Under the Editorship of
GARDNER MURPHY

THE PSYCHOLOGY OF LEARNING

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CHAPTER XX

Cats in a Puzzle Box

In the years following the first publication of *The Psychology of Learning* George P. Horton and I entered on a long series of observations of the behavior of cats in a puzzle box to see whether the general theory of learning described in that book seemed to be a practical description of behavior. We were particularly concerned with finding out whether all the observed behavior was consistent with the principle of conditioning or whether behavior appeared which that principle would contraindicate. Is it a general principle, or is it only one of a number of behavior mechanisms which adapt behavior to the conditions of life?

There is a great weight of opinion to the effect that association is just an occasional feature of behavior and that alternate principles are involved. This was the opinion of John Locke, who believed man a creature who was governed mainly by reason but who occasionally fell into associative thinking and so into error. In our own day Allport has expressed the belief that insight and association are mutually exclusive processes. The stand taken in this book is that insight is achieved through association. Tolman holds that there are at least six types of connections that are learned; the theory presented here can be entertained only as explaining one of these types, the acquisition of motor patterns. Wheeler, Koffka, Köhler, Lewin, Razran, Culler, Hilgard, Skinner, Mowrer, Maier, and numerous others either reject association of cue and movement as a principle or hold that the effect of association in making a stimulus a signal for a movement is an alternative mechanism of learning and that association has two alternative forms. Allport, Wheeler, Koffka, and Köhler all refer to association as a low or primitive mode of behavior change which is occasionally in evidence, usually with no result of value to the animal. Culler believes that conditioning could associate some appropriate action with a signal, whether or not that action was the practiced one. In 1930 Miller and Konorski suggested a distinction between classical or Pavlovian conditioning (association between two stimuli, a new and an old) and instrumental conditioning in which a response is rewarded when it appears. Skinner (1938) based his argument for two different mechanisms of association on the highly practical ground that two radically different experimental methods are in use for studying association which of necessity result in different laws. One of these, the classical Pavlovian method, observes the application of a new stimulus signal such as the sound of a bell along with a stimulus which can be depended on to elicit a response, as food elicits the flow of saliva. The result is the association of the signal with the response. The bell acquires the power to elicit salivary flow.

In contrast with this are the experiments in which no attempt is made to observe or control the situation that occasions the response, but which provide that if and when a particular response occurs it will be rewarded.

Probably the best-known and most quoted experiments in learning, after Pavlov's experiments on the conditioned response, are Thorndike's studies of the behavior of cats in a puzzle box, which were described in his *Animal Intelligence* published in 1898. These studies were widely quoted in elementary texts and exerted a strong influence in directing the development of American theories of learning toward the use of reward or reinforcement as the basic concept in learning. A whole generation of American teachers were trained in Thorndike's views, and his ideas were the foundation for the theories

of Hull and Spence, of Skinner and of Mowrer, and in general for the theories which reject straight association for a concept of reinforcement.

The box which Thorndike used was a wooden one with slats across the front and top. The escape door was held shut by a catch which could be released by pulling on a loop hanging in the box, or, in some models, by turning a button from a vertical to a horizontal position.

The cat was placed in the box and the door was latched. The cat's movements were watched through the openings between the slats, and the time between entrance and final escape was recorded.

Thorndike reported that on being placed in the box the cats were excited and active, biting at the slats, clawing at various points, trying to push their heads between the slats. He referred to these as random movements or as trial and error. The word "random" has been misinterpreted by numerous writers as "chance" or lawless. This is of course not what Thorndike meant. He meant that from the point of view of an observer the movements can not be described as aimed at operating the mechanism because the cat has had no previous experience with the mechanism. Thorndike conceived the whole process as consisting essentially of the acquisition of an association between the sight of the loop and the movement by which it was pulled. "The time curve," he said, "is obviously a fair representation of the progress of the formation of the association, for the two essential factors in the latter are the disappearance of all activity save the particular sort which brings success with it, and perfection of that particular sort of act so that it is done precisely and at will. . . . The combination of these two factors is inversely proportional to the time taken, provided the animal surely wants to get out at once. This was rendered almost certain by the degree of hunger."

Some cats learned the successful movement of escape in two or in three or four trials. For the majority there was an irregular but generally progressive decrease in the time required for escape. Thorndike interpreted this as proof that the cats were establishing an association and not using ideas or thought. This assumption, that associations are not made all at once but are built up by repetition, is in my own opinion an error founded on the habits of thought established in the early studies of learning, in the acquisition of nonsense-syllable series as in Ebbinghaus' experiments. These activities are better described as complex skills than as simple associations, but such studies firmly established in American psychology the general concept that the strength of an association is a function of the number of repetitions and not an adjustment achievable on a single occasion.

Thorndike held that the cat's learning consisted of establishing an association between a sense impression and an act "with the impulse leading thereunto." This was eventually described by him in terms of bonds strengthened according to the law of effect.

In 1929 Donald Adams published an account of a repetition of Thorndike's puzzle-box experiments and of experiments of his own design which avoided certain objections to Thorndike's method.

Adams worked on the assumption that animals use "ideas" in ways not consistent with "mere" association, and this theoretical assumption directed his factual observations. The canon guiding his theory was adapted from Lloyd-Morgan. Adams' amended version was as follows: "Any experience or mental process in another organism can be inferred from structure, situation, history, and behavior only when a similar experience or mental process is or has been invariably associated with similar structure, situation, history, and behavior in oneself; and the probability of the inference will be proportional to the degree of the similarity." In other words, Adams' scientific model

is himself. To the choice of such a model there is one very important objection: We do not know enough about Adams' behavior (or our own behavior) to use him or ourselves as predictive models. It is really Adams (and also ourselves) that we set out to try to understand in the first place.

Horton and I began with a very different theoretical orientation. We were seeking to describe the cat's behavior in terms of public facts rather than in terms of our own consciousness. A useful theory of learning must start with such public facts and derive or construct a set of principles or a model with known behavior on which can be based a prediction of what the animal will do. This also must be stated in terms of public facts. Laws of learning stated in terms of what the animal thinks or believes are of no value to us unless the behavioral expression of these thoughts and beliefs is open to observation and in that case the behavioral expression is a much better medium of description.

