

# THE "SUPERSTITION" EXPERIMENT: A REEXAMINATION OF ITS IMPLICATIONS FOR THE PRINCIPLES OF ADAPTIVE BEHAVIOR<sup>1</sup>

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Replication and extension of Skinner's "superstition" experiment showed the development of two kinds of behavior at asymptote: *interim activities* (related to adjunctive behavior) occurred just after food delivery; the *terminal response* (a discriminated operant) occurred toward the end of the interval and continued until food delivery. These data suggest a view of operant conditioning (the terminal response) in terms of two sets of principles: principles of behavioral variation that describe the origins of behavior "appropriate" to a situation, in advance of reinforcement; and principles of reinforcement that describe the selective elimination of behavior so produced. This approach was supported by (a) an account of the parallels between the Law of Effect and evolution by means of natural selection, (b) its ability to shed light on persistent problems in learning (e.g., continuity vs. noncontinuity, variability associated with extinction, the relationship between classical and instrumental conditioning, the controversy between behaviorist and cognitive approaches to learning), and (c) its ability to deal with a number of recent anomalies in the learning literature ("instinctive drift," auto-shaping, and auto-maintenance). The interim activities were interpreted in terms of interactions among motivational systems, and this view was supported by a review of the literature on adjunctive behavior and by comparison with similar phenomena in ethology (displacement, redirection, and "vacuum" activities). The proposed theoretical scheme represents a shift away from hypothetical "laws of learning" toward an interpretation of behavioral change in terms of interaction and competition among tendencies to action according to principles evolved in phylogeny.

The field of learning has undergone increasing fractionation in recent years. Interest in "miniature systems" and exact theories of local effects has grown to the detriment of any attempt at overall integration. Consequently, as one perceptive observer has noted:

At times, one senses a widespread feeling of discouragement about the prospects of ever getting clear on the fundamentals of conditioning. Attempts to arrive at firm decisions about alternative formulations rarely produce incisive results. Every finding seems capable of many explanations. Issues become old, shopworn, and disappear without a proper burial [Jenkins, 1970, pp. 107-108].

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The present article outlines an attempt to redress this imbalance. It is organized around the problem of "superstitious" behavior, originated by Skinner some years ago, which plays a crucial part in the empirical and theoretical foundations of current views of learning. Discussion of a replication of Skinner's original experiment leads to an account of the relationships between evolution and learning, and a system of classification derived therefrom. The paper concludes with a theoretical account of "superstition" and some related phenomena.

### THE "SUPERSTITION" EXPERIMENT

In his classic experiment on "superstitious" behavior, Skinner (1948) showed that the mere delivery of food to a hungry animal is sufficient to produce operant conditioning. The pigeons in that experiment were allowed 5-second access to food every 15 seconds, and food delivery was independent of their behavior. Nevertheless, nearly every pigeon developed a recognizable form of stereotyped, superstitious behavior that became temporally correlated with food delivery as training progressed.

Skinner's (1961) analysis of this phenomenon is a straightforward application of the Law of Effect:

The conditioning process is usually obvious. The bird happens to be executing some response as the hopper appears; as a result it tends to repeat this response. If the interval before the next presentation is not so great that extinction takes place, a second "contingency" is probable. This strengthens the response still further and subsequent reinforcement becomes more probable [p. 405].

Skinner's observations were quickly repeated in a number of laboratories. The apparent simplicity and reliability of the phenomenon, coupled with the plausibility of Skinner's interpretation of it, and the more exciting attractions of work on reinforcement schedules then developing, effectively stifled further study of this situation. However, both the experiment and his explication played a crucial role in advancing Skinner's theoretical view of operant behavior as the strengthening of unpredictably generated ("emitted") behavior by the automatic action of reinforcers.

Two kinds of data obtained in recent years raise new questions about "superstition" in this sense. First, experiments with time-related reinforcement schedules have shown the development of so-called "mediating" behavior during the waiting period, when the animal is not making the reinforced response. Thus, on schedules which require the animal to space his responses a few seconds apart if they are to be effective in producing reinforcement (spaced-responding schedules), pigeons often show activities such as pacing and turning circles. Simi-

larly, on fixed-interval schedules, in which the first response  $t$  seconds after the preceding reinforcement is effective in producing reinforcement, pigeons may show a similar behavior during the postreinforcement "pause" when they are not making the reinforced response. Other species show activities of this sort in the presence of appropriate environmental stimuli; for example, schedule-induced polydipsia, in which rats reinforced with food on temporal reinforcement schedules show excessive drinking if water is continuously available (Falk, 1969). None of these activities is reinforced, in the sense of being contiguous with food delivery, yet they are reliably produced in situations similar in many respects to Skinner's superstition procedure. Possibly, therefore, some of the activities labeled superstitious by Skinner, and attributed by him to accidental reinforcement of spontaneously occurring behavior, may instead reflect the same causal factors as these mediating activities.

Second, a number of experiments have demonstrated the development of behavior in operant conditioning situations by a process more reminiscent of Pavlovian (classical) conditioning than Law of Effect learning as commonly understood. Breland and Breland (1961) reported a series of observations showing that with continued operant training, species-specific behavior will often emerge to disrupt an apparently well-learned operant response. In the cases they describe, behavior closely linked to food (presumably reflecting an instinctive mechanism) began to occur in advance of food delivery, in the presence of previously neutral stimuli ("instinctive drift"). Since these "irrelevant" activities interfered with food delivery by delaying the occurrence of the reinforced response, they cannot be explained by the Law of Effect. A description in terms of stimulus substitution—a principle usually associated with Pavlovian conditioning—is better, although still not completely satisfactory. More recently, Brown and Jenkins (1968) have shown that hungry pigeons can be trained to peck a lighted response key simply by illuminating the key for a few seconds before food delivery. Fewer than a

hundred light-food pairings are usually sufficient to bring about key pecking. The relationship between this "auto-shaping" procedure and Pavlovian conditioning is further emphasized by an experiment reported by Williams and Williams (1969). They found that auto-shaped key pecking is maintained even if the key peck turns off the light on the key and thus prevents food delivery on that occasion. All these experiments show the occurrence of food-related behaviors, in anticipation of food, under conditions more or less incompatible with the Law of Effect.

The auto-shaping procedure is operationally identical to Pavlovian conditioning with short delay (the light-food interval in these experiments is typically 8 seconds). Therefore the eventual emergence of food-related behavior, in anticipation of food delivery, is not altogether surprising—although the directed nature of key pecking has no counterpart in principles of conditioning that take salivation as a model response. The superstition situation is also equivalent to a Pavlovian procedure—in this case temporal conditioning, in which the UCS (food) is simply presented at regular intervals. Perhaps, therefore, prolonged exposure to this situation will also lead to the emergence of food-related behavior in anticipation of food. Possibly the superstitious behavior described by Skinner includes activities of this sort, that occur in anticipation of food, as well as mediating activities that occur just after food delivery.

The present experiment affords an opportunity to test these ideas. It provides comparative data on the effect of fixed versus variable interfood intervals on superstitious responding (Skinner used only fixed intervals), as well as allowing a comparison between response-dependent and response-independent fixed-interval schedules. The experiment also extends Skinner's work by recording in some detail both the kind and time of occurrence of superstitious activities. The emphasis is on the steady-state adaptation to the procedures, but some data on the course of development of superstition are presented.

We hope to show that careful study of the superstition situation makes necessary a re-

vision of Skinner's original interpretation and, by extension, requires a shift of emphasis in our view of adaptive behavior.

### Method

#### Subjects

Six pigeons were used: four white Carneaux, two with experimental experience (Birds 31 and 29) and two experimentally naive (Birds 47 and 49). Two other pigeons were of a local (Toronto, Ontario) breed and were experimentally naive (Birds 40 and 91). All the birds were maintained at 80% of their free-feeding weights throughout.

#### Apparatus

Two standard Grason-Stadler operant conditioning chambers were used. The response keys were covered with white cardboard except during the response-dependent condition when one key was exposed and transilluminated with white light. Data were recorded by a clock, digital counters, and an event recorder. Food delivery was controlled automatically by relays and timers. Behaviors were recorded via push buttons operated by an observer. A tape recorder was used to record comments and corrections. Except for the push buttons and the tape recorder, all programming and recording apparatus was located in a separate room. White noise was present in the experimental chamber and, together with the noise of the ventilating fan, served to mask extraneous sounds.

