

Reinforcement Revisited: A Structural View

KARL H. PRIBRAM

Stanford University

IN ITS SIMPLEST TERMS, the question that has bothered psychologists is this: An animal learning to solve a problem makes some right movements, some wrong. What is it that reinforces the right ones and discourages the wrong? Why are the right ones remembered in later trials, the rest forgotten? This has been a persistently baffling problem; and its solution is essential to the theory of behavior.

The difficulty may seem trivial. If so, however, the reader may be begging the question by saying to himself in a common-sense (and animistic) way, that the animal of course can *see* that one of his movements has had the right effect, others not. But if the question is asked: What neural processes constitute the "seeing"? the solution is evidently as far off as ever. The simplicity of the question of reinforcement, or selective retention of some responses and not others, is like the simplicity of the fundamental postulates of mathematics that are taking centuries to ravel out [Hebb, 1949, pp. 173-174].

Rewards and punishments; reinforcers and deterrents; successes and failures; utilities and futilities: in one form or another psychologists are concerned with Thorndike's Law of Effect. As

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is so often the case when basic conceptions are involved, the picture remains incomplete and clarification is difficult until *neuropsychological* facts are exposed and brought to bear on the problems at issue. And with respect to reinforcement a great deal has already been done. My task today will be to re-examine this body of evidence, add something to it, and propose a view that may reasonably account for it.

First some current definitions and conceptions: (1) a reinforcing event increases the probability of recurrence of a response (Skinner, 1938); (2) reinforcement occurs by contiguity (Estes, 1959; Guthrie, 1942); (3) reinforcement accompanies drive reduction (Hull, 1951; Miller, 1951); (4) reinforcement is related to dissonance reduction (Lawrence & Festinger, 1962); (5) reinforcement informs the organism (Postman, 1953; Tolman, 1932); (6) reinforcement is a warm puppy (Schulz, 1962). These statements range from closely circular descriptions to hedonistic propositions. All are made in good faith by thoughtful and intelligent experimentalists and all have, at one time or another, been voiced in this series of symposia. Hypothesis: they can't all be all wrong in all ways.

A STRUCTURALIST LOOKS AT OPERANT CONDITIONING

"Reinforcement increases the probability of recurrence of a response." This simple definition, proposed by Skinner, has provided an excellent beginning and a steady guide to those who use operant techniques in the study of behavior. Because of its tight circularity, however, the definition leaves something to be desired when the question is asked: What is the neurological process concerned in reinforcement? But even here, experiments of the operant type can fruitfully initiate the inquiry.

Much has been done in this direction. According to the definition, behavior occurs and is followed by a contiguous event. This may be called the outcome or consequence of that behavior (which may be the case or may be acted on by the subject as if it were the case, i.e., as in a superstition). The contiguous event may "shape" the behavior. When this happens, the probability of recurrence of the contiguous response increases (and in many instances the probability of recurrence of other

responses in the subject's repertoire in that situation decreases). Whenever this constellation of environment-organism interactions is observed, the event consequent to the behavior is described as reinforcing the behavior. Note that whenever this same constellation of events occurs and the event that increases the probability of recurrence of a response antecedes it, the event is called a discriminative stimulus. One property of reinforcers, therefore, is that they are consequent to responses, and are often consequences of actions.

But reinforcing events per se do not affect behavior in a completely predictable fashion. To take an extreme case, one can set up an operant conditioning experiment in such a way that the number of reinforcements obtained during two equal-length testing sessions is the same, though in one the reinforcements are programed according to a fixed ratio, and in the other, according to a fixed interval schedule. The behavior displayed by the organism in these two situations is strikingly different. On the ratio controlled task, the subject shows a continuous, linear, and stable performance curve. Performance controlled by the fixed interval program is cyclical, the curve showing scallop due to a crescendo of activity which reaches a maximum at the moment reinforcement is due. Reinforcements, therefore, have a second important property related to the first: they control behavior through their temporal organization, that is the way they are scheduled or programed. Reinforcers are *consequences.*

Productive experimental analysis of reinforcement based on the initial definition has been undertaken by David Premack (1959). Premack begins by measuring the rate of a response in a given situation (e.g., the rate of eating) and comparing this rate with another, obtained independently (e.g., the rate of lever pressing per se). He suggests that reinforcement occurs whenever the response with the lower independent rate (lever pressing) coincides, within temporal limits, with the stimuli that govern the occurrence of the response with the higher independent rate (eating). An ingenious set of experiments has been presented in support of this view. One of these is of especial interest (Premack, 1962):

Parameters were identified for the rat which both made drinking more probable than running and running more probable than drinking. In the same subjects, depending upon which parameters were used, running reinforced drinking and drinking reinforced running. This relationship suggests that a "reward" is simply any response that is independently more probable than another response [p. 255].

Specifically, an activity wheel equipped with a brake and a retractable drinkometer were used:

Drinking contingent upon running was arranged by retracting the drinkometer, freeing the wheel and making availability of the drinkometer contingent upon running. Conversely, running contingent upon drinking was arranged by locking the wheel, moving in the drinkometer, and making release of the wheel contingent upon drinking [p. 255].

Other instances of such reversals among reinforcers will be discussed below in relation to the means-ends problem. Here, I should rather turn to two other matters. First, Premack's experiments and his analysis clearly establish that a response sequence is reinforcing to the extent that it occurs in the context of another response sequence (of lower independent rate). This contextual relationships adds a new and important dimension to the definition of reinforcement.

Second, Premack discusses only the relation between responses. He fails to define fully the immediate operations that define response. Response, in an operant situation, is the indicator of behavior, the indicator that the organism has acted in and on the situation. The action includes not only the patterned muscular contraction (movement) of the organism but the consequences of that movement. (In fact the response, the indicator of the action, is one of these consequences.) The response rates studied by Premack refer not so much, therefore, to the rapidity with which the movements of the organism take place, but to the rapidity with which some reliably observed consequences of these movements can be recorded. For instance, in one set of experiments, a Cebus monkey was used. The monkey might well have been smacking his lips, circling in the cage, or turning somersaults. These were irrelevant movements—and not recorded in the situation as responses, since manipulation of lever, door,

and bin were the actions under study. And the particular movements involved in these actions are also pretty much irrelevant—the monkey could use his right or left hand, his feet, or even his head to accomplish the response.

What I want to emphasize is that the response, as used in the operant situation, is an indicator of the stimulus aspects of the action—that is, the consequences of that action. Premack's contribution—that response sequences occurring in the context of other response sequences are reinforcing—may thus be more generally restated: reinforcements are *con-sequences* of behavior, that is, event sequences that occur in the context of other event sequences.

THE STRUCTURE OF CONTIGUITY—SOME PSYCHOPHYSIOLOGICAL FACTS

Reinforcement occurs by contiguity. Assuredly, the proponents of togetherness could not mean just this. And yet when one reviews Guthrie and Estes, this is exactly what they mean and hope to mean. Let us listen for a moment to Guthrie: "A stimulus pattern that is acting at the time of response will, if it recurs, tend to produce that response" (Guthrie, 1942, p. 23). Estes' (1958) beautiful analysis, at the 1958 symposium, of the drive-stimulus explanation of drive is certainly in this vein. Guthrie and Estes differ, however, on one point. For Guthrie, "we learn only what we do" (p. 24), and "a student does not learn what was in a lecture or in a book. He learns only what the lecture or book caused him to do" (p. 55). For Guthrie, behavior becomes "its own chief guide" (p. 37) by way of its consequences (movement produced stimuli). For Estes, behavior (i.e., the probability of any response) is guided by sets of stimuli sampled probabilistically, each with its own weight (a parameter determined from the data). However, Estes does not do away with conditions of reinforcement—these "are such that drive cues and experimentally controlled signals will become conditioned (associated) to the same responses" (p. 46). More of this later. Here the point is that we meet contiguity again and again, yet there is a definite difference in emphasis! Guthrie emphasizes response consequences;

Estes, stimulus association. Perhaps in this difference lies the heart of the problem.

What happens when experimentally controlled signals are repetitiously presented to an organism in a constant situation? The organism habituates.

Habituation has received a good deal of attention from neurophysiologists and psychophysicists recently, with the result that our conception of the process has altered radically. One critical experiment was performed in Moscow by Eugene Sokolov (1960). A tone beep of specified intensity and duration was presented at irregular intervals to a subject whose electroencephalogram, galvanic skin response and plethysmographic record were traced. At the onset of such an experiment characteristic changes in these traces are observed. These accompany behavioral alerting and are known as the orienting reaction. As the experiment proceeds, these indices of orienting become progressively more attenuated until the beep of the tone no longer seems to have any effect. This is habituation. At this point Sokolov reduced the intensity of the tone without changing any of its other characteristics. Immediately the electrical traces from the subject signalled an orienting reaction. Sokolov reasoned, therefore, that habituation could not be simply some type of fatiguing of sensory and neural elements. Rather, a process must be set up in the central nervous system against which incoming sensory signals are matched. Any *change* in signal would result in the orienting reaction. He tested his idea by habituating his subjects anew and then shortening the tone beep. Now the orienting reaction occurred at the moment the shortened beep ended. The electrical traces showed the alerting reactions to the period of silence.

