an arbitrary and false method.

It will simplify our further discussion if we leave out of account the effects upon reaction of inherited structure and deal only with learning. As we have intimated, the problem of the environment determines the shaping of a learned reaction. It is shaped *from the outside in*. This takes place in two ways, it seems. The *spacial distances* in the maze from 'position of starting' to 'place of turning,' or from 'one turning' to 'another,' stamp their effects into kinesthetic sensation under which the learned reaction is run off. Also points of stimulation in the environment—colored spots, lights, bells, doors, latches, etc.—become outstanding stimuli 'seen' or 'heard,' under which certain problem reactions are run off. Doubtless both processes go on together in motor learning and are more or less present in all cases of motor learning. In some cases as in that of the rat the kinesthetic sensation appears to be more important to learning than stimuli 'seen' or 'heard.' With lower animals generally 'location of stimulus' and 'distance covered' are important in learning. These depend on kinesthetic sensations which in turn have come from the structure of the environment. Likewise reactions that depend on stimuli seen or heard depend on the structure of the environment, and these reactions are even more interesting for present purposes of illustration. Stimuli seen or heard—such as a light or a bell—come to have a more or less *discursive* use as stimuli guiding motor behavior. In this respect they become 'signs' of adaptive behavior,—that is, they come to be an '*organic*' sign system¹ under which certain reactions are run off. This type of behavior reveals the *organic background* of the 'sign system' as we know it in human psychology.

¹ Since kinesthetic learning is based upon constant or repeated facts in the environment quite as much as is learning from stimuli seen or heard, we shall consider only the latter, as it makes more clear the comparison we are making.

Finally, that which most truly expresses an organism is the reaction which *adapts* it as all behavior shows. Such adaptive acts do not come to be merely by muscular reaction, but by acquiring reactions shaped *quantitatively to the environment or guided by an 'organic' sign system learned from stimuli seen or heard in the environment.* Even if it were possible to account for these acquisitions under the head of muscular reaction, we must still face the fact that the particular reaction comes to be what it is by reason of the structure of the environment and is run off under the stimulation of the environment. Since random reactions of muscles are not in themselves important to science but adaptive reactions are both that toward which learning tends and with which science can deal, and since adaptive reactions are both shaped by the environment and run off under environmental stimulation, our notion of 'expression' ought surely to be enlarged to include *reactions with reference to the environment,* and not remain limited to the abstraction 'muscular and glandular reaction.'

It appears that 'expression' should not only include *adaptive* reactions as such but should *especially* mean these as we have implied. This does not seem to be an arbitrary position. All experience of the animal tends toward such reactions. They are a sort of summing up of activities. They possess the elements of repetition and pattern which science can assess. If we get our orientation to the idea of expression from these facts we will not be content to limit it to muscular and glandular reaction. To do so would not only compel us to assume that all the powers by which we receive and transmit stimulation and by which the responses to these are integrated, abbreviated, or eliminated are exhausted by muscular and glandular reaction, but it would compel us to overlook that an adaptive reaction is run off with reference to a problem of the environment and is an *expression to something.*

Therefore to abstract, in a thoroughgoing way, 'expression' from the organic sign system in which it arises and without which it would not come to be what it is, is false to the facts. The adaptive reactions of the rat in the maze are run off point by point in accord with a process of integration and abbrevia-

tion under a constant series of stimuli in the environment. The muscular and glandular reactions are not the most significant thing, for they might be at a maximum in fruitless, random reactions in which no results for science are available. Fitting into the environment is the important thing for the animal and for scientific observation. I suspect that this is not done by reactions which are *wholly* muscular and glandular in the usual sense of the terms, and it is in any case done in a pattern which is conditioned by the static structural parts of the animal and in definite functional reference to high points of the environmental problem. The *adaptive reaction* is the whole, which is vastly more than the sum of its parts.

This less 'conceptual,' more 'organic' view of expression gives to our problem a new face when we come to apply it to the problem of the relation of thinking to language habits, or better still,—consider the relation of 'thinking' to 'expression.' We do not feel so strongly the tyranny of the muscular reaction theory. Language habits do not consist merely of laryngeal reactions with a minimum of larger musculature reaction. The babble of a child comes thencest to this. Language considered solely as motor reaction is first of all a *functional* reaction which is the result of environmental stimulation, transmission, integration, abbreviation and is used with reference to environmental situations. For this reason alone to abstract it into 'muscular activity' is not the most meaningful method even if true.

But language is a *result* as well as a reaction. It has been conditioned by the vocal chords in its development. So also the bony structure of the head and chest give resonance and furnish the signs of emotional placement. Likewise the language habits have grown up with reference to the ears of the subject and social associates and have been shaped by a great variety of conditions in the organic and inorganic environment. In other words, the most important thing about language behavior is not the muscular and glandular reactions upon which it surely depends, but rather those integrated *sign reactions* which make it what it is—a most valuable tool of adaptation.

Language has become this valuable tool of adaptation surely not because there is any *muscular tendency* toward laryngeal reaction rather than reaction of larger musculature, but because it has been shaped by a constant process of correction to the problems of the environment. It has grown out of the babble of mere vocal noise-making, not because laryngeal reactions tended to become substituted for the more overt muscular reactions but because *language came to be heard* as a sign system most valuable for adaptation. We should therefore in a scientific study of language habits in relation to thinking, shift the emphasis from abstract subtleties of muscular relation apart from function, to *word sign system heard* in which language grew up and by which it functions. If we do this it will be natural to think of language habits as the sign-system behavior of the 'thinking' behavior,—or the *sign system of thinking*.