#### Procedure

Three schedules of food delivery were used: (a) A response-independent fixed-interval (FI) schedule in which the food magazine was presented at 12-second intervals. (b) A response-independent variable-interval (VI) schedule in which the food magazine was presented on the average every 8 seconds. The following sequence of interreinforcement intervals was used, programmed by a loop of 16-millimeter film with holes punched at appropriate intervals: 3, 6, 6, 12, 9, 7, 3, 10, 21, 6, 5, 11, 8, 5, 3, 9, 7, 9, 5, 13, 3, 8, 9, 4, 7, 12, 11, 3, 6, 5, and 9 seconds. (c) A response-dependent FI schedule in which food was delivered (reinforcement occurred) for the first key peck 12 seconds or more after the preceding reinforcement.

Food delivery involved 2-second access to mixed grain. Sessions ended after the sixty-fourth food delivery and the pigeons were run daily.

*Habituation sessions.* All the birds were given a number of daily 10-minute sessions when no food was delivered; the birds were simply placed in the chamber and their behavior observed and recorded. The birds received the following numbers of such sessions: Bird 31, 3; Bird 29, 3; Bird 47, 15; Bird 49, 15; Bird 40, 7; Bird 91, 7. Note that the

experimentally naive birds received more habituation exposure.

*Response-independent training.* Four of the pigeons were then given a number of sessions on each of the two response-independent procedures, either FI followed by VI, or the reverse. The birds received the procedures in the indicated order (number of sessions in parentheses): Bird 31: FI 12 (26), VI 8 (111); Bird 29: VI 8 (26), FI 12 (109); Bird 47: VI 8 (36), FI 12 (36); Bird 49: FI 12 (37), VI 8 (36).

*Response-dependent training.* Two of the birds already trained on the response-independent procedures were then switched to the response-dependent FI 12 for the following numbers of sessions: Bird 31, 37; Bird 47, 52. The two naive birds (40 and 91), after their habituation sessions, were given one session of response-independent FI 12 when the food magazine operated every 12 seconds, but contained no grain. This was to habituate these birds to the sound of the mechanism. The next day these two birds were placed in the experimental box with the food magazine continuously available for 10 minutes. The following day the two birds were introduced to the response-dependent FI 12-second schedule. A small piece of black tape was placed on the lighted response key, as an inducement to pecking, for this first session only. All the birds pecked the key during the first session of the response-dependent procedure. This rather elaborate key-training procedure was designed both to prevent the experimenter from shaping the naive birds' behavior, and to avoid the possibility of "superstitious" conditioning, which might be entailed by some form of auto-shaping. Bird 40 received a total of 45 sessions of the response-dependent FI 12-second schedule, Bird 91 received 38.

*Response description and scoring.* Response categories were arrived at on the basis of initial observation during the habituation sessions and were altered as necessary to accommodate new behaviors. The names, descriptions, and numbers of the categories appear in Table 1. Responses were scored (by pushing the appropriate button) in two ways, either discretely or continuously. If a response tended to occur in discrete units (e.g., pecking), then the appropriate button was pushed each time an instance of the response occurred. The observer was the same throughout (VLS), and the maximum recordable rate for discrete responses was 3-4 per second. A continuous response is one which took an indefinite amount of time (e.g., facing magazine wall); the appropriate button was pressed throughout the duration of a continuous response. Discrete responses were pecking (wall, key, or floor) and quarter circles, all the rest were continuous responses. In general, the response categories were mutually exclusive. The only exception is facing Magazine wall ( $R_1$ ), which at various times occurred with Flapping wings ( $R_5$ ), Moving along magazine wall ( $R_6$ ), and Pecking ( $R_7$ ).

## Results

The data of interest in this experiment are the kind and amount of behavior at different points in time following the delivery of food. As training progressed, a systematic pattern of behavior as a function of postfood time began to emerge. The properties of this steady-state pattern for VI and FI schedules is discussed first, followed by a description of the changes that took place during acquisition.

### Steady-State Behavior

In the steady state, the behavior developed under both the FI and VI procedures fell reliably into two classes: (a) The *terminal response* was the behavior that consistently occurred just before food delivery. It began 6-8 seconds after food delivery on the FI procedures, and about 2 seconds after food on the VI procedure, and usually continued until food delivery. (b) A number of activities usually preceded the terminal response in the interval. These activities are probably indistinguishable from what has been termed mediating behavior, but we prefer the more descriptive term *interim activities*. These activities were rarely contiguous with food.

Figure 1 shows the performance averaged across three sessions of steady-state responding under all conditions for all the pigeons. The left-hand panels show the response-dependent FI schedule; the middle panels, the response-independent (superstitious) FI schedule; and the right-hand panels, the response-independent VI procedure, for the four birds exposed to each. The graphs show the probability (relative frequency) with which each of the activities occurred during each second of postfood time. Each bird shows the clear division between terminal and interim activities already alluded to. Excluding  $R_1$ , and the results for Bird 29 on VI, Pecking ( $R_7$ ) was the terminal response for all the response-independent procedures. For Bird 29, the terminal response Head in magazine ( $R_{11}$ ) became an interim activity following the shift from VI to FI and was replaced as terminal response by Pecking. Pecking remained the terminal response for this bird throughout a sequence

TABLE 1  
DESCRIPTION OF OBSERVED ACTIVITIES

Response no.	Name	Description
R <sub>1</sub>	Magazine wall	An orientation response in which the bird's head and body are directed toward the wall containing the magazine.
R <sub>2</sub>	Pecking key	Pecking movements directed at the key.
R <sub>3</sub>	Pecking floor	Pecking movements directed at the floor.
R <sub>4</sub>	$\frac{1}{4}$ circle	A response in which a count of one $\frac{1}{4}$ circle would be given for turning 90° away from facing the magazine wall, a count of two for turning 180° away, three for 270°, and four for 360°.
R <sub>5</sub>	Flapping wings	A vigorous up and down movement of the bird's wings.
R <sub>6</sub>	Window wall	An orientation response in which the bird's head and body are directed toward the door of the experimental chamber containing the observation window.
R <sub>7</sub>	Pecking	Pecking movements directed toward some point on the magazine wall. This point generally varied between birds and sometimes within the same bird at different times.
R <sub>8</sub>	Moving along magazine wall	A side-stepping motion with breastbone close to the magazine wall, a few steps to the left followed by a few steps to the right, etc. Sometimes accompanied by (a) beak pointed up to ceiling, (b) hopping, (c) flapping wings.
R <sub>9</sub>	Preening	Any movement in which the beak comes into contact with the feathers on the bird's body.
R <sub>10</sub>	Beak to ceiling	The bird moves around the chamber in no particular direction with its beak directed upward touching the ceiling.
R <sub>11</sub>	Head in magazine	A response in which at least the beak or more of the bird's head is inserted into the magazine opening.
R <sub>12</sub>	Head movements along magazine wall	The bird faces the magazine wall and moves its head from left to right and/or up and down.
R <sub>13</sub>	Dizzy motion	A response peculiar to Bird 49 in which the head vibrates rapidly from side to side. It was apparently related to, and alternated with, Pecking (R <sub>7</sub> ).
R <sub>14</sub>	Pecking window wall	Pecking movements directed at the door with the observation window in it.
R <sub>15</sub>	Head to magazine	The bird turns its head toward the magazine.
R <sub>16</sub>	Locomotion	The bird walks about in no particular direction.

of response-independent procedures after the ones reported here, lasting for a total in excess of 90 sessions. A curious idiosyncratic head movement accompanied pecking by Bird 49 (R<sub>13</sub>: Dizzy motion) on response-independent FI, although it disappeared following the switch to VI. The locus of Pecking on the magazine wall differed from bird to bird and varied both across and within sessions for some birds. The stable features of this response were its topography and its restriction to the general area of the magazine wall. A variety of interim activities occupied the early parts of the FIs and the period within 2 or 3 seconds after food on

the VI schedule: Pecking floor (R<sub>3</sub>),  $\frac{1}{4}$  circles (R<sub>4</sub>), Flapping wings (R<sub>5</sub>), Moving along magazine wall (R<sub>8</sub>), and Beak to ceiling (R<sub>10</sub>) were the most frequent. The interim activities were therefore more variable from bird to bird than was the terminal response.

