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ON THE GENERALITY OF THE LAWS OF LEARNING[1]

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That all events are equally associable and obey common laws is a central assumption of general process learning theory. A continuum of preparedness is defined which holds that organisms are prepared to associate certain events, unprepared for some, and contraprepared for others. A review of data from the traditional learning paradigms shows that the assumption of equivalent associability is false: in classical conditioning, rats are prepared to associate tastes with illness even over very long delays of reinforcement, but are contraprepared to associate tastes with footshock. In instrumental training, pigeons acquire key pecking in the absence of a contingency between pecking and grain (prepared), while cats, on the other hand, have trouble learning to lick themselves to escape, and dogs do not yawn for food (contraprepared). In discrimination, dogs are contraprepared to learn that different locations of discriminative stimuli control go–no go responding, and to learn that different qualities control directional responding. In avoidance, responses from the natural defensive repertoire are prepared for avoiding shock, while those from the appetitive repertoire are contraprepared. Language acquisition and the functional autonomy of motives are also viewed using the preparedness continuum. Finally, it is speculated that the laws of learning themselves may vary with the preparedness of the organism for the association and that different physiological and cognitive mechanisms may covary with the dimension.

Sometimes we forget why psychologists ever trained white rats to press bars for little pellets of flour or sounded metronomes followed by meat powder for domestic dogs. After all, when in the real world do rats encounter levers which they learn to press in order to eat, and when do our pet dogs ever come across metronomes whose clicking signals meat powder? It may be useful now to remind ourselves about a basic premise which gave rise to such bizarre endeavors, and to see if we still have reason to believe this premise.

The General Process View of Learning

It was hoped that in the simple, controlled world of levers and mechanical feeders, of metronomes and salivation, something quite general would emerge. If we took such an arbitrary behavior as pressing a lever and such an arbitrary organism as an albino rat, and set it to work pressing the lever for food, then *by virtue of* the very arbitrariness of the environment, we would find features of the rat's behavior general to real-life instrumental learning. Similarly, if we took a dog, undistracted by extraneous noises and sights, and paired a metronome's clicking with meat, what we found about the salivation of the dog might reveal characteristics of associations in general. For instance, when Pavlov found that salivation stopped occurring to a clicking that used to signal meat powder, but no longer did, he hoped that this was an instance of a *law,* "experimental extinction," which would have application beyond clicking metronomes, meat powder, and salivation. What captured the interest of the psychological world was the

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possibility that such laws might describe the general characteristics of the behavior acquired as the result of pairing one event with another. When Thorndike found that cats learned only gradually to pull strings to escape from puzzle boxes, the intriguing hypothesis was that animal learning in general was by trial and error. In both of these situations, the very arbitrariness and unnaturalness of the experiment was assumed to guarantee generality, since the situation would be uncontaminated by past experience the organism might have had or by special biological propensities he might bring to it.

The basic premise can be stated specifically: In classical conditioning, the choice of CS, US, and response is a matter of relative indifference; that is, any CS and US can be associated with approximately equal facility, and a set of general laws exist which describe the acquisition, extinction, inhibition, delay of reinforcement, spontaneous recovery, etc., for all CSs and USs. In instrumental learning, the choice of response and reinforcer is a matter of relative indifference; that is, any emitted response and any reinforcer can be associated with approximately equal facility, and a set of general laws exist which describe acquisition, extinction, discriminative control, generalization, etc., for all responses and reinforcers. I call this premise the assumption of equivalence of associability, and I suggest that it lies at the heart of general process learning theory.

This is not a straw man. Here are some quotes from three major learning theorists to document this assumption:

> It is obvious that the reflex activity of any effector organ can be chosen for the purpose of investigation, since signalling stimuli can get linked up with any of the inborn reflexes [Pavlov, 1927, p. 17].
>
> any natural phenomenon chosen at will may be converted into a conditional stimulus . . . any visual stimulus, any desired sound, any odor, and the stimulation of any part of the skin [Pavlov, 1928, p. 86].
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> All stimulus elements are equally likely to be sampled and the probability of a response at any time is equal to the proportion of elements in S′ that are connected to it. . . . On any acquisition trial all stimulus elements sampled by the organism become connected to the response reinforced on that trial [Estes, 1959, p. 399].
>
> The general topography of operant behavior is not important, because most if not all specific operants are conditioned. I suggest that the dynamic properties of operant behavior may be studied with a single reflex [Skinner, 1938, pp. 45–46].

