

THE PSYCHOLOGICAL REVIEW

A PSYCHOLOGY WITHOUT HEREDITY

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A CONFESSION OF FAITH

In view of the fact that at present there is no generally accepted point of view in psychology, it seems necessary that one should state definitely where he stands in the science before he can proceed to the discussion of a specific psychological problem; for unless there is an agreement on a general viewpoint among those who join in the discussion, there is little hope of settling the question at issue. I feel obliged, therefore, to make a confession of my psychological faith before I take up the problem of inheritance of behavior, which is the main theme of the present paper.

I shall define psychology as *the science which deals with the physiology of bodily mechanisms involved in the organismic adjustment to environment with special emphasis on the functional aspect of the adjustment.* (By functional aspect, I mean the effect, or result, or adjustment-value—positive, negative or indifferent—of a response which establishes a new functional relation of the reacting organism to its environment, social or otherwise.) Psychology adopts the methods of the exact sciences, stressing the supreme importance of objective and quantitative experiments for permanent progress of the science. Its subject-matter—behavior—is solely physical and mechanical events. It denies (*but does not disregard*) the existence of anything mental or subjective; the so-called consciousness, if it exists at all, must be reducible

to physical terms and capable of objective and quantitative treatment when we have better methods and technique than the ones in existence at present; there is nothing unique about consciousness, nor does it need any special explanation (16).

It naturally follows that in any kind of psychological discussion, the laboratory viewpoint should always be kept clearly in mind. Any controversy in psychology must be capable of promoting experimental researches so that the issue can be settled in the laboratory, or it must at least have some particular value for laboratory procedure. Otherwise there is no justification for the existence of any such controversies or problems in the science. It was for this reason that I attempted to repudiate the concept of instinct (6, 7) and it is this same reason which has led me to question the validity and usefulness of the whole concept of heredity in a laboratory psychology.

From the above brief statement—which is, by the way, obviously dogmatic and mechanistic in the extreme and which will certainly shock our metaphysical opponents, notably McDougall—as a confession of my general viewpoint in psychology, one can readily see that my controversy in this paper is with the strict behaviorists alone, for they are the only ones who are likely to agree with me on such a platform. *It is the main thesis of the present paper that in a strictly behavioristic psychology, with its emphasis on laboratory procedure and with its insistence on physiological explanation of behavior, there is practically no room for the concept of heredity.* With McDougall and other vitalists and with the mentalists I have no quarrel. My chief difficulty with this group of writers is purely a metaphysical one; and so long as the philosophy of my psychology is irreconcilable with theirs, I do not think we can get together and discuss profitably the problem of heredity or any other specific problem in psychology.

To repeat my main thesis, I insist that the problem of psychological heredity should be attacked solely from the laboratory standpoint; the concept of heredity in psychology must be a proved or provable fact in the psychological lab-

oratory, or it must at least be a valuable assumption for laboratory procedure; beyond the laboratory viewpoint I confess my inability to discuss the problem.

THE DIFFICULTY OF THE CONCEPT OF HEREDITY IN PSYCHOLOGY

For most of the strict laboratory students of heredity, the problem of psychological inheritance seldom exists. Their primary interest lies in the inheritance of morphological features of the organism; they are interested merely in those facts which can be stated definitely in morphological and physiological terms, so that they can readily bring them into the laboratory for testing; they seldom deal with facts of heredity in the abstract. But there is a group of biologists, notably the eugenicists, who with most of the present-day psychologists insist that there is another kind of heredity, namely, the heredity of responses. It is this last notion of heredity that I wish to question in this paper; I have not the slightest intention of questioning the well-established results of recent Mendelian experiments on heredity, or even the superimposed theories so long as they are not uncritically applied to psychology; *what I am here concerned with is the problem of neuromuscular patterns—the physiomorphological basis of hereditary responses—and the problem of the mechanism of psychological heredity.* Unless they can describe all hereditary responses in physiomorphological terms, the behaviorists are not justified in talking about heredity in psychology at all, although the mentalistic and vitalistic psychologists can talk a great deal about it, because the latter are under no obligation to deal with objective and concrete facts, while the former are required to describe psychological phenomena in objective terms.