These results do not stand alone. For instance, Leitvin et al. (1961) have shown, by use of microelectrodes, that there are nerve cells in the frog's optic lobe that respond by a burst of activity whenever a novel object enters the frog's visual field. The activity of these cells returns to baseline fairly rapidly when the object remains in the field or is repetitiously presented.

There is thus ample evidence for the occurrence of some process in the central nervous system and its appendages against which incoming signals are matched. The process is gradually

built up; it may be conceived as a coded representation of prior signals generated by organism-environment interaction; it is subject to alteration by signals of mismatch; (i.e., a partial match); it leads to "expectancies" of the environment by the organism. Such a process has been stated mathematically (MacKay, 1956); its implications for psychology (e.g., in perceptual readiness) have been detailed (Bruner, 1957). Here it is necessary only to point to the facts of the process and to ask what its existence means for the contiguity position.

Contiguity theorists work on the assumption that behavior can be predicated from lawful relations between simultaneously occurring stimulus events and between these and responses. The facts of habituation show that a stimulus event arises from a partial match between a central process and an environmental occurrence. The central process in turn has been formed by the prior occurrence of partial matches between central process and environmental events. Another way of stating this is to say that at any moment in time the central process provides the context in which stimuli arise. Contiguity of stimuli comes to be seen not as some vague "association" but a process occurring as a context-content relationship. And if this is so, stimulus-contiguity theory and expectancy theory become brothers under the skin—that is, in the central nervous system.

The question is raised whether the habituation paradigm holds more generally when behavior (that is, responses) is under consideration. Although no answer can now be given, and work is badly needed in this area, the suspicion has been voiced that habituation and extinction have factors in common. For instance, Premack and Collier (1962), in an analysis of the nonreinforcement variables affecting response probability, find it necessary to state that:

There are at least several reports of unconditioned responses failing to show complete recovery following repeated elicitation. Although the topic has been little investigated, in one of the few pertinent studies, Dodge (1927) reported a partial but apparently irreversible decrement in both latency and magnitude of no less than the unconditioned patellar reflex. Further, the biological literature on habituation contains several cases in which presumably unconditioned responses, having undergone decre-

ment with repeated elicitation, failed to return to initial latency. . . . The question is . . . whether some degree of irreversible decrement is not more widely characteristic of behavior than is customarily assumed [p. 13].

Let us return to Guthrie and Estes. The emphasis in both the "behavior is its own guide" and the "stimulus sampling" approach must, in the light of these results, begin to stress the importance of the *temporal* organization of contiguity. Organisms do not respond to *any* occurrences that happen simultaneously, contiguously. Their behavior is guided by *stimuli*, including those consequent to behavior. And stimuli are context-determined events, "sampled" on the basis of a central process (a neural "set") determined by *prior* experience and by other central events. An organism's behavior is thus context determined, and is, as well, context determining; response produced events, the outcomes of behavior, consequences, are more than likely (i.e., have a high probability) to partially match a central process and thus act as stimuli—behavior thus becomes its own guide.

In summary, the organization of contiguity is a context-content structure. For the contiguity position, therefore, reinforcements come to be response sequent events occurring in context, that is, con-sequences.

DRIVE STRUCTURES AND THE "REAL" CNS

The most talked about conception of reinforcement is probably the drive-reduction hypothesis. This notion is based on the two-factor theory of drive—that physiological needs set up tensions in the organism (these, as a rule, are manifested in increased general activity); that behavior which reduces such tensions is reinforced. Some have argued (e.g., Sheffield et al., 1955) that organisms seek tension increase—that reinforcement thus accompanies tension increase. This argument does away with the need for the second factor. Drive and reinforcement are still considered covariant, however. Meanwhile, Estes (1958) has convincingly stated the case for a drive-stimulus rather than a drive-tension theory of drive. The question remains, what happens to reinforcement if drive stimuli are conceived to guide behavior directly and not via some tension-state mechanism. A partial answer has

been given in the last section: What constitutes a "stimulus" is not as simple as it seems on the surface—that is, external to the organism. But there is much more that can be said. In this area of problems especially, neurologically oriented psychologists have made their mark, and it would be a shame were their work all in vain just because of Estes' lovely analysis. It is not.

First, though, some comments on the organization of a drive-stimulus. This structure is fairly well worked out in many instances. Essentially, it results from the operation of a biased homeostat (Brazier, 1962; Pribram, 1960; Von Euler, 1961). Such an apparatus has a sensitive element—a receptor. Specialized areas sensitive to temperature, osmotic equilibrium, estrogen, glucose, and partial pressure of carbon dioxide are located around the midline ventricular system; these areas are connected to mechanisms which control the intake and output of the agent to which they are sensitive. The areas are embedded in a reticulum (the famous reticular activating systems) which can act to set the level (a bias) at which the sensitive mechanism throws the system into operation. As will become clear, other biases also come to regulate drives. In addition, the entire homeostat is often supplied with secondary, peripherally sensitive mechanisms which aid in the more finely calibrated regulations of the agents in question (e.g., to shunt the blood in vessels of the finger tips so as to provide greater cooling or to avoid extreme cooling, on the basis of a blood-finger temperature differential biased by the body's main thermostat).

Still more is known. Electrodes placed in the area sensitive to glucose show an increase in neural activity to occur not when the organism is deprived, but when he has just completed a meal (or has been given intravenous glucose). The idea has been expressed that the activity of the area is proportional to the amount of glucose actively metabolized in the liver. On the other hand, when electrodes are placed in a region lateral to the sensitive area, the converse is found. The activation of the sensitive area inhibits the activity of the lateral region to which it is connected. Whereas destructions around the sensitive mechanism lead to overeating, those in the laterally placed region produce an animal who will not eat at all. And it is the amount

of electrical activity that can be recorded from this lateral region that correlates directly with the amount of deprivation of the organism. This region is largely composed of several crossing tracts of fibers in passage—which suggests that the inhibitory effects of the activity of the sensitive area are distributed among several locations in the central nervous system, that is, to the cells of origin of the fibers in question.

But the most striking contribution to the neuropsychology of the drive related theories of reinforcement has not been in the specification of drive stimuli but in producing reinforcement directly with central electrical excitations. Before reviewing these results, however, it becomes advisable to take one more look at the drive-induces-tension, or activation, notion which generated the experiments and has been used in their interpretation. Perhaps I can present my own views on the matter best by using an analogy.

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The seashore is a favorite haunt. In northern California, ocean swimming is unfortunately considerably restricted along most of the coast, not only by the recent appearance of sharks but by an extremely dangerous undertow caused by the surf. The beauty of this tremendous, awesome, powerful, cyclical oceanic activity has inspired many (e.g., *The Ninth Wave*, Burdick, 1956). I was therefore particularly impressed one stormy day while flying, to observe a peaceful coastal scene composed in part by series of stable, standing wave patterns. Could it be that those who concern themselves with drive solely as activation fail to see its structure because they maintain too close a view of the particular data they wish to describe? If this is so, a more complete encompass should, without denying the validity of the narrower set of problems, be able to include them in the wider formulation. And having said this, perhaps it will be possible to take the longer view without losing the feel of excitement and turbulence of participation in the storm-blown scene below. Be that as it may, the distinction, recently succinctly presented by Roger Brown (1962), between homeostatic and drive (i.e., tension) theories will come up more and more frequently in the next sections.

Olds and Milner (1954) discovered that an animal would

press a lever in order to electrically excite certain parts of its brain. In his presentation in this series of symposia, Olds (1955) distinguished between physiological need, drive, reward, and punishment, and stated that the last three might all covary, or that any two might, or that each might require separate treatment. His own view was that reward and punishment were probably separable and that each would have a drive component. For Olds, physiological needs were drive stimuli; drive resulted in general activity of the organism; reward and punishment served as response selectors.

More recently, Deutsch (1960) has examined the self-stimulation effect in the light of his own theory, which also demands separation of drive and reinforcement (i.e., reward and punishment) factors. In Deutsch's theory, drive is directly derived from need and therefore can be equated with Estes' and Olds' drive stimuli. However, in the Deutsch mechanism, excitation derived from drive-stimuli and that derived from reinforcing stimuli (initiated in peripherally located receptors) are apt to display different properties.

He has made good use of this presumed difference. In a recent series of studies, he has clearly manipulated the self-stimulation effect in such a way as to produce "drive" effects that are different from "reinforcement" effects. These have been recently reviewed (Deutsch & Howarth, in press). Essentially, three sets of ingenious experiments are reported: (1) extinction of lever pressing and maze running was found to be a function of time from the last brain excitation, and not of the number of unreinforced lever presses or maze runs; (2) extinction of lever pressing (and maze running) was found to be delayed or interrupted by interposing conditions that would "normally" produce lever pressing (or maze running) e.g., by an aversive stimulus; (3) extinction was delayed by low voltage or low frequency, but hastened by high voltage or high frequency trains of excitation, administered to the electrode independent of response. These results show that the central nervous system referents of drive and reinforcing events can be separately manipulated—that any exclusive definition of one in terms of the other (as in the drive related theories) is difficult to maintain.

Deutsch
Howarth

What then is the relation between reinforcement and drive? Deutsch suggests that afferent excitation derived from peripheral sensory receptors converges on a neural "link" which has already been connected to the drive stimulus. Another way of stating this is that the reinforcing event acts on a neural mechanism preset by the drive stimulus. M. D. Egger and Neal Miller (1963) have recently reported a series of experiments which they interpret in somewhat similar terms: the reinforcing event gives information about the drive state. In both statements the drive structure is conceived to preset the organism—to provide the context within which the reinforcing events are to operate.

