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The two most radical doctrines put forward by the behaviorist are, first, that consciousness does not exist, and second, that psychology should wholly abandon the introspective method. My chief object this evening is to inquire in what degree the second thesis necessarily follows from the first. But before entering on a discussion of the value of introspection to a behaviorist, it seems desirable that I should state, as briefly as possible, my attitude towards the first thesis, namely, that consciousness does not exist.

I was trained as a structural psychologist. Now it is an odd circumstance that while historically behaviorism in America is the child of functional psychology, logically the point of view of structural psychology leads more naturally to sympathy with behaviorism than does that of functional psychology. Structural psychology and behaviorism have the tremendous bond of accepting the world of physical science as a closed system. Structural psychology in addition to the world of physical science accepts a parallel or epiphenomenal world of consciousness; behavioristic metaphysics rejects this conscious concomitant. But both behaviorism and structural psychology refuse to allow the interference of non-physical causes in the physical system.

As a structural psychologist, then, I am prepared like the behaviorist to defend the unity of the physical world. It is [p. 90] a world of motions, the sum of which is constant. The signal triumph of this conception is that it is the only hypothesis ever framed by man upon which predictions may be made that will infallibly be verified. On no other hypothesis can man command obedience from his environment. Functional psychology, like much recent philosophizing, violates this closed system of science in the interests of monism: I would rather sacrifice monism than the physical order. In one of Dr. Kantor's ably written articles,^[1] 'Psychology as a Science of Critical Evaluation,' the author, discussing memory, rejects the structuralist

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interpretation because it involves 'an empty dance of idea-atoms,' and behaviorism because it involves 'what is just as bad, a neural mechanics.' If we ask what Dr. Kantor would substitute, he answers that 'when we consider the function of ideas in memory, we must immediately freight those ideas heavily with human content.' My difficulty with this statement is just that I do not know what it means. A neural mechanics can be linked to the mechanistic system of the universe, which seems to me to have, by virtue of its success in controlling phenomena, so compelling a prestige; sensations I can fit into a relation of concomitance with the neural mechanics, but freights of human content are anomalies unless reduced to other terms. They seem to be of the same stuff as that which Münsterberg used to call 'will-attitudes,' and these, as he used earnestly to maintain, have no place as such in the world of science.

The structural psychologist feels, then, more at home in the behavioristic world of physico-chemically determined behavior than he does in a world where movements are causally determined by freights of human content, or by active selves. How did it happen, then, that functional psychology and not structural psychology was historically the parent of behaviorism? Why, the behaviorist learned from the functional psychologist to ignore the mind-body problem; he learned that in a world where ideas can act as such directly on movements there is no difference between ideas and the physical world of movements; hence he felt at liberty to take [p. 91] a further step and say that ideas are movements and nothing else. By simple assertions the old concepts were redefined. Consciousness does not accompany a certain degree of complexity among systems of muscular movements: it *is* a certain degree of complexity of movement. What an animal means *is* what he does; thinking *is* subvocal talking. This fervor of assertion unaccompanied by argument, which recalls the methods of Mrs. Eddy, certainly points to the sincerity of the behaviorist's conviction that talking and thinking are one.

Believing with the behaviorist that the system of physical nature is a closed system, and envying as we all must the triumphal march of physical science, the structural psychologist must sympathize with the behaviorist's desire to attach himself to the mechanistic procession. There was at a certain university where I had charge of the psychological laboratory an intelligent janitor who had picked up many bits of scientific information by serving the various laboratories of the institution. While waiting in the laboratory of psychology one day he gazed meditatively about, and remarked: "This is a queer place. It somehow gives you the impression that the thing can't be done." Now the experimental work of the behaviorist has the great advantage that it plainly can be done; the untrained and casual observer can behold it being done. Upon this gratifying obviousness of his results the behaviorist is to be congratulated. The scientific worker who does not come into the world of physical science and observe merely movements is bound to be lonely and to miss the comradeship of the physicists and chemists.

But though we are sympathetic with the good fortune of the behaviorist in obtaining admission to the world of physical science, the rest of us cannot accept his statement that nothing but the world of physical science exists. For the world of physical science leaves out colors, tones, odors; the whole system of sensation qualities. A system of movements, it admits, ultimately, no qualitative differences in the bodies which do the moving. Solar systems, living organisms, molecules, atoms, are only different patterns or arrangements [p. 92] in space: neither in the stimulus acting upon the organism, nor in the organism itself with its responses to stimulation, which are of course movements, do we get anything but rearranged patterns of qualitatively uniform particles. In reviewing Watson's first book, in 1915,^[2] I said: "Once allow that a human being can experience a sensation of color or of taste or of anything except his own movements, and you have admitted the existence of a phenomenon not to be explained in behavioristic terms." It seems singular that behaviorists should overlook the fact that their world has no sensation, qualities; they seem as it were to play at hide and seek with them. When it is a question of the reaction of the organism to blue light and they are thinking about the stimulus side of the stimulus-response situation, they accept the physicist's account of the stimulus as a series of ether disturbances and tacitly assume that the blueness is in the organism's response: when they are thinking about the response side of the situation, they accept the physiologist's account of it as molecular readjustments in nerve and muscle, and tacitly assume that the blueness was in the stimulus. Obviously, however, the blueness is neither in the stimulus nor in the response, for these are movements and blueness is not a movement. There is no escape, if the behaviorist wishes to accept the physical scientist's account of stimulus and response, from

