HOW DO ANIMALS LEARN? SOME PRESENT PROBLEMS

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Learning has become one of the major interests of experimental psychology, and the experiments on learning in animals have forced a wide departure from conceptions and methods based on the older associationism. Associationism displaced faculty psychology, which proceeded by means of "word magic"—naming some process and assuming that the name is in itself an explanation. Thus "memory" satisfied as an explanation of the experiences called by that name; "reasoning" or "creative imagination" accounted for man's reconstruction of his environment or his creative and inventive work, etc., just as "gravitation" is popularly used as an explanation of why bodies on the earth tend to fall toward its center. But associationism, which displaced this faculty psychology, was itself only a logical construct, based on introspection and the vaguest hypotheses as to neural activity, but it was analytic and was so convincing to theorists as even in earlier experimental days to escape rigid experimental tests. It is true that Miss Calkins (1894) devised a well controlled experiment to verify the theoretically constructed laws of association. She employed, however, only given series of nonsense syllables to be memorized in the fixed order of their exposure.

Experiments with animals while solving real problems, present, nowadays, a very different problem. From such experiments, especially since it became evident that mind cannot be assumed to be an original force directing the process, it soon became clear that both the motivation or "drive" to activity and the directing factors were actually physical or chemical stimuli—not desire, purpose, will, perception of results, etc. Some psychologists, notably Thorndike (1911), have adhered rather closely to association principles as explanations of learning, and have continued to invoke the old pleasure-pain view to help them out of the numerous difficulties in which they became entangled. They have had much opposition to their assumptions that pain (or discomfort) corrects 'wrong' acts (*i. e.*, inefficient acts with respect to the reaching of any objective in a problem situation) and that pleasure (or satisfaction) facilitates the successful acts in the excessive activity aroused by any obstruction to behavior. Apparently they follow here a view, well stated in the terminology of his time, by Hartley (1749): "But it appears . . . that God has so formed the world, and perhaps (with reverence be it spoken) was obliged by His moral perfections so to form it, as that virtue must have amiable and pleasing ideas affixed to it; vice odious ones" (*Observations on Man*, etc., p. 504). Thus to attribute the operation of these subjective qualities to something innate is of course not to give a scientific explanation. There are actually many situations in which the painful act is biologically the more life-enhancing

There is no denying that associative factors are operative in fixing certain responses, and that frequency and recency (both of stimulus and response) play important roles in learning series of acts controlled by the experimenter. Thus Thorndike (1911, p. 48) has shown that if a cat in a problem box is regularly let out when, and only when, it chances to lick itself, it learns to perform this act in a relatively short time. The conception of "trial and error" procedure in the learning of a series of acts not rigidly controlled came from Lloyd Morgan, but was well worked out in Thorndike's early experimentation on animal learning.

Jennings (1906) in his *Behavior of the Lower Organisms* offered for the explanation of such learning the so-called law that *"The resolution of one physiological state into another becomes easier and more rapid after it has taken place a number of times"* (p. 291). This statement has been effectively criticised by Thorndike (1911, 268 ff.). It is, in fact, not correct; for trial and error learning processes invariably involve lopping off of irrelevant or circuitous responses, often described by the word 'errors.' The series of original responses does *not* become fixed by repetition of the acts, but numerous ineffective elements drop out as learning advances. Exact repetition in such learning is therefore impossible, and Jennings' law is contrary to fact.

Stephenson Smith attempted to explain learning strictly on the basis of "the laws of chance and habit. The law of habit is that when any action is performed a number of times under certain conditions, it becomes under these conditions more and more easily performed" (1908, pp. 503f). The essence of his explanation is this: Let us suppose that the appropriate response in any situation is x, and that in any number, n, of possible responses, none but x will succeed but will have to be followed by other acts until x occurs. Whenever the reaction x chances to occur, no other one of these n reactions will follow in that series, because x was appropriate and ended the series. Now, since x *must* occur in every series of trials and will end the series, no other reaction will, in general, occur as often as x; and therefore this reaction, x, will survive over all the others as the series gradually shortens in successive repetitions or trials. Thorndike three years later (1911, 270 ff.) shattered the hypothesis by pointing out that on the basis of Smith's main assumption (the law of habit), *any* act that by chance occurs first will have the greatest tendency of all the acts possible to occur again. He concluded his criticism in these words: "The law of effect is primary, irreducible to the law of exercise" (p. 272). But his law of effect is based on the assumed efficacy of satisfaction and discomfort (p. 244).