The pattern of behavior characteristic of each interval was little affected by whether or not food was dependent on key pecking. The similarity between the patterns during response-dependent and response-independent FI is particularly striking for Bird 47, who was switched directly from the response-independent to the response-depend-

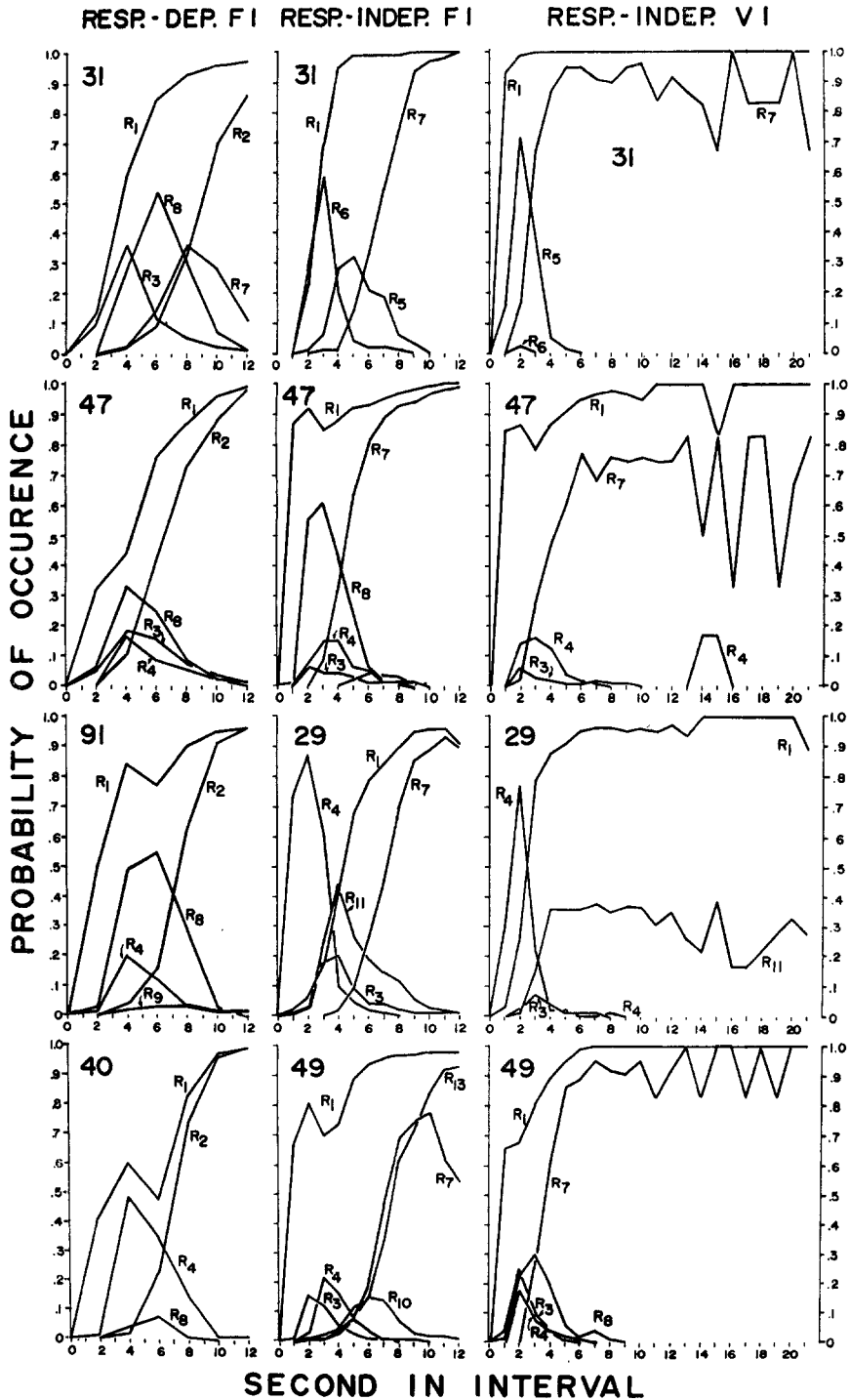


FIG. 1. Probability of each behavior as a function of postfood time for all birds for all three experimental conditions, averaged over three sessions of steady-state responding under each condition. (Each point gives the probability that a given behavior occurred in that second of postfood time. Data for the response-dependent condition are averaged across 2-second blocks. Behaviors ( $R_i$ ) are identified in Table 1.)

ent procedure. Bird 31, for whom the response-independent and response-dependent FIs were separated by response-independent VI, shows more variation in the interim activities under the two conditions. Birds 91 and 40, who were exposed only to the response-dependent FI, show a similar pattern of interim activities to the response-independent birds, although again there is some variation as to details. This similarity cannot be attributed to the procedure used to shape key pecking (see Procedure). After the imposition of the key-pecking contingency, Bird 31 retained the old Pecking response as an interim activity restricted to a period in the interval just before key pecking. Bird 47 showed a similar effect of his response-independent experience in that a high proportion of his key pecks failed to depress the key sufficiently to activate the automatic recording circuitry, although they were recorded as key pecks by the observer. Overall, these data provide no evidence for substantial changes in the pattern of terminal or interim activities traceable to the imposition of a key-pecking requirement.

The general pattern of terminal and interim activities during the response-independent VI schedule was similar to the FI procedures. Differences were restriction of the interim activities to the first 2 or 3 seconds of postfood time rather than to the first 6 or 7 seconds (with one exception, to be discussed), and the smaller number of interim activities, in most cases. Under both fixed and variable procedures, once the terminal response began in an interval, it continued until food delivery. The exception is Bird 47 who showed a drop in the probability of the terminal response (Pecking,  $R_7$ ) accompanied by a transient increase in the interim activity of  $\frac{1}{4}$  circles ( $R_4$ ) at the 14–15-second postfood time. A similar slight drop in the probability of Pecking, although not accompanied by an interim activity, was also shown by Bird 31. These differences are related to the properties of the VI schedule (see Discussion).

### *Sequential Structure*

Figure 2 indicates something of the sequential structure of the behavior occupying

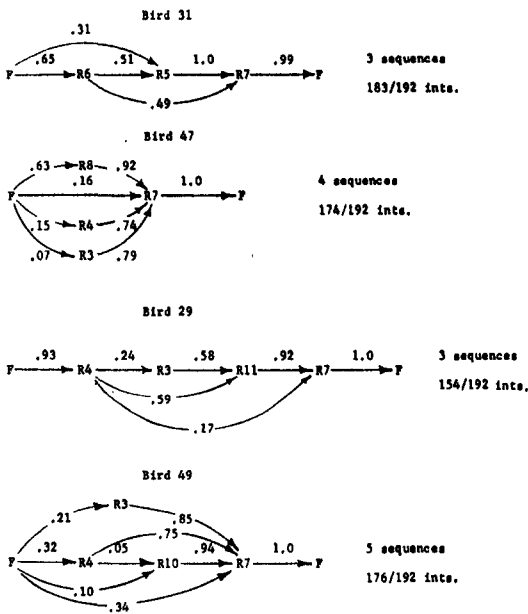
each interfood interval for each bird during the response-independent procedures. The figure summarizes the two to five behavior sequences that account for most of the intervals during the three steady-state sessions. For simplicity, no account is taken either of interbehavior times or of the duration of each activity in this method of representation. The most striking characteristics of these sequences are: (a) that each bird showed only a small number of typical sequences (usually three or four); (b) that the sequencing was very rigid, so that although a given behavior might fail to occur during a particular interval, it never occurred out of sequence—this is indicated by the absence of return arrows ("loops") in these diagrams; and (c) that the variability of the sequences was greatest early in the interval and least at the end, in the period just preceding food delivery—this is indicated by the absence of "forks" (ambiguous transitions from one behavior to two or more others, as in the diagram for Bird 31 where  $R_6 \rightarrow R_6$  or  $R_6 \rightarrow R_7$  with approximately equal probability) late in the sequence of behaviors shown in the diagrams. This regular sequencing did not occur early in training, as indicated in Figure 4, discussed below.