Our own theoretical position led us to observe the detail of movements and not merely record successful outcomes of movements (acts). We wished to know whether or not the detail of movement is predictable as well as the end result. The "goal" psychologies (Köhler, Koffka, Lewin, Maier) have no basis and no provision for predicting such detail. Because common sense is interested not in how things get done but only in whether or not they get done, many psychologists have failed to show any interest in means; they are interested only in the attainment of ends.

There was another theoretical bias which directed our experiments. We were not concerned with improvement for its own sake (which many psychologists have identified with learning) but we were concerned with the nature of the changes in behavior whether they were improvements in behavior or not. We were therefore interested primarily not in the curve of learning which Thorndike considered the important result of his observations, but in the changes in the pattern of the animal's movements from one trial to the next. Thorndike believed that the curve of learning showed a gradual reduction in the time required for escape and that this represented a gradual increase in the strength of an association between situation and successful response. Our theoretical position was worlds apart from this. Thorndike himself had noted that the successful response often occurred on the first or second trial and that no learning was required. This is no evidence for the strengthening of an association. The association appears to spring full-blown like Minerva from the head of Jove. By Thorndike's own acknowledgment, the lessening of the time required for escape is due to the omission of movements that do not contribute to the escape, although these movements appear and are repeated just as the final movements are.

The unsuccessful movements do not fade out or show any gradual weakening. The successful movements are often as strong at the beginning as at the end, and often appear in an early trial and continue indefinitely.

It was our conviction that there is evidenced in both animal and human behavior a strong tendency for action to exhibit a certain all-or-none character, a strong tendency toward dichotomy of choice. Just as people either sit or stand, either rise or remain seated (with occasional but atypical amusing compromises), either go to the right of the obstacle or go to the left, either speak or remain silent, either say the word or do not say it, either go to the party or stay home, so do animals, once a movement pattern has been executed, tend to exhibit the whole pattern or none of it. Compromise responses exist—Stevenson Smith and I described them in 1921—but behavior tends strongly to be organized into definite patterns and these patterns occur or fail to occur.

Horton and I therefore began our observations with the intention of observing not the total time required for escape, or

the curve of diminishing errors, but whether or not recognizable patterns of behavior appeared according to any rule, and whether such patterns could be best described in all-or-none terms or showed evidence of waxing and waning consistent with the general concept of habit strength or of response as a continuous function of such variables as elapsed time, trial number, number of previous practices.

Our choice of a puzzle box rather than a maze was based on an interest in specific behavior in one situation. Studies of maze learning have by the nature of the experimental device strongly prejudiced theory in favor of the learning curve as a basic concept because the maze discourages observation at specific points and lends itself to records of total errors, total time required to traverse. We believed that the phenomenon of learning was lost sight of in such observations. The recording of total errors conceals such essential phenomena as the shift to a correct turn at choice point E and the subsequent adoption of the correct turn for the balance of the experiment. Miss Voeks' experiment (1948), in which this feature, so far as I know, was for the first time made a matter of record, showed that the response at a maze choice point could be predicted with significantly greater certainty from the response last made at that choice point than from the amounts of previous practice of right and wrong responses at that point. For instance, if a subject turns left at point E (wrong) on trials 1 to 10 and on trials 11 to 20 turns right, the prediction of trial 12 on the basis of previous practice will be strongly in favor of "left" as it will on trials 13 and 14; and even on trial 20 the conventional betting will be 10 to 9 on the false premise that habit strength is a function of previous practice.

But it is characteristic of maze behavior that, except for such special patterns as a tendency to alternation or to exploration, responses at a choice point change suddenly and the new response is maintained. By recording only total errors, a completely artificial continuity is introduced into the record and association is given a false appearance of depending on the number of previous trials.

We were equally discontented with the classical conditioned response experiment which we believed was likewise highly misleading in its results. The fact that response certainty (the percentage of trials in which the signal is followed by the response in question) shows a gradual curve of increase with repetition we thought did small justice to the learning capacity of dog or man or even lower animals. We know very well that in everyday life animals as well as man characteristically acquire new behavior in one trial. The rat probably never repeats on the same trap if it escapes the first time. It does not require 100 springings of the trap to learn caution. Once handed a hot bone by his master, the dog refuses to take the next bone. This refusal is not a response whose strength gradually builds up with repeated burnings. Hudson's rats (1939), once shocked while eating from a metal dish, one month later when again exposed to the situation started (13 out of 14 of them) toward the dish and then withdrew before touching it. Köhler (1925) so takes for granted that a solution once hit upon will be repeated that he makes a special note when one of the chimpanzees fails to act accordingly.

The puzzle box which Horton and I chose for our experiments had certain advantages. Its front was a large pane of glass which allowed us to observe all the animal's movements, to photograph the final successful movement, and (so far as our funds would allow) to take motion pictures of all the behavior.

The release mechanism by which the cat could cause a glass door in the glass front to open and thus permit escape, was either a post on the floor or a tube suspended from the top of the puzzle box. By an electric contact or by the interruption of a beam of light, a slight movement of the post operated the release of the door.

At the rear of the puzzle box was a starting box in which the cat was placed. In this way we avoided putting the cat in by hand, which would have introduced a great variety of initial behavior.

The movement of the post which opened the door also operated the shutter of a Contax camera and took the cat's picture in the act of effecting its release.

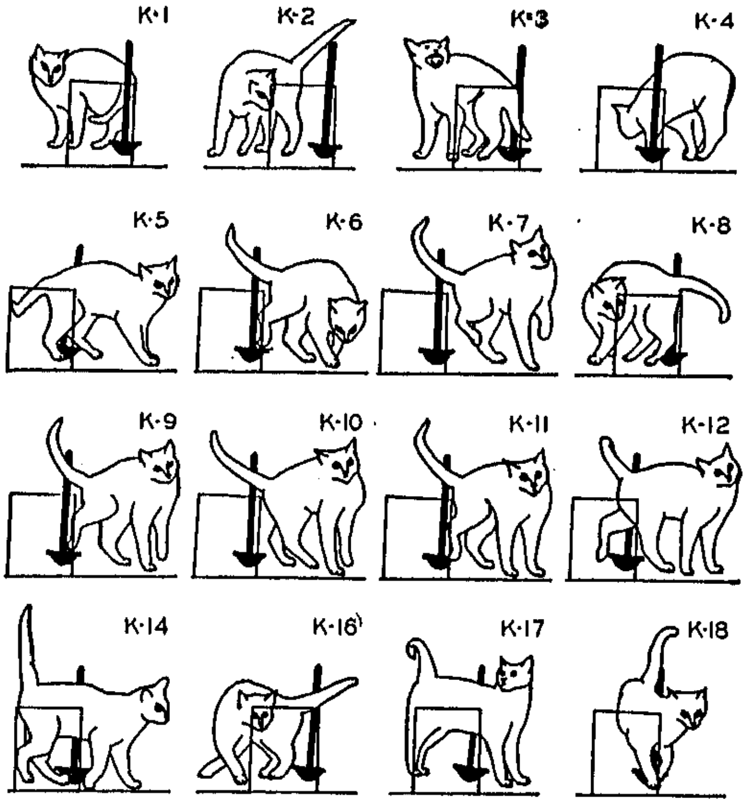
The animal was placed in the starting box and kept there from ten seconds to a minute. A pull on a lanyard opened the doors into the puzzle box. For the first three trials the glass escape door was left open. A saucer containing a small bit of canned salmon was outside, in view of the cat.

