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PSYCHOLOGICAL FACTS AND PSYCHOLOGICAL THEORY*

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A fact has a peculiar and intricate structure. It belongs to two worlds, the world of objects and events, and the world of human discourse. Facts are invisible and inaudible. They can not be burned in furnaces or used to roof houses, or shot from big guns.

Objects and events are not facts; they are merely objects and events. They are not facts until they are described by persons. And it is in the nature of that description that the quintessence of fact lies. Only when an event has been given a very specific kind of description does it become a fact.

When we say, "Let's get down to the facts," what we are saying is much more than that we should look at or listen to or smell or touch real objects, or that we should all observe an event. What we are really proposing is that we all try to find certain statements on which we can all agree. Facts are the basis of human cooperation.

A fact is an event so described that any observer will agree to the description. There are, of course, no facts that meet this too general requirement. We are satisfied—we have established our fact—if any observer within the circle of persons with whom we discuss events will agree. There are always feeble-minded persons, ignorant persons, insane persons, apathetic persons, whom we disregard. There are, therefore, no absolute facts, and a universe without men and human discourse would be a universe without facts.

It may be readily granted that this definition of a fact is no fact itself. There are many men who would hold that facts are just events and objects, and will so continue to assert even after they hear this definition. The definition is made, however, with the hope that certain elite hearers will immediately accept it. They are the audience to whom the definition is addressed.

Psychological facts are events so described that any psychologist

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will accept the description. This description is done perforce by psychologists and for the benefit of other psychologists. Laymen will not necessarily be familiar with the language itself, or with what should be looked at (for, after all, facts are based upon what is seen and heard and are not pure inventions of men.)

Facts are of particular importance to science. Science is founded on an interest in fact. By that I do not mean an interest in nature. I mean an interest in getting down to a factual basis which starts with descriptions acceptable to any observer. This agreement must not be limited to the members of a cult or the partisans of a cause, or the speakers of a language. Sun worshippers may all agree that the Sun God has reappeared with the new day. Members of other cults will not accept his divine attributes. But all men in any language will agree that the circular disc of light is again present.

The agreement that is essential to facts can not depend upon skill or judgment or taste, unless we have assurance that that skill or judgment or taste can readily be acquired with training. The expert tea-taster, the connoisseur of pictures, the skilled diagnostician does not contribute to the advance of science unless he can discover a factual basis for his judgments open to all interested men.

As psychologists we face in the near future the onrush of a torrent of new facts. The number of psychologists has doubled in a few years and if we read the signs correctly is about to double again in an even shorter period. Even the present members of the Association will be contributing to the deluge if we can interpret the fact that during one year approximately three-fourths of us (three thousand of us in round numbers) have changed our addresses and presumably have encountered new persons and problems and new scenes. Industrial psychologists, school psychologists, personnel psychologists, clinical psychologists, will soon be filling the pages of our journals with statements which will, it is to be hoped, include the right proportion of facts. We hope to remain on a factual basis.

But that is not to be taken for granted. A flood of new publications is not automatically a flood of new facts. And it may include many facts which do not contribute materially to the science of psychology. Collections of facts are not science. They are the material out of which science can grow, but they are only the raw material of science, and sometimes they are not even that.

Psychological facts are events described in psychological terms and therefore by and for psychologists. The descriptions which facts require have not been lying about waiting to be noticed. They are the result of hard work and careful and devoted attention. And their value depends

on the insight and good judgment of their collectors. There are useless and misleading facts as well as useful and enlightening facts. There can be a great wastage of paper and of human effort in the publication of facts. Collections of facts for their own sake are of no more value than the collections of old objects.

Some facts are useful in themselves. Medical knowledge includes many facts about the effects of drugs and treatment for which there is no rationale, and as psychology extends into new fields it will welcome many facts about human behavior that cannot be fitted into any theory. The success of applied psychology will depend on the accumulation of much knowledge of this sort. And knowledge of this sort must be taught to students if they are to become practitioners. The open-mindedness of the physician toward facts of this sort which have no bearing on theories has saved the lives of many of us and is the real mark of the physician as distinguished from the scientist. The physician is interested in the cure of his patients. His success depends on his acquaintance with thousands of medical facts. The discovery of penicillin may lead to a saving of human life years that will match the loss of the fifty million lives in the war just finished. But until many researchers have patiently collected the relevant scientific facts that enable them to make scientific generalizations about how penicillin brings about its results, the discovery of its healing effect is not yet a contribution to the sciences of medicine and physiology. It is only a contribution to the tools of the physician.