In the summer of 1916 the author carried out in the University of Chicago the first objective, well controlled experiment devised to test the explanations of learning as formulated by the associationists. The experiment had been planned during the author's first year of teaching in the University of Minnesota. The subjects used were white rats, which evidently were in no way governed in their behavior by any presuppositions. Their problem was simply to get to the food box in the mazes used. It was, of course, impossible to see or measure the pleasure or satisfaction of these animals, and even if it be granted that they had satisfaction on reaching the food box and eating, it would still be very puzzling, as Thorndike pointed out in 1898, how pleasure could fix the correct ones among several more or less random acts performed in solving a problem, since "*There is no pleasure along with the association. The pleasure does not come until after the association is done and gone*" (1911, p. 148). The undesirability of assuming such subjective factors in scientific work is obvious. Twenty-four white rats were used in the first experiment on four different mazes (Peterson, 1917). The mazes in one pair were identical in all respects save that one maze had shorter blind alleys than the other; those in the other pair differed only in the fact that certain of the blind alleys were shortened in the one maze and a different set were shortened in the other one. Each of the four mazes was used with two different groups of rats, one group having already been trained on a different maze and one group being entirely untrained. The data showed that the maze with short blind alleys was learned more easily than the one with longer blind alleys, and that errors of entrance into the short blind alleys were more readily eliminated than errors of entrance into longer alleys. These results are inexplicable on the basis of recency and frequency association factors, but they support quite a different hypothesis which had been formulated during the previous year (Peterson, 1916). This hypothesis was based on the assumption that impulses resulting from any stimulus and reaction did not immediately fade away but persisted and interacted with later impulses aroused as the animal made its way through the maze. These impulses were thought to be largely proprioceptive, being aroused by the responding muscles and passing up to higher nerve centers of the brain. This would make possible the combination of numerous nerve impulses, both from recent acts and from present externally stimulating objects, into various pattern forms consistent both with bodily attitudes and external conditions, so that the resulting responses would be made to situations and not merely to immediately stimulating objects in a seriatim fashion. Such proprioceptive impulses from the muscles have recently been demonstrated by means of amplifying devices (Travis and Lindsley, 1931). Volleys of such impulses are sent back to higher centers, beginning with but a few at first, then increasing in number to a certain maximum point and beyond that gradually decreasing. Over any single nerve fiber they pass as a succession of impulses the frequency of which is a function of the refractory phase of the fiber and of intensity of stimulation.

In the second experiment (Peterson, 1917a), involving a detailed analysis of the responses of 17 rats in the first two trials at learning a maze (during the period when learning was evident and rapid), it was found that the rats failed totally to conform in their 'choices' at the various bifurcations of the maze to the laws of association (or the expectancy based on frequency and recency factors). In fact, it was found that frequency and recency factors definitely tended to fix wrong responses as well as right ones, and that the learning was accomplished (*i. e.*, wrong responses eliminated) by reactions that were contrary to expectations based on both frequency and recency of previous responses at any bifurcation where a wrong response was first made. Later (Peterson, 1920) these results were confirmed on adult human subjects in experiments with a "mental maze"—used to eliminate irrelevant spatial factors—and it was further found (Peterson, 1922), by the use of a modification of this maze problem making it impossible for one to learn unless his responses contradicted directly the expectations based on frequency and recency factors, that learning was not obstructed and was only impeded a little even under such conditions. Further than this, it has been demonstrated in a recent study, yet unpublished but reported in abstract (Peterson, 1930), that learning goes on very little hampered even when frequency and recency factors are made to operate negatively and the subjects (college students) are punished with an electric shock whenever correct responses are made. Kuo (1922) has shown that in a maze with two different pathways leading to the food box—a long and a short one—rats which at first by chance choose the long one will eventually change rather suddenly to the short one, without further exploratory activities. It is obvious that such departures from original habits are contrary to expectations based on associations by frequency and recency factors.

Thorndike (1927), ten years after the first demonstration (on rats in maze learning) of the inadequacy of association laws (Peterson, 1917a), became convinced by his own experiments on human learning of the inadequacy of his "law of exercise" or repetition as a selective factor in learning, but he still contends that "a satisfying after-effect strengthens greatly the connection which it follows directly and to which it belongs, and also strengthens by a smaller amount the connections preceding and following that, and by a still smaller amount the preceding and succeeding connections two steps removed" (1933, p. 174). His assumption of a "satisfying after-effect" as the agent of such establishment of connections in the nervous system is purely gratuitous; he has not even hinted at the means by which this is accomplished. It should be emphasized that such non-testable hypotheses are of little value in science (but see Thorndike, 1932).

In the meantime the conditioned reflex enthusiasts are carrying on many interesting and important experiments on the substitution of stimuli in glandular secretion, on the control of emotions by secondary stimuli, etc. These processes are called *conditioning*, and there is a tendency to assume that all learning is finally reducible in its elemental aspects to simple conditioning. Hunter (1932) at the conclusion of his radio talk on *How Animals Learn*, concludes "that there is probably only one fundamental method of learning, that of conditioned reflexes" (p. 169). Watson (1916) enthusiastically championed the same view in his presidential address before the American Psychological Association seventeen years earlier. Against this view are the following facts: (a) that there has been no evidence produced to show any more complex learning by conditioning than mere contiguity association of a rather simple order; (b) that conditioned responses soon weaken on repetition whenever the natural stimulus is withheld a short time while only the conditioning stimulus operates (Culler, 1933), making it necessary frequently to strengthen the response by a simultaneous or nearly simultaneous presentation of the natural with the conditioning stimulus; and (c) that higher degrees of conditioning—secondary, tertiary, etc.—seem to be very limited indeed.

Maier (1931, 1932) has recently devised experiments on the differentiation in rats of two factors which he calls L (learning) and R (reasoning), and he holds that he has differentiated these two factors so successfully as to show in progressive cerebral destruction, by the Lashley methods, on large numbers of animals, that R activities are affected increasingly with increase in size of lesions of the posterior cortex appreciably more rapidly and seriously than are L activities. It seems probable that his alleged two factors are but extremes of learning activities, and the results can thus be accounted for without any assumptions of faculties. In human beings, neither learning nor reasoning is a unitary or unique function. On the contrary, *given pairs of activities* within either of these two classifications correlate not at all highly with each other, so that prediction of the one based on performance in the other is still far from encouraging, and is often not significantly better than prediction of learning ability from performances in reasoning, or *vice versa*.

These various problems in learning are in several respects live issues today and indicate that much more technical experimentation must be made before anything conclusive can be arrived at. The problems are inviting to neurologists and biologists as well as to psychologists.

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