The routine of the experiment consisted of setting a visible electric timer at zero, closing the escape door, setting the still camera, placing the salmon in the dish. The doors of the starting box were then opened by a lanyard and notes taken on the actions of the cat.

In most experiments in maze or puzzle-box learning the experimenter's interest has been on the decrease in the total time required by the animal or the gradual reduction of "errors." We considered these to be results of learning, not learning itself. We were interested in the process, not in the result.

Our release mechanism could be operated by an indefinite variety of movements and, unlike Thorndike's loop, permitted successive escapes by an exact repetition of movements. A loop requires very different movements, depending on whether it has one side or the other toward the operator.

When the cat moved the pole by bumping into it, by falling on it, by lying down and later rolling into it, by biting, by clawing, or by turning and touching it with its tail, the picture that was automatically taken was supposed to represent a cross section of the escape movement. In spite of our hopes, this was not always taken at the same point of the escape movements even when these represented startlingly exact repetitions. The escape

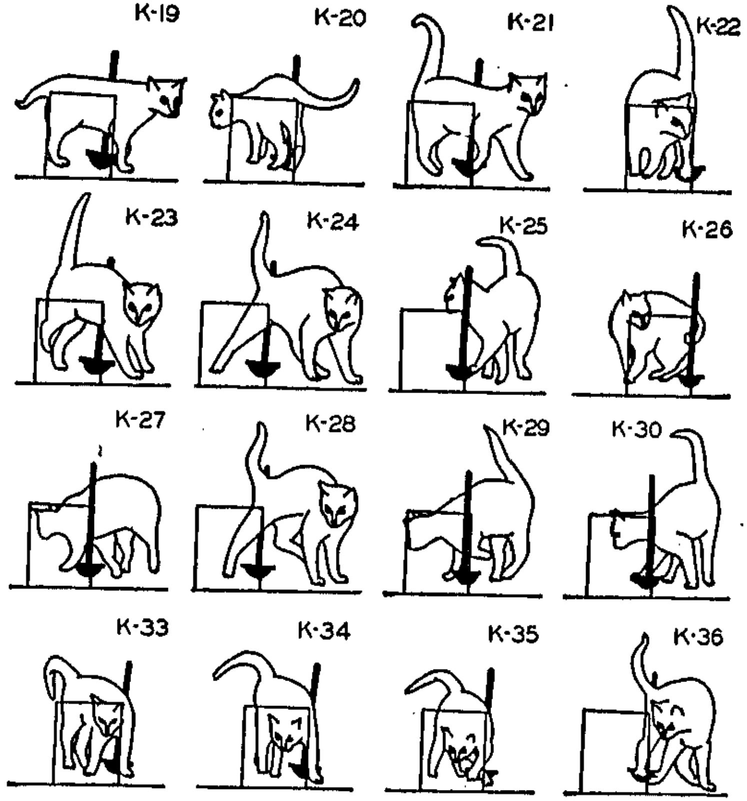


Record of Cat K

In trial 4 the routine used in the first three escapes was repeated, but without success, and the cat escaped by striking the pole with the left shoulder. On trial 5 the original routine again failed to

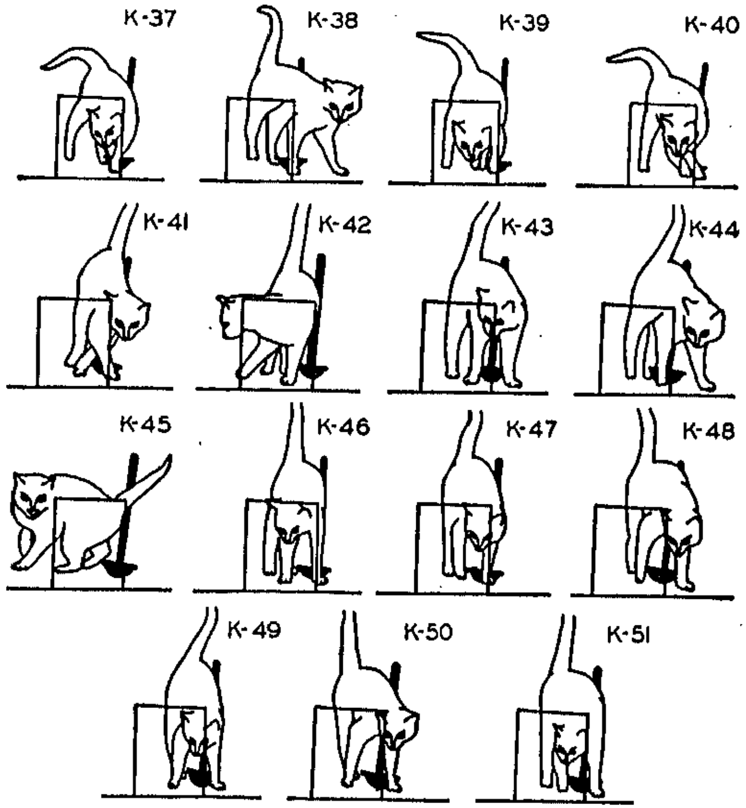
movement might be practically identical with the preceding one but initiated from a position an inch farther from the post so that the picture was taken farther on in the series; in some cases the exact repetition of the movement series was begun from a point too far away and failed to operate the release.