I

THE PROBLEM OF THE RELATION OF BEHAVIOR PATTERN TO NEUROMUSCULAR PATTERN

By behavior pattern we mean the integration of separate bodily activities into an organismic adjustment. In phys-

iology—or better, in psychophysiology—we deal with such bodily activities as patterns of a lower order, namely, neuromuscular patterns; but in psychology proper we deal with them as an integrated whole, the organismic adjustment or behavior pattern. In other words, neuromuscular patterns are the material or elements out of which the behavior pattern is built. Thus, every organismic adjustment or behavior pattern can be analyzed into its elements, the neuromuscular patterns, although the properties of the former patterns are not inherent in the latter patterns. Now, the problem which confronts the student of objective psychology in connection with heredity can be stated thus: (1) *Are there any neuromuscular patterns corresponding to the supposed hereditary behavior patterns? And (2) assuming that there are definite neuromuscular patterns corresponding to the hereditary behavior patterns, how are they related to the germ-plasm? I.E. how are they correlated with the germinal organization?*

(1) Answering the first question, the inheritance psychologist of the laboratory type has a twofold task: (A) he must determine whether every behavior pattern has a definite, fixed, and invariable neuromuscular pattern, and if so, then (B) he must determine, locate and demonstrate such neuromuscular patterns. Until this twofold task is accomplished, he can not legitimately talk about heredity in psychology.

A. Recent studies in human and more particularly in animal behavior as well as in physiology have brought about a very definite and conclusive fact, namely, that a behavior pattern has no definite, fixed, and invariable neuromuscular patterns. Variability in the constituent elements of an organismic adjustment is a rule rather than an exception. The same behavior pattern of different individuals, or of the same individual at different times, may be made up of different movements, different receptors, effectors, and adjustors, while the same bodily mechanisms may be involved in different behavior patterns. Curiously enough, this fact has been conceded not only by the instinct deniers but by many instinct defenders as well (4, 9, 12, 13). To show the complete

breakdown of the concept of non-variability of behavior pattern let me quote Dr. Tolman (13, p. 210-202), who has recently come out to defend strongly the concept of instinct. "It (the charge against the non-variability concept) calls attention to the extreme *flexibility* of most actual animal behavior. It asserts that nothing like real *reflex patterns* are to be found anywhere in nature. The solitary wasp, *ammono-philin*, does not sting her caterpillars always in exactly the same degree of resultant paralyzation. Birds do not build their nests by means of a precise and invariable order of movements. Indeed these and countless like observations have given the pure *reflex pattern* theory its final *coup de grâce*." This is a really fatal indictment against the concept of hereditary responses in a type of psychology which always insists that every behavior pattern is analyzable into its physiological segments, and which maintains that nothing but physiomorphological features can be inherited. But for the sake of further discussion, let us waive for the moment this indictment and assume that every behavior pattern does always involve the same definite and fixed bodily mechanisms, so that we can immediately proceed to the consideration of the second task of our heredity psychologist, namely, that of locating and demonstrating the neuromuscular mechanisms for hereditary responses.

B. Though definite physiological correlates with hereditary responses have been assumed by a great many psychologists, no one has taken the trouble even to state, much less to show, what these correlates consist of. Watson, in his chapters on instinct (14, 15), evades the whole problem with a vague definition that instinct is an hereditary pattern of reaction, the separate elements of which are movements principally of the striped muscles, and makes no attempt to show what movements and what striped muscles each instinct involves. When he defines the other hereditary mode of response, the so-called emotion, he does, however, seem to point out some definite sort of physiological mechanisms involved in the response, namely, the visceral and glandular systems. Other psychologists make great use of the concepts

of neural connections and synaptic resistance. According to them the neural connections of the hereditary responses are inborn and unlearned, while those of habits are acquired during lifetime. And, further, the synaptic resistance is low for hereditary responses; while new reactions have high resistance; learning or formation of habit consists, physiologically, in reducing synaptic resistance. Still others will assert that, viewed physiologically, hereditary responses are due to the predispositions of the nervous system. Let us examine carefully each of these various assertions.