the recognition of something over and above these and of different stuff, namely, colors, tones, and the other sensation qualities; namely, consciousness, -- abhorred word!

Annoying as it is to have to admit that we cannot eliminate dualism from our conceptions of the world, it can be shown, on the behaviorist's own theory of perception, that such a dualism arises from the very nature of the relations between the organism and its environment. The forces to which the organism reacts are of two kinds, molecular movements and mass movements. Where the stimulus is a mass movement, the movement of the organism in response is comparable to the movement of the stimulus. Now it would be good behaviorism to say that such stimuli can be adequately perceived because they can be adequately responded to: our [p. 93] movements in response to the stimulus are like those of the stimulus. The successive blows of raindrops on the hand are perceived with accuracy as distinct, discontinuous impulses, for the same rate and extent of movement is possible to the hand itself. It is obvious that all the properties which have been ascribed to stimuli, that is, to the physical whether by modern science or by the philosophers who wrote of the distinction between primary and secondary properties of matter, are derived from our own experience of our own movements; they are kinaesthetic. Locke said that the bulk, figure, number, situation, motion, and rest of their solid parts are in bodies whether we perceive them or not; and a modern physicist would describe matter in the same terms now, bulk, figure, number, situation, motion, and rest are all experienced through our own movements. But when the hand is subjected to the action of heat, we perceive, not the dance of molecules striking against it, but heat, a continuous sensation quality, something quite different from any movement whatever, and this inaccurate and inadequate perception rests on the fact that we cannot make movements in response to the movements of these separate particles. We substitute a difference in the quality of something continuous for differences in the frequency of what is intermittent, because we are infinitely too large to make discontinuous movements corresponding to the discontinuous movements of molecules, atoms, or electrons. Thus there comes into existence something that has no resemblance to movement: colors, tones, odors, in a word, sensation qualities or consciousness. The only way to avoid recognizing this dualism is to abandon the idea of the physico-chemical nature of stimulus and response; to say either that movement is not the essential nature of the stimulus or that the reaction of the organism is something other than movement. This latter alternative seems to be the one chosen by philosophical behaviorists like Professor R. B. Perry: when they say that consciousness is a form of behavior, they mean that behavior is something other than movement. But if you held that behavior is anything other than movement you have got to show what relation it has to [p. 94] movement: you give up the whole physico-chemical theory of behavior, and behaviorism becomes as far out of harmony with all the rest of the sciences as the most cloudy mysticism could possibly be.

The metaphysics of behaviorism, therefore, I cannot accept. But it is not the main purpose of this address to attack a metaphysical problem. I should like, rather, to do something to further the harmonious working of behaviorists and introspective psychologists in company, by persuading the former that there is a point of view from which introspective results are important to behaviorism. It seems to me a deeply discouraging phenomenon, and one discreditable to psychology, that a great body of experimental data obtained through the expenditure of as much care and patience as have in other sciences resulted in discoveries of enduring worth, should be thrown aside by one school of psychologists as wholly valueless. May one who accepts -- as I do not -- the thesis that mental states are not a proper subject of scientific investigation, that they do not even exist, still extract trustworthy and important results from introspection? In order to answer this question, I shall try throughout to assume the attitude of a behaviorist and to speak behaviorese.

Of course introspection is for the behaviorist a form of language behavior. It is, by the way, a curious fact that in spite of the signal importance which language behavior assumes in the behaviorist's system (thinking being subvocal talking), the adherents of this system have so far shown little interest in the experimental work that has been done on language. The literature of memory experimentation is, from their point of view, of course, a literature dealing with the formation of language habits, and as such full of significance, yet Watson's treatment of this literature in his 'Psychology,' for example, is not only far less adequate than his discussion of

work in other fields, but in one important instance, his statement regarding the confirmation by later workers of Ebbinghaus's finding that the number of repetitions increases more rapidly than the amount of material to be learned, it is incorrect.[3] Yet nearly all of the experimental [p.95] work on memory has been done under scientific conditions and well deserves to be assimilated into the body of facts with which the behaviorist must deal. His break with traditional psychology, however, is so recent that he is happier on the field of salivary reflexes and extensor thrusts than in any region where introspective psychologists are likely to be met.