There were therefore many more close repetitions of escape movements than our camera record shows.



operate the release and the cat continued to turn. Striking exceptions to the prevailing routine can be seen in trials 8, 16, 26, and 45. In each of these the cat had backed out of the starting box and followed a routine different from the usual one, but remarkably the same in

We observed and photographed about 800 escapes. One of the noteworthy features of the escape behavior was its conformity to the descriptions by other men. All the features noted by Thorndike and by Adams were noticeable in our own cats. Lewin's general description of behavior at a barrier also applied. The cats spent most of their time at the front of the box through which they could see the salmon or the "outer world." It would



the four isolated cases in which the start had been through backing into the box. Motion pictures of this cat are included in the film, *Cats in a Puzzle Box*, by Horton and Guthrie.

have been very difficult to describe the cat's goal, which was not an object, but escape itself. Only when the cat had spent considerable time at the glass front did it turn to other parts of the box; and the post, as Lewin would probably have predicted, was not noticed until the whole periphery (barrier) had been thoroughly explored over and over. The cats were also promising subjects for description, in Tolman's terminology, of cathexes (the attachment of interest in escape to features of the

door), of equivalence beliefs (which I have trouble distinguishing from cathexes), or field expectancies (perhaps also illustrated by the cat's evident set for escape through the door). Field-cognition modes were also in evidence—perception, at least. Perhaps the perception of the door as openable is an illustration. Our interest, of course, centered on what Tolman calls motor patterns, the cat's behavior in the box.

In general the cats, on being admitted to the box, paused at the threshold, entered cautiously, proceeded to the front door and clawed, sniffed, looked about. Any outstanding features such as the crack around the door got attention and were pushed at or bitten at. Approximately an average of fifteen minutes was spent in such exploratory behavior. This meant many excursions about the box.

Eventually most cats did something that moved the pole and opened the escape door. The noise of the door was followed by the cat's looking at the door and then (usually) by its leaving the box through the open door.

After brief freedom the cat was picked up and replaced in the starting box and the process was repeated. Evident from the very first were startling repetitions of previous behavior. If, on entering the puzzle box from the starting box, the cat had paused and sniffed at the lower right corner of the doorway there was high probability that this routine would be repeated. If, on entering, the cat turned left, this also was in order on the next trial. In some cases long series of movements—a triple tour of the periphery of the cage including numerous stops—would be repeated in detail.

The time required for escape was irregularly reduced so that by the end of the 20th trial escape usually took less than a minute and in some cases as little as five seconds.

The outstanding feature of the series of escapes was a strong tendency for escape routines (the final movements leading up to touching the post) to be repeated with high fidelity. A typical cat would have a number of these routines and its 20 escapes could be classified as consisting of 12 cases of routine B, 3 cases of routine A, 4 cases of routine C, and one unique solution. As time went on, most cats settled on one routine to the exclusion of others.

It is perhaps worth giving a play-by-play account of the first cat's behavior in a series of 34 trials with the post in one position and a series of 15 with the post in a different position. (This is quoted from *Cats in a Puzzle Box* by Guthrie and Horton, Rinehart, 1946.)

Cat A, the first animal used, represents one of the most variable records. There were certain errors in the operation of the apparatus which may account for variability. The mechanisms opening the door and activating the still camera were not so finely adjusted as in the runs of the following cats. The displacement of the pole necessary to open the door was not the same on each trial, and this proved an essential condition of some of the behavior which will be described. The speed with which the door opened was variable and the entrance door did not operate smoothly. All these factors were corrected in later experiments.

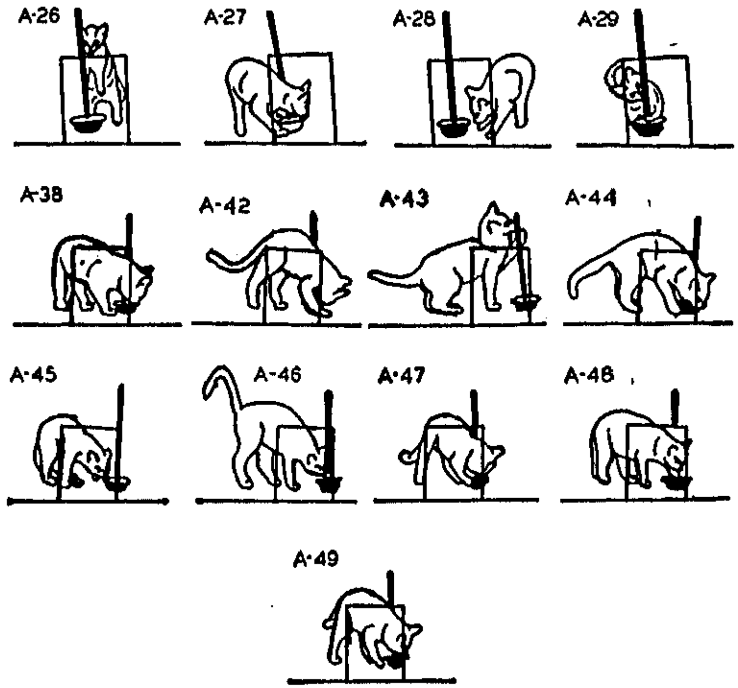
Cat A was given thirty-four runs in Box A on October 21, 1936. A motion-picture record was also made, and this has been issued by the Psychological Film Exchange with the title, "Cats in a Puzzle Box." Time records are lacking because we depended on the cinema, and this had to be interrupted occasionally when film ran out.

Each cat was first placed in the enclosed starting box at the rear of the apparatus; the doors were then opened, and the cat was allowed to exit by the open door. Three such "free runs" were usually allowed in order to speed up the trials by establishing the location of the exit. All trials numbered from the trial following these "free runs."

Cat A, stopped by the closed glass door on its third trial (the camera failed on the first two trials), spent many minutes exploring the box and finally brushed the pole with its flank while moving back and forth across the front of the box. On the fourth trial the escape movement was entirely different. The cat was headed in the opposite direction and was pawing the pole. The fifth trial finds it in almost the position of the first escape except for the angle at which the tail

is held. Escapes seven and eight were accomplished by what we judged to be the "same" movement, but this resembled no others in the series.

Trials 14, 18, 19, and 20 have a like substantial identity of posture at the moment of escape. The cat has approached the door with the pole on its right and gone rather promptly to the door and had struck



Record of Cat A

Trials 26, 27, 28, 29, 38, 42, 43, 44, 45, 46, 47, 48, and 49.

the pole with its rear while examining the door. Trials 38, 42, 44, 45, 46, 47, 48, and 49 were accomplished by almost identical movements (pawing), times being twenty, nine, fifteen, ten, and nine seconds. In trial 43, which failed to conform to this series, the time is longer, thirty-six seconds, and the action was a use of the left paw as in trial 37 but at the top of the pole rather than at the base.