A Reexamination of Equivalence of Associability

The premise of equivalence places a special premium on the investigations of arbitrarily related, as opposed to naturally occurring, events. Such events, since they are supposedly uncontaminated by past experience or by special propensities the organism brings to the situation, provide paradigms for the investigations of general laws of learning. More than 60 years of research in both the instrumental and classical conditioning traditions have yielded considerable data suggesting that similar laws hold over a wide range of arbitrarily chosen events: the shape of generalization gradients is pretty much the same for galvanic skin responses classically conditioned to tones when shock is the US (Hovland, 1937), and for salivating to being touched at different points on the back when food is the US (Pavlov, 1927). Partial reinforcement causes greater resistance to extinction than continuous reinforcement regardless of whether rats are bar pressing for water or running down alleyways for food. Examples of analogous generality of laws could be multiplied at great length.

Inherent in the emphasis on arbitrary events, however, is a danger: *that the laws so found will not be general, but peculiar to arbitrary events.*

The Dimension of Preparedness

It is a truism that an organism brings to any experiment certain equipment and predispositions more or less appropriate to that situation. It brings specialized sensory and receptor apparatus with a long evolutionary history which has modified it into its present appropriateness or inappropriateness for the experiment. In addition to sensory-motor capacity, the organism brings associative apparatus, which likewise has a long and specialized evolutionary history. For example, when an organism is placed in a classical conditioning experiment, not only may the CS be more or less perceptible and the US more or less evocative of a response, *but also the CS and US may be more or less associable.* The organism may be more or less prepared by the evolution of its species to associate a given CS and US or a given response with an outcome. If evolution has affected the associability of specific events, then it is possible, even likely, that the very *laws* of learning might vary with the preparedness of the organism from one class of situations to another. If this is so, investigators influenced by the general process view may have discovered only a subset of the laws of learning: the laws of learning about arbitrarily concatenated events, those associations which happen in fact to be equivalent.

We can define a continuum of preparedness operationally. Confront an organism with a CS paired with US or with a response which produces an outcome. Depending on the specifics, the organism can be either prepared, unprepared, or contraprepared for learning about the events. *The relative preparedness of an organism for learning about a situation is defined by the amount of input* (e.g., numbers of trials, pairings, bits of information, etc.) *which must occur before that output* (responses, acts, repertoire, etc.), *which is construed as evidence of acquisition, reliably occurs.* It does not matter how input or output are specified, as long as that specification can be used consistently for all points on the continuum. Thus, using the preparedness dimension is independent of whether one happens to be an S-R theorist, a cognitive theorist, an information processing theorist, an ethologist, or what have you. Let me illustrate how one can place an experimental situation at various points on the continuum for classical conditioning. If the organism makes the indicant response consistently from the very first presentation of the CS on, such "learning" represents a clear case of instinctive responding, the extreme of the prepared end of the dimension. If the organism makes the response consistently after only a few pairings, it is somewhat prepared. If the response emerges only after many pairings (extensive input), the organism is unprepared. If acquisition occurs only after very many pairings or does not occur at all, the organism is said to be contraprepared. The number of pairings is the measure that makes the dimension a continuum, and implicit in this dimension is the notion that "learning" and "instinct" are continuous. Typically ethologists have examined situations in the prepared side of the dimension, while general process learning theorists have largely restricted themselves to the unprepared region. The contraprepared part of the dimension has been largely uninvestigated, or at least unpublished.

The dimension of preparedness should not be confused with the notion of operant level. The frequency with which a response is made in a given situation is not necessarily related to the associability of that response with a given outcome. As will be seen later, frequent responses may not be acquired when they are reinforced as readily as infrequent responses. Indeed, some theorists (e.g., Turner & Solomon, 1962) have argued that high-probability, fast-latency responding may actually antagonize operant reinforceability.

The first empirical question with which this paper is concerned is whether sufficient evidence exists to challenge the equivalence of associability. For many years, ethologists and others (for an excellent example, see Breland & Breland, 1966) have gathered a wealth of evidence to challenge the general process view of learning. Curiously, however, these data have had little impact on the general process camp, and while not totally ignored, they have not been theoretically incorporated. In view of differences in methodology, this is perhaps understandable. I do not expect that presenting these lines of evidence here would have any more effect than it has already had. More persuasive to the general process theorist should be the findings which have sprung up within his own tradition.. Within traditional conditioning and training paradigms, a considerable body of evidence now exists which challenges the premise. In reviewing this evidence, we shall find the dimension of preparedness to be a useful integrative device. It is not the intent of this article to review exhaustively the growing number of studies which challenge the premise. Rather, we shall look within each of the major paradigms which general process learning theorists have used and discuss one or two clear examples. The theme of these examples is that all events are not equivalent in their associability: that although the organism may have the necessary receptor and effector apparatus to deal with events, there is much variation in its ability to learn about relations between events.