Drive, structured as a biased homeostat, thus serves in turn as the bias or setting for the consequences of behavior. These, as will be described in the following section, have a structure of their own—a structure not dissimilar to that of the biased homeostat. For the present, it suffices to make clear that the drive structure can, and does in the experiments cited, bias the consequences of behavior.

But there is more. To turn again to the nervous system, Olds, in his presentation to this series of symposia (1955), detailed the anatomical systems of the forebrain from which self-stimulation could be obtained in his hands. These structures, formally called rhinencephalic, have become more widely known as the limbic areas since their relation to olfaction is relatively limited. Olds reviews the evidence:

As we mentioned earlier anatomical and physiological evidence can be cited to indicate that structures related closely or remotely to olfaction are divided into three systems: System I has direct connection with the olfactory bulb. It contains none of the structures we have discussed. System II including septal area and some amygdaloid nuclei, is connected with system I but not with the olfactory bulb. Further, it is related to the anterior hypothalamus. This system has been implicated in diverse functions: olfactory, gustatory, metabolic, socioemotional.

Finally, system III is defined by having connections to system II but none to the first system or to the olfactory bulb. It includes the hippocampus, and the cingulate gyrus; and it is connected anatomically to the posterior hypothalamus and the anterior thalamus.

Now, the interesting fact is that our Skinner box tests, which

were not even conceived when Pribram and Kruger (1954) classified limbic structures, validate the distinction between second and third system [pp. 120-121].

The validation to which Olds refers is that the rate of lever pressing increased over 50 per cent when electrodes were implanted in system II; when the implant location was system III rates increased from 20-32 per cent. No endbrain stimulations in locations other than these reliably gave an increase in response rate—of eighty-six electrode placements there were only two (one on the edge of the lateral geniculate nucleus, the other in the white matter of the cerebrum) from which a spurious increase in response rate occurred, and this sporadically (not every day). I have reviewed elsewhere (Pribram, 1960) in considerable detail the evidence that concerns the presumed functions of these limbic systems. This evidence need not, therefore, be restated here. The generalization suggested from the review, relevant here, was that these systems function in an especial way in the execution of behavior sequences, particularly those involved in feeding, fleeing, fighting, mating, and maternal behavior. This portion of the review concludes:

Analysis of the neural mechanism that underlies the execution of sequences of actions has just begun. Electrical changes have been recorded from the amygdaloid complex of the limbic systems whenever the organism is exposed to a novel event or one that has meaning in terms of reward and punishment (Grastyan, 1959; John & Killam, 1959). These electrical changes subside once the organism is familiar with the event unless the hippocampal formation of the limbic systems has been ablated, in which case electrical changes continue to occur when this or any other event takes place. The amygdaloid complex is necessary to the establishment of electrocortical conditioned responses. The suggestion has been made that the hippocampal formation inhibits (perhaps by way of the reticular core of the brain stem) the succession of unrelated inputs to the amygdala that might occur and so allows this structure to maintain the neural activity necessary to the conditioning process. In a conditioning or learning situation, electrical changes are recorded from the hippocampal formation during the initial trials; later, no such changes accompany successful action; they occur only when errors are made (Adey, 1959) [Pribram, 1960, pp. 13-14].

Currently, the evidence continues to accrue. Kimble (1963) compared the behavior of rats who had had the hippocampus removed bilaterally with that of isocortically operated and unoperated control groups. He showed an increase for the "hippocampal" group in repetitive running in an open field situation; poorer performance of these subjects on a successive brightness discrimination task; and a greater number of errors in Hebb-Williams mazes.

Qualitative differences between the hippocampal Ss and the other two groups were also observed. The hippocampal Ss initially ran rapidly along the perimeter of the open field, stopping only rarely. They typically traversed the interior of the field only after 2-5 min. The most striking characteristic of their behavior was an extremely repetitive running pattern. The behavior of the other two groups differed radically from that of the hippocampal group. It consisted of "bursts" and "stops." A typical performance was to run to one wall, explore around the perimeter of the field once or twice, stop and groom, stand up on the hind legs and sniff, run out into the center of the field, explore in a seemingly random fashion, and return to a corner for more grooming and occasional crouching [p. 274].

Subsequently Kimble has gone on to examine more directly the effect of hippocampal removals (Fig. 1), this time in monkeys. For this he made use of a new automated discrimination apparatus (DADTA). This allows ready trial by trial computational analysis of performance (Figs. 2 & 3) (Pribram et al., 1962). Groups of monkeys were trained on (1) a simple visual discrimination task; (2) two types of sequential tasks, in one of which the order of "correct" choice was completely predetermined, while in the other "incorrect" choice was limited to those occasions where the animal chose a cue repetitiously; and (3) discrimination tasks in which trials were spaced from five seconds to six minutes. No difference between a "hippocampal" group of four subjects and operated controls appeared in the performance of any of the discriminations (tasks 1 & 3) (Fig. 4); the hippocampal group was, however, markedly defective (Figs. 5 & 6) in the performance of both sequential tasks (Kimble & Pribram, 1963).

Interestingly, improved performance could be obtained if the consequences of each panel press were accentuated by dim-

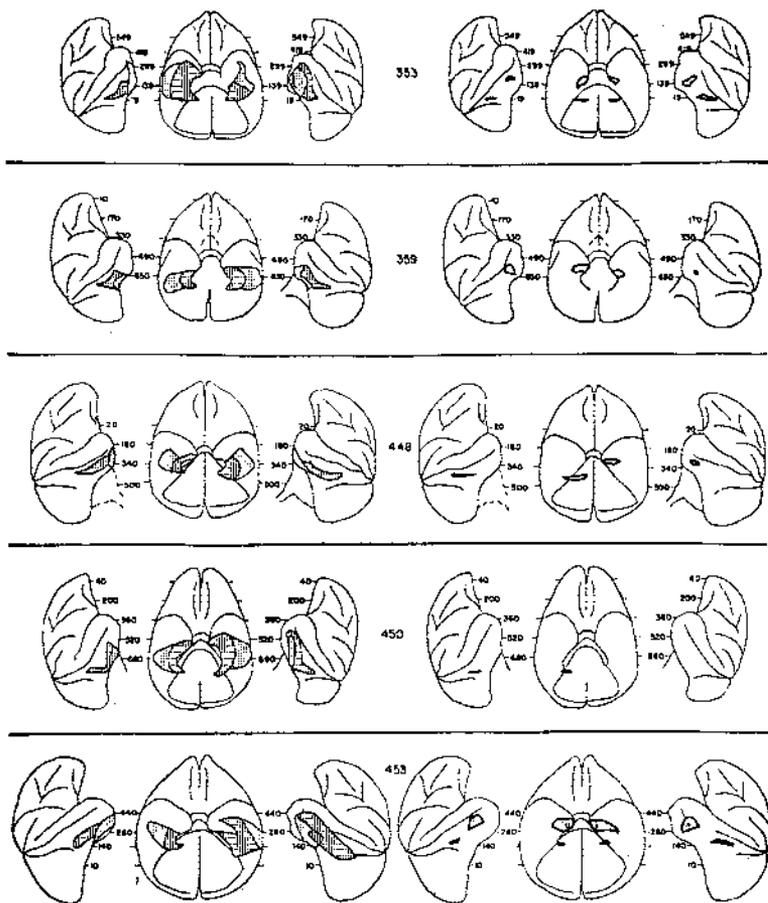


Fig. 1.

ming the "housetlight" in the test cage. Both the control subjects and those with hippocampal lesions showed improvement—that of the hippocampal group sent their performance above the chance level.

Habituation and dishabituation (orienting) is also affected by limbic lesions (Kimble & Bagshaw, unpublished data). Amygdalectomy markedly reduces the number of galvanic skin responses to a novel stimulus. The time-course of habituation and

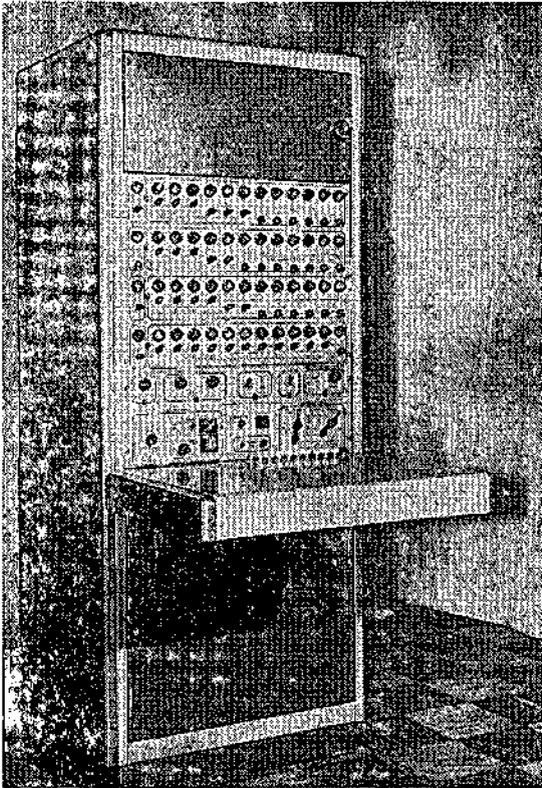


Fig. 2.

subsequent dishabituation appears approximately normal. Bilateral removal of the hippocampus on the other hand appears to alter only dishabituation (Kimble & Bagshaw, unpublished data). The first change in stimulus conditions produces no reaction; a subsequent change, however, results in a marked increase in the number of galvanic skin responses, at least double that of the controls. This lowered reactivity to the initial change may account for the failure to perform the behavior sequences unless each consequent event is doubly signaled.