An inviting possibility raised by these regular sequences is that this behavior may be described by some kind of Markov chain (cf. Cane, 1961). Although the argument cannot be presented in full here, this assumption cannot be sustained for a number of reasons, the most important of which are (a) that the duration of a bout of a given activity was shorter the later the activity began within an interval, and (b) that the time between two successive activities was shorter the later in the interval the first activity ended. These and other considerations suggest that postfood time was the most important factor controlling both the onset and offset of each activity in the sequence.

### *Acquisition*

Figure 3 shows acquisition data for naive Bird 49, through and beyond the period when his behavior became stable on re-

## FIXED - INTERVAL



## VARIABLE - INTERVAL

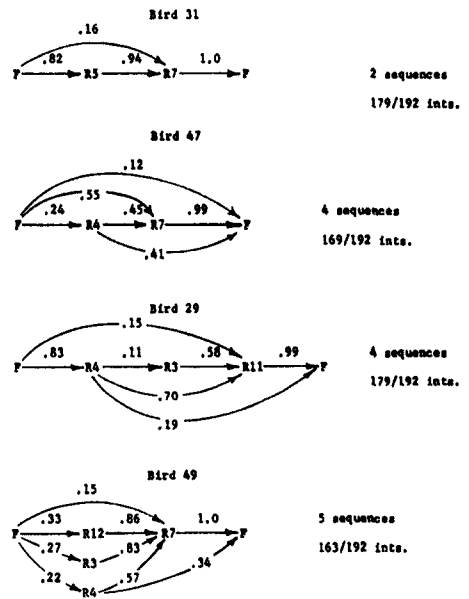


FIG. 2. Steady-state sequences: Sequential relationships among behaviors during the last three sessions of each response-independent procedure for all four pigeons. (Fixed-interval is on the left, variable-interval on the right. Numbers give probabilities of the indicated transitions. Number of different sequences on which the diagrams are based and number of intervals (out of 192) accounted for by these are indicated. "F" is food delivery, and behaviors ( $R_i$ ) are identified in Table 1. Note: probabilities at each "fork" do not always sum to 1.0 because not every sequence is accounted for.)

response-independent FI. The graphs show (as a function of sessions) the probability of the various behaviors in each of the six 2-second periods making up the FI. The most noteworthy characteristic of acquisition is the relatively sudden disappearance of the behavior Head in magazine, which was almost the only behavior to occur during the first few sessions for Bird 49, in favor of the terminal response Pecking on magazine wall. For Bird 49 this transition took place between the seventh and eighth sessions of FI without any prior history of pecking during earlier sessions. Once pecking became established as a terminal response, the only further change was a slow decline in the probability of the response during early parts of the interval (e.g., between 3 and 8 seconds). The other birds showed similar results on their first exposure to the response-independent procedures; for each bird, pecking on the magazine wall

first occurred during the following sessions: for Bird 31, on the first FI session; Bird 29, twelfth session of FI following the switch from response-independent VI; Bird 47, twenty-eighth session of VI; Bird 49, eighth session of FI. Thus, although one of the experienced birds pecked during the first response-independent session (31), the other did not until being switched to a different schedule (29). No bird shifted to a different terminal response once he began pecking; the total number of sessions of response-independent experience (FI and VI) for each of the four birds following the onset of pecking was as follows: Bird 31, 140; Bird 29, 96; Bird 47, 45; Bird 49, 66. For Bird 29, the terminal response during the response-independent FI procedure (i.e., before the onset of pecking) was Head in magazine. As with the other birds, once pecking appeared it was in full strength almost immediately and further experience



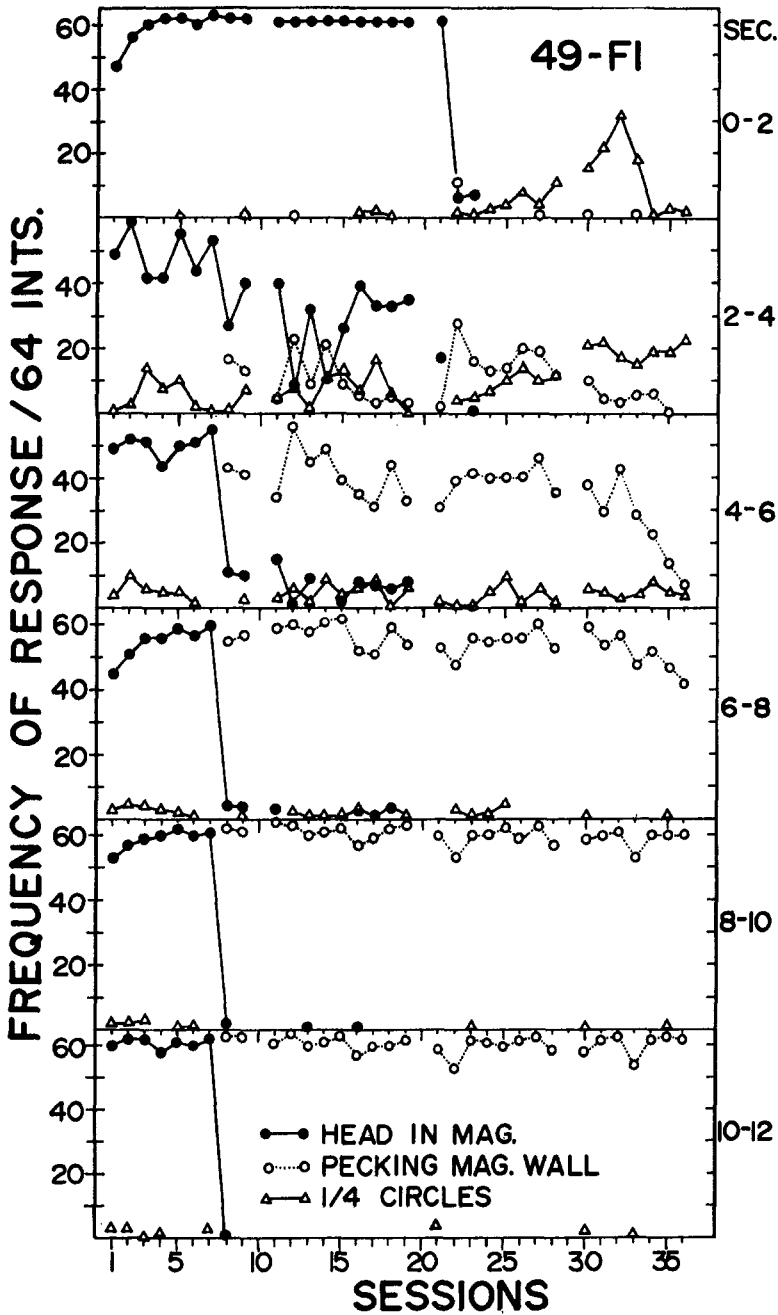
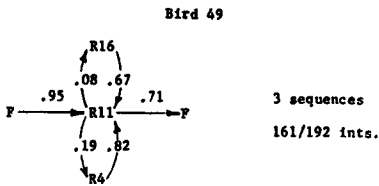
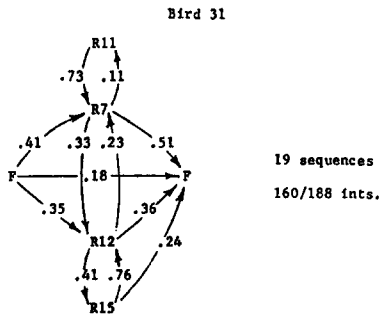


FIG. 3. Development of the terminal response. (The graph shows, for Bird 49 on the response-independent fixed-interval procedure, the transition from Head in magazine ( $R_H$ ) to Pecking ( $R_T$ ) as terminal response, and includes one interim activity,  $\frac{1}{4}$  circles ( $R_I$ ), for comparison. Each panel covers 2 seconds of the 12-second interval and indicates the number of intervals (out of 64) in which a given response occurred in that 2-second block for each session over the first 36 sessions. Gaps indicate days for which data are not available. Bird 49 was not run for a 9-day period between Sessions 21 and 22.)