CLASSICAL CONDITIONING

The investigation of classical aversive conditioning has been largely confined to the unconditioned response of pain caused by the stimulus of electric shock (cf. Campbell & Church, 1969), and the "laws" of classical conditioning are based largely on these findings along with those from salivary conditioning. Recently, Garcia and his collaborators (Garcia, Ervin, & Koelling, 1966; Garcia, Ervin, Yorke, & Koelling, 1967; Garcia & Koelling, 1966; Garcia, McGowan, Ervin, & Koelling, 1968), and Rozin and his collaborators (Rodgers & Rozin, 1966; Rozin, 1967, 1968, 1969) have used illness as an unconditioned response and reported some intriguing findings. In the paradigm experiment (Garcia & Koelling, 1966), rats received "bright-noisy, saccharin-tasting water." What this meant was that whenever the rat licked a drinking tube containing saccharine-flavored water, lights flashed and a noise source sounded. During these sessions the rats were X-irradiated. X-irradiation makes rats sick, but it should be noted that the illness does not set in for an hour or so following X-raying. Later the rats were tested for acquired aversions to the elements of the compound CS. The rats had acquired a strong aversion to the taste of saccharine, *but had not acquired an aversion to the "bright-noise."* The rats had "associated" the taste with their illness, but not the exteroceptive noise-light stimuli. So that it could not be argued that saccharin is such a salient event that it masked the noise and light, Garcia and Koelling ran the complementary experiment: "Bright and noisy saccharin-tasting water" was again used as a CS, but this time electric shock to the feet was the US. The rats were then tested for aversion to the elements of the CS. In this case, the bright noise became aversive, but the saccharin-tasting water did not. This showed that the bright noise was clearly perceptible; but the rats associated only the bright noise with the exteroceptive US of footshock, and not the taste of saccharin in spite of its also being paired with shock.

In the experiment, we see both ends as well as the middle of the preparedness continuum. Rats are prepared, by virtue of their evolutionary history, to associate tastes with malaise. For in spite of a several-hour delay of reinforcement, and the presence of other perceptible CSs, only the taste was associated with nausea, and light and noise were not. Further, rats are contraprepared to associate exteroceptive events with nausea and contraprepared to associate tastes with footshock. Finally, the association of footshock with light and sound is probably someplace in the unprepared region. The survival advantage of this preparedness seems obvious: organisms who are poisoned by a distinctive food and survive, do well not to eat it again. Selective advantage should accrue, moreover, to those rats whose associative apparatus could bridge a very long CS-US interval and who could ignore contiguous, as well as interpolated, exteroceptive CSs in the case of taste and nausea.

Does such prepared and contraprepared acquisition reflect the evolutionary results of selective pressure or does it result from experience? It is possible that Garcia's rats may have previously learned that tastes were uncorrelated with peripheral pain and that tastes were highly correlated with alimentary consequences. Such an argument involves an unorthodox premise: that rats' capacities for learning set and transfer are considerably broader than previously demonstrated. The difference between a position that invokes selective pressure (post hoc) and the experiential set position is testable: Would mating those rats who were most proficient at learning the taste–footshock association produce offspring more capable of such learning than an unselected population? Conversely, would interbreeding refractory rats select out the facility with which the taste–nausea association is made?

Supporting evidence for preparedness in classical conditioning has come from other recent experiments on specific hungers and poisoning. Rodgers and Rozin (1966) and Rozin (1967, 1968) have demonstrated that at least part of the mechanism of specific hungers (other than sodium) involves conditioned aversion to the taste of the diet the rats were eating as they became sick. Deficient rats spill the old diet and will not eat it, even after they have recovered. The association of the old taste with malaise seems to be made in spite of the long delay between taste of the diet and gradual onset of illness. The place and the container in which the old diet was set, moreover, do not become aversive. The remarkable ability of wild rats who recover from being poisoned by a novel food, and thereafter avoid new tastes (Barnett, 1963; Rozin, 1968), also seems to result from classical conditioning. Note that the wild rat must be prepared to associate the taste with an illness which does not appear for several hours in only one trial; note also that it must be contraprepared to associate some contiguous CSs surrounding the illness with malaise.

Do these findings really show that rats can associate tastes and illness when an interval of many minutes or even hours intervenes or are they merely a subtle instance of contiguity? Peripheral cues coming either from long-lasting aftertastes or from regurgitation might bring the CS and US into contiguity. Rozin (1969) reported evidence against aftertaste mediation: rats received a high concentration of saccharin paired with apomorphine poisoning. Later, the rats were given a choice between the high concentration and a low concentration. The rats preferred the low concentration, even though the aftertaste that was purportedly contiguous with malaise should be more similar to the low concentration (since it had been diluted by saliva) than the high concentration.