In brief, let us say then: When we observe any behavior—for example, a rat in a maze—we do not observe it abstracted from the environment and we should not therefore say that 'muscular reaction' exhausts its expression. We observe behavior with reference to the organic sign system which regulates it, and this sign system is built up by reactions which are probably not *all* muscular and it has reference in function to *facts or points in the environment*. The random and meaningless reactions of muscles and glands become fixed into meaningful pattern because of these constant points of stimulation from the environment.

In the same way we should observe thinking not as merely the reaction of the muscular language mechanism but with reference to language *as an audible sign system*. We observe motor behavior with special reference to a sign system *seen*; we observe mental behavior—thinking—with special reference to a sign system *heard*, for this is the manner *in which these respective systems have been built up in function into integrated sign systems*. We merely in the one case assume the *eyes* of the psychologist and in the other his *ears*.¹

To treat expression then from the point of view of its sign

¹ Of course speech is translated into a written sign system and can then be seen.

system is to seize upon the most important approach as the development of behavior indicates. To take this approach is also to take hold upon the most objective data from which to assess behavior. This is equally true in motor and in mental behavior. It is perfectly apparent that language habits do not grow up because there is any muscular tendency for laryngeal reactions to become substituted for larger muscular reactions. The substitution takes place because the sign system of *language heard* has more adaptive value than *motor reaction seen*.

This brings us to the final significant question: Why speak of behaviorism as if it had to do only with muscular and glandular reaction, since in practice the study of behavior always depends for progress not upon muscular reactions as such but upon the other factors we have mentioned, which make these reactions show repetitions (patterns) and thereby make them of scientific importance? This means that behaviorism in practice keeps in mind not muscles and glands alone but the environment and the powers of the organism to synthesize in learning the effects of the stimulations from the high points in the environment. These latter powers involve the use of muscles no doubt, but they appear to involve more. They are in any case enough different from muscular reaction as generally understood to make us hesitate out of both respect for truth and scientific clearness to include them within the general notion of 'muscular reactions.'

But even so, muscular and glandular reaction without reference to constant elements in the structure of the organism and the environment remain random and meaningless. Since all orderly reaction that is not inherited has its pattern marked for it by a sign system from the environment, and all pattern reaction, inherited and learned, has relation to the bony structure of the organism, it is difficult to see how behavior can be correctly described by abstracting it under the notion 'muscular and glandular reaction.'¹

It appears then that 'behaviorism' is not at fault in

¹ We of course do not deny the importance of the elemental approach. The organismal is however more significant.

method but in a too abstract and intellectual definition of data. It is doctrinaire. Not even in lower animals are we limited in actual study of behavior to the reaction of muscles and glands, while, with respect to the human behavior we call thinking, muscular reaction is a fiction *as data* by the admission of the behaviorist himself. The sign system structure of the reaction is both more important to the organism's expression, and more accessible to science than any amount of muscular reaction. This means that the *word signs heard* in language are both more important to language function and scientific observation than the muscular reactions involved. We should therefore especially study 'thinking' (I like the word 'reflection' better) as it is objectified in the use of language *as heard*. The use of spoken language *as heard* is as much a matter of behavior and 'expression' as is the pattern reaction of the rat in the maze *as seen*. We must therefore frankly accept as expression *language as heard* and attempt to study thinking *through its expression in its audible sign system—language*.

Just how far this will get us cannot be considered at this time.¹ We at least have the statement of a real problem in the sense that we have both an *observable datum and an objective method*. This it seems to me has hitherto not been the case. The data of muscular behavior when we come to distinctively human psychology—the psychology of thinking—is hidden in the depths of physiology beyond the reach of observation. The older psychology on the other hand has depended upon a method of pseudo-subtlety which cannot without radical revision become an objective method. Any other method than that of behaviorism can, it seems to me, have only the semblance of objectivity at best. Behaviorism is *the* scientific method for psychology. It however must be applied consistently to its full data.

The task therefore which we have before us is to discover the technic of 'thinking' especially from the manner in which it is objectified in the use of language as a vocal sign system.

¹ I am working on this problem. The background study 'The Structure of Animal Learning,' appeared in the May number of the *PSYCHOLOGICAL REVIEW*, 1921. An attack upon distinctively human psychology is in process.

This involves eliminating all the effects which appear in language shaping except those which come from the inner technic or structure of thinking; or in other words, it involves isolating the fundamental pattern of language use from those elements which come into the language habits from timely factors or lower types of learning. Success in this task will give us that part of the pattern of language use and habit which comes from the factor of thinking alone—that is, give us the structure of thinking—its technic. It will in short evaluate the *X* which in human thinking lies between 'situation' and 'end result' adjustment.

This task may seem an heroic one because of the subtlety of the factors and the discursive nature of thought and language—its sign system. No one can deny however that it is a definite task. Indeed if we approach the problem involved in a thoroughly pragmatic manner the facts point unmistakably to this shaping of the problem, it seems. The only way to get at the *unique* technic of human psychology in a scientific manner is to observe, with full respect for all the facts, *distinctively* human behavior. Distinctively human behavior is the *adaptive use of a vocal sign system known as language*. We should study 'thinking'—reflection—through this expression. This applies the scientific *method* of behaviorism while it avoids the theoretic weakness of the *doctrine* of behaviorism as we have come to know it.

If from this point of view the problem seems difficult, this is nothing new. If the woods ahead are as dark as ever and appear to be as trailless, our feet are at least upon the ground. We have some place to stand, from which to make a beginning, which it appears to me is not the case with respect to this central problem of psychology, in either the doctrine of behaviorism as it has been defined, or that of the orthodox school.