Now it remains to be shown how the results of these experiments on behavior sequences and habituation relate to those that have demonstrated reinforcement by way of self-stimu-

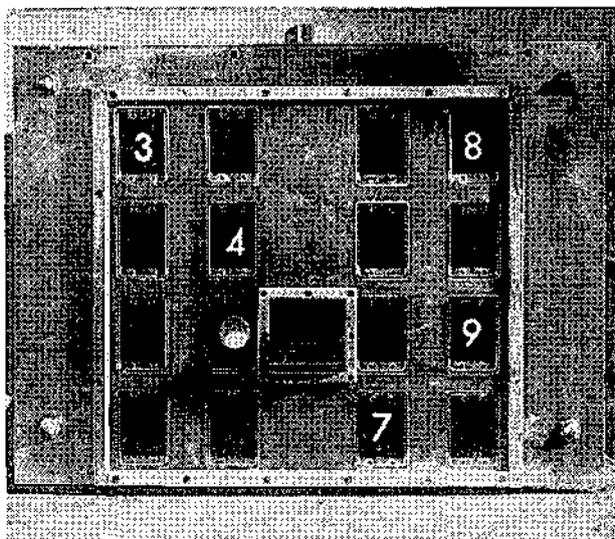


Fig. 3.

lation of limbic formations. Again, a suggestion has come from the laboratory.

Ross Adey (1960) has studied the electrical activity that can be recorded from the hippocampal formation of cats during learning of a simple visual discrimination task. Very careful but complicated analysis has led him to venture that the phase relations between wave patterns recorded from the deeper and the more superficial portions of the hippocampal cortex change as a function of task performance. Early, while many errors are made, the activity recorded from the deeper layers of the hippocampal cortex precedes that from the more superficial layers; later, when performance contains many error-free runs, the reverse is the case. Input to the deeper layers is from other core structures of the brain; input to the more superficial layers is from the adjacent entorhinal and cingulate cortex.

Despite the preliminary nature which this datum must have because of the state of the computing art in neurobiological science, it nonetheless strikes a responsive chord. This is especially so since Flynn, MacLean, and Kim (1961) had concluded in their

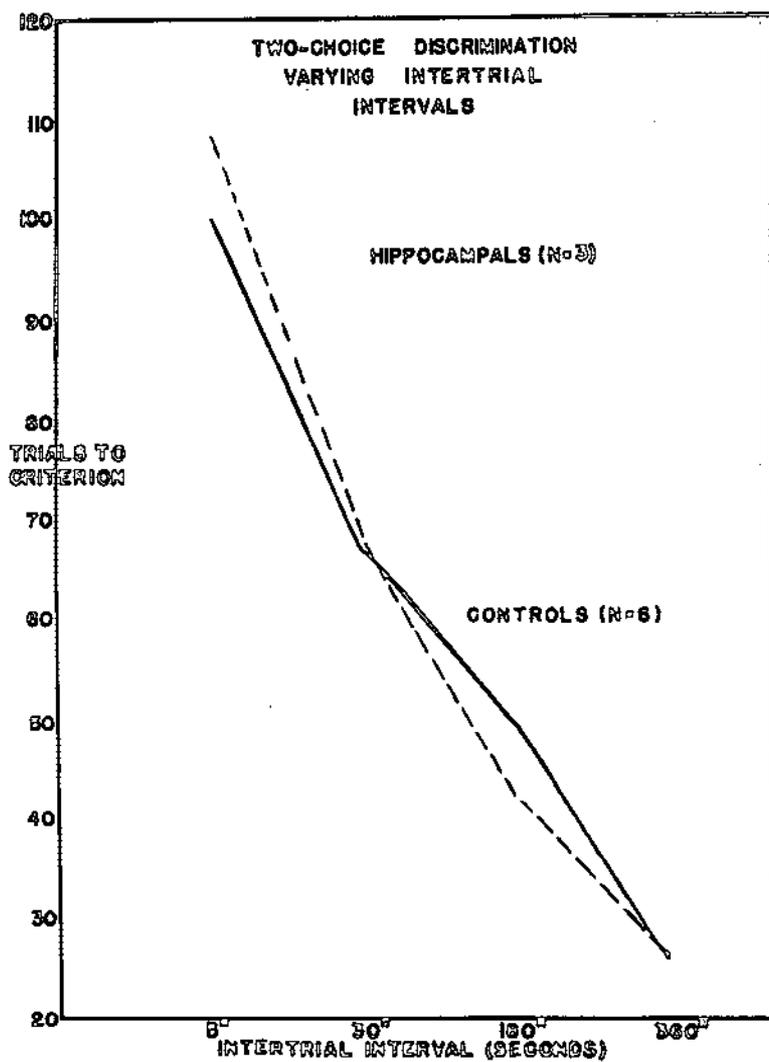


Fig. 4.

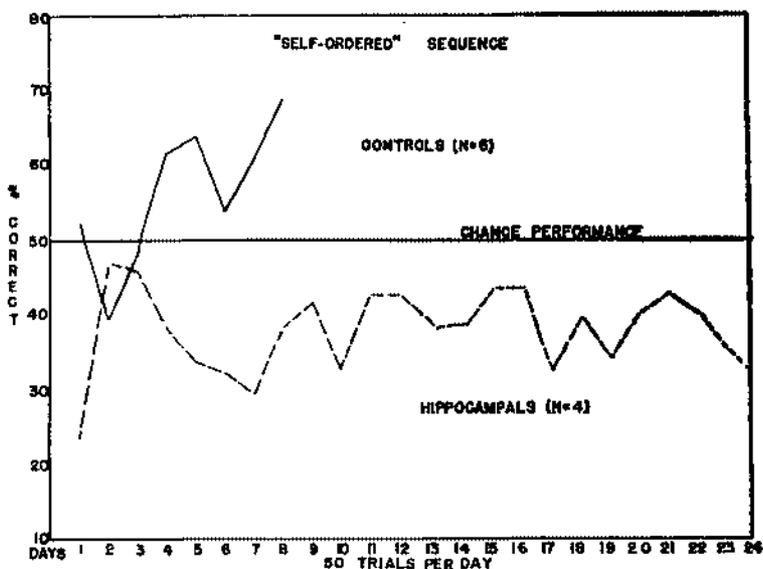


Fig. 5.

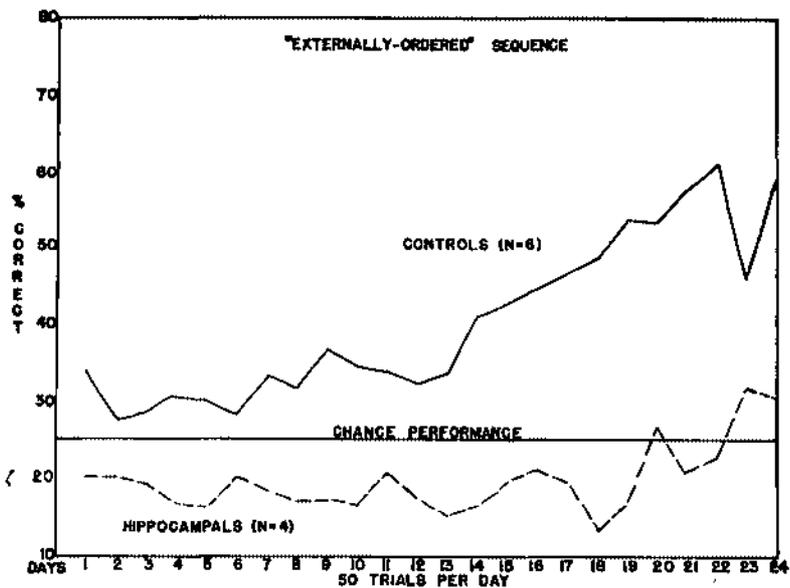


Fig. 6.

pioneering work on the effects on behavior of afterdischarges produced by electrical stimulation of the hippocampus:

Is it possible that the neural perturbations remaining in these structures after sensory stimulation allow a more ready association of two temporally separated events than is possible in the neocortex, where one does not see a comparable phenomenon [p. 386]?

In addition, Freeman (in press) has, using an entirely different technique, reported a somewhat similar "comparator" process to account for electrical phenomena recorded from the pyriform cortex (cats) just prior to the performance of a conditioned response.