(a) *Predisposition of the Nervous System.*—I really do not know just what is meant by such a vague phrase, nor can any one who has made use of this concept assure himself of its real implications. If by predisposition of the nervous system is meant the preformed arrangements of the system, then it is merely another name for the neural connection concept or the reflex pattern concept. On the other hand, if it refers to the readiness of the nervous system to respond to certain stimuli, it should be a very similar concept to that of synaptic resistance. Other than these two concepts, I have not been able to make out just what the expression 'predisposition of the nervous system' implies. We will take up the concepts of neural connections and of synaptic resistance in turn.

(b) *Neural Connections.*—Whenever this concept is used by the heredity psychologist, it always refers to the structural arrangement of the nervous system, the preformed or inborn pathways. It implies two things: first, that inherited pathways are open at birth, or before birth, or can function merely as a result of growth; and second, that learning consists in formation of new pathways. Although the evolution and development of the gross divisions of the nervous system in the embryo are more or less definitely known, modern embryologists have very little to tell us about the differentiation and development of particular reflex arcs in pre-natal life. At present we have no way of telling which paths are present at birth or before birth or merely as a result of growth, and which are formed as a result of learning. All this points

to the highly speculative character of the concept of neural connections in heredity. Aside from the question of actual evidence, there is a certain theoretical difficulty in this conception which has been pointed out by Watson (15, p. 272): "The conclusion is forced upon us that in habit no new elementary movements are needed. There are enough present at birth and more than will be combined into complex unitary acts. Since so many of the psychological texts speak freely of the formation of new pathways in habit it seems well to call attention to the simple mathematical fact that the number of permutations and combinations of, say, one hundred unit acts is a staggering number. Such speculations, though, are futile. One needs only to examine the five or six-day-old infant to be reasonably convinced that there is no need for the formation of additional reflex arcs to account for all later organization."

(c) *Synaptic Resistance*.—First of all it must be pointed out that the concept of synaptic resistance is little more than a hypothesis in neurology. We know very little about the nature of opposition in the hypothetical synaptic membranes. It is very questionable whether we should apply unproved theories of other sciences to psychology. Granted, however, that the theory of synaptic resistance is a well-established fact, we shall still ask: What has the theory to do with hereditary responses? In the first place, the theory implies only that there is a difference in threshold value between responses; high resistance means high threshold value and vice versa. This amounts to nothing more than saying that actions of low threshold can be called forth more readily than those of high threshold. On what ground, then, can we conclude that actions of lower threshold value are inherited responses while those of higher threshold value are not? Action *A* can be called out more readily than action *B* on account of the lower synaptic resistance of the former, so that action *A* is an inherited response. But why not call action *B* an inherited response also, since it is also a possible action and can certainly be called forth when the intensity of the stimulus is sufficiently increased? Secondly, the difference

in synaptic resistance between reflexes is merely a difference in degree. What, then, will be the degree of resistance which marks off an inherited reaction from a non-inherited one? I pray that those who have made great use of the synaptic resistance concept in explaining hereditary reactions may consider this question seriously.

(d) Finally, we come to the consideration of visceral and glandular organs as bases of inherited responses. We have pointed out that Watson has made a rather unsuccessful attempt to locate the so-called emotions in the visceral organs and glands. Other psychologists have recently made a great fetish of the endocrines. Due partly to failure of the reflex pattern theory in instinct and partly to some promising works on the function of the ductless glands, autacoids have become, for many of the present day psychologists, the *moving spirits* of the behaving organism. 'Pep,' 'drive,' 'driving adjustment,' 'determining tendency,' 'instincts,' 'emotions,' 'libido,' 'personality,' and what not, all can be located in the organs governed by the autonomous nervous system. Of course, we all more or less admit the importance of the functions of these organs which influence our behavior. But their importance should not delude us into thinking that they are the physiological basis of 'instincts' and 'emotions.' A little attention to the fact that the same glands may be involved not only in different instincts and emotions but also in a great many other activities, and that all the visceral organs and glands are in activity practically in every moment in our life, will convince one that differentiation between instincts and emotions cannot be made on the basis of these organs. This implies again that the supposed inherited reactions have no definite physiomorphological basis.