But this is a digression. The problem of introspection from the behaviorist's point of view is not concerned with the investigation of language habits for their own sake. Introspection to the behaviorist is not simply language behavior, but language behavior regarded as evidence of other behavior. The introspective side of memory investigations, for instance, consists not in the speaking of nonsense syllables or verses of poetry, but in the speaking of remarks about visual or kinaesthetic imagery, distractions of attention, feelings of satisfaction or dissatisfaction accompanying the working of the language habits principally involved in the learning. Professor Weiss,[4] in an otherwise admirable study of the relations of behaviorism and functional psychology, calls introspection a minor speech reaction. He distinguishes between the major reactions studied in an experiment, and various minor reactions, including vasomotor and respiratory changes, processes in the sense-organ, and speech reactions both actual and incipient, these speech reactions constituting introspection and being to him, apparently, of negligible importance. Such a view overlooks, it seems to me, the fact that speech reaction may be taken as indications of the presence of other types of reaction. The remarks made by a person introspecting would indeed be of minor importance both to the behaviorist and to the introspective psychologist if they indicated nothing but themselves. I should like to [p. 96] suggest that the behaviorist use as his equivalent to the term introspection the term 'symptomatic language behavior.'

If this expression be accepted, clear thinking will be further helped by an attempt to reach a behavioristic definition of language. Obviously, not all behavior that is symptomatic of other behavior is language. The sounds and movements made by an animal or a human being under the influence of emotion, 'involuntarily,' to use the common term, are not language, because they would be made just the same if no other living being were present. Symptomatic behavior is not language unless a part of the effective stimulus is the presence of some other living being: a cry of pain may have as its sole stimulus the injury that is being inflicted by an inanimate force, but a cry of pain that is also a cry for mercy or help has as a part of its stimulus the behavior of some other living thing, and a cry for mercy or help is language. Further, true language is not dependent wholly on inherited mechanisms, but is determined in part by the effects of previous stimulation of the individual. In some cases even where the stimulus is the behavior of another living being the vocal response is not language: an animal may cry out with pain on being attacked by another animal; such a cry of pain is not language any more any more than if the stimulus were an inanimate force, because the victim's reaction is purely innate. If on the other hand he cries for mercy or help he is reacting in a way that shows the influence of past situations; in popular terms, to secure results, and not merely because he cannot help it. The following, then, may serve as a behavioristic definition of language: sounds or movements made by the striped muscles of the body, and determined in part by the behavior of other animal organisms and by the effects of past stimulation through the behavior of other animal organisms. The youngster who bawls with one eye on a sympathetic audience has transformed his bawls into true language because his action rests partly on his previous experience of the effect of bawls on the audience's conduct.

It must also be realized that while all symptomatic [p. 97] reactions are not language, not all language is primarily symptomatic. In ordinary popular parlance, we use language for two purposes. The first is to produce action that satisfies any one of a varied range of our needs, as when we ask persons to perform services for us and give them directions: while such language may be regarded as a symptom of our particular need at the time, its function is not so purely symptomatic as in the second case, where we use language to bring about action *like our own*, as when we describe our experiences to another person. This use of language is based on the special need for sharing experience, for obtaining sympathy; on a peculiarity of socially constituted creatures whereby they are uneasy unless the creatures about them act as they do.

True symptomatic language reactions rest on the need for communicating and sharing reactions, quite apart from any ulterior results.

Now, further; is all symptomatic language reaction introspection? The textbooks say that introspection means a looking within. Should it be maintained that symptomatic language reactions are introspective only when the behavior that is shared is inner behavior, referred to the reacting organism itself, and not that behavior which constitutes a description of the stimulus? Shall we say that when a person's language reactions are symptomatic of his inner reactions they are introspective; when on the other hand they are symptomatic of the character of external objects, as when he describes to us a new species of animal, they are not introspective?

Such a distinction between description of process and report of meaning, between true introspection and introspection which commits the 'stimulus error,' is current in structural psychology, but it does not seem to me fundamental for the present discussion. For one thing, as description of an external object becomes more minute and detailed it always tends to pass over from description of object to description of processes set up by the object. We describe an object as hard and hot, the qualities being referred to the object: more detailed attention analyses the hardness into [p. 98] contact pressure *plus* deep pressure and the heat into warmth *plus* cold or warmth *plus* pain, these being processes set up by the object. Again, whether a given quality (let us forget for the moment that behaviorists do not experience qualities) is regarded as belonging to the stimulus or to the reacting organism depends on whether it is being treated in its setting with other qualities from the external senses, such as colors received from other parts of the object, or in its setting of qualities that are experienced only from within the organism, such as feelings and kinaesthetic sensations. We may, I think, safely maintain that all description is really introspection.