The responsive chord is this. Earlier, the suggestion was made that drive structures provide the context within which reinforcement can occur. Adey's experiments demonstrate the possibility that during learning this relationship between drive and reinforcement could be reversed. Our experiments (Kimble & Pribram, 1963) indicate that the external, situational stimulus events consequent to behavior are considerably less effective in guiding subsequent responses after hippocampal lesions. These consequent stimuli may, in the absence of the hippocampal formation, in the experienced organism, have difficulty in establishing the context in which drive stimuli can be reinforcing. The proposal is that the limbic systems serve in reversing, as a function of experience, the context-content relationship between drive-stimuli and other reinforcing events. There is some evidence that other than drive-stimuli are involved in this limbic system function. The stimuli in question may perhaps be only those close relatives of drive-stimuli such as olfaction (Freeman, 1960) and taste (Bagshaw & Pribram, 1953; Benjamin & Pfaffmann, 1955; Pfaffmann, 1955, 1961); but behavioral evidence (deficits on alternation tasks that follow hippocampal and cingulate resections [Pribram, Wilson & Connors, 1962]) suggests that the stimuli affected are of a still wider range. The full meaning of this type of reversal in the context-content relationship among stimuli will become more evident in the next sections. For the present, the fact suffices that a mechanism exists within which such reversal between drive and external stimuli is demonstrated to take place.

To summarize: (1) The neural structure of a drive-stimulus is a biased homeostat. (2) Electrical self-stimulation has been shown to be analyzable into at least two components: one probably related to drive stimuli, the other to reinforcement. (3) The limbic systems of the forebrain, prominent loci from which the electrical self-stimulation effect is obtained, are also essential to the proper performance of behavior sequences. (4) Some evidence is at hand that these systems make possible the juxtaposition of two sets or classes of inputs. These certainly include the neural consequences of drive stimuli, and the neural consequences of actions on experimentally arranged task stimuli—but there is also evidence (e.g., Schwartzbaum & Pribram, 1960) that the juxtaposition of any two sets of experimentally arranged stimuli (as in a transfer of training task) may also be accomplished through the office of limbic system mechanisms. (5) The analysis of the electrical concomitants of task performance suggests that such juxtaposition is effected through a "comparison" mechanism, not through a blending (or simple compounding) of signals. (6) Apparently, a reversal of precedence of stimulus events occurs as a function of learning. This suggests that a reversal of the context-content relationship among stimulus sets is effected and is reminiscent of Premack's experiment on the reversal between the reinforcing role of drinking and running. (7) On this basis, the suggestion is made that the outcomes of actions initially biased by drive stimuli can, through experience, come to bias them. Apparently, the brain behaves truly as it must, if sequences of events in the context of other sequences of events, guide behavior; that is, if consequences reinforce.

PERFORMANCE THEORY: ADDICTIONANCE AND EFFECTANCE

But there is more to the problem of reinforcement than to meet the concerns of drive theorists. This past year, Lawrence & Festinger (1962) have made an important contribution: faced with an impasse in the learning theory derived from animal experiments, they have made a statement about rat behavior in the context of a theory designed to cope with the behavior of man.

The impasse is this: response strength, that is, the probability

that a response should recur, ought to be, according to learning theory, proportional to the occurrence and immediacy of appropriate reward and inversely related to the effort expended to obtain that reward.

Recently, there has accumulated a considerable body of experimental evidence suggesting that these common assumptions underlying learning theory fail to give an adequate description of changes in response strength. In fact, there is the suggestion that under some circumstances the variables of reward, temporal delay, and effort may have just the opposite effects from those predicted by the assumptions . . . [p. 6]

This impasse is not a new one for those working in a physiological orientation. Miller, Bailey, & Stevenson (1950) found that the lesions of the ventromedial region of the hypothalamus which produce the syndrome of hypothalamic hyperphagia (overeating) do *not* induce the subject to work more for food; on the contrary, these animals will work *less* than their controls under similar conditions of deprivation. The conditions that determine this dissociation have been fully explored (e.g., Teitelbaum & Epstein, 1962). In another series of experiments, performed in our laboratories, the variables that determine the response sequence displayed in the "fixed interval" reinforcement situation were analyzed. Over-all rate of response, dependent on deprivation, was found to be sensitive to limbic system lesions, but the distribution of per cent of responses in a "scallop" was unaltered by either deprivation or limbic area destructions. Frontal isocortical lesions, on the other hand, altered the "scallop" without changing over-all rate of response (or, for that matter, the rate of reinforcement).

The evidence to which Lawrence and Festinger address themselves, however, concerns what happens during extinction. In essence, the more the effort, and the fewer and more delayed the rewards, the slower the learning, but the more resistant the behavior will be to extinction. In operant situations, behavior established under conditions of continuous reinforcement extinguishes almost immediately; behavior established under conditions of variable schedules of reinforcement is more resistant to alterations of scheduling.

In their experimental and logical analysis, Lawrence and Festinger dispose of several prior explanations forwarded to resolve this impasse. The resistance to extinction cannot be simply conceived as dependent on a failure in discrimination, an insufficiency of information, or the development of competing responses. Rather, they propose that, when "competing," "incongruent," "dissonant" sets of information are supplied to the organism, its behavior persists beyond the point expected if either set were alone operative: an animal that, during a non-choice situation, experiences a set of conditions that he is shown to choose *less* frequently in a free-choice situation, will show increased resistance to extinction. In man, the "dissonant" state is stated to arise when two cognitions, that is, two sets of information, would suggest two different actions. Most of the body of experiment and, I believe, much of the spirit of the argument, is concerned with the state aroused when such dissonant sets occur *in sequence*. For instance, dissonance is aroused when the result of taking some action does not lead to consequences which sufficiently "justify" the action taken. If the set of information consequent to the action were available beforehand, and choice were free, the action would not have been undertaken. Another way of stating this is to say that expectations were not met by consequences—that antecedent and reinforcing events do not match—that dissonance results from this mismatch.

Dissonance reduction can come about in two ways: (1) the organism "can convert the consequences of the act into something that 'justifies the action'" or (2) it "can change its behavior so that it becomes consonant with the consequence experienced (e.g., the animal may, during extinction, refuse to run, provided this does not lead to consequences still more dissonant). The question remains as to what consequences "justify" action (Lawrence & Festinger's "extra attractions") and what it means to say that behavior "becomes consonant with the consequences experienced."

A few observations are in order. It is common knowledge among morphine addicts that very often the strength of the addiction is proportional to the amount of "hustling" that is required to obtain the drug. In fact, in most cases, patients who

have had prolonged morphine therapy and who go through withdrawal symptoms when treatment is discontinued, have an (understandable) aversion to the drug and addiction is not a particularly severe problem. Recent experiments with rhesus monkeys suggest that the situation is not much different here (Clark & Polish, 1960; Clark, Schuster, and Brady, 1961). ("Personality" variables, of course, play a considerable role; yet the over-all observation holds sufficiently to affect the laws regulating morphine distribution, e.g., in the British Isles.) The similarity to the dissonance producing paradigm is unmistakable (thus the term "addictionance" theory). And the observation thus leaves us with the same unsettled and unsettling questions.

Could it be that activity per se is rewarding? This makes little sense, for it would not account for the difference between, say, "hustling" and unordered, random hyperactivity. And here we may have a clue: Could it be that *ordered* activity per se is rewarding? And again, what can be meant by "ordered activity"?—certainly not patterned muscular contractions, since these are equally manifest when we observe random activity. No, clearly, when the *consequences* of action become orderly, consonant, that is, sequences appearing *in context*, then and only then is activity ("judged") rewarding, that is, reinforcing.

Support for this view comes from another quarter. Mace (1961) has called attention to the fact that, in an affluent society, man and beast tend to reverse the means-end relationship.

What happens when a man, or for that matter an animal, has no need to work for a living? . . . the simplest case is that of the domesticated cat—a paradigm of affluent living more extreme than that of the horse or the cow. All the basic needs of a domesticated cat are provided for almost before they are expressed. It is protected against danger and inclement weather. Its food is there before it is hungry or thirsty. What then does it do? How does it pass its time?

We might expect that having taken its food in a perfunctory way it would curl up on its cushion and sleep until faint internal stimulation gave some information of the need for another perfunctory meal. But no, it does not just sleep. It prowls the garden and the woods killing young birds and mice. It *enjoys* life in its own way. The fact that life can be enjoyed, and is most enjoyed, by many living beings in the state of affluence

(as defined) draws attention to the dramatic change that occurs in the working of the organic machinery at a certain stage of the evolutionary process. *This is the reversal of the means-end relation in behavior.* In the state of nature the cat must kill to live. In the state of affluence it lives to kill. This happens with men. When men have no need to work for a living there are broadly only two things left to them to do. They can "play" and they can cultivate the arts. These are their two ways of enjoying life. It is true that many men work because they enjoy it, but in this case "work" has changed its meaning. It has become a form of "play." "Play" is characteristically an activity which is engaged in for its own sake—without concern for utility or any further end. "Work" is characteristically activity in which effort is directed to the production of some utility in the simplest and easiest way. Hence the importance of ergonomics and work study—the objective of which is to reduce difficulty and save time. In play the activity is often directed to attaining a pointless objective in a difficult way, as when a golfer, using curious instruments, guides a small ball into a not much larger hole from remote distances and in the face of obstructions deliberately designed to make the operation as difficult as may be. This involves the reversal of the means-end relation. The "end"—getting the ball into the hole—is set up as a *means* to the new end, the real end, the enjoyment of difficult activity for its own sake [pp. 10–11].

A somewhat similar statement has been presented during this series of symposia by Robert W. White (1960). He emphasizes the role played by the progressive achievement of competence in the maintenance of behavior, and makes a strong case that the "feeling of efficacy" is an important guide to behavior.