In passing, it may be noted that the autacoid substances, important as they are in behavior, act upon the organism as nothing more than intraorganic stimuli; they produce effects on the organism very similar to the effects of drugs. Feeding or injection of certain drugs will produce the same restlessness and uneasiness in the organism as do the internal secretions, and of course, no one will take such drugs as the source of

'instincts' or 'emotions'! Professor Tolman, in his article on the 'Nature of Instinct'(13) and in a recent correspondence with the writer, has pointed out the importance of the results of C. R. Moore's experiments on the testicular and ovarian transplantation (10). [In this connection see also an excellent historical summary by Stone (11).] Tolman says: "Such facts [Moore's results] certainly seem to demand some sort of instinct-hypothesis. The teleological hypothesis, with its allowance of more or less innate reflex patterns as required, would seem to satisfy this demand as well as any other." Professor Tolman does not seem to fully realize one serious implication of this conclusion. It implies that sex instincts can be exchanged between male and female, merely by exchanging the sexual glands. Well, then, what is an instinct? Shall we identify sex instincts with the sex glands? Allen and Doisy (1) injected active substances from the ovarian follicles of hogs and cattle into spayed animals and found that the injection caused typical oestrus changes. "During this artificially induced oestrus animals experience mating instincts, the female at times taking the initiative in the courtship, which culminates in copulation and the formation of a typical vaginal plug." Here we have an actual case of sexual reaction which can be produced by the injection of the active substance of the follicles. Will such a substance demand some sort of instinct hypothesis also? What is the teleology of such a substance? Moreover, the functions and growth of the sexual glands are also related to many other things than sexual reactions. The secretions of the Leydig cells may also produce other types of behavior than sexual intercourse. It is, therefore, physiologically incorrect to locate the sex instinct in the sexual glands. At any rate, personally I am inclined to think that the results of Moore's experiments as well as others tend to weaken rather than strengthen the instinct concept.

All the above discussions point to the urgent need for a psychophysiology. At present, we are more or less completely ignorant of the particular kinds and extent of the physiological apparatus involved in a given adjustment. Such igno-

rance offers the psychologists an opportunity to make use of general and vague theories, such as neural connections, synaptic resistance, internal secretions and what not, to explain general and ill-defined behavior categories. In this connection, I am in hearty agreement with Lashley (8, p. 351) that the chief handicap to the progress of behavior psychology is the lack of an adequate physiology. We need no longer delude ourselves, as did Watson and other behaviorists, into believing that the behaviorist with entire ignorance of physiological processes, can write an adequate description of behavior (15, p. 195). Just as biochemistry is essential to the progress of physiology, so is psychophysiology to the science of behavior. The general knowledge of the structures and functions of physiological apparatus is of little value to the psychologist; we need to know the particular reflex arcs, particular sense organs, muscles, and glands involved in each particular adjustment. We should endeavor to determine physiological facts as definite segments or components of behavior. We ought to have a technique of our own. We must not follow the example of the present-day psychologists, especially the inheritance psychologists, who borrow general and vague concepts from general physiology and merely use them as explanatory theories to conceal our ignorance of the real nature of physiological basis of behavior. We are interested in the actual facts of physiological correlations with particular reactions, rather than in the application of the general physiological concepts in our science. Let us cease to guess at any physiology of behavior until we can demonstrate it in the laboratory.

II

THE PROBLEM OF THE MECHANISM OF PSYCHOLOGICAL INHERITANCE

Even granted that the students of heredity in psychology have fulfilled all our requirements for a definite physiomorphological basis of hereditary responses, we will still have to inquire into the mechanism of such an inheritance. Are the neuromuscular patterns of hereditary responses preformed or

inherent in the germ-plasm? Just how does the germinal organization determine such patterns? Perhaps the problem of the mechanism of heredity has been a sufficiently difficult one even for the biologist. Most of the theories proposed by the biologists to solve the problem are highly speculative. The modified form of the preformation doctrine, namely, the doctrine of genes or the factorial theory, has been rather widely accepted by modern biologists to explain Mendelian heredity. But as a consequence of the acceptance of this theory the problem concerning the patterns and the chemical nature of the genes in correlation with the organismic pattern immediately arises, which problem probably cannot be solved until the much-dreamed of ultra-microscope is invented, which is powerful enough to study the structure, grouping, and behavior of the genes. In this connection it should be borne in mind that the most that both biometry and the method of experimental breeding can do for genetics is to bring out problems in heredity. Neither can solve any hereditary problem. The fundamental problem of heredity is the problem of mechanism. This problem can be solved only by cytology and developmental physiology. But at present these two sciences are not advanced enough to allay all our doubts about the validity of the new preformation theory. In fine, the present state of knowledge of the biologist concerning heredity and the germ-plasm is too meager to be of any use for the psychologist.