My own personal bias in psychology is toward an understanding of learning and habit formation. In the two fields of learning and of motivation will be worked out the basic theory that will eventually make the science of psychology a much more powerful instrument than it now is. When we are able to state the general principles which govern human learning we shall have the most important tool needed for the prediction and control of human behavior.

Nothing is more familiar to men than human learning and habit formation. Men have lived intimately with the phenomenon since long before they reached the human state, and they have in myriad ways remarked and described it. But a scientific theory of learning has yet to be agreed upon by psychologists. Such a theory is essential to progress for several reasons. One of these is that unless the beads of fact can be strung in order and pattern on the threads of a theory, there is a strict limitation upon imparting psychological knowledge to others. Theories are mnemonic devices that make science teachable. And theories are the basis of working concepts. They enable men to confront new facts and deal with them successfully. Furthermore, theories are

required to direct the search for relevant facts. It is theories that endure, not facts. Events are ephemeral and their descriptions also may be ephemeral. It is theory that lasts for years or for generations. It is theory rather than fact that leads to new controls over nature and events. From theory inferences can be made and new applications devised. Facts are likely to be local and temporary. Their applications are limited.

A blacksmith may have collected much skill at his trade, and a few facts. He has learned that iron bought from a certain firm has certain good qualities. He can, by watching the color, judge the moment for making his weld. He knows how to temper his steel and how to draw its temper. The science of metallurgy goes in for a very different collection of facts. The science of metallurgy lies in making a different collection of facts. It is not interested in subjective color but in temperature. It substitutes chemical analysis for facts about market source. Market sources are ephemeral and smiths report colors less reliably than thermometers and thermocouples report temperature.

Like metallurgy, a scientific psychology consists in a new orientation toward psychological facts, a weeding out of subjective descriptions and an avoidance of descriptions colored by values and prejudices that are not universally shared. At the present moment our science is entirely too tolerant of such concepts as adjustment, reward and punishment, success and failure. These are all strongly flavored by values that are non factual. The approval of our own group or cult determines what we shall call an adjustment or what we shall call success.

It is my own conviction that in the field of learning the great majority of studies have been collecting unpromising kinds of facts. They have collected facts analogous to the blacksmith's lore concerning how long a particular tempering of his iron will wear upon the horse's hoof, how well pleased his patrons are with his wagon tires, how fatigued he will be with one method of welding as compared with another.

The reason for this is that we have allowed ourselves to be too much influenced by the desire for results of immediate practical application. This has led to the common acceptance by psychologists of a definition of learning in terms of practical value. Most psychologists, when they use the word *learning*, mean the acquisition of socially approved modes of behavior, improvement in performance, in economy of effort and of time in attaining conventional goals. The early writers on learning, Thorndike, Lloyd Morgan, Hobhouse, defined learning in terms of achievement. The animal learns a task set for him by the experimenter. He improves his accomplishment.

This conception, of course, is in good accord with practical common

sense. It is what gets done that is of practical importance, not the response of the person, but the results of that response. But to use practical achievement, goal attainment, success, as the essential criterion of learning, and to turn our search for facts to the observation of success and the conditions under which it is attained is analogous to the use of money value by the chemist as his chief descriptive term in observing a chemical reaction, or the definition by the physicist of work in terms of useful work or valuable work. All the psychologies which are written in terms of "least effort" or of goal achievement are by that choice rejecting the possibility of developing an objective and scientific psychology. They are, of course, following public interest which is turned toward securing quick results in training, or toward the abolishment of obnoxious habits, the acquisition of paying skills. We shall never learn how skills are acquired if we confine our attention to "improvement" in behavior and use as the criterion of learning the elimination of bad behavior and the acquisition of good, or the accomplishment of praiseworthy results. We must understand the processes through which behavior is changed, whether for better or for worse. A clinical psychologist is properly interested in the "cure" of an enuresis. If he is a real psychologist as well as a clinician, he will be interested in just what alteration in behavior was brought about; the fact that the alteration was acceptable to his patient's family may contribute to his income, but not to his science.

The conception of learning in terms of socially valuable outcomes of action led to the collection of learning curves which indicated the reduction of time and of waste motion with practice. It even led to a perversion of Pavlov's conditioning experiments in which as many as 1500 pairings of stimuli are recorded along with the resulting change in certainty or intensity of response. During that series the phenomenon of learning has occurred at each pairing. The massed effect of the 1500 trials may totally obscure or totally miss what happens at each trial. Studies with the maze, the puzzle box, the acquisition of skills, all record some end-result, but do not collect facts involving the animal itself.