Effectance is to be conceived as a neurogenic motive, in contrast to a viscerogenic one. It can be informally described as what the sensory-neuro-muscular system wants to do when it is not occupied with homeostatic business. Its adaptive significance lies in its promotion of spare-time behavior that leads to an extensive growth of competence, well beyond what could be learned in connection with drive-reduction [p. 103].

White is concerned with the implications of effectance in clinical psychology; here our concern is with what the sensory-neuro-muscular system "wants."

According to the foregoing analysis, the common problem for dissonance theory, addiction theory, means-end theory, and effect-

ance theory is that activities of a certain type appear to be self-maintaining in the face of situations which in a drive-reduction frame of reference would be predicted to extinguish the behavior. In the previous section, the relation between drive and reinforcement was shown to be one of sequentially occurring events set in context (context provided either by the drive-stimuli or the environmental stimuli, "comparison" taking place in the limbic systems). In the present section, the relation between action and reinforcement has been found to be somewhat similar—in the case of action, however, the consequences of the actions must provide their own set within which a subsequent event will be consequent—that is, reinforcing.

In many respects, what has been discussed in the latter half of this section is the development of behavior differentiation—that is, skill. Effectance and competence, play and gamesmanship, demand precise timing of actions within larger sequences of actions, so that consequences—sequences in context—will form a harmonious production. And a great deal is known about the neurology of skill. Here, perhaps, more than anywhere else, the model of "sequence in context" can be realized in tissue—and, in fact, the model was originally devised to handle some new neurological facts in this area [Miller, Galanter & Pribram, 1960].

At the reflex level, control of muscular contraction can no longer be conceived simply in terms of the reflex arc (some excitation of receptors, transmission of the signal aroused by such excitation to the central nervous system, and back again to the muscle in question). The change in conception is necessitated by the discovery that the activity of the γ efferent fibers, fibers that transmit signals from the central nervous system to the receptors in the muscle (muscle spindles), acts as a feedback, that is, controls the amount of activity recordable from the afferents that signal the state of the receptor to the central nervous system. The presence of this feedback loop makes it difficult at any moment in time to assess the origin of a particular amount of activity in the afferent nerves, and thus the state of the receptor. That state could reflect the state of contraction (isomorphic or isotonic) of its muscle group or it could reflect the amount of activity of the γ efferent system, or both. Only a

comparison between states at successive moments, in the context of γ efferent activity, will give a signal of the state of contraction of the muscle group. The γ efferent activity provides the setting, the context, the bias on the muscle receptor. (On occasion, the reverse may well be the case. The bias may be set by the muscle contraction and changes in γ efferent activity computed.) The feedback model, very similar to the biased homeostat, applies, therefore, not only to behaviors where drive stimuli are involved, but also to behaviors where the consequences of muscular contractions per se are under consideration.

Sherrington, in his classic lectures on the Integrative Action of the Nervous System (1906), was not unaware of the problem, and his statement of it is worth repeating (though his solution is cast in simple associative terms—reinforcement for Sherrington occurs through immediate spinal induction [summation through increased intensity and coextensity of convergent inputs]):

We note an orderly sequence of actions in the movement of animals, even in cases where every observer admits that the coordination is merely reflex. We see one act succeed another without confusion. Yet, tracing this sequence to its external causes, we recognize that the usual thing in nature is not for one exciting stimulus to begin immediately after another ceases, but for an array of environmental agents acting concurrently on the animal at any moment to exhibit correlative change in regard to it, so that one or other group of them becomes—generally by increase in intensity—temporarily prepotent. Thus here dominates now this group, now that group in turn. It may happen that one stimulus ceases coincidentally as another begins, but as a rule one stimulus overlaps another in regard to time. *Thus each reflex breaks in upon a condition of relative equilibrium, which latter is itself reflex.* In the simultaneous correlation of reflexes some reflexes combine harmoniously, being reactions that mutually reinforce [p. 120; italics supplied].

At the cerebral level, also, neurology has a great deal to say about skill. Removals of the precentral "motor" cortex of primates (including man) certainly results in awkward performance (Pribram et al., 1955-56). Equally disruptive are lesions of the cerebellar hemispheres, those portions of the cerebellum that are directly connected with the precentral cortex through the ventrolateral portion of the dorsal thalamus. The functional rela-

tionship between precentral cortex and cerebellum has been the subject of considerable work. An overbrief summary runs as in the following paragraph.

Each part of the precentral motor cortex controls the muscles, movements and action of a portion of the body. This part of the cortex also receives a direct input from the portion it controls. In addition, this same portion receives an input via the cerebellum. The assumption has been that a match between these inputs is made and that the signal resulting from this match activates the pyramidal tract through which muscle control is effected. And there is ample evidence to support the assumption. Awkwardness due to cerebellar lesions results in "intention tremor"—smooth performance is converted to a jerky sequence (almost as if a biased homeostatic mechanism were thrown into oscillation). And the effect of precentral cortex removals has been suggested to be a change in facilitation of reflex actions—in some experiments, disinhibition resulting in spasticity (Denny-Brown, 1948), in others, defacilitation (Lashley, 1924).

The suggestion is that the organization of action resembles the biased homeostat, the structure of drives. It follows that the bias of the neural mechanisms in control of action should be resettable, much as is the bias of the drive homeostats to produce the phenomenon of self-stimulation. This has been accomplished by John Lilly (1959). Prolonged trains of excitation (subliminal to those that would produce movement) were delivered to the precentral motor cortex whenever the lever was depressed by the subject (a monkey). Lever pressing had to be paced so that the on-off nature of the excitation could be maintained. The monkey learned to do this, however, and spent many (may I say "happy"?) hours at this occupation.

Obviously, the interrelations among the neural systems that regulate skilled, highly differentiated action are complex. It may be that careful perusal of the literature and a few critical experimental results could make out a clear hierarchical arrangement among systems in such a way that each locus of match between inputs serves as a setting within which the occurring events must operate. This is not the place for such an extensive analysis. It suffices to pose the problem and to point out in summary, that

a neural mechanism does exist whereby order can be achieved among actions and their consequences—again the brain is as it must be, since skilled behavior is possible.

This has been a long way from dissonance to effectance to skill. The point is simply that these areas of interest pose a common problem: how is it that selective behavior is maintained in the absence of guides from drive stimuli—or, in the extreme, when behavior apparently goes in a direction contrary to one plausibly related to drive stimuli? The suggestion made in this section is that the consequences of actions are truly stimulus events that occur in sequence and that, once some order has been initiated in this sequence of stimuli, this order per se can provide the set or context for the occurrence of the next or sub-subsequent event. Actions have consequences and the consequences of actions are reinforcers. Behavior, thus, becomes its own guide.

PERCEPTUAL PERFORMANCES: REINFORCEMENT AS INFORMATION PROCESSING

Would that this were all there need be said about reinforcement. But in 1960 I offered the suggestion that reinforcement is "the reverse of the coin of similarity" and, of course, many others, including Tolman (1932) and Postman (1953) in his contribution to this group of papers, have conceived of reinforcement as information. According to the hypothesis which guides this presentation, "we couldn't all have been all that wrong." Let me review the statement:

But perhaps the model has its greatest power in the description of what constitutes reinforcement for the organism. The posterior intrinsic mechanism, because of the hierarchical nature of its selective control over its own modification, allows a change in the representation to occur by trial and error. Whenever the error signal is such that the corrective change is not uniquely specified, the representation is modified to include this information, and trials continue. Thus an organism that possesses this mechanism can, given a relatively unchanging or slowly changing environment, search that environment for the additional information that is needed to make the organism fully informed. The neural model would thus account for the search through negative instances

as these are defined in the stimulus-sampling type of stochastic learning theories (Bush & Mostellar, 1951; Estes, 1950, 1955; Green, 1958), search by an information-hungry organism reinforced or satisfied only when corrective change of the representation is immediate and can be deduced uniquely from the error signal. Stated in this way, reinforcement becomes one side of the coin of similarity [Pribram, 1960, p. 18]!

There I was concerned with what Bruner (1957) has called perceptual readiness and Postman calls the arousal of perceptual response dispositions (1953, p. 86). Here I have been discussing reinforcement in relation to learning and to performance theories. Perhaps in this distinction lies the key to some of the problems that have remained puzzling.

The concept "reinforcement" was spawned by learning theory to account for the fact that performance-change is effected through consequences of actions as well as by the cues in a situation. The set of problems presented in the last section came to light when an impasse in learning theory was encountered. Facts of performance failed to fit the facts of learning in any simple fashion. However, as was shown, the concept "reinforcement" continued to be a useful one and did not need redefinition. The laws that govern the operation of reinforcing events in this area of problems are apparently somewhat different from those derived in classical learning theory—a distinction is therefore in order: *performance theory* might well encompass this body of data.

A somewhat similar smudging of distinctions has taken place in perceptual theory. Hebb (1949) and Postman (1953) are both initially responsible and many others have fallen in with the current vogue in talking about perceptual learning. Should it turn out that the definition of reinforcement which has served thus far holds for perceptual theory as well, this could clear the air. The element common to all three areas would be established and the reason for treating them together, exposed. This would then allow renewed effort in the direction of specifying differences between them: for assuredly perception, performance (behavior differentiation), and learning (behavior change) do have some elements to distinguish them.