The above discussion has revealed the fact that heredity in psychology is not a fact, but merely an assumption. But of what value in the psychological laboratory is such an assumption? Is it not merely a great cloak devised by the psychologists to conceal their ignorance of the origin and development of behavior? At present, even the foundation of the science of psychology itself has not been firmly established; the fields of psychophysiology and developmental psychology have not as yet been touched upon. Why, then, assume so much? Why not go ahead in the laboratory and try to devise ways and means to study the developmental phases of behavior, together with their physiological correlates?

Certainly, it will not be too late for the psychologists to wait at least a few decades before they appeal to such ultra-microscopic gods for help; to wait until the psychophysicologist can locate definitely the neuromuscular patterns of each response and an ultra-microscope for the study of genes has been invented, or to wait until we have exhausted all the possible experimental methods of development of behavior as results of interactions between the organism and stimulations, intraorganic or extraorganic.

The fact is that if we assume heredity as an explanation of behavior we will have to explain, as already pointed out, various difficulties concerning the neuromuscular patterns and the cellular basis of hereditary responses; the explanation itself needs explanation. In brief, it does not explain behavior but simply explains away all problems in behavior in terms of heredity, and the problem of heredity remains.

The time seems to have come when we can no longer tolerate the tyrannic domination of biology in psychology; when we feel that there is need for a clear division of labor between biology and psychology, and that neither one should encroach upon the field of the other. Psychology as an independent science must have a system of its own, together with its own explanatory concepts. The geneticist is concerned with the problem of the origin and development of the organism; while *the psychologist takes the organism as given*, and investigates its adjustment relation with the environment. Behavior is always an interaction between the organism and its environment. Given an organism with its behavior history and given a stimulation, the psychologist has the task of determining the response. He needs the concept of heredity as much or as little as the concept of god. In fact, it makes very little difference to the psychologist whether the ultimate cause of behavior is heredity, nature, god, or soul, since heredity of behavior can never be proved as long as there is no one-to-one correlation in a fixed, definite, and invariable way between neuromuscular patterns and behavior patterns.

III

CONCERNING THE SPECIFIC TYPES OF HEREDITY IN
PSYCHOLOGY

The Problem of Instinct.—It is impossible at this time for me to attempt an extensive historical review of the arguments pro and con of the instinct concept during the last three years. What I wish to do here is (1) to amend some of the statements which I made in my earlier articles (6, 7), and (2) to consider carefully the arguments in favor of instinct of those who have tried to attack the problem more or less from the laboratory point of view.

(a) *An Amendment.*—In my article on 'Giving Up Instincts in Psychology' (6) one of my main arguments against instinct is that all the so-called instincts are in the last analysis acquired responses. Such an argument would imply, in fact it admits, the traditional distinction between inherited and acquired responses, and would create certain difficulty when cases of unlearned responses are cited. Furthermore, such a distinction has led me to admit the inheritance of certain of the elementary responses out of which our complex reaction systems are built, namely, the units of reactions. This amounts to an abandonment of my original contention, for so long as there are inherited reactions, simple as they may be, there is justification for the use of the term instinct, although we may rightly object to its use for more complex responses. My confession is not that I had gone too far, but that I had made too much concession to the instinct psychologists, and had given them an open gap for attack. I gladly take this opportunity to amend my contention as follows: The traditional sharp distinction between inherited and acquired responses should be abolished. All responses must be looked upon as the direct result of stimulation, as interactions between the organism and its environment. We cannot attribute unlearned reactions to heredity, any more than we can so attribute other types of reactions. The units of reactions are no more inherited acts than are the complex habits of the adults. The problem of heredity is not a psy-

chological problem, since inheritance of psychological features can neither be proved nor disproved in the laboratory. This leads us to the considerations of the concepts of universality and non-acquisition as criteria of instincts.