Symptomatic language behavior will then be a fair synonym in behaviorese for introspection. It may be attacked as a scientific method upon two grounds. In the first place, the objection may be made that language is unreliable as an idea to other types of behavior; that introspection is untrustworthy. In the second place, it may be said that the types of behavior revealed only by introspection are unimportant and scientifically worthless.

The problem as to the trustworthiness of introspection opens the whole question regarding the limitations of language. Of these the most fundamental is that language, whether of speech or of gesture, is always a system of successive happenings, while the experience it is supposed to communicate involves complex masses of simultaneous happenings. A language cannot be learned unless its symbols are fairly simple, so easily do associative dispositions interfere with and confuse one another; hence the condensed and complicated single moments of reality must in description be drawn out into long series of words and thereby falsified. This defect, however, is far more serious for aesthetics than for science, which deals in any case with a transformed reality. From the point of view of science, the chief requisite is that a given symbol shall always carry the same significance. And the conditions that interfere with the satisfaction of this requirement lie in the circumstances under which language has developed. [p. 99]

These circumstances were, of course, the great practical needs of mankind. Such aspects of the external world and such inner states of the human organism as had most importance for welfare were those to which the reactions of language became attached. Thus the naturally and spontaneously acting human being will use the word 'loud' when acted upon by a certain tone stimulus, and those who hear this vocal response know with very fair approach to certainty what other responses on his part may be expected: 'loud' and 'soft' refer to aspects of the stimulus-response situation that are of practical importance. But a person will not use the expression, "having the vocalicity of l" when acted on by a tone stimulus, unless he has had training in the artificial surroundings of the laboratory, nor will the naïve listener know what he is talking about, that is, what other behavior accompanies the unusual phrase. Similarly, the natural man may say that he is uncertain what to do, and the natural listener infers with reasonable assurance the inner reactions that accompany the words: uncertainty is an inner state of the organism sufficiently important practically to have become firmly associated with its

verbal expression. But only the sophisticated frequenter of laboratories will say, 'I experience kinaesthetic imagery,' or, 'I am conscious of a fiat that precedes my action,' and the untrained listener will not know what he would be at, for practical life has created no fixed association between language and such practically unimportant details of the inner attitude of the organism. The more remote from every-day life and its requirements are the reactions which language accompanies, the less trustworthy is language as an index of these reactions, and the greater the danger that different observers will use the same term in different situations and different terms in the same situation.

Further, not only has our vocabulary developed under the limiting influence of practical needs, but our use of the vocabulary we have is modified by a special set of practical conditions, the social instincts: that is, our need of adjusting ourselves in special ways to the behavior of our fellow-beings. Here belong the effects of imitation and suggestion, which [p. 100] may render a verbal formula symptomatic not of the individual's inner reactions, but merely of the particular external copy that happens to have been set him. Here belong also those modifications of language that spring from the self-exhibiting instinct, the Freudian censor, or, in ordinary language, the desire to appear well in the eyes of others. I will not attempt the hard task of stating in purely behavioristic terms the equivalent of such influences as the self-exhibiting instinct, or the inferiority complex, or the will to power: both behaviorism and structural psychology may well envy functional psychology, or self psychology, their justification in leaving such concepts unanalyzed. But the desire to appear well in other eyes does exist, however it is described, and does produce such mechanisms as displacement, compensation, and symbolism. The working of these mechanisms has been held to invalidate the testimony of introspection in the field of the emotions. I may say at once that there seems to me an error in this view. It is claimed, for instance, that when a person declares that he hates another, psychoanalysis may reveal his true attitude to be one of thwarted love. Now to call the hatred unreal and the thwarted love real involves the assumption that our emotions are not actually determined in part by social conventions and the self-exhibiting instinct. I believe that they really are so determined. When an individual has a certain emotional attitude towards another, this attitude is due in very large part to the whole body of instincts and habits which he brings to bear on the stimulus. The Freudian censor does not repress an emotion: it is a normal part-determinant of the emotion. Suppose that a person hates another because his self-esteem is inhibited by the other's coolness to his previously manifested love; his self-esteem is just as genuinely a part of him as his love: he really does hate. What psychoanalysis does is not to reveal a true emotion and substitute it for a false one: it transforms one emotion into another by abolishing the patient's self-respect and regard for appearances, which are among the normal determinants of emotion. Or to speak more accurately, the psychoanalyst [p. 101] alters the stimulus for the self-exhibiting instinct in his patient: it was originally the standards of his friends and family, and is now the standards of the psychoanalyst.