To return for a moment to performance theory. Lawrence and Festinger point out that one way the organism responds to dissonance is to seek "extra attractions" to maintain the behavior. I have made the case that such "extra attractions" can result from the consequences of the actions themselves, provided they have an opportunity to become progressively orderly: that behavior differentiation can become "the extra attraction." Lawrence and Festinger, I am sure, would not want to be limited to this mode of dissonance reduction. The fact that they speak of dissonant "cognitions" suggests that in a situation, any set of events that can be progressively ordered might serve as well. And these events could arise in the receptors by which the organism engages his environment, that is, the events could be perceptual.

The evidence that perceptual differentiation takes place need not be reviewed here. The work of the Gibsons (1955) is well known, as are Hebb's (1949), Piaget's (1955), Postman's (1953), and Bruner's (1958) contributions. I should rather address myself to two questions: (1) is the concept of reinforcement tenable in this area of problems; and (2) what is the connection between progressive differentiation, hierarchy, and the model of the biased homeostat?

If reinforcement is conceived as a consequence of action, what is the action involved in perceiving? Operant conditioning has admitted "looking responses" to the repertoire. Looking certainly involves muscular contractions—the muscles of the eye must move it to make and maintain contact with the exciting energy. Focusing the retinal image also involves the muscles of accommodation. In hearing, the muscles of the middle ear have been shown important to the process of habituation in this modality. And sniffing is certainly a part of perceiving olfactory cues. Further, the experiments of Ivo Köhler (in press) and of Held (Held & Hein, 1958; Held & Schlank, 1959) have shown that perceptual performance depends in good part on more general performance experience in the situation. Finally, there is the well-known fact that passive pushing of the eyeball leads to blurring of vision, while active movement "presets" the perceptual mechanism in such a way that vision is possible. Evidence of this sort has been collated by Teuber (1960) and given a

theoretical base. A somewhat similar model for the process has recently also been detailed by MacKay (1957 a, b; 1958 a, b). And the model is essentially the model of reinforcement with which we are concerned (Pribram, 1960). The consequences of looking must involve events subsequent to context—context active in the movement of the eye. This context must be some self-adapting error-sensitive mechanism that has control over its own input; in other words, a biased, settable, homeostatic-like structure. However, I need not detail anew such a model here. Floyd Allport (1955), after his classic, critical and comprehensive review of theories in perception, was led by his analysis to propose the outlines of a structural model. Let us look in on his event-structure theory of perception:

The first step of the theory is the conceptualization of a complete and potentially repetitive cycle of ongoings and events. . . . Both these effects [positive interaction and inhibitory interaction between cycles] might be present in one . . . system, as in the principle of negative feedback, if the main line production is also considered as a cycle. . . .

We now proceed to a further step. The closed chain of ongoings and events, since it is neither open-ended nor indefinitely extended in space, but a real (cyclical) entity, can be used as a unit of a structure of a larger or "compounded" type. . . . The total structure is the "including" structure. . . . In other words we can build a "higher order" of structure made up of a cycle of cycles of ongoings and events. Still higher orders can be structured as cycles of these larger cycles, and so on. This development of the model will be called the property of "order." It should now be noted that the principle of ordering is not limited to event-cycles; it can apply also to event-systems [pp. 635-636].

Allport presented his theory of event-structure as a way of looking at the problems of perception—not as a definitive theory. The similarities of his view to those expressed in the past two sections leaves little doubt that event-structures in perceptual theory, and reinforcement (viewed structurally) in learning and in performance, have many essential properties in common. I should like to believe, in fact, that event-structure and the mechanism of reinforcement are structurally identical. So conceived, event-structuring is a process through which the consequences of

perceptual acts (e.g., looking) become ordered, much as this ordering takes place in the development of skills. Perceptual problems can, in this light, be approached as if they were sensory performances. Differences between motor (i.e., instrumental) and sensory performances would quickly show themselves—for sense organs and the striped musculature of the organism are differently related to their environment.

But it remains now to be shown that the neural organization of sensory performance partakes of the properties assigned to reinforcing events. In discussing the structure of contiguity, the facts of the orienting reaction to "novel" stimuli and of habituation were presented. Is there any direct evidence that these processes take place in a neural mechanism closely connected to sense-organ function? There is. Lettvin et al. (1961) describe the following reactions of cells in the frog's optic tectum:

"Newness" neurons: These cells have receptive fields about 30 degrees in diameter. . . . They are distributed so as to map continuously the visual field with much overlap. Such a neuron responds a little to sharp changes in illumination. If an object moves across the receptive field, there is a response whose frequency depends on the jerkiness, velocity, and direction of the movement, as well as on the size of the object. There is never an enduring response [p. 773].

"Sameness" neurons: Let us begin with an empty gray hemisphere for the visual field. There is usually no response of the cell to turning on and off the illumination. It is silent. We bring in a small dark object, say 1 to 2 degrees in diameter, and at a certain point in its travel, almost anywhere in the field, the cell suddenly "notices" it. Thereafter, wherever that object is moved it is tracked by the cell. Every time it moves, with even the faintest jerk, there is a burst of impulses that dies down to a mutter that continues as long as the object is visible. If the object is kept moving, the bursts signal discontinuities in the movement, such as the turning of corners, reversals, and so forth, and these bursts occur against a continuous background mutter that tells us the object is visible to the cell.

When the target is removed, the discharge dies down. If the target is kept absolutely stationary for about two minutes, the mutter also disappears. Then one can sneak the target around a bit, slowly, and produce no response, until the cell "notices" it again and locks on [p. 774].

My interest in "perceptual readiness" and a model that would account for it stems from the results of a series of neuro-behavioral experiments. Some years ago, in our analysis of the functions of the posterior "association" cortex of monkey, our group discovered (cotemporaneously with Harlow [1953]) an area on the inferolateral surface of temporal lobe that functions in vision (Blum, Chow, & Pribram, 1950). Additional work showed that lesions in this locus disturbed visual behavior only—somesthetic, taste, and auditory functions could be assigned elsewhere. Further, all sorts of visual choice behaviors were disrupted: choices among colors, patterns, objects, luminances were equally affected, provided the task was equally difficult (number of trials to learn) for unoperated, normal monkeys (Pribram, 1954). Finally, vision was not affected if *choice* was not involved: the monkey could groom, pick a gnat out of midair, and gauge his rate of lever pressing according to the level of illumination in a "ganz-field" (Ettlinger, 1959). The variables that influence choice were also investigated: stimulus object similarity (Mishkin & Hall, 1955) and factors influencing response difficulty (Pribram & Mishkin, 1955) were found important. But in a test of the effect of varying the number of alternatives in the situation an interesting fact came to light. The performance difficulty of the lesioned monkeys was related not to the number of alternatives in the situation but to the number of alternatives sampled—the lesioned group of monkeys consistently made their choices from among a smaller set of stimulus objects than did the control subjects (Pribram, 1959).

The anatomical relations of this inferolateral portion of the temporal lobe are also of interest. There is no specifically visual input to this area as far as is known today. The area is three neurons removed from the striate optic-cortex—and no further away from the auditory cortex of the supratemporal plane, and not much further, neuronwise, for that matter, from the post-central somatosensory areas. Also, severance of the connections between striate and inferolateral cortex by circumsection of the occipital lobe fails to affect visual choice behavior. On the other hand, there is an efferent tract that arises both in the occipital and inferolateral temporal cortex and ends in the neighborhood

of the superior colliculus—an important structure in the visual mechanism which, among other things, has to do with the regulation of eye movements. The real possibility exists, therefore, that the restricted sampling and related effects on visual choice behavior produced by inferolateral temporal lobe lesions are due to a defect produced in the presetting of the visual mechanism: a defect in the process of establishing the context within which a subsequent event can become consequent, that is, reinforcing.

Enough has been said to show that reinforcement viewed as a structure has use in the area of perceptual theory. There remains another problem, however, which has been repeatedly touched upon but which, so far, has been only vaguely formulated: namely the relation between a cyclic, homeostatic-like process and progressive differentiation. Homeostats can be hierarchically arranged. The blower on the home-furnace of a hot-air system is controlled by a thermostat separate from, but subordinate to, the main thermostat. There is some evidence that the food appetitive and general activity mechanisms of the organism are both contained within the larger regulation of basal temperature (Brobeck, 1945, 1948, in press). But, I believe, this simple statement of a hierarchical relationship does not give a full account of the progressive differentiation process which is of concern here. What seems to happen in performance differentiation and in perceptual differentiation is a true reversal of means and ends; of context and content; of bias and the mechanism biased. Differentiation can take place in the biases placed on the mechanism—the temperature of a home will be controlled by several thermostats, each of which biases the main mechanism but is in turn biased by it. This complex yet orderly interrelation among subsystems and system achieves stabilities beyond those possible for the simpler systems. The suggestion is that the biased homeostat becomes differentiated, mainly through differentiation of its bias, perhaps because of inherent imperfections. These imperfections must be in the control the mechanism has over the variables to which it is sensitive. This poses a paradox—for differentiation occurs most readily when such control appears to be accomplished. But just at these junctures, increased sensitivity is also achieved: namely, the thermostatic system that has

allowed temperature to vary between 65 and 75 degrees Fahrenheit is insensitive to temperature changes of 1 or 2 degrees. When the system is sufficiently stable to control temperature at 70 degrees it becomes exquisitely sensitive to a 2 degree change. And these new sensitivities cause the system to react where it would not have on prior occasions. Thus, though this is a structural, even a homeostatic, view of the behavioral process, its design certainly does not lead to stagnation.