(b) *Universality as Criterion.*—My contention is that universality of reaction is due either to universal organismic pattern or to universal environmental demands or to both. Upright walking is an universal response for the primates because they all possess legs and erect posture. Flight is universal in birds because they all possess wings. Food getting, respiration, rejection of waste products are universal and *inevitable* responses, to use Wells' terms (17), for every and all organisms including plants, *because of universal and inevitable organic demands.* *But does universality prove heredity? Just how is it related to the germ-plasm? What has universality in behavior to do with the germinal organization? Wells (17, 18), while admitting that constant environmental conditions are partly responsible for the universal or inevitable responses, insists that such responses are dependent upon determiners transmitted through the germ-plasm. But what are such determiners and how are they transmitted through the germ-plasm? Are they the very determiners that determine the development of legs and erect posture in the case of upright walking, and wings in the case of flight? If so, it amounts to attributing universal responses to the universal organismic pattern. But of what value is it to distinguish on the basis of the universal organismic pattern between inherited and acquired responses? Furthermore, universality of organismic pattern does not necessarily mean the presence of universal reactions; it merely indicates the possibility of such reactions; whether they will actually appear or not depends entirely on whether or not there are *universal, constant, and inevitable* stimulating conditions. The emphasis is always upon the environment. In a word, while it is admittedly true that there are universal reactions due to fundamental and inevitable needs of the organisms, and due to the universality in their bodily make-up, the organismic pattern, their presence is no evidence for heredity, nor is*

there any justification for making them the basis of distinction between habits and instincts.

(c) *Non-Acquisition as Criterion.*—The fact that there are reactions which can be performed without learning cannot be denied. But here again *does non-acquisition prove heredity?* Is non-acquisition also due to ‘determiners, factors or genes transmitted through the germ-plasm’? Is there any correlation between the germinal organization and the unlearned behavior-pattern? If so, in what way? One may argue that non-acquisition is due to preformed nervous connections determined in the germ-plasm. We may briefly summarize the main points of our argument previously set forth against this contention: (1) There are no fixed and invariable nervous connections corresponding to an unlearned adjustment; (2) there is no embryological evidence to mark off the so-called inherited neural pathways from the acquired pathways; and (3) there is no actual evidence nor theoretical justification for the assertion that unlearned reactions are the result of any particular group of inborn neural pathways and that learning or habit-formation is a matter of establishing new pathways.

My second question concerning non-acquisition as a criterion of instinct is: What is the pragmatic or laboratory value in psychology which can be attributed to such a criterion? Suppose we observe three actions *A*, *B*, and *C*. Action *A* is *effectively* performed on the first trial, action *B* requires two or three trials in order to be *effectively* performed, and action *C* requires on hundred or more trials. On the basis of this criterion we should conclude that action *A* is inherited while actions *B* and *C* are acquired. Now, from this conclusion the danger and invalidity of the criterion can be readily seen. The difference in number of trials between actions *A* and *B* is only one or two trials whereas the difference between actions *B* and *C* is ninety-eight or more trials. And yet, in conformity with the criterion, actions *B* and *C* should be classified together as one type of reactions, the learned type as against the other type of reaction, action *A*, the unlearned type. Why is action *B* more akin to action *C* than

it is to action *A*? This I cannot really understand. My point is that for experimental purposes the distinction between reactions on the basis of non-acquisition is too crude to be of any value. As a matter of fact, actions do not just fall into these two opposite types, they vary in various degrees with respect to the ease, readiness, and rapidity with which they can be effectively performed. And so we should have a scale to measure the degree of relative ease and relative rapidity of acquisition of new reactions; such a scale should range from zero trials to hundreds or thousands of trials. Actions which can be performed without practice are no more instinctive acts than actions which require more than one hundred or one thousand trials; nor are actions with only two or three trials acquired responses any more than are the unlearned reactions. This is not intended as an *argument for the identity of instinct and habit*, but simply a plea for a *finer discrimination of reactions*; not to classify actions into two opposite groups, learned *vs.* unlearned, inherited *vs.* acquired, instinct *vs.* habit; but to have all actions fall in a scale so that the relative degree of ease and rapidity with which new reactions are acquired can be compared and studied.