It is perhaps significant that this cat had twice in the first series escaped by "advertent" use of its left forepaw. Tolman's description in terms of "means-end-expectancy" appears to fit the case very well in view of the added fact, not shown in the pictures, that the cat turns promptly from the pole to the door after pawing the pole. This action must have been prepared while the cat was pawing the pole.

But this description is highly superficial. Why should the actual movement series and the postures in which the picture catches the cat be so remarkably alike? Surely this cannot be explained in terms of the cat's knowledge of the necessary muscular contractions any more than a human being's movements can be explained in such terms. The cat does not use names for its movements and choose a movement by name. The cat has no insight into or hypotheses concerning these movements. The theories of Tolman and Lewin have no place for the movements by which goals are reached, and this occasional highly stereotyped repetition of actual movements has no place in their theories. We may take occasion here to mention that there were many more of these stereotyped repetitions than appear on the record or in the pictures because it many times happened that this repetition was not successful in operating the door. Movement series were in some instances repeated as often as forty times, but a slight change in the position of the cat now rendered the movement useless.

A movement-by-movement account of eight hundred escapes is out of the question because, among other reasons, it would not be read. This account must be limited to certain outstanding features noticed by the experimenters and in most cases based on the picture record.

How are we to interpret what the cats were seen to do in the puzzle box? The problem is not how they achieve success. No two cats in the fifty and more we watched used identical methods. The variety of names which can be given the escapes has already been mentioned—clawing, biting, bumping, pushing. The variety of actual movements is indefinitely great. Every cat has its own pattern and most cats have several patterns.

No theory of learning explains success. The actual movements which operate the release mechanism are part of the animal's repertoire, determined by the animal's structure and its neural equipment and receptor capacities and connections. Some of its movements—in an adult cat most of its movements—are the end product of much previous integration through learning. The

cat has learned differential responses which will achieve pawing a seen object in a multitude of different positions. Pawing an object requires an elaborate integration of posture and limb movements. Maintaining balance while the limb is moved requires a delicate adjustment of balance in many dozens of muscles. What happens in the puzzle box is that regardless of what order these elements from the repertoire occur in on any one occasion in the box, the same order tends to be preserved on the next occasion. This is what the principle of association by contiguity would lead us to expect if we reflect that movement complexes themselves are stimuli and can serve as cues for response as well as what is seen and heard.

We can not predict what a particular cat will do on its first tour of the box except on the basis of our knowledge of cat behavior in general. A racoon will have a different repertoire to call on the first time through.

But after watching the cat through one trial we can bet rather heavy odds that the second trial will repeat most of the routines of the first. There are exceptions to this generalization. One trial is not at all what Thorndike tended to think it, namely, one association. In its wanderings around the box on the very first occasion the cat has established many associations but at the same time has replaced many of these with others. Much unlearning occurs on the first trial if that is protracted.

For instance, if the cat makes three rounds of the box and then is led to do something different, the end of a round may now have a new consequence and the cat will not keep circling.

While the cat is in the box there are continual opportunities for establishing new associations. Behavior tends to arrange itself in what we may call "episodes." These episodes are often self-terminating. When the cat sees and then bites the post, the post is in the cat's mouth and can not be seized. When a paw is raised, it can not be raised again until it has been lowered. When the cat has eaten the salmon it can not repeat the act until the salmon has been replaced. A cat that has lain down can not lie down. It can only remain lying down or rise. What we are doing is an important determiner of what we will do next, and we may be faced with the same external situation; but if we are not in the posture and action in which we faced the scene, our general situation is not being repeated. By "general situation" is meant the total stimulation that is acting.

This is a long excursion into interpretation and theory and far away from the factual description of what the cat was doing, but Horton and I believed that the behavior we watched lent itself readily to this interpretation.

There was one movement routine that changed less on successive escapes than the others. This was the final series of movements that led to the opening of the door and escape from the box. Why should this successful movement be so much more permanent than other movements which did not contribute to escape?

It is our belief that the escape routine, the essential movements of escape, are repeated because they remove the cat from the puzzle box and, being removed, *no new associations with the puzzle-box situation are possible.*

Note that this is a theoretical position radically different from the reward and reinforcement theories. The present theory holds that all response complexes are associated with their contiguous situations and will be repeated when the situations are repeated, but that the movement complexes which leave the animal in the puzzle box are subject to unlearning because, owing to changes in the external and internal situation, *the animal still confronts the box situation and may be led to different behavior which will replace the previous association.* The animal that claws at the door will eventually desist as a result of pain or fatigue. Now, looking at the door, it backs away instead of clawing. On its next approach to the door, this may completely do away with clawing. The cat has associated the sight of the closed

door (and a complex of its own behavior stimuli) with retreat from the door in place of approach and clawing.

The theoretical position taken in this book is that every action performed by the cat in the puzzle box is conditioned on the contemporary cues from the cat's own movements from the box and other external stimuli. There is observable on successive trials an astonishing amount of repetitiveness, some of the routines appearing for only a few trials, some persisting through many trials. Whatever the cat is led to do, whatever new behavior is introduced by the inevitable newnesses in the situation, tends to be learned as a response to the attendant circumstances. And this wealth of detailed behavior that appears and remains for few or many trials is not rewarded behavior. It seemed to us to be any action whatever.

But this behavior is exposed to replacement by other behavior *unless it ends in the removal of the cat from the box, in which case there is no opportunity under any theory of learning for the establishment of new behavior toward the interior of the box.* Even if one adopts the reward or reinforcement hypothesis the stimuli must be present in order to establish new responses to them.

A very conspicuous feature of the cat's behavior in the puzzle box with its repetitiousness. This has been noted by Thorndike and by Adams but no significance was attached to it. Muenzinger, Koerner, and Irey noted it in 1929 in the behavior of guinea pigs in a puzzle box, though their attention was on the variability rather than the stereotyping of behavior. They did not, however, do more than note, for example, that one animal (No. 17) brushed over the release lever nine times in 550 trials; it rested its paw on the lever three times; it operated the lever with a circular movement four times, and placed the right foot over the left six times. Other animals had other idiosyncrasies. These authors concluded that the mechanization of habitual movement "is still accompanied by variability of its pattern

. . ." and that accessory movements, "while on the whole exhibiting much plasticity, show some mechanization of a brief and unstable kind."