Much remains to be done in the way of clarification of the neural mechanisms involved in such a cyclic process that leads to perceptual (and behavioral) differentiation. But the problem is stated: According to the view put forward here, perceptual and performance differentiation occurs since biased homeostatic processes—mechanisms of reinforcement—continually operate to achieve stability. Once control of a certain order has been momentarily established, new sensitivities within the context of the old appear, and these in turn must be dealt with: that is, these new (novel) events become the consequences of the behavior—the events sequent and in context—the reinforcers.

THE ANATOMY OF HAPPINESS

The theme is reinforcement. Each of the preceding sections is a variation on that theme, that variation produced by a set of techniques and problems. In this section I hope to draw out the theme itself more fully.

Reinforcements are considered consequences of instrumental, motor, and sensory acts—event sequences that occur in the context of other event sequences. The model of event structure presented is the biased homeostat, a feedback unit which has the capacity to differentiate. This model has been spelled out in a variety of ways: one way is as a set of test-operate-test-exit units, hierarchically organized into a branching program or Plan, suggested by George Miller, Eugene Galanter, and myself (1960). This formulation has the advantage of analogy with computer information processing, so that problems generated can be tested by hardware as well as software operations.

The suggestion has also been made that under some circumstances reinforcing events function as biases on feedback units,

homeostats. Further, differentiation of performance and of perception has been attributed to the differentiation of this bias mechanism, the reinforcing process. Is there any direct support for equating reinforcement and bias? I believe there is.

Whalen (1961) has used a situation similar to that already described in which drive and reinforcing stimuli were teased apart. In Whalen's experiment, reinforcement resulted not from electrical self-stimulation of the brain, but by sexual activity. Whalen showed that the choice between two alleys of a maze depends on the occurrence of an intromission, but running speed in the maze increases with the number of such occurrences.

Spence (1955, p. 127-148) has reported a similar result when pellets of food are used as reinforcers. These results are congruous with Miller's (already mentioned) proposal that choice—and the rate of learning of that choice—depend simply on the information given by the reinforcing event. But Whalen's and Spence's results demonstrate that, once performance is established, reinforcing events display another property: namely, an increase in the number of reinforcements causes performance rate to increase monotonically (over a range). Another way of stating this effect on performance is to say that reinforcers place a value on the performance—reinforcement biases the performance.

In *Plans and the Structure of Behavior* (Miller, Galanter, & Pribram, 1960) we discussed two aspects of motivation: Plan and Value. Plan clearly directs performance, that is, Plans program choices. Value played some other, less well-specified role in the initiation and the "seeing through" of Plans. When Value is stated to be a bias on performance (and of course, this includes perceptual performance), a clearer conception is attained.

However, another problem is immediately raised. If reinforcements bias performance, place values on them, how are reinforcers then informative? The solution to this problem lies again in the reversal of the context-content relationship. When, during learning, reinforcements give information they must operate, by the definition of the term information, within a set or bias already established. When, on the other hand, reinforcements bias performance, they establish the set within which other events become informative. Thus the consequences of actions and perceptions

are informative or valuative according to whether they take place within another context or they themselves serve as the context.

Some of the conditions that govern the occurrence of context-content reversals have been detailed in each of the earlier sections. Much more could have been added. When mechanisms of drive serve as context, utility theory applies: preferences form the contextual matrix within which the consequences (outcomes) of actions and perceptions are informing. When, due to affluence, the means-ends reversal has taken place, "futility" and performance theory must take over—the consequences of action become the context within which other consequences, drive, and perceptual stimuli give information about the gamesmanship or skill. And when perceptual stimuli provide the context, values are recognized to bias the operation of both drives and actions.

But, perhaps more important would be a summary of the similarities and differences between the structure of reinforcement proposed here and the conceptions of others working in each of the areas covered by a section:

Experimentalists working in the tradition of operant conditioning have pointed the way to a descriptive definition of reinforcement and have demonstrated the reversibility of the response relationships that result in reinforcement. However, the distinction has been blurred between a "response" as an indicator of action and a "consequence" of an action conceived as a stimulus event. This blurring has led to the possible confusion that reinforcement is composed by the concatenation of muscular events. Once consequences of actions are seen for what they are—stimulus sequences that occur in the context of other stimulus sequences—the confusion is no longer possible.

Contiguity theorists have depended heavily on the simple idea that stimulus events, whether of muscular, extero- or interoceptive origin, must somehow come into contact in order to guide behavior. This faith has been amply rewarded, not only by their own work at the behavioral level, but by neurophysiologists who have detailed the loci and systems where such contact occurs. Neurophysiology and psychophysiology have gone a step further, however. It turns out that what constitutes a

stimulus event is itself a contiguity affair. A stimulus is seen to result *only* when there is a partial match between some coded neural representation of prior inputs to the organism (or some innately operating mechanism awaiting "release"). A stimulus thus is shown to arise by contiguity, that is, within the context of some already established process. Contiguity becomes a context-content relationship among stimulus sets, *temporally* ordered. And so theoretical distinctions between contiguity and expectancy theory vanish. A reinforcing event is any consequence of behavior that alters the context-content relationship, that is, any consequent stimulus. The proposal here presented should allow better specification of what constitutes a stimulus (reinforcing or otherwise) by those who hold the contiguity position—perhaps the central problem for their theory and for behavior theory in general.

Drive theorists have been concerned primarily with physiological need-produced tension states. Estes' analysis cleared the air considerably by demonstrating that many of the problems faced could be solved by considering drives as stimuli. The neuropsychological work presented here takes this analysis a step further. Drive stimuli are seen to arise from the operation of homeostats, which control the production of the physical or chemical substances to which they are sensitive. The level at which these mechanisms become sensitive can, within limits, be set, that is, biased. Self-stimulation by electrical currents delivered to the brain was suggested to operate by setting this bias. But the regions of the endbrain, the limbic systems, from which self-stimulation is obtained, do not deal only with drive-stimuli. In later sections of the paper, the structure of action-produced stimuli and even perceptual structure was shown to be similar to the structure of drives—that is, to biased homeostats. And reversals among these several classes of potentially contextual structures were shown to be impaired when lesions are made in the limbic systems. Reinforcers, the consequences of action, were shown to become, on the basis of experience, the bias or context within which a drive stimulus is informing where, initially, the set of drive-stimuli were the context within which the reinforcing event informed.

Activation, the concept of tension, was not completely done away with, however. As detailed in the section on perception, homeostats, even biased ones, are imprecise, faulty mechanisms. In a relatively constant environment, however, they manage to become progressively adapted—that is, they stabilize the inputs to their sensitivities. Once this is accomplished, however, they are also more sensitive to smaller perturbations in these inputs and so new problems of stabilization arise. The suggestion is made that one mechanism for achieving stability and therefore sensitivity is the differentiation of the bias—more homeostats are brought to bear on the input. These homeostats must, however, be interconnected, that is, each biased by the other, to achieve a maximum of control. Again, as this level of control is achieved, new sensitivities develop and new mechanisms differentiate to cope with them. A structural view of this sequence of operations shows it to be that of a biased homeostat; a closer view, however, would discern a cyclic pattern of coping followed by control and new sensitivity—and another cycle of coping. The coping phase might well be called activation, since neural and behavior unrest would characterize this phase.

For the rest, this proposal, by focussing on reinforcement, makes possible a view of dissonance, addiction and effectance as performances which need not necessarily follow the laws established for learning by behavior theory. In the same fashion, "perceptual learning" is unravelled: the clear statement of what reinforces perceptions may allow perceptual theory to return to other matters. And, what may be even more important, reinforcement is seen to be a part of a stimulus event-structure irrespective of the receptor or origin of the stimuli: problems in the domain, "motivation," can be stated in terms familiar to those working in perception.

A final thought. Pfaffmann (1960) has stated the case for the pleasures of sensation elegantly. Perhaps *this* paper has brought us a bit closer to understanding happiness. Classically, happiness is conceived as "entelechy," that is, "self-actualization" (Maslow, 1955) and not hedonistically. Cyclically recurring phases have been described to characterize achievement (Toman, 1960; Erikson, 1962): during each cycle, progressively more control is gained

over a set of events; when this control reaches a certain level, new sensitivities develop. Then satiety sets in, reorganization (of context-content relationships?) takes place, and the cycle starts afresh. This cyclic process is, of course, most clearly apparent in behavior related to physiological needs. There is some evidence that feeding and sexual satiety are accompanied by marked activation of the electrical patterns recorded from the brain, and especially from limbic structures, despite the fact that behaviorally the organism is asleep (Sawyer, in press). These "paradoxical sleep records" are similar to those obtained when man dreams during sleep (Jouvet, 1961; Dement, in press). In fact, persons routinely awakened whenever the paradoxical sleep waves appear, fail to feel rested after sleeping.

The suggestion is that happiness is slowly achieved through the progressive gain in control over stimulus events, through the structuring of con-sequent performances, that is, through reinforcement. In the course of human existence, many moments recur when control reaches the point, just before the reorganization of satiety, when sensitivity is maximal. At such moments, "happiness is a warm puppy."

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