The literature of mental tests had over twenty years ago collected some ten thousand titles and the number must be at least three times that figure by now. For the most part the testers have limited their collection of facts to the marks put on paper by persons being tested, and to the association of these marks with some criterion. They have not examined the behavior of the child taking the test, nor has this enormous literature advanced our understanding of what goes on in a child who marks the third of four possible choices. In other words, the

testing movement has been absorbed in highly useful and practical work, but it has not contributed to psychological theory. It has not advanced our knowledge of how the child's mind works.

In the same way studies in learning have been dominated by practical considerations and their facts collected center in practical outcomes of behavior rather than in the behavior process itself. We must undertake to examine the nature of changes in behavior before we shall have a proper understanding of success and failure.

My first suggestion for directing our attention toward facts that will lead to the development of good theory applies chiefly to the field of learning. It is that we look for facts in the behavior of the organism rather than in the operation of a latch, an arrival at a goal, the "learning" of a lesson. We should transfer our interest from the goal achievement to the behaving organism. It is the muscles of the organism that are innervated, and not the lever of the problem box. The machinery through which solutions are arrived at is contained within the skin of the solver.

May I illustrate what here is meant. Studies of maze learning have kept records of the time and number of errors required on successive trials to get the animal to a particular area. Learning curves have been plotted and learning assumed to be a direct function of the number of trials. Practically no experimenter has taken account of the fact that each animal may radically alter its behavior on successive trials, or that the alteration may have been evident only between the eleventh and the twelfth trials and exhibited no curve at all. The curve is only the resultant of many cumulative learnings, which may have included a number of "unlearnings" as well. The picture of learning as a function of the number of trials may be totally altered when we examine behavior at each choice point separately.

Dr. George P. Horton and I occupied ourselves two pre-war winters in observing and recording some eight hundred escapes of cats from a puzzle box. One startling result of an examination of our photographic records of the posture of the cat at the moment of release is the discovery that a series of escapes often displays a highly routinized pattern and stereotyped posture which appears at widely separated points in the series. Here is an elaborate series of movements extending over a period of many seconds or even minutes which has not disappeared from the cat's repertoire, although it has not been in evidence in the cat's behavior for many trials. It was not unlearned or forgotten, as is proved by its accurate reproduction.

If we had contented ourselves with a record of the time required to escape, we should have missed the real nature of the learning process.

So far as we can judge, improvement in the sense of time reduction consisted in the gradual elimination of movement routines that left the cat in the box. The successful act itself always appeared suddenly, either in the very first trial or in some subsequent trial. It required no long series of repetitions for its establishment.

It has been suggested that it will be profitable to give more attention to the behaving organism if we are to understand learning. There is a second admonition which might well be taken seriously. This is that we may profitably give more attention to stimuli as the occasions for response. No psychologist has seriously challenged the conception that the normal occasion for muscular contraction, and hence for all that an animal does, is the activation of sensory receptors. There are psychologists, however, who believe that the stimulus-response formula has had its day. For myself, I do not believe that it has been yet properly exploited. It requires that, if we are studying learning, we observe the response actually following stimulation. Many recent experimenters have, instead, followed Pavlov and observed not the sequence of stimulus-response but the conjunction of two stimuli like bell and food, or buzzer and shock, and have not believed it necessary to notice what actual response followed the signal.

Psychologists who think in terms of punishment and reward have almost uniformly neglected to note how the animal at the time responded to the punishment or to the reward, and the role this played in subsequent behavior. The resulting generalization is inevitably an attempt to link the intentions of the experimenter (intentions to reward or punish) with good or bad behavior on the part of the animal. Punishment and reward are, objectively viewed, stimuli acting on the animal's sense organs, and their effect must be mediated through the animal's nervous system and appear in muscular contraction or glandular secretion. Since levers and loops and mazes are not innervated, the operations of these devices are incidental to the actual learning which the living animal performs.

This failure to examine facts in the field of stimulus-response sequence is, of course, a tradition of psychology. Lloyd Morgan, Hobhouse, Thorndike, responsible for our first careful observations of learning, all were interested primarily in success rather than in response, and all speak in terms of "confirming results." It has occurred to none of them to regard these confirming results as possible stimuli, followed by possible response. Hull, who has endeavored to make the concept of reward over into something much more objective and immediate, so far as I can understand leaves the determination of what it is that will serve to confirm or reinforce quite vague. I believe it will be very

profitable to examine his reinforcements as possible stimuli with close attention to their subsequent responses.

There are many fields in psychology in which the injunction to note and formulate facts concerning the stimulus-response sequence might well be followed. Freud, who makes associative learning the foundation of his whole system, has at no point even asked what it is that is associated, or under what circumstances association is effective. A number of psychologists have in recent years insisted in interposing an O for "organism" between S for "stimulus" and R for "response." There can be no objection to this, provided we make a vigorous effort to determine the classes of fact that we agree to include in this O for "organism" and are not content to leave it as O. There can be no doubt that what is intended to be included in this O is often reducible to a dependence of response on interoceptive and proprioceptive stimuli, and O is a symbol for groups of relevant facts that should be noted and recorded rather than given up.