The various disturbing influences which affect the trust worthiness of symptomatic language reactions will vary in the amount of harm they do according to the degree in which such language reactions can be correlated with other reactions. There are cases where the behavior of external objects bears out the testimony of symptomatic language reactions. Examples are almost too numerous to mention. Such verbal responses as 'blurred' or 'distinct' made to stimulation by light can be shown by experiments with lenses and screens to correspond with actual peculiarities of the image cast on the retina. When an observer reports that fainter and higher tones accompany the fundamental tone produced by setting a metal plate into vibration, sand strewn on the plate will show that it actually does vibrate in fractional parts as well as in one piece. There are other cases where the symptomatic language reaction can be correlated, not indeed with physical observations of the stimulus, but with other observable reactions of the organism. Not infrequently the direction of attention, for instance, can be inferred from signs other than the observer's verbal report of it. Emotional states of course have a variety of manifestations other than language, although unfortunately most of the easily observable signs of emotion are so readily controllable that when they are allowed to appear they practically constitute gesture language. Sometimes one set of language reports can be compared and checked against a different type of language report: thus Friedländer^[5] and Fernberger^[6] have shown that differences in the discrimination of weights (these differences of course being expressed in language) are regularly correlated with differences in the attitude of the observer,

introspectively reported. Now where there exists a regular correlation between symptomatic language reactions and other symptoms, the trustworthiness of introspection cannot be seriously impugned. [p. 102] There remains, however, a considerable number of phenomena where symptomatic language reactions are unconfirmed by any other sort of evidence. Here, certainly the sources of error which affect language will have nearly unchecked play. The behaviorist may well say that in these cases the expression 'symptomatic language reactions' is inappropriate, for these language reactions are symptomatic of nothing but themselves. A doctor calls an increase of body temperature a symptom of infection, but he has other means of verifying the existence of infections besides the behavior of a thermometer.

There is no denying that these criticisms have weight. Let us see, however, if mitigating circumstances can not be found. To begin with, and parenthetically, as it were, we must recognize that in this class of phenomena, whose existence rests wholly and entirely on the testimony of language, there are included a number which are so common, so generally agreed upon, that the burden of proof is thrown justly upon the person who denies their existence. The mental image is of course the most striking instance of this. The testimony of mankind to its reality as an experience, though only verbal testimony, goes far towards balancing the purely theoretical objections of the behaviorists. But there are other examples besides the mental image: we have no evidence but introspection for the existence of after-images, of binocular double images, of flicker effects, of temperature and pressure spots on the skin, of the fusion into one of two touch impressions in the Weber compass experiments, and of a number of other comparatively simple observations. Secondly, it must be noted that rejection of the whole group of phenomena for whose existence symptomatic language is the only evidence would be inevitable if the language used in connection with them were not the same language which is used in connection with reactions resting on other evidence. But of course it is the same; all our descriptive terms are derived from externally observable responses. When a person is engaged in introspecting his organic and kinaesthetic responses, which are not externally observable, he has to use [p. 103] such expressions as strain, pressure, warmth, contraction, which apply equally to his reactions to external stimuli. If the meaning of these terms is well established in connection with externally observable responses, it may indeed be rash to conclude that the inner responses are exactly like the externally observable ones, but it certainly seems safe to infer that the use of the same term implies a common element. Köhler,^[7] by the way, in closing the report of his investigation on auditory sensation, makes an interesting suggestion which bears on this point. He proposes the hypothesis that what he calls the compelling tendency to call high tones 'bright' and low tones 'dark' means a real similarity between the processes in auditory and visual cortical centres. Since the term 'bright' is also applied to touch sensations by some investigators, it would, if there is anything in Köhler's suggestion, indicate a peculiarity of the nervous processes that may be common to three cortical regions, sight, hearing, and touch. Our present interest in this idea of Köhler's is due to the firm reliance it shows on the existence of processes for which no other indication but language can be found.

These considerations, however, can only mitigate, they cannot abolish, the difficulty of finding clear and unambiguous terms to indicate the subtler reactions. The precaution adopted by those who use the introspective method consists in giving the observers a careful preliminary drill in the use of terms. Now this proceeding, of course, provides immediate occasion for the behavioristic enemy to blaspheme: he points to the obvious danger from suggestion. The introspective psychologist may, however, say in defense that other descriptive sciences also have to train their inexperienced observers in the use of terms. Take, for example, the terminology used in reporting the very difficult observations on the structure of protoplasm; or such astronomical observations as those on double stars, needing years of practice; take the observations of planetary markings, or, in chemistry, the use of the Wheatstone bridge, which requires liminal discriminations of noise. These observations are wholly analogous [p. 104] to certain types of introspective work, where an actual external stimulus is given, but where the reactions to that stimulus are complicated and may vary greatly according to the degree in which the stimulus is reacted to in its details. Compare the careful introspective observations of Boring^[8] on cutaneous sensations or of Murray^[9] on the qualitative analysis of the sensation of tickle, or of Köhler^[10] or Rich^[11] on the properties of tone sensations. Whatever sources of error affect such language reactions as these studies involve must be admitted to apply equally to any difficult observations in the other observational sciences.