Not only do rats acquire an aversion for the old diet, on which they got sick, but they also learn to prefer the taste of a new diet containing the needed substance. This mechanism also seems to involve prepared conditioning of taste to an internal state. Garcia et al. (1967) paired the taste of saccharin with thiamine injections given to thiamine deficient rats, and the rats acquired a preference for saccharin. So both the rejection of old foods and acceptance of new foods in specific hungers can be explained by prepared conditioning of tastes to internal state.

INSTRUMENTAL LEARNING

E. L. Thorndike, the founder of the instrumental learning tradition, was by no means oblivious to the possibility of preparedness in instrumental learning, as we shall see below. He also hinted at the importance of preparedness in one of his discussions of classical conditioning (Thorndike, 1935, p. 192–197): one of his students (Bregman, 1934) attempted to replicate the results of Watson and Rayner (1920), who found that little Albert became afraid of a white rat, rabbit, and dog which had been paired with a startling noise. Bregman was unable to show any fear conditioning when she paired more conventional CSs, such as blocks of wood and cloth curtains, with startling noise. Thorndike speculated that infants at the age of locomotion were more disposed to manifest fear to objects that wiggle and contort themselves than to motionless CSs.

Thorndike's parallel views on instrumental learning rose from his original studies of cats in puzzle boxes. As every psychologist knows, he put cats in large boxes and investigated the course of learning to pull strings to escape. What is less widely known is that he put his cats in not just one puzzle box, but in a whole series of different ones (incidentally in doing this he seems to have discovered learning set—Thorndike, 1964, pp. 48–50). In one box the cats had to pull a string to get out, in another a button had to be pushed, in another a lever had to be depressed, etc. One of his boxes—Box Z—was curious: it was merely a large box with nothing but a door that the experimenter could open. Thorndike opened the door in Box Z whenever cats licked themselves or scratched themselves. The cat is known to use both of these frequently occurring responses instrumentally: it scratches itself to turn off itches, and licks itself to remove dirt. In addition, Thorndike had established that getting out of a puzzle box was a sufficient reward for reinforcing the acts of string pulling, button pushing, and lever clawing. In spite of this, Thorndike's cats seemed to have a good deal of trouble learning to scratch themselves or lick themselves to get out of the boxes.

A reanalysis of the individual learning curves presented by Thorndike (1964) for each of the seven cats who had experience in Box Z documents the impression: of the 28 learning curves presented for these seven cats in the boxes other than Z, 22 showed faster learning than in Z, three showed approximately equal learning, and only three showed slower learning. While all of the cats eventually showed improved speeds of licking or scratching for escape, such learning was difficult and irregular. Thorndike noted another unusual property of licking and scratching:

> There is in all these cases a noticeable tendency . . . to diminish the act until it becomes a mere vestige of a lick or scratch . . . the licking degenerated into a mere quick turn of the head with one or two motions up and down with tongue extended. Instead of a hearty scratch, the cat waves its paw up and down rapidly for an instant. Moreover, if sometimes you do not let the cat out after the feeble reaction, it does not at once repeat the movement, as it would do if it depressed a thumb piece, for instance, without success in getting the door open [Thorndike, 1964, p. 48].

Contemporary investigators have reported related findings. Konorski (1967, pp. 463–467) attempted to train "reflex" movements, such as anus licking, scratching, and yawning, with food reinforcement. While reporting success with scratching and anus licking, like Thorndike, he observed spontaneous simplification and arhythmia in the responses. More importantly, he reported that reinforcement of "true yawning" with food is very difficult, if not impossible. Bolles and Seelbach (1964) reported that rearing could be reinforced by noise offset, but not punished by noise onset, exploration could be modified by both, and grooming by neither. This difference could not be accounted for by difference in operant level, which is substantial for all these behaviors of the rat.

Thorndike (1964) speculated that there may be some acts which the organism is not neurally prepared to connect to some sense impressions:

> If the associations in general were simply between situation and impulse to act, one would suppose that the situation would be associated with the impulse to lick or scratch as readily as with the impulse to turn a button or claw a string. Such is not the case. By comparing the curves for Z on pages 57–58 with the others, one sees that for so simple an act it takes a long time to form the association. This is not the final reason, for lack of attention, a slight increase in the time taken to open the door after the act was done, or *an absence of preparation in the nervous system for connections between these particular acts and definite sense impressions* [italics added] may very well have been the cause of the difficulty in forming the associations [p. 113].