There is another more legitimate excuse for O. Very properly classed as characters of the organism affecting the stimulus-response sequence are the facts of past learning which can be known only through the record of past behavior. We have as yet no way of noting the brain changes that we assume to be the actualities responsible for changed response to stimulation. There are also legitimately included under O the determiners of behavior sought out by tests, which may be interpreted as behavior samples and assumed to be prognostic of response for varying periods of time. O may also include the material being offered through the more objective methods of examination and interview, and, by inference, the information furnished through the history of the individual.

In all these classes of fact it is of first importance to remember that facts are events so described that any competent observer will accept the description. We should recall that acceptance within limited groups, like the staff of a hospital working under an aggressive leader, or any department under an aggressive chief, may exhibit acceptance on grounds that do not insure that intelligent and informed outsiders will be able to agree to the asserted facts. Stimuli and movements are relatively objective and the agreement necessary to the establishment of fact is relatively easy to obtain. Attitudes and the meaning of behavior are less objective and more likely to produce disagreement among observers. Only on a factual level can the foundations of science be laid. Progress toward scientific psychology must be founded on agreed facts and public facts. The psychoanalysts' interpretations of dreams and of

motivation in general are notably remote from the factual basis that must precede the development of a scientific psychology in that field. But that a factual basis is not unattainable in a field so remote from basic theory as psychotherapy is established by Carl Rogers' recent book, *Counseling and Psychotherapy*, in that he has succeeded in reducing his account to descriptions of events which should prove to be acceptable to psychologists of varied interests and varied theoretical background. This he has even achieved in a number of his quantitative generalizations. He illustrates the type of fact collection which, though made with a highly practical aim, may furnish a basis for the theory that must eventually be developed in order to give system and order to our facts. That theory will extend far beyond psychotherapy into many fields of psychology.

My first suggestion concerning the factual basis for learning theory was that we give more attention to the organism itself, and that we recognize that such classes of fact as improvement, success and failure, reward and punishment, are external and incidental features of learning. The mechanism of learning is within the organism. These external features should be examined only in their role as stimuli to sense organs.

My second suggestion was that the promising factual field for observation is the stimulus-response sequence, and that we should meticulously note such sequences. My third suggestion was that part of what some writers insert in that stimulus-response formula, namely the organism, can, with diligence, be examined in terms of interoceptive and proprioceptive stimuli, often observable and often inferable (as in the case of so-called drives like hunger). Much of the rest of O names facts of the organism's past history, from which we infer changed tendencies to reaction. Such of O as is left over in the form of attitudes we must endeavor to place on a basis of public fact and seek for descriptions which are acceptable to all psychologists.

There is a further admonition. This is that we should undertake more consistently and thoroughly to note what I may call response-stimulus sequences, the stimulus changes following upon the responses of the organism. I have already expressed the opinion that learning and motivation represent the two fields most fundamental to an understanding of behavior and thought. Through close attention to stimulus-response sequences we may formulate the rules of learning, the circumstances under which such sequences change. Through close attention to response-stimulus sequences we may solve many of the problems of motivation and the direction of learning.

It is through observation of the effects of response on stimulation

that we may avoid those vague references to drive and motive that have done so much to obscure the understanding of behavior. A tense bladder through reflex paths operates to relax a sphincter muscle, but that relaxation is inhibited through associative learning by numerous situations. When these associative cues are removed or facilitating associative cues are added, the act occurs. The original stimulation is removed. The incident, save for its effects on future behavior through associative learning is for the time being closed. To invent a drive to explain this act is unnecessary as soon as we are familiar with the stimulus-response antecedents. To allow the disappearance of the restlessness that follows sphincter relaxation to force us to speak in terms of a drive that has attained its goal and is now satisfied is unnecessary when we observe the effects of the response on the new stimulus situation, the R-S sequence.

Every response alters the stimulus situation of an animal. Some responses remove the persistent and insistent stimulus that has been responsible for general activation as well as specific action tendencies. Such responses have a profound effect on the behavior following and on the mode of response that will be acquired by the animal through training.

Other responses leave the stimulus goad in action and the effect is to bring new goads into play. In fact the whole direction of behavior is set by the effects of responses on stimuli. The advocates of the law of effect (Thorndike) or the law of reinforcement (Hull) state the foregoing sentence differently. Their version would be: The whole direction of behavior is set by the effects of responses. You will recall that the version here suggested is: The whole direction of behavior is set by the effects of responses on stimuli. Punishment and reward have no effect on behavior as mere rewarders or reinforcers, but only in so far as they stimulate new behavior. We learn to do what punishment and rewards make us do. We do not necessarily learn to do what was rewarded or learn to abstain from what was punished.