When the relative degree of ease and rapidity with which new reactions can be effectively performed is compared, the psychologist immediately faces a genuine experimental problem: What are the factors which are responsible for the fact that one action can be acquired without any practice, another with two or three trials, another with ten trials, another with one hundred trials, etc? This is a very important problem in the study of acquisition of new reactions, and should be worked out in the laboratory of developmental psychology. But the concept of instinct with non-acquisition as its criterion has obscured this problem; in fact, such a problem will never appear under the concept of instinct. This is the main reason for my indictment of the view that instinct is a *finished* psychology, and I am still holding to the view which I expressed more than a year ago, that in the laboratory the instinct psychologist ends his investigation where the non-instinct psychologist begins (7).

Tropisms, Reflexes, and Emotions.—While the meaning of tropism and reflex can be definitely understood, the meaning of the so-called emotions is a very mooted question in psychology. For the present purpose, however, we may safely assume that the term emotion is used in psychology to designate a definite type of reaction which is assumed by many psychologists to be inherited through the germ-plasm. Now the chief argument for the inheritance of tropisms, reflexes, and emotions is also based upon the fact that they belong to the unlearned type of reactions. In the discussion of instinct we have presented our reasons for objecting to using non-acquisition as a criterion for determining hereditary responses. The same objections can be applied to the cases of reflex, emotion, and tropism. It is not necessary therefore for us to enter into any detailed argument against the notion that tropisms, reflexes, and emotions are inherited reactions.

The statement is often made by the inheritance psychologists that the organism, due to heredity, is so constituted that it will react tropismically or reflexively or emotionally under appropriate stimulation. Frankly, I have failed to grasp the significance of such a statement. Regarding this statement Kantor, whose chief attack is on emotion, has the following to say: "How informing is this statement? Such a statement is on a par with the assertion that the human individual is born to think, to perceive, to wear clothes, as well as to undergo various other experiences" (5).

Mental Traits.—When the study of heredity of morphological and physiological features began to be in vogue, a great many biologists and psychologists who were laboring under the delusion of the old-fashioned psychophysical parallelism thought that mental qualities must also be inherited and are inherited in the same manner as physical characters. Thus, one modern biologist dogmatically asserts, "There is no longer any question that some kinds of feeble-mindedness, epilepsy and insanity are inherited, and that there is often an hereditary basis for nervous and phlegmatic temperaments, for emotional, judicial, and calculating dispositions. Nor can it be denied that strength or weakness of will, a tendency

to moral obliquity, or rectitude, capacity or incapacity for the highest intellectual pursuits, occur frequently in certain families and appeared to be inherited" (2, p. 71). Working upon the assumption that mental inheritance goes parallel with physical inheritance, many biologists and psychologists, and more recently the mental testers, attempted many investigations of the heritability of individual differences, mental traits and racial characters. By juggling with statistical figures, many of these investigators conclude not only that mental traits are heritable but that many of them, such as insanity, feeble-mindedness, sexual crime, alcoholism and the like are inherited in Mendelian ratios. In this connection we must also mention the works by Yerkes (19) and Coburn (2) upon the heredity of wildness, savageness, and timidity in mice. The methods of these two investigators are relatively superior to those of Galton, Wood, Weeks, Goddard and others, in that while the latter used the historical and questionnaire methods, the former used the method of experimental breeding. But we are not concerned here with the reliability of the methods and of the sources of their information; for, even if we grant that all their information was gathered under strictly controlled conditions, we will still question the validity of their conclusions.