**FIXED - INTERVAL**



**VARIABLE - INTERVAL**

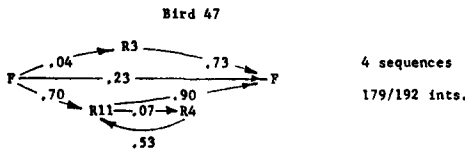
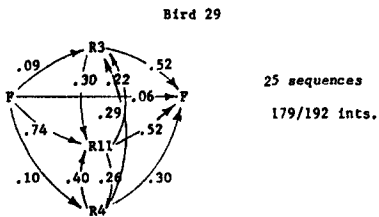


FIG. 4. Sequence data, in the same form as Figure 2, for the first three sessions under the first response-independent procedure to which each bird was exposed. (Details as in Figure 2.)

had the effect merely of restricting it more to the later parts of the interval.

Figure 4 shows sequence data, in the same form as Figure 2, for the first three sessions of the first response-independent schedule to which each bird was exposed. Approximately the same proportion of the total number of sequences observed within 192 intervals is accounted for by these dia-

grams as in the diagrams of Figure 2 for the last three sessions. The data for the first three sessions are much more variable, showing repetitions of a behavior within an interval and reversals of sequence; three out of four birds show no single terminal response. Thus, the regularities apparent in Figure 2 are evidently a real effect of training and not simply an artifact of this method of representation.

*Discussion*

The results of this experiment confirm the suggestion that the "superstition" situation generally produces two distinct kinds of activity: interim activities that occur at short and intermediate postfood times, and the terminal response that begins later in the interval and continues until food delivery. It is not always clear from Skinner's original discussion just which kind of behavior he was observing. In one case, he briefly describes an experiment in which the interfood interval was 60 seconds, and the "superstitious" response (a "well-defined hopping step from the right to the left foot") was automatically recorded. In this case, the behavior was evidently a terminal response, since it occurred with increasing frequency through the interval:

The bird does not respond immediately after eating, but when 10 or 15 or even 20 sec. have elapsed it begins to respond rapidly and continues until the reinforcement is received [Skinner, 1961, p. 406].

On other occasions, however, Skinner may have been observing interim activities, as, in our experience, they are sometimes much more striking than the terminal response, especially early in training when they may include actions like jumping in the air, vigorous wing flapping, and idiosyncratic head and limb movements. We have also sometimes observed interim activities during sessions when there was no obvious terminal response.

*Nature of the Terminal Response*

The data from both FI and VI schedules of food presentation indicate that the probability of the terminal response at different postfood times was a function of the prob-

ability of food delivery at those times: the probability of the terminal response increased just in advance of increases in the probability of food delivery, and decreased just after decreases in that probability. Thus, on the FI schedule, the terminal response began at postfood times greater than about 8 seconds, corresponding to a probability of food delivery of zero at times less than 12 seconds, and one thereafter. On the VI schedule, probability of food delivery was zero at postfood times less than 3 seconds (the shortest interfood interval in the VI sequence), and increased thereafter. Correspondingly, the birds began their terminal response after 2 seconds or so of postfood time and continued until food delivery. The data for Birds 29 and 47 on the VI schedule provide additional confirming evidence. Both birds showed a small decline in the probability of the terminal response between Seconds 12 and 17 of postfood time, accompanied by a brief reappearance of an interim activity (between Seconds 13 and 16) in the case of Bird 47. This brief decrease in the terminal response, and accompanying reappearance of an interim activity, corresponds to the zero probability of food in the region of postfood time between 13 and 21 seconds (because the VI sequence used had no interfood interval between 13 and 21 seconds in length—see Method). Catania and Reynolds (1968) showed a similar relationship between rate of key pecking and probability of reinforcement, as a function of postreinforcement time, on a variety of conventional (response-dependent) VI reinforcement schedules. This similarity and the similarities between the response-independent and response-dependent conditions of this experiment emphasize that the terminal response must be regarded as a discriminated operant in Skinner's (1958) sense.

Herrnstein (1966) reported an experiment in which the rate of key pecking on response-independent FI 11 seconds was lower than on response-dependent FI 11. However, in the present experiment, considering just the terminal response of Pecking on magazine wall ( $R_T$ ), we found no evidence for a difference in either probability

or rate of response favoring the response-dependent procedure. Indeed, rate of key pecking (defined as switch operations) under the response-dependent FI was generally lower than the observer-defined rate of pecking under the response-independent FI because many pecks failed to break the switch contact. On the other hand, because the location of pecking was much more variable under the response-independent condition, a comparison between key pecking (in the response-dependent condition) with pecking on a comparable area of the magazine wall (in the response-independent condition) would show more responding under the response-dependent condition. This result raises the possibility that the effect of the response-dependency, in operant conditioning experiments using interval reinforcement schedules, may be largely one of determining (perhaps imperfectly) the *location* of pecking, rather than either its form or its frequency of occurrence.

#### *Nature of the Interim Activities*

Falk (1969, 1970) has coined the term *adjunctive behavior* for a variety of activities that are induced in a number of different animal species (rats, pigeons, monkeys, chimpanzees) by intermittent schedules of reinforcement. These activities generally occur just after reinforcement, when the reinforced (i.e., terminal) response is not occurring. However, the important variable in determining their temporal location seems to be the low probability of further reinforcement in the immediate postreinforcement period (rather than the interruption of eating), since they also occur following brief stimulus presentations on second-order interval schedules (Rosenblith, 1970), following each response on spaced-responding (differential reinforcement of low rates) schedules (Segal & Holloway, 1963), during time-out periods (Wüttke, 1970), only after the last pellet when a number are delivered consecutively at the end of each FI (Keehn, 1970), and later in the interval during long FI schedules (Segal, Oden, & Deadwyler, 1965). They are not elicited (e.g., by frustration) in any obvious sense, since they take some time to develop (Reynierse &

Spanier, 1968). They are related to motivational systems, since a number of activities—for example, polydipsia (excessive drinking), pica (eating nonfood material), wheel running, and schedule-induced aggression—may occur more or less interchangeably depending on the presence of appropriate stimuli; and animals will learn an operant response in order to obtain an opportunity to engage in one of these activities (Falk, 1970).

The interim activities in the present experiment appear to reflect the same causal factors as adjunctive behavior: they occur at times when reinforcement is not available and the terminal response is not occurring; they occur on both response-dependent and response-independent schedules (cf. Azrin, Hutchinson, & Hake, 1966; Burks, 1970; Flory, 1969); and they occur on both FI and VI schedules. Adjunctive behavior requires appropriate stimuli (water for polydipsia, wood shavings for pica, etc.) to which it is directed and by which it can be modified, within limits. This is unlikely to be a crucial difference, however, since appropriate stimuli were not available in the present experiment (had water been available, schedule-induced drinking would almost certainly have been observed in lieu of the interim activities; cf. Shanab & Peterson, 1969), and behavior resembling interim activities has occasionally been reported under conditions when most animals show adjunctive behavior, thus

[this rat] was atypical in that it did not develop polydipsia . . . exhibiting instead a number of stereotyped behaviors, like rearing and running to the corners of the experimental chamber, between reinforcements [Keehn, 1970, pp. 164–167].

We return to a theoretical account of interim and adjunctive behavior in the concluding section.

An analogy can be drawn between the terminal and interim activities here and the classical dichotomy between consummatory and appetitive behavior (Craig, 1918). Thus, pecking, the stable terminal response, is a food-elicited (consummatory) activity in pigeons, and the interim activities were quite variable, as might be expected of appetitive behavior. Moreover, the variability of

the sequences (as measured by the number of “forks” in the sequence) was greatest at the beginning of the sequence and decreased toward the end (cf. Figure 2). More or less unlearned sequences terminating in consummatory acts show a similar reduction in variability toward the end of the sequence (e.g., Morris, 1958).

In a similar vein, Falk (1970) has compared adjunctive behavior with displacement activities (Tinbergen, 1952):

In both adjunctive behavior and displacement activity situations, the interruption of a consummatory behavior in an intensely motivated animal induces the occurrence of another behavior immediately following the interruption [p. 305].