The account given by Muenzinger, Koerner, and Irey is quite consistent with what Horton and I observed in our cats. We were interested in the routinizing, they in the exceptions and new behavior. We were convinced that whole segments of movement appear and are repeated all-or-none on succeeding trials.

It is the final movement series that is least subject to change. By the final movement we do not mean just the momentary action that operated the release. We mean a long series of movements which took the cat into a position from which the releasing movement could be made. The variety of these final movements, which were alike in no two cats and were probably as characteristic of each cat as a signature is of the human signer of a document, is sufficient evidence that no "law of least effort" was in evidence. Some modes of release consumed far more energy than others.

Our summing up of our observations and our interpretation is as follows: (1) There was a strong tendency to repeat movement series on succeeding trials and to repeat them in remarkable detail; (2) new behavior often appeared, sometimes involving a whole session in the box and a new solution; (3) the final movement series was more stable than others and was often a pattern which had appeared in early trials.

We interpret these observed facts to mean that the cat's first trip through the box is subject to accidental determiners, many of which inhere in the accidents of the entrance to the box, the features of the box, the past experience of the individual cat, and other factors. Once repeated, a series of movements in the box becomes in a sense independent of minor distractions and tends strongly to reproduce itself if it is initiated on a later occasion. This is understandable in terms of the principle of association,

for each movement phase may tend to become the signal for the next phase. The possibility that temporal as well as spatial patterns of stimuli may serve as integrated signals is an issue for future research and interpretation to settle.

On the second trip through the puzzle box the cat ends to repeat its behavior on the first, errors and all, except such behavior as has been reconditioned during the trial. When the cat is in the box for a long period, much relearning tends to take place. Cats which hit upon a solution quickly on the first trial tended to maintain their pattern through an indefinite number of following trials. We could always force new behavior by holding the release mechanism so that what had been a successful escape movement now left the animal in the box with a novel situation.

Our main conclusion from the series of experiments was that the prediction of what any animal will do in a given situation is most securely based on a record of what the animal was observed to do in that situation on its last occurrence.

If it is objected that this rule applies to physical objects and is not a psychological generalization, we should add a clause which has been taken for granted, and restate the rule as follows: What an animal will do in a given situation is best predicted by the record of what it last did in that situation even though the last occurrence of the situation was long ago. Voeks (1948, 1950) has called this the principle of postremity.

Watching cats in a puzzle box raises a problem which every investigator of learning has faced. How shall he name what he sees? What elements or units shall he remark? What shall he use as the weather signs of behavior, the bases of prediction? What are the predictable behaviors? The answers to these questions will shape the observer's theory. They will determine what goes into the record and therefore into the laws developed. In his earlier experiments Skinner chose to place the rat in a closed, opaque box with a bar and a food tray. The antecedent items of record were some measure of the hunger drive, such as number of hours without food and recent starvation to 80 percent of normal body weight. The consequent item recorded was the rate and total number of movements of the bar.

We found other items of great interest but they lacked the definite character of Skinner's items. We noted that long sequences of movement around the puzzle box were occasionally repeated in great detail. This repetition by its very nature was not an item built up by practice. The first repetition was the best and most exact. If one is interested in these second appearances of complex and extended behavior patterns there is no learning curve or curve of extinction to record. The pattern appears or it does not appear. The question of degree is subordinate to the question of presence or absence.

Furthermore, in predicting what would happen next as we watched the cats, we depended on such behavior sequences. Our own basis for prediction was what we saw the animal doing; this can be thought of as a pattern of serial stimulation of exteroceptors and proprioceptors activated by the animal's movements and the consequences of those movements. We would recognize the early part of a sequence observed once before but not on recent trials. We would then predict that the cat would carry on the action as it had on the last occasion when this sequence had occurred. This rule held whether the action led to any observable reward or not. It even held when the action led to acute discomfort—for example, the cat that jumped for the wire mesh covering the cage and clung there for a time. It should be remarked parenthetically that the establishment of these sequences of movement around the box is essentially like the establishment of sequences in the maze that are the basis of Tolman's *latent learning*. The demonstration of latent learning consists in allowing an animal to stay in a maze without reward and showing later that the effect of its consequent familiarity

with the maze is to reduce sharply the time required to run it when reaching the goal is rewarded by food.

Occasionally long sequences were observed in our puzzle box, but these were all eventually shortened or omitted. Most cats in time reduced their pattern to a brief series of movements. In one case at least, the series was dependent on external stimuli for its completion. This was cat K which, as shown by the motion-picture record, had been escaping by leaning toward the post. On one occasion this movement was not effective—did not result in the click of the released catch and the opening of the door. The cat made the movement again, and in succeeding trials it made a double pass at the post even when the first was successful.

Another cat used in the preliminary development of the box design had exhibited a pattern which consisted of walking across the box and stepping on the base of the post with the right hind foot and then walking out the opened door. When the post was set up in another part of the box this same routine was followed to the point of bumping into the closed glass door. In the first case (cat K), the repeated pushing of the post with the flank, evidently there were at least two serial units—the approach to the post and pushing, and, as a second integrated series, walking to the door at the signal of either the sound of the latch or the sight of the opening door.

In Tolman's terms, the double push is evidence of a negative equivalence belief (that it doesn't pay to try to exit until the door opens) and a field expectancy in that the push has acquired a "sign-gestalt" character connecting it with escape or making it the understood cause of the opening of the door, or evidence of a field-cognition mode in which the cat uses its "innately strong" tendency to infer that "if a certain sequence of events has occurred on one occasion, this same sequence of events is likely to occur on subsequent occasions." The observed event itself gives real point to Tolman's probable refusal to treat the whole series as a motor pattern. The motor pattern was broken when the door failed to click and *the push was repeated*, which had never happened before.

I contend that the eventual motor pattern, which now included a double push of no actual utility, did constitute a motor pattern and was acquired by straight association. Each movement had become the associative response to the stimuli produced by the last movement, including posture and the sight and sound of the door. The categories of behavior described by Tolman as kinds of learning all exist and are abundantly illustrated. My own inclination is to regard them as items to be explained and not as ultimate explanations.