This speculation seems reasonable: after all, in the natural history of cats, only behavior such as manipulating objects which maximized chances for escaping traps would be selected, and licking is not in the repertoire which maximizes escape. At minimum, Thorndike demonstrated that the emission of licking paired with an event which could reinforce other emitted acts was not sufficient to reinforce licking equally well. In the present terms, Thorndike had discovered a particular instrumental training situation for which cats are relatively contraprepared.

Brown and Jenkins (1968, Experiment 6) have reported findings which appear to come from the opposite end of the dimension. Pigeons were exposed to a lighted key which was paired with grain delivered in a lighted food hopper below the key. But unlike the typical key-pecking situation, the pigeons' pecking the key did not produce food. Food was contingent only on the key's being lit, not on pecking the key. In spite of this, all pigeons began pecking the key after exposure to the lighted key, followed by grain. Moreover, key pecking was maintained even though it had no effect on food. One can conclude from these "autoshaping" results that the pigeon is highly prepared for associating the pecking of a lighted key with grain.

There is another curiosity in the history of the instrumental learning literature which is usefully viewed with the preparedness dimension: the question of why a reinforcer is reinforcing. For over 20 years, disputes raged about what monolithic principle described the necessary and sufficient conditions for learning. Hull (1943) claimed that tissue-need reduction must occur for learning to take place, while Miller (1951) held that drive reduction was necessary and sufficient. Later, Sheffield, Roby, and Campbell (1954) suggested that a consummatory response was the necessary condition. More recently, it has become clear that learning can occur in the absence of any of these (e.g., Berlyne, 1960). I suggest that when CSs or responses are followed by such biologically important events as need reducers, drive reducers, or consummatory responses, learning should take place readily because natural selection has prepared organisms for such relationships. The relative preparedness of organisms for these events accounts for the saliency of such learning and hence the appeal of each of the monolithic principles. But organisms *can* learn about bar pressing paired with light onset, etc.; they are merely less prepared to do so, and hence, the now abundant evidence against the earlier principles was more difficult to gather.

Thus, we find that in instrumental learning paradigms, there are situations which lie on either side of the rat's bar pressing for food on the preparedness dimension. A typical rat will ordinarily learn to bar press for food after a few dozen exposures to the bar press—food contingency. But cats, who can use scratching and licking as instrumental acts in some situations, have trouble using these acts to get out of puzzle boxes, and dogs do not learn to yawn for food even after many exposures to the contingency. On the other hand, pigeons acquire a key peck in a lighted key–grain situation, even when there is no contingency at all between key pecking and grain. These three instrumental situations represent unprepared, contraprepared, and prepared contingencies, respectively. Later we shall discuss the possibility that they obey different laws as a function of different preparedness.

DISCRIMINATION LEARNING

The next two paradigms we consider—discrimination learning and avoidance learning—combine both classical and instrumental procedures. In both of these paradigms, findings have been reported which challenge the equivalence of associability. We begin with some recent Polish work on discrimination learning in dogs. Lawicka (1964) attempted to train dogs in either a go right–go left differentiation or a go–no go differentiation. Whether such differentiation could be acquired depended on the specific discriminative stimuli used. For the left–right differentiation, if the S− and the S+ differed in location (one speaker above the dog; one speaker below), the dog readily learned which way to go in order to receive food. If, however, the stimuli came from the *same* speaker and differed only in pitch, the left–right differentiation was exceedingly difficult. Topographical differences in stimuli, as opposed to qualitative differences, seem to aid in differentiating two topographically different responses. The dog seems contraprepared, moreover, for making a left–right differentiation to two tones which do not also differ in direction. Lest one argue that the two tones coming out of the same speaker were not discriminable, Lawicka (1964; like Garcia & Koelling, 1966) did the complementary experiment: dogs were trained to go and receive food or stay with two tones coming out of the same speaker. One tone was the S+ and the other tone the S−. The dogs learned this readily. Thus, using the same tones which could not be used to establish a left–right differentiation, a go–no go differentiation was established. The author then attempted to elaborate the go–no go differentiation to the same tone differing in location of speakers. As the reader should expect by now, the dogs had trouble learning the go–

no go differentiation to the difference in location of S+ and S−. Dogs, then, are contraprepared for learning about different locations controlling a go–no go differentiation although they are not contraprepared for learning that the same locations control a left–right differentiation. Dogs are contraprepared for learning that qualitative differences of tone from the same location control a left–right differentiation, but not contraprepared for using this difference to govern a go–no go differentiation. Dobrzecka and Konorski (1967, 1968) and Szwejkowska (1967) have confirmed and extended these findings.