In stimulus-responses there is to be found the key to associative learning. In response-stimulus sequences we may discover the motivation and direction of behavior. That we learn is insured by S-R. Stimulus patterns active at the time a response is initiated become inciters of that response. Because inciters of rival responses may also be active, the response does not always occur; but what effect such stimulus patterns contribute is toward the production of the response with which they were last associated.

That we learn is insured by the association of a stimulus with a re-

sponse. Whether that learning is retained depends on what then follows. It depends on the effect of the response on the new stimulus situation. May I illustrate this with an anecdote of animal learning. The anecdote is not factual in that it describes an event witnessed by only one psychologist and he would be too humane to repeat it. But its analogue is very familiar in the puzzle box behavior which George Horton and I have extensively photographed. The anecdote is this: The psychologist in question has a cat which on entering the kitchen before mealtime limps with a very noticeable limp. This limp is not observed in the cat at other times. Its history is that the cat on one occasion entering the kitchen at mealtime had its foot pinched in the swinging door. The cat made a terrific outcry and continued to limp about and put forth noise. After a quick examination to assure himself that no bones were broken, the psychologist offered the cat its dinner which had been standing ready. Why does the cat persist in limping on later visits to the kitchen?

Horton and I found that every cat we dealt with, between fifty and sixty in all, exhibited very similar behavior. When escape from the puzzle box followed almost any behavior, colliding with the release, pawing it, backing into it, jumping to the top of the box and falling on the release, lying down and inadvertently rolling to contact with the release, heavy odds could be placed that the same movement would be repeated soon after the cat was returned to the box. None of these behaviors had a learning curve. Each appeared suddenly full blown. In only a few cases could anything like an improvement of the successful act be recognized.

It is our belief that this characteristic of learning is explainable in terms of the effect of the response in question on the stimulus situation. Responses which left the cat in the box tended to disappear from its behavior, though in some cases they were very persistent. But the response which opened the escape door was generally preserved. We suggest that this is explainable through the fact that escape removes the cat from the puzzle box but does not allow a new response to be associated with the stimulus situations within the box. The cat has no way to forget. R remains faithful to its association with S because unfaithfulness would require that some rival response become associated with S, but S is now out of the picture. No new associations can be established with an absent stimulus situation.

It is my contention here that we shall gain much new light on behavior if we devote ourselves more zealously to observing the effects of response on stimulation. Every response must have such effects.

Through movement an animal changes its view, the sound pattern affecting its ears, its own pattern of proprioceptive stimulation from muscles and joints. In this radical change which results from action lies the explanation of the direction of our learning.

Our own responses not only bring about changes in the external world. They furnish cues for our further action. They eliminate, or sustain, or produce stimuli to action. And the consequences of this elimination, sustaining, or production are far-reaching.

My dog reaches out and paws my foot as I sit reading. To get my attention, the ordinary observer would say. Of course, the dog does it to get my attention. This is not a fact, however, but an interpretation. Its factual basis is that the dog makes movements or takes a posture that was in the past formed and originated by my attention. Without this factual basis we are speaking on the level of Little Red Riding Hood who is satisfied by the wolf's explanation that his great ears are the better to hear her with.

What would we find the explanation of the dog's gesture if we were to follow the rules that are here suggested and note the history of the event in terms of stimulus-response sequences and response-stimulus sequences? I reject immediately those softer accounts in terms of insight. There must have been a first use of the gesture, and I do not for one moment believe that dogs come into this world equipped with so strange a power for getting results. My own notion of what has happened is that first, I have on a number of occasions scratched the dog behind the ear. The effect of this on the dog is to interrupt all other activities and keep him motionless in place. Common speech uses the word enjoyment; but we might try to stick to psychological facts. The stimulation of my scratching is an essential element of the dog's response. It is what serves to maintain his quiet pose. When my scratching stops, the dog is released, but the sight of me, and the nature of his own response to me serve to call out in him a repetition or prolongation of his "behavior of being scratched." When scratching stops, he is no longer kept quiet by the scratching and is free to move. Whatever movement takes place will be within the limits of his present stance. I do not expect sudden barking or violent action. He has been standing quietly. He may move his head. If his movement had attracted my attention and brought a resumption of the scratching, I should expect that on the next occasion on which the stimulus situation was substantially the same, there would be a repetition of the head movement. This response of the dog's would not be unlearned, because the stimulus situation (waiting unscratched) which had become its cue with one repetition

is gone with the recurrence of the scratching. It is more or less of an accident that his original head movement was unrewarded, and that reward was reserved for a movement of his paw.