But there is one type of introspective work that does occupy a position more exposed to falsifying influences than any other scientific investigation. It is the type where there is no external stimulus at all. A trained and an untrained observer may vary greatly in their reports of double stars or of a cutaneous sensation, but at least we do know that the two observers are being acted upon by the same external force: there is one common factor in the situation. When the language report relates to purely internal processes like conscious attitudes, complex feelings, imageless thought, there being no external stimulus, we have no positive guarantee of any common factor in the situation at all. Persistent and spontaneous use of the same language by different observers, as in the case of the mental image, gives presumptive evidence of similarity of inner reaction, but it must be admitted that this is the realm where introspection is most vulnerable.

We may turn from the problem of the trustworthiness of symptomatic language reactions to that of the value of what they tell us.

The proper introduction to a discussion regarding the importance of the information gained through a psychological method is a definition of what constitutes important information.[p. 105] There are, it would appear, three directions in which the results of a psychological experiment may be important. First, they may be useful in practical life; thus an improved method of testing intelligence, or the discovery that a given method of advertising brings in more results than another method, or a demonstration that fatigue sets in sooner when a task is performed in a certain way, is practically important. Secondly, a psychological investigation is important if it throws any light on the workings of the nervous system; on the functioning either of sense organs or of the sensorimotor system as a whole. Thirdly, to psychologists of the non-behavioristic schools, the result of a psychological investigation is important if it conduces to the better description of a mental process. To the structural psychologist especially the analysis and description of mental processes is worth while for its own sake, apart from its relation to practical welfare, and even apart from its immediate results on knowledge of nervous function. The functional psychologist does not share this interest in the mere description of mental processes, although he believes in their existence and in their capital importance for the welfare of the organism. The behaviorist, of course, denies that mental processes exist. He will, however, properly be interested in any language reactions that furnish information bearing on practical needs, or on the functioning of the nervous system. From the behaviorist's point of view, then, the question as to the worth of introspective results reduces to the following terms: Can any information be gleaned from the language reactions which accompany inner and less accessible reactions of the organism, that bears either on the working of the nervous system, or on the practical responses of the individual to the needs of life?

We have noted the fact that there are certain reactions of the organism, as for example those involved in attention and emotion, where the testimony of language may be confirmed or corrected by that of other symptoms; while there are other reactions for which language furnishes the only accessible evidence. In the case of the first type, the [p. 106] value of symptomatic language reactions both for practice and for physiological theory cannot be disputed. The convenience of language as an indication of the direction of attention in a person undergoing mental tests, as evidence of the nature of an advertisement's appeal, as indicating the early stages of fatigue in experiments to determine the effect of various working conditions on efficiency, is too obvious to need discussion. As for the emotions, Professor Perry says, indeed, that, "almost every recent advance in this field has resulted from the more or less complete abandonment of the introspective method of description"[12]; but I do not know how far the Freudian psychology would have progressed if it had had to deal with animals incapable of language: most of its evidence seems to involve symptomatic language reactions. And that bulwark of behaviorism, the James-Lange theory of emotions, makes its most convincing appeals always to introspective evidence.

If we turn to the group of phenomena for which language is the only evidence, we find, as has been seen, that they may be subdivided into those which like after-images, binocular double images, difference tones, have an immediate relation to a known external stimulus, and those which like mental imagery and thought processes have a much remoter relation to outside

stimuli. Now two examples, at least, will occur to us where phenomena of the first sub-class have been used to support important conclusions regarding the functioning of the nervous system. The first is Sherrington's appeal to observations on binocular and monocular flicker. There is absolutely no way of determining the existence of flicker effects except by language reports. Sherrington found, it will be recalled, that when the two eyes are separately stimulated by interrupted flashes of white light, in such a way that the flash received by one retina coincides in time with the dark interval on the corresponding point of the other retina, there is no interference or summation effect between the two phases in the two eyes. Hence he draws the conclusion, of the greatest possible significance for the theory of nervous action, [p. 107] that while the fact of the connection of the right half of each retina with the left half of the cerebrum means a final common path for the control of movements, it does not mean a final common path so far as sensory processes are concerned. And on the basis of these observations he warns us "against any hasty conclusion that the neural mechanisms which synthesize reflex movements illustrate in their arrangement also those concerned where sensual fusion is the phenomenon."^[13] This warning of course applies especially to the behaviorist.