Emlen (personal communication, 1969) reported discrimination (or at least perceptual) learning that is prepared. It is known from planetarium experiments that adult indigo buntings use the northern circumpolar constellations for migration, since blocking these from view disrupts directed migration. One might have thought that the actual constellations were represented genetically. If young birds are raised under a sky which rotates around a fictitious axis, however, they use the arbitrarily chosen circumpolar constellations for migration and ignore the natural circumpolar constellations. Thus, it appears that indigo buntings are prepared to pay attention to and learn about those configurations of stars which rotate most slowly in the heavens.

AVOIDANCE LEARNING

Data from avoidance learning studies also challenge the equivalence of associability. Rats learn reasonably readily to press bars to obtain food. Rats also learn very readily to jump (Baum, 1969) and reasonably readily to run (Miller, 1941, 1951) from a dangerous place to a safe place to avoid electric shock. From this, the premise deduces that rats should learn readily to press bars to avoid shock. But this is not so (e.g., D'Amato & Schiff, 1964). Very special procedures must be instituted to train rats to depress levers to avoid shock reliably (e.g., D'Amato & Fazzaro, 1966; Fantino, Sharp, & Cole, 1966). Similarly, pigeons learn readily to peck lighted keys to obtain grain: too readily, probably, for this to be considered an unprepared or arbitrary response (see Brown & Jenkins, 1968). But it is very difficult to train pigeons with normal laboratory techniques to key peck to avoid shock. Hoffman and Fleshler (1959) reported that key pecking was impossible to obtain with negative reinforcement; Azrin (1959) found only temporary maintenance of key pecking in but one pigeon; and Rachlin and Hineline (1967) needed 10–15 hours of patient shaping to train key pecking to remove shock. This probably attests more to a problem specific to the response and reinforcer than to some inability of the pigeon to learn about avoidance contingencies. Ask anyone who has attempted to kill pigeons (e.g., by electrocution or throwing rocks at them), how good pigeons are at avoiding. Pigeons learn to fly away to avoid noxious events (e.g., Bedford & Anger, 1968; Emlen, 1970). In contrast, it is hard to imagine a pigeon flying *away* from something to obtain food.

Bolles has recently (1970)—and quite persuasively—argued that avoidance responses as studied in laboratory experiments are not simple, arbitrary operants. In order to produce successful avoidance, Bolles argues, the response must be chosen from among the natural, *species-specific* defensive repertoire of the organism. Thus, it must be a response for which the organism is prepared. Running away for rats and flying away for pigeons make good avoidance responses, while key pecking and bar pressing (which are probably related to the appetitive repertoire) do not.

It might be argued that these difficulties in learning avoidance are not due to contrapreparedness but to competing motor responses Thus, for example, rats have trouble pressing levers to avoid shock because shock causes them to "freeze" which is incompatible with bar pressing. A word of caution is in order about such hypotheses: I know of no theory which specifies in advance what competes with what; rather, response competition (or facilitation) is merely invoked post hoc. When, and if, a *theory* of topographical incompatibility arises it may indeed provide an *explanation* of contrapreparedness, but at the present time, it does not.

Let us review the evidence against the equivalence of associability premise: in classical conditioning, rats are prepared to associate tastes with nausea and contraprepared to associate taste with footshock. In instrumental learning, different emitted responses are differentially associable with different reinforcers: pigeons are prepared to peck lighted keys for food, since they will acquire this even in the absence of any contingency between key pecking and food. Cats are contraprepared for learning to scratch themselves to escape, and dogs for yawning for food. In discrimination learning, dogs are contraprepared to learn that different locations control a go–no go differentiation, and contraprepared for different qualities controlling a left–right response. In avoidance learning, those responses which come from the natural defensive repertoire of rats and pigeons are prepared (or at least unprepared) for avoiding shock. Those responses from the appetitive repertoire seem contraprepared for avoidance.

TWO FAILURES OF GENERAL PROCESS LEARNING THEORY: LANGUAGE AND THE FUNCTIONAL AUTONOMY OF MOTIVES

The interest of psychologists in animal learning theory is on the wane. Although the reasons are many, a prominent one is that such theories have failed to capture and bring into the laboratory phenomena which provide fertile models of complex human learning. This failure may be due in part to the equivalence premise. By concentrating on events for which organisms have been relatively unprepared, the laws and models which general process learning theories have produced may not be applicable beyond the realm of arbitrary events, arbitrarily connected. This would not be an obstacle if all of human learning consisted of learning about arbitrary events. But it does not. *Homo sapiens* has an evolutionary history and a biological makeup which has made it relatively prepared to learn some things and relatively contraprepared to learn others. If learning varies with preparedness, it should not be surprising that the laws for unprepared association between events have not explained such phenomena as the learning of language or the acquisition of motives.