I have used the word "reward." That is not a psychological word. My scratching was not effective because it was a reward. It was effective because it prevented the dog from unlearning his gesture with his paw. If I had, instead, cuffed him hard, the cuff would have undone the gesture with the paw as a habit, not because the cuff is a punishment but, in psychological terms, because a hard blow would have established in the dog a tendency to back away from me and there would have been no recurrence of the situation that led to the gesture with the paw.

Once we have let ourselves in for scratching a dog's ears, there is no natural end to the incident save our own fatigue. The dog's response, his quiet posture permitting the scratching, can be continued indefinitely and will be interrupted only by eventual fatigue or an adventitious external event. But there are other actions that are self-terminating. Descending a stair cannot go on indefinitely even though the first steps have established associative serial connections, because one of the peculiarities of staircases is that they have a bottom step and it would be only with the help of a miraculous steamshovel and a corps of engineers that we could be kept provided with steps to descend. If we raise an arm, the arm is now raised and the situation radically changed. The arm can not be again raised until it has first been lowered.

This introduces a new admonition for our fact-collecting. Not only should we note S-R sequences. We should further note that any R in progress, whether that response is active movement or merely the maintenance of a posture, sets strict limits on what can next be done. At any moment there are severe restrictions on the behavior possible to elicit, no matter what new stimuli are offered.

We recall in this connection the work of Magnus on postural reflexes. When a cat has been decerebrated and is stood upon a surface, slight manipulation of its head can result in alterations of its whole muscular set. If the head is turned slightly to the right, the right fore-leg is flexed, the left extended, and the whole posture is made an obvious set for moving to the right. Older members of the profession can recall taking advantage of this postural adjustment by using leathern straps attached to the head of one of the larger animals in order to induce locomotion toward the side on which the rein was pulled.

It is probable that an intact cat in the posture appropriate to taking off toward the right, can not be directly stimulated to take off toward the left. The original posture must be first relinquished and a second taken

on. A whole field here calls for research. What movements are possible from a given stance? What responses are elicitable when a person is maintaining a given attitude? I would here include not only obvious physical attitudes but the more covert states which limit behavior. We must not give up the investigation of attention and readiness.

What are the effects on a going action of sudden irrelevant stimuli? I confess that I do not even know the answer to so simple a problem as what a diner in a restaurant, just raising his cup of coffee to his lips, will do if a police whistle is suddenly shrilled just behind his head. Will the movement in process be suddenly energized and the cup thrown over his shoulder, or will the cup be dropped from his fingers? We should know enough about the rudiments of behavior to answer such questions without waiting for some drunken guest to conduct the experiment for us.

Certain recent experiments in conditioning applied a signal under circumstances which allowed the animal either to have the leg flexed or to have it extended at the time the signal was given. There is small wonder that the results were ambiguous, since response was bound to be ambiguous. Extension is impossible when the leg is already extended. Flexion can occur as an active response only when the leg is not already flexed. This reminds us also of the original admonition to note the facts of stimulus-response sequence. When a signal is alternately or at random presented during extension and flexion or during running and during cowering, but no record is kept of the response following the signal, we should not expect to find any definite generalization in our returns.

Horton and I had a very considerable amount of fact-trouble in making our observations of our cats. Though we made notes and in a number of cases a motion picture record, there was often doubt whether or not a sequence of movements of the cat in the box could be reported as substantially the same as a previous sequence. The statement just made that the major determiner of the animal's actions is the present state of action or rest is an interpretation rather than a fact. It is an interpretation to which we found ourselves compelled; but it is not an interpretation to which we could be sure other psychologists would be forced. A large part of the time we could tell at any moment what this particular cat would do next. Our ability to do so was based on having seen it execute the same routine in a previous trial or earlier in the current trial. Having started any former routine, we could predict its continuance. It is possible that this prediction should have been undertaken, to compare with prediction on any other basis. This seemed, however, a bit absurd, since we knew no other basis on which to base

predictions. The behavior of other cats allowed such prediction only in the most general terms, and did not apply to specific movement series.

The response-stimulus sequence to which I have been referring includes, of course, the familiar concept of "set." What I am urging is that the development of basic psychological theory demands an extension of our collection of facts in this class. We should extend radically our knowledge of sets and their consequences, and we should do that by observing what responses are elicitable from a given stance or set and what are not. Social psychologists are collecting facts about attitudes and their patterns, particularly those organized about words. This must eventually be reinforced by further theory of the elementary behaviors out of which attitudes are made. The watchful therapist is full aware of attitudes in his patient. The skilled mental tester learns to direct a child's behavior into attitudes that permit testing. We must eventually know more of the facts of these attitudes. What is the factual description of negativism or of resistance? To what extent are such attitudes general in their effects and to what extent specific? About what cues are they organized? What occurs when an attitude of resistance changes to one of acceptance? How do prevailing attitudes control attention and perception and learning? In other words, what effect does our own behavior have on our own behavior?