Again, take the blending of two touch impressions into one: the Weber compass experiment. It is evidenced in no other way than by language reactions, it is a typical introspective datum, and of course one of the oldest in experimental psychology. The behaviorist will recall that it has recently played a leading rôle in the construction of an elaborate theory of the working of the nervous system. Head makes great use of it in developing his hypothesis regarding the relative functions of the thalamus and the cortex; namely, that the cortex is the organ of discriminative sensibility, while the thalamus gives sensory impressions that are poorly discriminated. Head ^[14] tested patients with variously located brain lesions, and found that the ability to distinguish two compass points was lost when the cortex was thrown out of function. Boring's^[15] successful attack on Head's earlier observations by the compass method renders the accuracy of Head's observations on pathological cases highly doubtful. But it will be remembered that the issue between Head and Boring on the compass point experiment rested not on a difference in introspective testimony, in which case the enemy of introspection might have an excuse for ignoring the controversy, but on a difference in the mode of applying the compass points. Head, ^[16] in his earlier observations on his own sensations during recovery from section of a [p. 108] cutaneous nerve, reported entire loss of the power to discriminate compass points at ordinary separations, and deduced from this fact the existence of a special form of sensibility which he called protopathic and later referred to the thalamus. Boring, after a similar nerve section, found the two-point threshold not affected at all, and explains Head's results as due to lack of adequate control of the pressure exerted by the compass points. What interests us is that here is a purely introspective phenomenon which has assumed capital importance for a theory of the nervous system. It does not matter that anyone who reads Head's articles is likely to think that never was a huger theory built on a shallower foundation of facts: the trouble is not that Head's evidence is introspective, but that he gives us so little assurance regarding the precautions he has observed in obtaining it.

The behaviorist will be most likely to reject as unimportant for his purposes those introspective studies where there is less immediate reference to an external stimulus. Take for example the work of Moore,^[17] Weld,^[18] and Tolman^[19] on the relations between meaning and image, or the group of studies made at Clark under Baird's direction, whose object was to describe the stages in a mental operation and the changes which they undergo as the mental operation becomes more practiced; the work of Woods^[20] on recognition, of Fisher^[21] on generalization, or of Crosland^[22] on forgetting. No doubt from the behaviorist's point of view, the fact that persons should actually pay money for the opportunity as research students to perform and induce others to perform language reactions so remote from contact with any kind of reality, and that they should be given for such performances doctor's degrees which have a money value, though a very slight one, must appear a behavior phenomenon abnormal enough to throw light on many problems of morbid psychology. Needless [p. 109] to say, this large body of investigation was not performed with the object of interesting the behaviorist, but in the belief that mental processes really exist and therefore are worth analysis and description. But is there really nothing here for the behaviorist except a challenge to find out how people come to be so foolish)

Practical significance for ordinary life this work obviously lacks, although conclusions of a practical bearing might be drawn here and there if the investigators were interested to draw them. For example, Weiss[23] criticises Fisher's work on the process of classifying because it does not help a man to classify any better to know that in classifying "the regions of essential group features were stressed in consciousness and these regions behaved in consciousness in a fashion which depended upon their resemblance or lack of resemblance to the corresponding features in the group members. In the former event, the regions in question passed in and out of consciousness in rapid and ready fashion, without retarding the course of attention. In the latter case, on the other hand, the course of attention was arrested sharply; these regions often persisted in consciousness, and they were frequently accompanied sooner or later by more or less focal and intensive kinaesthetic, organic, and affective contents which functioned in their conscious settings as definite rejectings of the figure." [24] However, Fisher's finding that there are two types of classifiers, one more cautious and the other with a more pronounced 'response tendency' might have yielded practical advice on the best method of classifying, if the author's aim had been practical: she notes that the 'response-tendency' classifiers were the more rapid, and probably could also have furnished data on the comparative accuracy of the two types.

Now it seems to me that although this type of introspective work is directed primarily towards an end of which the behaviorist does not recognize the value, it has nevertheless contributed very largely to a result which is of the utmost worth to the behaviorist's theories as to the working of the nervous [p. 110] system. This result is the recognition of the importance of kinaesthetic processes.

The whole behavioristic theory of learning rests, of course, upon kinaesthesia, or, to use a more objective term, upon proprioceptive processes. It reduces all forms of association to habit, and habit means such a linking of nervous pathways that the performance of one movement stimulates, by what Sherrington called proprioceptive paths, the performance of the next movement in the series. What is the evidence that this explanation, which I myself believe the true one, is correct? Watson, who of course holds the theory without question, offers so far as I can see no evidence for it except the anatomical fact that there are sensory endings in muscle. Sherrington's observations indicate the importance of proprioceptive processes in the maintenance of tonus, but Sherrington does not discuss habit formation at all. Do we not really owe largely to introspection our belief that in a thoroughly established habit kinaesthetic processes play the leading rôle? That is, in behavioristic terms, is it not the evidence of language reactions associated with the contraction of the muscles of various parts of the body which is most important for the theory that habit and association rest on a kinaesthetic or proprioceptive basis? I feel quite sure that such language reactions are the main evidence of the existence of what Watson calls 'implicit language habits'; or subvocal talking: it is, the introspectionists would say, because we can feel ourselves subvocally talking under certain circumstances that the doctrine is plausible; or, translated into behaviorese, because we make certain explicit speech reactions which report the existence of these slight and incipient speech reactions. Now it is precisely the highly trained and sophisticated type of introspection that has done most to extend our conception of the role played by proprioceptive excitations. To be sure, a hundred years ago, Thomas Brown, using introspection of the old arm-chair kind, devoid of modern subtleties, declared in the strongest terms that "our muscular frame is not merely a part of the living machinery of motion, but is also truly an [p. 111] *organ of sense*." [25] But how extensive the effect of this sense-organ is could not be revealed by ordinary introspection, because kinaesthetic excitations do not as a rule demand attention on practical grounds, and symptomatic language reaction, or introspection, as we have seen, has developed like all language under the pressure of practical needs.