At the present stage of knowledge, these comparisons do little more than group together a number of puzzling phenomena that cannot as yet be convincingly explained, either by ethological principles or by the Law of Effect. We can choose either to accept these behaviors as anomalies within our present conceptual system, hoping that further research will show how to reconcile them, or revise the system in a way that will accommodate them more naturally. The first alternative is becoming increasingly hard to maintain, as it becomes clear that these behaviors are of wide occurrence, and as they continue to resist attempts to explain them in conventional terms. Polydipsia is the most widely studied adjunctive behavior, and Falk (1969) summarizes research results as follows:

It is not explicable in terms of any altered state of water balance initiated by the experimental conditions. It cannot be attributed to adventitious reinforcing effects. Since it cannot be related to abnormal water losses, chronic internal stimulation arising from unusual states . . . or from injury to the central nervous system, the overdrinking would be classified clinically as primary or psychogenic polydipsia [p. 587].

A revision of the conceptual foundations of operant behavior, which will deal naturally with these behaviors, as well as guide research into more profitable channels, seems called for.

#### *Development of the Terminal Response*

The development of the terminal response provides some clues toward an alternative

conception. Changes in the terminal response throughout acquisition rule out an unqualified application of the Law of Effect as a description of the process. For example, three of the four birds made the response Head in magazine at a higher frequency and for a larger fraction of the total time than any other response, for the first few sessions of exposure to the superstition procedure (either FI or VI, depending on the bird). This behavior was also the one most often contiguous with the delivery of food. Yet during later sessions, it dropped out abruptly and was replaced by Pecking. Thus the development of Pecking as terminal response resembles the findings of Williams and Williams and of Breland and Breland, much more than it does the strengthening of an emitted response, in Skinner's terms, by the automatic action of food as a reinforcer. In all these cases, the presentation of food at a predictable time resulted, after training, in the regular occurrence of a food-related behavior in anticipation of food delivery, quite independently of the demand characteristics of the situation (i.e., the reinforcement schedule). Given that food is a stimulus that elicits pecking by pigeons, the appearance of pecking, in anticipation of food, in the present experiment is an instance of the principle of stimulus substitution (Hilgard & Marquis, 1940). The stimulus is, of course, a temporal one (postfood time), and the situation is analogous, therefore, to Pavlovian temporal conditioning (Pavlov, 1927), both operationally and in its conformity to the substitution principle. Therefore, it is tempting to attribute the appearance of pecking to a classical conditioning mechanism, and leave it at that.

A number of considerations suggest that this explanation will not suffice, however: (a) The behavior of one bird (29) is a partial exception. He showed a terminal response (Head in magazine) different from Pecking, although this response was metastable (Staddon, 1965), in the sense that it was displaced by Pecking following a schedule shift. In addition, Skinner's results and informal observations in a number of laboratories indicate that the superstition procedure may generate behaviors other than

pecking that persist for considerable periods of time. Presumably, many of these behaviors are terminal responses, in our sense, and, metastable or not, they cannot be dismissed in favor of a Pavlovian account of all terminal responses in superstition situations. (b) The results of Rachlin (1969), who was able to obtain spontaneous key pecking by means of the auto-shaping procedure of Brown and Jenkins, but using electric shock-reduction rather than food as the reinforcer, also complicate the picture, since it is not clear that pecking is elicited by shock, as it is by food. The results of Sidman and Fletcher (1968), who were able to auto shape key pushing in rhesus monkeys, are also not readily explicable by stimulus substitution. (c) Williams and Williams (1969) note that the directed nature of the key peck in the auto-shaping situation is not readily accommodated within a Pavlovian framework:

the directed quality of the induced pecking does not follow naturally from respondent principles (see also Brown and Jenkins, 1968). It is unclear, for example, why pecking would be directed at the key rather than the feeder, or indeed why it would be directed anywhere at all [p. 519].

A similar objection can be raised here, since the terminal response of pecking was always directed at the magazine wall, although it is important to notice that this objection is more damaging to an interpretation in terms of classical conditioning than one couched simply in terms of stimulus substitution. (d) Finally, there is the problem of the skeletal nature of pecking. Ever since Skinner's (1938) original suggestion, it has become increasingly common to restrict the concept of classical conditioning to automatically mediated responses. Some criticisms of this convention are presented below, and Staddon (1970a) has argued against the operant/respondent and emitted/elicited dichotomies. For the moment, let it be said that the objection to the skeletal nature of the response is only a problem for an interpretation of the development of pecking in terms of classical conditioning, as traditionally conceived. It does not conflict with a stimulus substitution interpretation.

In summary, it is clear that while the principle of stimulus substitution describes the development of the terminal response in a majority of cases, and is more successful than an appeal to Pavlovian principles modeled on the salivation reference experiment, it is not adequate as a universal account. We turn now to the possibility of a general scheme to deal with these anomalous facts.

#### EVOLUTION AND LEARNING

Objectively considered, the subject matter of the psychology of animal learning—the behavior of animals—is a part of biology. This commonality does not extend to terms and concepts, however. Although the ethologists have investigated unlearned behaviors in a variety of species, learning remains almost exclusively the possession of psychologists. Consequently, the theoretical foundations of the study of learning, such as they are, have evolved almost independently of biology. It is now 15 years since Verplanck (1955) wrote: “the structure of the theory of unlearned behavior and that of learned behavior must prove to be similar if not identical [p. 140],” but little progress toward a unified set of concepts has been made. Yet the facts discussed in the previous section seem to demand an interpretation within the context of adaptive behavior as a whole.

The principles of evolution by natural selection provide a unifying framework for biology. Pfaffman (1970) has recently commented:

I am impressed by the extent to which evolution, the genetic machinery, and biochemistry provide for biologists a common language and unity of theory that overrides the molecular versus organismic debate within biology. In contrast, it is obvious that there is no unified theory of behavior for all students of behavior [p. 438].

These considerations—the commonality of subject matter between biology and animal psychology, the probable common basis of learned and unlearned behavior, the unifying role of evolutionary processes in biology—all suggest the application of evolutionary principles to the psychology of learning in animals. In recent years, several papers

have drawn attention to the similarities between evolution and learning (e.g., Breland & Breland, 1966; Broadbent, 1961; Gilbert, 1970; Herrnstein, 1964; Pringle, 1951; Skinner, 1966a, 1966b, 1969), but no version of the evolutionary approach has proved influential as yet. The main reasons for this failure, perhaps, have been the comparative effectiveness of traditional versions of the Law of Effect in dealing with the limited phenomena of laboratory learning experiments and the lack of a substantial body of facts clearly in conflict with accepted theory. To some extent, of course, the second factor reflects the first—although it would be cynical to speculate that the kind of experiments psychologists do are often such as to preclude data that might go beyond current theory. In any event, neither of these reasons now holds true.

The growing number of facts on auto-shaping, instinctive drift, adjunctive, and superstitious behaviors are not readily accommodated within traditional views. In addition, the history of the Law of Effect as a principle of *acquisition* (as opposed to the steady state, i.e., asymptote) has not been a distinguished one. Little remains of the impressive edifice erected by Hull and his followers on this base. We are no longer concerned with the production of learning curves, nor with the measurement of habit strength or reaction potential. Hullian theory has proven effective neither in the elucidation of complex cases nor as an aid to the discovery of new phenomena. Indeed, the opposite has generally been true: new learning phenomena such as schedules of reinforcement, learning, and reversal sets, etc. have typically been the result of unaided curiosity rather than the hypothetico-deductive method, as exemplified by Hull and his students. This represents a failure of Hullian theory rather than a general indictment of the hypothetico-deductive method, which has proven every bit as powerful as Hull believed it to be—when used by others (e.g., Darwin, cf. Ghiselin, 1969). A similar, although less sweeping, verdict must be handed down on stochastic learning theory, which represents perhaps the most direct attempt to translate the Law of Effect into

a quantitative principle of acquisition. With the exception of predictions about the steady state, such as Estes' ingenious deduction of probability matching (Estes, 1957), the early promise of this approach as a way of understanding the behavior of individual animals has not been fulfilled. It seems fair to say that the rash of theoretical elaboration that has occupied the years since Thorndike first stated the Law of Effect has told us almost nothing more about the moment-by-moment behavior of a single organism in a learning situation. Although the alternative we will offer may be no better than earlier views, it does accommodate the anomalies we have discussed—and the need for *some* alternative can hardly be questioned.

The close analogy between the Law of Effect and evolution suggests an approach that may be a step toward the general framework which the psychology of learning so obviously lacks. At the present stage of knowledge, this approach is simply an analogy, although a compelling one, and cannot yet be called an evolutionary theory of learning. However, it provides a beginning and may lead to a truly evolutionary account which can securely imbed the study of learning in the broader field of biology.