Reflection on the behavior of our cats escaping from their puzzle box has led to some clarification in my own mind of the meaning of the word *stimulus*. There are two requirements which stimuli must satisfy in order to serve as scientific data. The first is that they must be physical changes affecting and activating sense organs. The second requirement serves a different purpose. In order to be usable as psychological data stimuli must be describable in terms acceptable to all scientific observers, whatever their theoretical position. In this particular, afferent nerve impulses do not satisfy us. At present we have no means of observing patterns of afferent impulses. Nor have we any means of directly observing any stimuli to retina or to ear or to temperature organs in the skin. What we can actually observe and agree upon are such events as "the sound of the whistle," "the sight of the pattern on the door," "the smell of peppermint," "being pushed forward," "turning the corner of the maze alley," "entering the food box," "being recumbent,"—all phrases that describe highly complex events already presumably organized by perception or organized as symbols for concepts and cognition. The only requirement for their scientific use is that we can expect any observer to accept them as described, perceptual or cognitive organization and all. When we say, "Let's get down to facts," we mean, "Let's find what events are involved on whose description we can agree." A Lewinian barrier is not a barrier in the psychological sense without being perceived or recognized as a barrier. This perception or recognition implies the perceiver's past experiences with barriers. A barrier can be accepted by the observing psychologists as a stimulus provided they can agree that it is functioning as a barrier.

Tolman's field expectancies obviously involve similar perceptions and cognitions. We recognize such perceptual and cognitive qualities of events in the common words we use for objects and events. We speak of food as a stimulus, or of the sight of the lever, or of the sound of the food pellet dropping into the tray. In actual experiment or in applied psychological work, stimuli are of necessity described in such terms. We can observe and agree upon our subject's posture and movement, which we usually also describe in terms which imply their incorporation into action and goal behavior.

As with stimuli, so with responses. The response as used in theory has two necessary characteristics. It must consist of movement or change of state in the subject brought about through the effectors, muscles, and glands. An allergic reaction is not in this sense a response, nor is a change in weight as a result of a disturbance of metabolism. The second necessary characteristic is like the similar requirement of stimuli, for, in order to qualify as scientific data or psychological facts, responses must be so selected and named and described that observers will all accept the description and agree on the use of the name. Skinner's bar pressing, which is actually bar movement with the pressing inferred rather than observed, qualifies on the second requirement but, so far as my own theory is concerned, not on the first.

The principle of association by contiguity or conditioning, as was mentioned before has possible ambiguities attached to nearly every word. *A pattern of stimuli acting at the time of a response* *will on a subsequent occasion tend to evoke that response* has many concealed assumptions. One is an absence of competing stimuli. Another is the presence of the same general situation on the second occasion as on the first. In fact, the principle might possibly be stated at greater length as follows: When a general situation which has a certain component, S_c, is followed by a response which does not include a certain movement component, R_n, and this general situation is repeated on a later occasion with the presumptive addition of S_n, as well as S_c, and followed by a response including R_n, a repetition of the general situation without S_n but with S_c is followed by a response that includes R_n. This is the mode of behavior change that is involved in learning.

Even this far more ponderous statement leaves indeterminate what is meant by a situation component or stimulus pattern. I believe that part of Tolman's concern is that we recognize that the stimuli act not as new elements but as perceptual or cognitive units or wholes. In escaping from the puzzle box, the post by which the cats effected their release was merely encountered by some cats (as by turning about and striking the post with the tail) and not perceived. That other cats perceived the spatial characteristics of the post is proved by the fact that without other experience of this particular post these cats reached out with their paws and clawed the post. The post was at sight already an object-that-could-be-clawed.

To this contention of Tolman's I can only agree. But it is my own contention that even if all this structuring is accepted, there remains a strong presumption that these perceptual attitudes and responses were acquired by associative learning where they were not innate, and that Tolman's cathexes, which are associations of drive and goal object, are built up out of associations and do not represent a nonassociative category of learning at all. Studies of perception and cognition are important, but we should undertake them with the possibility in mind that they involve associa-

tive learning. This approach will, I feel sure, prove more rewarding than the type of experiment offered by the Gestalt psychologists, who investigate perception as a phenomenon of the present moment and make no inquiry into its origins.

In the chapter on Hull's reinforcement theory mention was made of a possible tendency for behavior to be organized into all-or-nothing patterns and this was used as a serious objection to the conception of learning as a continuous function of such items as number of reinforcements or number of unreinforced pairings. In this respect Hull, Skinner in his earlier work with rats, Spence, Mowrer, and probably the great majority of learning theorists take a position different from the one here outlined. Tolman is an exception. Tolman's views admit the possibility that any one of his six varieties of learning could occur all at once, or in one trial. He has expressed no definite view on the problem of frequency.

Viewing our cats escape from their puzzle box strongly suggested that one-trial association was a characteristic of their behavior. We are reminded of Skinner's opinion that in a chain of responses only the first is open to Type R conditioning. After the first movement of the series is "emitted" the rest follow as a series of respondent movements, one movement being the signal for the next.

Consider the implications of this notion. At certain points in its first tours of the box the cat is somehow led to one of several possible movements. Suppose that one of these, A, elicits a chain, B, C, D, E, etc., in which each movement is responsible for the next because on some previous occasion it was followed by the next. All this, after A, would be *respondent* behavior in Skinner's language. Suppose that another of the possible movements, A, would have been followed by the chain B^1, C^1, D^1, E^1, etc., a chain or serial response established on some previous occasion.

There is here no question of degree or strength of the responses in either series. They occur because each of them once followed the previous response and the stimuli which that response evoked. Many such alternate series are observable in the behavior of our cats. They develop not simply alternate responses in a given situation but alternate lines of behavior. For instance, as was said earlier, in trials 8, 16, 26, and 45 cat K was caught by the camera in almost the same position, a position radically different from any others in the whole series. Examination of our notes showed that on these four trials only, the cat had turned around in the starting box before the door was opened, had backed into the puzzle box, and followed that awkward beginning by the same extended series of movements in the box.

It is here suggested that these movements did not occur because they were in any sense stronger than other responses, but because their cues had occurred. They were obviously just as *strong* on the intervening trials when they did not occur. The question of strength is irrelevant. The nonappearance of the series on the intervening trials is explained not by its weakness but by the fact that the animal entered headfirst and so there was no revival of any one of the series of movements which was begun by backing into the puzzle box.