I am reminded here of an extremely interesting paper of Heidbreder's on cognition recently published. In that she reports the results of some experiments which establish a certain hierarchy among concepts. Confronted with three sets of material in one of which the cognition is of an object, in another of a common form, and in the third, of a common number, the three are arrived at by subjects uniformly in that order—object, form, number. My own interest would be in the process through which cognition is attained. That process probably includes "trial and error" naming, and is not hopelessly un-get-at-able. My point is that cognition is not a mystery which involves a hidden thought process and a mental concept for which eventually a word is found, but is a case in which the subject uses his verbal repertoire in trial and error fashion until the word that "works" is hit upon. Proper search may discover that it is the careful noting of stimulus-response sequences that will furnish the factual basis for a theory of cognition. The cognition of objects may prove to be more ready than cognition of form or number because physical objects have a way of offering stable and recurrent, dependable, patterns of stimuli. Cognition of number may prove to be dependent on the initiation of counting which may be injected early in the process by an associative hint, or wait until trials

of object names prove futile. Concepts will turn out to be language in use, and use will turn out to follow the changes of learning by the same laws that will be worked out for action.

Up to this point a number of suggestions have been made for the direction of our search for psychological facts, for the ultimate purpose of understanding learning. One of these was that we have tended to neglect the behaving organism and to give undue attention to the external results of movement on the outer world. A second was that we would do well to recall that stimuli are the normal occasions for all response. A third was that we should note carefully the sequence of stimulus and response if we hope to get at the basic principles of associative learning. A fourth was that our interest in the role played by the organism in determining the response should be responsible for stern efforts to develop an objective, factual basis for our descriptions of the states of the organism that enter into the determination of behavior. A fifth suggestion was that closer attention to the response-stimulus sequence would be profitable in explaining motivation and the direction of learning.

My final concern is harder to name than these. I am in entire sympathy with the belief that quantitative treatment is to be aimed at in all scientific fact gathering. Number is the chief tool of science. But in our zeal to be scientific, I am convinced that we have been led into certain lines of experiment in the field of learning because these lines promised at least to yield numerical comparisons, curves. Because repeated trials can be given a series of ordinal numbers we have too readily fallen into the practice of treating the number of trials as a quantity, the more trials the better. We have been led to neglect what I am convinced is the central problem of learning, namely, what change occurs in behavior as the result of a single action.

In the laboratory we glory in experiments with fifty to fifteen hundred repetitions and their resulting curves. In nature these repetitions, as exactly duplicated as possible, simply do not occur. But learning does occur. The experimental results with a long series of repetitions have all the desirable characteristics of scientific fact. Their numerical analysis can be made by agreed methods. We must all recognize a mean and a standard deviation, or a difference and the standard deviation of a difference. And we are fairly agreed on the inferences that can be drawn from such analysis.

In the field of learning this very commendable effort to be scientific has led us toward studies of success, the trend of errors with repetition,

the reduction of time with practice. But it is a characteristic of a score of total errors (in a maze, for instance) to omit examination of the successive changes that constitute learning. Indefinite amounts of unobserved learning may enter into our final result. Before our criterion of success may be reached, learning, unlearning, relearning may have occurred over and over again.

We expect our friends to remember an engagement after one notice. We expect clinic patients to be different at each interview because of the last. We expect one quarrel to change attitudes. We expect one reading of a paper before this Association to leave some auditors with an impression. One spoiled egg may leave us for a time cautious. Once a rat has visited our grain sack we can plan on its return. Terror is called out once in the bird dog by the report of his master's gun, and the dog is now gun-shy.

But in the laboratory we assume that the response fixed in fifty trials was one fiftieth fixed in each trial.

Repetition has its place in learning, but repetition is effective only in those complicated instances in which what is learned is not a response to a stimulus, but a whole repertoire of responses to a large variety of stimuli. We have learned to achieve some result by means which vary according to the circumstances. Learning skills takes time and practice and furnishes beautiful learning curves and admirable data for statistical analysis. This is because they involve many and complicated learnings. It is here being suggested that the development of a scientific psychology requires that we investigate learning in its simplest forms. What happens as the result of one pairing of a stimulus pattern with a response that alters the previous effect of that pattern?