#### *Behavioral Variation and Reinforcement*

The Law of Effect, suitably modified to take account of advances since Thorndike's original formulation, can be stated so as to emphasize acquisition, or the steady state, and learning (e.g., S-R bonds), or performance. We will take as a point of departure a neutral version of the law that emphasizes performance in the steady state, as follows: "If, in a given situation, a positive correlation be imposed between some aspect of an animal's behavior and the delivery of reinforcement, that behavior will generally come to predominate in that situation." The term "correlation" is intended to include cases of delay of reinforcement and experiments in which the response acts on the rate of reinforcement directly (e.g., Herrnstein & Hineline, 1966; Keehn, 1970). This formulation does not take account of more complex situations where more than

one behavior and more than one correlation are involved (i.e., choice situations), as these complications do not affect the present argument. A discussion of three aspects of this law—(a) the initial behavior in the situation before reinforcement is introduced, (b) the process whereby this behavior is transformed into the dominant reinforced behavior, and (c) reinforcement—follows:

(a) The behavior in a situation before the occurrence of reinforcement reflects a number of factors, including past experience in similar situations (transfer), motivation, stimulus factors (e.g., novel or sign stimuli), and others. We propose the label "principles of behavioral variation" for all such factors that originate behavior. These principles are analogous to Darwin's laws of variation, corresponding to the modern laws of heredity and ontogeny that provide the phenotypes on which selection (analogous to the principles of reinforcement, see below) can act. Thus, the term "variation" is intended to denote not mere variability, but the organized production of novelty, in the Darwinian sense.

(b) Transition from initial behavior to final behavior is the traditional problem of learning theory and, as we have seen, is essentially unsolved at the level of the individual organism. However, it is at this point that the analogy to the mechanism of natural selection becomes most apparent. Broadbent (1961) makes the parallel quite clear:

Since individual animals differ, and those with useful characteristics [with respect to a particular niche] survive and pass them on to their children, we can explain the delicate adjustment of each animal's shape to its surroundings without requiring a conscious purpose on the part of the Polar bear to grow a white coat. Equally each individual animal [under the Law of Effect] tries various actions and those become more common which are followed by consummatory acts [i.e., reinforcement] [p. 56].

Thus, the transition from initial to final behavior can be viewed as the outcome of two processes: a process that generates behavior, and a process that selects (i.e., selectively eliminates) from the behavior so produced. Since there is no reason to suppose that the process which generates behavior following

the first and subsequent reinforcements is different in kind from the process that generated the initial behavior (although the effects will generally be different, see Extinction, below), we can include both under the head of principles of behavioral variation. We propose the label "principles of reinforcement" for the second, selective process.

(c) As we have seen, the Darwinian principle of selection is analogous to the process that transforms initial behavior into final behavior—the "principles of reinforcement." The notion of reinforcement (more exactly, the schedule of reinforcement) itself is in fact analogous to an earlier concept, one that preceded evolution by natural selection and can be derived from it: the Law of Conditions of Existence, that is, the fact that organisms are adapted to a particular niche. This is apparent in the form of the statements: "The Polar bear has a white coat *because* it is adaptive in his environment." "The pigeon pecks the key *because* he is reinforced for doing so." It is important to emphasize this distinction between *reinforcement* and *principles of reinforcement*, and the analogous distinction between adaptation to a niche and the process of selection by which adaptation comes about. In the case of adaptation, the process of selection involves differential reproduction, either through absence of a mate, infertility, or death before reproductive maturity. In the case of reinforcement, the distinction is less obvious, since the principles of reinforcement refer to the *laws by means of which* behaviors that fail to yield reinforcement are eliminated, rather than the simple fact that reinforced behaviors generally predominate at the expense of unreinforced behaviors.

Thus, both evolution and learning can be regarded as the outcome of two independent processes: a process of variation that generates either phenotypes, in the case of evolution, or behavior, in the case of learning; and a process of selection that acts within the limits set by the first process. In both cases, the actual outcome of the total process is related to, but not identical with, the material acted upon: phenotypes reproduce more or less successfully, but a gene pool is the outcome of selection; similarly, behaviors

are more or less highly correlated with reinforcement, but learning (i.e., an alteration in memory) results.

The three aspects of this process—variation, selection, and adaptation—have received differing emphases at different times, depending on the prevailing state of knowledge. Before Darwin, adaptation, in the form of the Law of Conditions of Existence, was emphasized, since the only explanation for it—the design of the Creator—was not scientifically fruitful. Following Darwin, selection, both natural and artificial, received increased attention, in the absence of firm knowledge of the mechanism of variation (i.e., inheritance). With the advent of Mendelian genetics, variation has been most intensively studied (this is one aspect of the "molecular vs. organismic" debate referred to by Pfaffman).

In terms of this development, the study of learning is at a relatively primitive level, since the Law of Effect, although a great advance over the level of understanding which preceded it, simply represents the identification of environmental events—reinforcers—with respect to which behavior is adaptive. Lacking is a clear understanding of both selection (the principles of reinforcement) and variation (the principles of variation).

Space precludes exhaustive elaboration of all the implications of the classificatory scheme we are suggesting. However, in order to provide some context for our account of the facts already discussed, it seems essential to briefly summarize some possible candidates for principles of variation and reinforcement. It should be obvious that current knowledge does not permit the categories used to be either exhaustive or clear-cut.

*Principles of behavioral variation.* 1. Transfer processes: One of the main sources of behavior in a new situation is obviously past experience in similar situations. Transfer has been most exhaustively studied under the restricted conditions of verbal learning (e.g., Tulving & Madigan, 1970), but the principles of memory thus derived—proactive and retroactive interference, primacy and recency, retrieval factors, etc.—are presumably of general applicability.



With few exceptions (e.g., Gonzales, Behrend, & Bitterman, 1967), these principles have been little used to interpret animal learning experiments. Other principles of transfer are stimulus and response generalization (induction) and what might be called "compositional transfer," in which several past experiences are combined to generate a novel behavior, as in insight learning and other forms of subjective organization of past input.

2. Stimulus substitution: This principle, which is usually identified with Pavlovian conditioning, has already been discussed as a description of the origin of the terminal response of Pecking. It may also describe the origin of the metastable terminal response Head in magazine, since this response is also elicited by food under these conditions, and the animal which showed this response most persistently (Bird 29) had had considerable experimental experience. The difference in the persistence of Head in magazine in the case of this bird, as compared with others, cannot be explained in this way and might reflect a difference in other transfer processes. The final dominance of Pecking, in every case, may reflect a special susceptibility of consummatory responses to the stimulus substitution principle, or the action of other transfer principles in this particular situation in ways that are presently unclear. The indefinite persistence of key pecking following only three peck-contingent reinforcements, found by Neuringer (1970b), tends to support the simpler conclusion, as does the data of Wolin (1968), who reports a similarity between the topography of operant pecking for food or water reinforcers, and the appropriate unconditioned response. We return later to a general discussion of this principle in relation to these data (pp. 33-34).

3. Preparatory responses: This principle is also frequently associated with classical conditioning, and in that sense is related to, and to some extent overlaps with, the stimulus substitution principle. Thus, some conditioned responses, such as salivation, can be equally well described by either principle. Others, also respondents (such as heart rate, which increases following electric shock, but

usually decreases in anticipation of it [Zeaman & Smith, 1965]) may be classified as preparatory responses. Skeletal responses observed in classical conditioning situations are often preparatory in nature (see discussion of classical conditioning, below).

4. Syntactic constraints: There are often sequential constraints among behaviors, so that a given behavior is determined by some property of the sequence of preceding behaviors. Examples are spontaneous position alternation observed in rats and other rodents, stimulus alternation observed in monkeys on learning set problems (e.g., Levine, 1965), and sequential dependencies observed in most species which cause responses to occur in runs rather than alternating randomly (e.g., position habits and other perseverative errors). Human language provides the most developed example of syntactic constraints.

5. Orienting responses: This category includes all those transient behaviors, such as exploration, play, curiosity, etc., that expose the organism to new stimuli and provide the possibility of transfer to future situations.