In opposition to Skinner, it is here argued that even the initial movement has its signal, whether we discover it or not. I believe it more profitable to take a theoretical position that encourages search for the signals of emitted behavior than to assume there are no such signals, particularly in view of the fact that we can often discover them if we look.

Even in terms of Skinner's theory this conception, that elaborate response series may be set in action by the "hair-trigger" release of a single initial response which determines the rest, makes rather meaningless a description of elementary learning in terms of a continuum of response strength, a function of number of reinforcements. The fact that response probability is

demonstrably a function of number of reinforcements is dependent on accidents of the stimulus situation. We can describe situations and record situations and control situations only in general terms, not in all their details. The accidental details are ground enough for occurrence or nonoccurrence of the response. The curves of learning are discovered when we attempt the repetition of situations over and over again. They are more rationally explained as dependent on the accidents of a situation that is not controlled, and as representing the development of skills, than as increasing the strength of an association.

What I am trying to say can be illustrated by the four extraordinary responses of cat K which followed backing into the puzzle box. Other responses were stronger if we measure strength by percentage of occurrence. This one odd response occurred 4 times in 60. When it did not occur, the explanation is not that it was weak. When it does occur it is obviously as strong as the responses which did not occur but which have "strengths" higher than 4 in 50. Four out of 50 really represents the observed incidence of a bad start in the trial.

If we accept this theoretical analysis and recognize that whether or not a particular response occurs depends on the occurrence or nonoccurrence of a "trigger response" which starts a chain of movements linked by the fact of one previous occurrence, we then in turn examine the nature of this trigger response. Is it basically different from the following responses in that it is the result of a different kind of conditioning? Skinner would say that it is different. It is an emitted response, whereas the others are "respondent" to stimuli.

Whether or not it is different, it is obviously of peculiar importance because its occurrence or nonoccurrence determines whole trains of action. The theory in this book represents this important determining response as an "all-or-nothing" event like the conduction of a nervous impulse or the ringing of an electric bell which depends on the making of contact in a switch. The responses made by our cats in the puzzle box were about as rigorous on the first occasion as on later ones. Their latency, in the sense of the period between stimulus and response, was not directly observed, but since a reduction in all the latencies of a chain response would result in speeding up the action, we judge that the latencies were not reduced. The action did not speed up. The total action, it is true, was enormously reduced in time and extent, but this was the effect of the progressive elimination of whole series of movements. How this elimination could occur has been discussed. The reader will remember that it was believed to follow the establishment of new associations while in the puzzle box, and the consequent elimination of the behavior involved in the old associations. This establishment of new associations and new responses to the features of the puzzle-box situation is possible so long as the animal remains in the box.

The odd behavior of cat K in trials 8, 16, 26, and 45 *did not increase or decrease its strength of association.* The association was probably just as strong six months after trial 45 as it was on that trial. The point is that the behavior simply can not occur unless the signals occur. The occurrence or nonoccurrence of responses in general need not be interpreted in terms of strength of association because their frequency is so obviously a function of situation rather than response.

The problems of perception and cognition which concern Tolman are not met in our present theory. We may note in passing that Tolman attempts no theory of how they occur but is content to call attention to the fact that they do occur. What our theory does is to recognize that somehow the learning organism can establish stimulus patterns as functional units which elicit response and that response patterns likewise are functionally integrated by one occurrence and can from then on be elicited as whole patterns. How this is achieved is not explained here. Many machine analogies are available. Research in this

field has been conducted recently in the Gestalt tradition without speculation as to underlying mechanisms. The doctrine of *Prägnantz* is like the Aristotelian doctrine of entelechy which calls attention to the tendency toward a given outcome with no attention to the means by which it is achieved.

The present theory takes this ability for granted. The principle of association was stated thus: A stimulus pattern accompanying a response will on a later occasion tend to elicit that response. This assumes that the pattern of stimuli somehow becomes a functional signal *as a pattern* and that the response has similarly become a functional unit. We may note that a perception consists in the establishment of a response toward a class of stimulus patterns. We may note further that conceptions are response features or aspects attached to language symbols. We may also note that both are always instances of associative learning.

There is one further item of interest in the behavior of the cats in the puzzle box. In the original report we distinguished what we called "advertent responses" from "inadvertent responses." By this we meant that the final successful escape movement was in some instances an act like clawing or biting the post. This involved looking at the post and reaching out for it. Such an act has the characteristics that Wiener has described as involving a feedback control like the control of an automatic pilot. As the cat reaches for the post either ready to bite or ready to claw, the movement is continuously corrected by vision and therefore may be executed from a small variety of stances or of distances from the post. But when the escape was achieved by an inadvertent contact such as backing into the post or striking it with the flank in executing a turn, success was restricted to a narrower range of movements.

Advertent solutions transferred more readily to the post in a new position. The cat did not bite or claw the air in the old spot. The animal was more likely to look around and on seeing the post in a new position approach it and perform a guided act like biting or clawing. When escape had been inadvertent, this transfer to a new position did not take place and the cat repeatedly backed into the place where the post had been.

Advertent solutions obviously have many of the qualities that interest Tolman (and should interest others). They can be described as expectancies or as perceptions of means-end relationships. It is our belief that in associating the act of reaching with the sight of the post, tendencies to reach out may through previous practice be conditioned on vision and visual orientation, and serve as maintaining stimuli for a sustained reaction to the post which has the same trial-and-error components as has the automatic pilot of the plane or steamship. Reaching out and touching is a skill with much practice behind it, and it is also a behavior mode which exhibits cybernetic control. When the telephone rings we ultimately reach the instrument even if our chair is in a new spot and we must follow a course which never before has been followed. We respond to the bell by rising and by being ready to grasp the telephone, perhaps by being set to say "Hello." Seen obstacles are avoided. That avoiding seen obstacles is based on past training is evident from recent operations on children for cataract in which seen obstacles are not avoided.

In other words, association may result in acts as well as movements, and this is evident in cats as well as in men. The basic nature of the learning may be just as much an association of stimulus and response in an act that includes sustaining stimuli and cybernetic correction as it was in Pavlov's salivary responses. The automatic pilot, the thermostat, the governor of the engine—all illustrate the fact that physical analogies are available in which by setting a control we govern the later behavior of a complicated machine. In animal behavior we have only to assume that the setting, which can itself be a physical response, is itself subject to associative learning.