I confess that to me, although I believe in the existence of mental processes and therefore think their precise description worth while for its own sake, the most interesting results obtained from the more elaborate pieces of introspective research are those which bring into prominence the rôle of kinaesthesia. It is hardly necessary to give examples of the way in which recent work constantly does this. An investigation to determine whether a certain experience shall be classified as 'attribute' or 'meaning' decides for the latter term because the experience in question is 'carried' by kinaesthetic and organic processes. Descriptions of successive stages in various mental operations contain a high percentage of allusions to the appearance and

disappearance of kinaesthesia. If the behaviorist wishes to show that the so-called higher mental processes can be supplanted on his theory by systems of movements, he will have to turn his attention to these introspective studies, or else repeat them himself, for nothing but symptomatic language reactions will furnish evidence of the proprioceptive processes required for such an explanation.

Introspection, or symptomatic language behavior, appears thus to give results as trustworthy as those accepted in other descriptive sciences, which themselves, indeed, often rely on introspective evidence. It is most vulnerable in cases where the reactions reported by language alone have no immediate external stimulus. It has furnished data which the behaviorist cannot afford to disregard; which, in fact, he takes for granted. We learn from the highest of all ethical authorities that the peacemaker is blessed, but experience shows that [p. 112] the blessing is seldom bestowed upon him by either of the previously contending parties. So this address will seem to the introspective psychologist a pitifully inadequate defense of introspection, while the behaviorist will pay no heed to it, preferring to cut his garments from new cloth rather than thriftily to salvage what of last year's wardrobe can still be worn. Its writer, however, felt the need of clarifying her own mind on the points which have been discussed, and the audience must console itself with the thought that she, at least, has gained some satisfaction from the process.

Footnotes

[1] PSYCHOL. REV., 1919, **26**, 12-13.

[2] *Phil. Rev.*, 1915, **24**, 212.

[3] He says on page 337, "Since Ebbinghaus's investigation was completed a large amount of such work has been done, but in general his conclusions have been many times verified." The fact is that all later investigators, Meumann, Weber, Radossawljewitsch, Knors, Reuther and Gambe, far from verifying Ebbinghaus's conclusion, established just the opposite result, namely, that the number of repetitions increases less rapidly than this amount of material.

[4] PSYCHOL. REV., 1917, **24**, 301-317.

[5] *Zsch. f. Psychol.*, 1920, **83**, 129-210.

[6] *J. of Exper. Psychol.*, 1921, **4**, 63-76.

[7] *Zsch. f. Psychol.*, 1915, **72**, 182.

[8] *Quarterly Journal of Experimental Physiology*, 1916, **10**, 1-95.

[9] *Amer. J. of Psychol.*, 1908, **19**, 289-344.

[10] *Zsch. f. Psychol.*, 1910, **54**, 241-289; 1911, **58**, 59-140; 1913, **64**, 92-105; 1915, **72**, 1-192.

[11] *Amer. J. of Psychol.*, 1919, **30**, 121-164.

[12] *J. of Phil.*, 1921, **18**, page 92.

[13] 'Integrative Action of the Nervous System,' 1906, 386.

[14] *Brain*, 1918, **41**, 58-253.

[15] *Loc. cit.*

[16] *Brain*, 1908, **31**, 323-450.

[17] *PSYCHOL. REV.*, 1915, **22**, 177-225.

[18] 'Studies in Psychology in honor of E. B. Titchener,' 1917, 181-208.

[19] *PSYCHOL. REV.*, 1917, **24**, 114-138.

[20] *Amer. J. of Psychol.*, 1915, **26**, 313-387.

[21] *Psychol. Monog.*, 1916 (no. 90).

[22] *Psychol. Monog.*, 1921 (no. 130).

[23] *PSYCHOL. REV.*, 1917, **24**, 364-365.

[24] *Amer. J. of Psychol.*, 1917, **28**, 115.

[25] Extracted in Rand's 'The Classical Psychologists' from Brown's 'Treatise on the Philosophy of the Human Mind,' 1827, Part II, Chapter V, Section I. The parallel passage in his 'Lectures on the Philosophy of the Human Mind,' 1824, is 1, 461.

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