Lenneberg (1967) has recently provided an analysis of language, the minimal conclusion of which is that children do not learn language the way rats learn to press a lever for food. Put more strongly, the set of laws which describe language learning are not much illuminated by the laws of the acquisition of arbitrary associations between events, as Skinner (1957) has argued. Unlike such unprepared contingencies as bar pressing for food, language does not require careful training or shaping for its acquisition. We do not need to arrange sets of linguistic contingencies carefully to get children to speak and understand English. Programmed training of speech is relatively ineffective, for under all but the most impoverished linguistic environments, human beings learn to speak and understand. Children of the deaf make as much noise and have the same sequence and age of onset for cooing as children of hearing parents. Development of language seems roughly the same across cultures which presumably differ widely in the arrangement of reinforcement contingencies, and language skill is not predicted by chronological age but by motor skill (see Lenneberg, 1967, especially pp. 125–158, for a fuller discussion).

The acquisition of language, not unlike pecking a lighted key for grain in the pigeon and the acquisition of birdsong (Petrinovich, 1970), is prepared. The operational criterion for the prepared side of the dimension is that minimal input should produce acquisition. One characteristic of language acquisition which separates it from the bar press is just this: elaborate training is not required for its production. From the point of view of this paper, it is not surprising that the traditional analyses of instrumental and classical conditioning are not adequate for an analysis of language. This is not because language is a phenomenon *sui generis,* but because the laws of instrumental and classical conditioning were developed to explain unprepared situations and not to account for learning in prepared situations. This is not to assert that the laws which govern language acquisition will necessarily be the same as those governing the Garcia phenomenon, birdsong, or the key peck, but to say that species-specific, biological analysis might be fruitfully made of these phenomena.

It is interesting to note in this context the recent success that Gardner and Gardner (1970) have had in teaching American sign language to a chimpanzee. The Gardners reasoned that earlier failures to teach spoken English to chimpanzees (Hayes & Hayes, 1952; Kellogg & Kellogg, 1933) did not result from cognitive deficiencies on the part of the subjects, but from the contraprepared nature of vocalization as a trainable response. The great manual dexterity of the chimpanzee, however, suggested sign language as a more trainable vehicle. Hayes (1968) has recently reanalyzed the data from Vicki (the Hayes' chimp) and confirmed the suggestion that chimpanzees' difficulty in using exhalation instrumentally may have caused earlier failures.

Language is not the only example of human learning that has eluded general process theory. The extraordinary persistence of acquired human motives has not been captured in ordinary laboratory situations. People, objects, and endeavors which were once unmotivating to an individual acquire and maintain strongly motivating properties. Fondness for the objects of sexual learning long after sexual desire is gone is a clear example. Acquisition of motives is not difficult to bring into the laboratory, and the extensive literature on acquired drives has often been taken as an analysis of acquired human motivation. A rat, originally unafraid of a tone, is shocked while the tone is played. Thereafter, the rat is afraid of the tone. But the analogy breaks down here; for once the tone is presented several times without shock, the tone loses its fear-inducing properties (Little & Brimer, 1968; Wagner, Siegel, & Fein, 1967). (The low resistance to extinction of the conditioned emotional response should not be confused with the high resistance to extinction of the avoidance response. This inextinguishability probably stems from the failure of the organism to stay around in the presence of the CS long enough to be exposed to the fact that shock no longer follows the CS, rather than a failure of fear of the CS to extinguish.) Yet, acquired motivators for humans retain their properties long after the primary motivation with which they were originally paired is absent. Allport (1937) raised the problem for general process theory as the "functional autonomy of motives." But in the 30 years since the problem was posed, the failure of acquired human motives to extinguish remains unanalyzed experimentally.

The notion of preparedness may be useful in analyzing persistent acquired motivation. Typically, investigations of acquired drives have paired arbitrary CSs with arbitrary primary motivators. It seems possible that if more prepared CSs were paired with primary motivators, the motivational properties of such CSs might be unusually resistant to extinction. Seligman, Ives, Ames, and Mineka (1970) conditioned drinking by pairing compound CSs with injections of hypertonic saline-procaine in rats. When the CS consisted only of exteroceptive stimuli (white box, white noise), conditioning occurred, but extinguished in a few days. When the interoceptive CS of one-hour water deprivation was added to the compound, conditioning occurred and persisted unabated for two months. It seems possible that preparedness of mild thirst for association with rapidly induced strong thirst may account for the inextinguishability of acquired drinking.

Are humans prepared to associate a range of endeavors and objects with primary motivators, and are such associations unusually persistent after the original motivators have left the scene? Here, as for language, viewing persistent acquired motives as cases of preparedness may make human motivation—both adaptive and maladaptive—more amenable to study.