In the first place, are not all the so-called 'mental traits' merely terms for very vague and ill-defined categories of social valuation of the organism's responses? What is insanity, what is sexual crime, feeble-mindedness, etc., and what constitute savageness, etc. in mice? Can any of the 'traits' be reduced to definite physiomorphological facts? It must be remembered that practically all of the so-called mental traits both in men and in animals are social names, each of which includes tens or even hundreds of different reactions, and each of these reactions may be, in turn, included in other categories of mental traits. And, what is more, the same bodily mechanisms of the organism may be used in combination with other mechanisms to produce actions which are found in every category of mental traits. All this points to the impossibility of locating definite physio-

morphological mechanisms of mental traits, and so we will be forced to speak of their heritability in the abstract sense, which sense is totally unacceptable to the objective psychologists.

Secondly, are the mental traits investigated by Galton, Wood, Goddard, Weeks, Rosanoff and others unit characters in the Mendelian sense? The Mendelian experiments deal strictly with definite morphological features, and it is sheer nonsense to speak of mental traits in terms of Mendelian ratio, when such traits are not reducible to definite physiomorphological facts.

SUMMARY AND CONCLUSION

We may now summarize the main points of this paper.

1. The strictly behaviorist viewpoint in psychology is accepted by the writer as his theoretical foundation, upon which the whole discussion of the problem of heredity is based. So far as heredity is concerned, the behaviorist psychology has the following implications:

(a) That the problem of heredity should be considered strictly from the laboratory standpoint;

(b) And that since all facts of behavior are reducible to physiomorphological terms, and since biology does not deal with facts of heredity as abstract things, the behaviorist is required to state (*i.e.* to locate) hereditary responses not in general and vague, but in definite and exact physiomorphological terms.

2. But it is pointed out that behavior patterns do not usually have definite, fixed, and invariable neuromuscular patterns; and further, that the same physiological mechanisms may be employed in combination or in coöperation with other mechanisms to produce many varieties of reactions: the hands, the legs, the eyes, etc. are each and all used in innumerable ways to produce innumerable different reactions. This fact leads us to the conclusion that the neuromuscular patterns of hereditary responses cannot be definitely located in the organism in the same way in which the geneticist locates the color of hair or the shape and size of the eye,

or the length of garden peas. And so long as the same legs, the same hands, the same eyes, and ears, the same neurons, in short, the same physiological apparatus or organs are involved in all or many of the hereditary responses as well as of non-hereditary responses, such apparatus cannot be taken as hereditary units for any given responses, and psychological inheritance becomes a purely metaphysical abstraction.

3. The validity of the general concepts of neural connections, of synaptic resistance, of predisposition of the nervous system, and of internal secretions as applied to the problem of hereditary responses, has been seriously questioned in this paper.

4. The assumption of heredity is a stumbling-block in the way of the progress of experimental psychophysiology and developmental psychology. The inheritance psychologists try to solve the problem of the origin and development of behavior by postulating the existence of hereditary factors of a very mystical and non-experimental nature, and thus substitute a metaphysical problem for the scientific and laboratory problem.

5. The validity of universality and non-acquisition as criteria for measuring instincts and other modes of hereditary responses has also been questioned.

6. The so-called mental traits in men and animals are very vague and ill-defined socialized behavior categories, each of which includes a group of many different reactions. Hence no mental trait has any definite physiomorphological constituents which are not at the same time constituents of other traits.

From the above facts the conclusion is forced upon us that unless we are willing to accept the vitalistic or the mentalistic program in psychology, in which program heredity can be justly dealt with in the abstract, the entire concept of heredity should be dismissed from our science.

At present, we do not need so much the assumption of heredity in psychology as we need experimental technique for the study of psychophysiology and developmental psy-

chology. The success of behaviorism depends to a very great extent upon the success of these two branches of the science. The behaviorists have accepted the concept of heredity from the traditional psychology without realizing that such a concept is a lazy substitute for energetic and painstaking work in experimental psychophysiology and developmental psychology. Indeed, unless we are willing to admit that, judging from the tenor of recent publications, the whole behaviorist movement will amount merely to a rephrasing of the older psychological categories, re-interpretation of consciousness, re-interpretations of sensation, perception, emotion, and the like, unless we want to be content with pure speculations and unprovable assumptions, unless we do not want to build a constructive program of behavior psychology upon the basis of experimental results of psychophysiology and developmental psychology, I earnestly invite the attention of the behaviorists to the serious consequences of the application of those general and vague concepts of general physiology to our science and of the assumption of the heredity of responses.

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