No group of psychologists has done more toward investigating this phenomenon in its elemental form than the Yale group under the inspiration of Clark Hull. With that work I have only one quarrel. This is that they have not examined adequately the R-S sequences which I have mentioned. Hull's theory, which has dominated the collection of facts in the Yale laboratory, is the theory of reinforcement, not a straight associationism. It assumes that an association is formed, or is not formed by virtue of a subsequent reinforcement or reward which somehow works upon traces of the S-R event and confirms or destroys the associative connection.

This theory is in line with the great tradition of the psychology of learning. Thorndike in his *Animal Intelligence*, C. Lloyd Morgan in his book of the same title, and Hobhouse in his *Mind in Evolution* all

speak in terms of a confirming reaction, which determines whether or not the association will be made.

No one questions the effectiveness of reward and punishment, or the effect of after-effects of a reaction on learning. But this statement of learning theory has led to an entire neglect of the observation of R-S. The confirmation or reward or punishment is supposed to have its effect by virtue of simply being confirmation or reward or punishment, not by virtue of the effect which it has on the stimulus situation and therefore on subsequent behavior and the opportunities for further learning. There is excellent reason for believing that both reward and punishment are effective by virtue of what they make the animal do, not simply by virtue of their own nature. Adherence to the theories of confirmation or reinforcement has led to quantitative results, it is true. It is highly probable that close examination of the action caused by punishment and the action caused by reward will discover that the learning which takes place can be adequately described in terms of the new associations set up by the new action. Reward, as Thorndike has remarked, tends to leave the animal doing the same thing in the same situation,—eating while food is present. Punishment induces the animal to do something different in the same situation. A theory of associative learning in its straight form without appeal to after-effects would lead us to predict in these instances what happens. The animal does not unlearn its tendency to do what it previously did if rewarded because nothing has happened to establish rival responses to the situation. It does not learn not to eat when the food is finally presented although it does eventually desist, because either the food or the inner hunger is now absent and cannot be re-conditioned in their absence.

Culler's laboratory, like Hull's, has led in the investigation of relevant facts on learning in its elemental form. Some of that work I should like to see repeated with closer adherence to the S-R prescription. Stimuli are applied without observing what the animal's actual next behavior is. In a number of instances two rival responses, flexion and extension, take place following the signal. That the result of this mixed practice turns out to be not a straight exemplification of association is not to be wondered at.

In general the experimenters who work with what has come to be called instrumental conditioning also fail to observe the maxim to observe the response following the stimulus. It would seem obvious that an investigation of association would require in the first place that the stimulus and the following response be at least made a matter

of record, but experiments in instrumental conditioning seldom record what the animal was doing when the signal was given. The returns therefore throw no light on association, but only on the effects of reward on subsequent response to the signal. In these experiments not only does the S-R fail to be observed, but also the sequence, R-S′. None of the experimenters is interested in the immediate behavioral consequences of reward, but only in the remote effects of reward upon a previous stimulus.

May I here recall the initial theme of this paper. It has been concerned with the future development of psychology as a science and particularly with the possible effects of a sudden increase in the numbers of psychologists and a sudden enormous extension of the application of psychology to practical affairs. None of us doubts that human living will be improved by that extension. Most of us would accept that improvement as the final goal and justification of all human science. But we must remember that the sciences have developed through an objective detachment from immediate profit, and that, in the overwhelming majority of instances, steps forward in scientific theory have been independent of practical application.

The hope that is here being expressed is that the new psychologists will in general not allow themselves to become mere technicians using psychological methods and techniques for the accomplishment of practical ends, that in the training of the new generation of psychologists we take care to cultivate an interest in theory as well as practice. We are entering a period of increased usefulness. It is to be hoped that it will not be a period in which theory stands still. Our factual information is bound to increase at a greatly accelerated rate. For that increase to result in the advance of psychology as a science two things are necessary. One is that theory be continuously produced and continuously amended and continuously used to guide the collection of fact. The other is that we remember to conform to the rules that have been responsible for the remarkable achievement of the scientific tradition, the use of objective evidence which means a basis in facts open to the observation of all who are interested and described in public terms that must be accepted by other scientists. These requirements may bear heavily on many current movements in psychology, in which recognition of events is claimed to be an art not communicable by ordinary means, open only to the inner members of a cult and closed to outsiders. Facts may accumulate without theory; but they will prove to be unstable and of little profit in the end. Theories may flourish if their basis lies not in scientific fact

but in opinions and interpretations acceptable only to the members of a limited faction; but they will be bad theories. Schools flourish only when theories are not carried back to public facts. Unless psychologists maintain an interest in general theory the fields of psychology will increasingly become independent collections of undigested information.