PREPAREDNESS AND THE LAWS OF LEARNING

The primary empirical question has been answered affirmatively: The premise of equivalence of associability does not hold, *even in the traditional paradigms for which it was first assumed.* But does this matter? Do the same laws which describe the learning of unprepared events hold for prepared, unprepared, and contraprepared events? Given that an organism is prepared, and therefore learns with minimal input, does such learning have different properties from those unprepared associations that the organism acquires more painstakingly? Are the same mechanisms responsible for learning in prepared, unprepared, and relatively contraprepared situations?

We can barely give a tentative answer to this question, since it has been largely uninvestigated. Only a few pieces of evidence have been gathered to suggest that once a relatively prepared or contraprepared association has been acquired, it may not display the same family of extinction curves, values for delay of reinforcement, punishment effects, etc., as the lever press for food in the rat. Consider again the Garcia and Koelling (1966) findings: the association of tastes with illness is made with very different delays of reinforcement from ordinary Pavlovian associations. Unlike salivating to sounds, the association will be acquired with delays of up to one hour and more. Detailed studies which compare directly the delay of reinforcement gradients, extinction functions, etc., for prepared versus unprepared associations are needed. It would be interesting to find that the extinction and inhibition functions for prepared associations were different than for unprepared associations. If preparation underlies the observations of functional autonomy, prepared associations might be highly resistant to extinction, punishment, and other changes in instrumental contingencies. Breland and Breland (1966) reported that many of the "prepared" behaviors that the organisms they worked with acquired would persist even under counterproductive instrumental contingencies. To what extent would the autoshaped key pecking responses of Brown and Jenkins (1968) be weakened by extinction or punishment, as bar pressing for food is weakened? Williams and Williams (1969) reported that autoshaped key-pecking responses persist even when they actually "cost" the pigeon reinforcement.

Does contraprepared behavior, after being acquired, obey the same laws as unprepared behavior? Thorndike (1964) reported that when he finally trained licking for escape, the response no longer looked like the natural response, but was a pale, mechanical imitation of the natural response. Would the properties of the response differentiation and shaping of such behavior be like those of unprepared responses? The answer to this range of questions is presently unknown.

Preparedness has been operationally defined, and it is possible that different laws of learning may vary with the dimension. How can the dimension by anchored more firmly? Might different cognitive and physiological mechanisms covary with dimension?

Acquired aversions to tastes following illness is commonplace in humans. These Garcia phenomena are not easily modified by cognition in contrast to other classically conditioned responses in humans (e.g., Spence & Platt, 1967). The knowledge that the illness was caused by the stomach flu and not the Sauce Bearnaise does not prevent the sauce from tasting bad in the future. Garcia, Kovner, and Green (1970) reported that distinctive tastes can be used by rats as a cue for shock avoidance in a shuttlebox; but the preference for the taste in the home cage is unchanged. When the taste is paired with illness, however, the preference is reduced in the home cage. Such evidence suggests that prepared associations may not be cognitively mediated, and it is tempting to speculate that cognitive mechanisms (expectation, attention, etc.) come into play with more unprepared or contraprepared situations. If this is so, it is ironic that the "blind" connections which both Thorndike and Pavlov wanted to study lie in the prepared realm and not in the unprepared paradigms they investigated.

We might also ask if different neural structures underlie differently prepared learning. Does elaborate prewiring mediate prepared associations such as taste and nausea, while more plastic structures mediate unprepared and contraprepared associations?

We have defined the dimension of preparedness and given examples of it. To anchor the dimension we need to know the answers to three questions about what covaries with it: (*a*) Do different laws of learning (families of functions) hold along the dimension? (*b*) Do different cognitive mechanisms covary with it? (*c*) Do different physiological mechanisms also covary with preparedness?

PREPARATION AND THE GENERAL PROCESS VIEW OF LEARNING

If the premise of equivalence of associability is false, then we have reason to suspect that the laws of learning discovered using lever pressing and salivation may not hold for any more than other simple, unprepared associations. If the laws of learning for unprepared association do not hold for prepared or contraprepared associations, is the general process view salvageable in any form? This is an empirical question. Its answer depends on whether *differences* in learning vary systematically along the dimension of preparedness; the question reduces to whether the preparedness continuum is a nomological continuum. For example, if one finds that the families of extinction functions vary systematically with the dimension, then one might be able to formulate *general* laws of extinction. Thus, if prepared CRs extinguished very slowly, unprepared CRs extinguished gradually, and contraprepared CRs extinguished precipitously, such a systematic, continuous difference in *laws* would be a truly general law of extinction. But before such general laws can be achieved, we must first investigate what the laws of prepared and contraprepared associations actually are. If this were done, then the possibility of general laws of learning would be again alive.

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