6. Situation-specific and species-typical responses: Certain situations seem to call forth specific responses, which are often typical of the species rather than the individual and do not seem to depend in any obvious way on any of the other principles of variation such as transfer, etc. Examples are the species-specific defense reactions discussed by Bolles (1970), which occur in fear-producing situations, the tendency to peck bright objects shown by many birds (Breland & Breland, 1966), and the digging shown by small rodents (Fantino & Cole, 1968). Other examples are given by Glickman and Sroges (1966).

*Principles of reinforcement.* Before the discovery of the mechanism of inheritance, evolution could be explained only in terms of a goal—adaptation—and a means sufficient to reach that goal—variation and selection; the generation-by-generation details of the process were obscure: "Our ignorance of the laws of variation is profound [Darwin, 1951, p. 170]." We are at present equally ignorant of the mechanisms of behavioral variation. Since learn-

ing must usually involve constant interplay between variation and reinforcement, we are not yet in a position to suggest anything specific about the moment-by-moment details of the process, either in its variational or selective aspects. However, just as Darwin was able to say something about selection by pointing out the adaptive role of various structures, so it is possible to learn something about the selective role of reinforcement by looking at steady-state adaptations to fixed conditions of reinforcement, that is, reinforcement schedules. On this basis, we suggest the following tentative generalizations about the effects of reinforcement:

1. Reinforcement acts directly only on the terminal response; activities which occur at other times (interim activities, adjunctive behavior, etc.) must be accounted for in other ways, to be discussed later. This assertion is perhaps closer to a definition than an empirical generalization, since it is equivalent to the assertion that the terminal response may, in general, be *identified* as the activity occurring in closest proximity to reinforcement in the steady state. Identification is, of course, no problem in conditioning situations that *enforce* a contingency between some property of behavior and the delivery of reinforcement. However, we will show later that there is no empirical or logical basis for separating situations that do impose a contingency between response and reinforcement from those that do not (see Classical Conditioning, below).

2. Reinforcement acts only to eliminate behaviors that are less directly correlated with reinforcement than others. This generalization, like the first, is also more like a definition, since all that is *observed* (under consistent conditions of reinforcement) is the eventual predominance of one behavior over others—which is consistent with either a suppressive or a strengthening effect. As Skinner (1966a) points out in a summary of the Law of Effect:

Thorndike was closer to the principle of natural selection than the [usual] statement of his law. He did not need to say that a response which had been followed by a certain kind of consequence was more likely to occur again but simply that it was not less likely. It eventually held the field because responses which failed to have such

effects tended, like less favored species, to disappear [p. 13].

Unfortunately, Skinner, and most other behaviorists, elected to follow Thorndike in considering reinforcement to have a positive, strengthening, or "stamping-in" effect. One of the main purposes of the present paper is to suggest that this decision was a mistake and has given rise to a number of problems and controversies that can be avoided if the effects of reinforcement are considered to be purely selective or suppressive.

There are three kinds of argument which support a purely selective role for reinforcement. The first, and most important, is that the overall conceptual scheme which results is simpler and more easily related to biological accounts of behavior than the alternative.

The second point is that situations such as extinction and shaping by successive approximations that might seem to require an active role for reinforcement can be interpreted in a way that does not require anything more than a selective effect. This point is discussed later (see Extinction, below).

The third point is that the superstition and related experiments suggest that the response contingency imposed by most reinforcement schedules is not essential for the production of *some* terminal response, but only for the selection of one response over others, or for directing a response which would probably predominate in any case—as in key pecking by pigeons. Our failure to find a consistent difference in rate of observer-defined pecking between the response-dependent and response-independent conditions of the present superstition experiment supports this view, as does a recent finding that the contingency between electric shock and responding is not necessary to the generation of behavior maintained by intermittent shock delivery to squirrel monkeys (Hutchinson, 1970; Stretch, personal communication, 1970).

The usual interpretation of the fact that there is a terminal response in the superstition situation, despite the absence of response-contingency, is the notion of accidental strengthening of a response by con-

tiguity with the delivery of reinforcement. Here we consider the general implications of adventitious reinforcement as an explanation, and its incompatibility with the notion of reinforcement as selection. Other problems related to adventitious reinforcement are discussed later (see Acquisition, Classical Conditioning).

First, adventitious reinforcement implies failure of constancy, in the sense that the animal is presumed to be unable, because of the stamping-in mechanism of reinforcement, to distinguish between real and accidental correlations between his behavior and the occurrence of reinforcement. This is a strong assumption, in view of the adaptive utility of the constancy process and its ubiquity in perceptual and motor mechanisms. In perception, a similar failure to distinguish changes in sensory input that are produced by our own behavior from changes that are independent of behavior might cause us to perceive the world as rotating every time we turn our head. It is of course true that on the basis of one or a few instances the animal may not be in a position to be certain about the reality of a contingent relationship between his behavior and reinforcement—and this kind of sampling limitation might account for a transient superstitious effect. It is less convincing as an account of a long-term effect.

Second, if reinforcement is considered as purely selective, it *cannot* be invoked as an explanation of behavior when *no* imposed contingency exists between reinforcement and behavior (i.e., in the *absence* of selection). To do otherwise would be like taking a population of white mice, breeding them for 20 generations without further selection for color, and then attributing the resulting white population to the results of "accidental selection." In this case, as in the case of response-independent reinforcement, the outcome reflects a characteristic of the initial population (i.e., the mice gene pool, the nature of the organism), and not a non-existent selection process.

In short, the notion of adventitious reinforcement is not a tenable one. The extent to which reinforcement can be invoked as an explanation for behavior is directly re-

lated both to the degree of *imposed contingency* between response and reinforcement and to the opportunities for that contingency to have some selective effect (i.e., the number of contingent reinforcements). If there is no contingency, or if few contingent reinforcements have occurred, the resulting behavior must owe more to principles of variation than to the selective action of reinforcement. On the other hand, if many contingent reinforcements have been delivered during a protracted period of shaping, the final form of behavior is obviously much less dependent on *particular* principles of variation, and the role of reinforcement (selection) may properly be emphasized.

3. There is considerable evidence for the generality of a principle implicating relative rate or proximity of reinforcement as the fundamental independent variable determining the spatial and temporal location of responding in steady-state conditioning situations. Thus, Herrnstein (1970) has recently reviewed a number of operant conditioning experiments involving differential reinforcement of simultaneous (concurrent schedules) and successive (multiple schedules) choices which support the idea of relative reinforcement rate as the independent variable most directly related to the rate of key pecking in pigeons. Shimp (1969) has presented an analysis which is formally different from Herrnstein's, but which also implicates differences in reinforcement rate as the crucial variable. An extensive series of experiments by Catania and Reynolds (1968) suggests a similar (although less exact) relationship between rate of pecking and relative temporal density of reinforcement on interval reinforcement schedules. Jenkins (1970) summarizes a series of studies with a discrete-trials procedure that led him to suggest relative proximity to reinforcement as an important determiner of the tendency to respond in the presence of a stimulus. Staddon (1970a) has suggested a similar principle to account for positive "goal gradients" that underlie the effects of reinforcement omission on a variety of interval schedules.

4. The concept of reinforcement implies a capacity to be reinforced. The fact that a

given stimulus may be reinforcing at one time, but not at another, requires the idea of a *state* corresponding to each class of reinforcers. The independent variable of which the strength of most of these states is a function is deprivation with respect to the appropriate class of reinforcers (food deprivation for the hunger state, water for thirst, etc.). This will not do for most negative reinforcers (e.g., the removal of electric shock), however, since there is no obvious counterpart to deprivation in this case. It is also unlikely that deprivation is the only independent variable sufficient to alter the strength of states associated with positive reinforcement. For example, evidence is discussed later in favor of reciprocal inhibitory interaction between states as a possible factor in polydipsia and other adjunctive behavior. Interactions of this sort may also alter the strength of states for which there is no deprivation requirement, as in audio anaesthesia (Licklider, 1961).

Thus, one may hope for a set of principles of reinforcement that will deal both with the proper classification of states and with the interactions among them.

The theoretical vocabulary of learning is full of terms with an uneasy conceptual status somewhere between explanation, definition, and category label. This terminology, which is not coherent or internally consistent, makes it difficult to approach particular topics with an open mind. It is too easy to dismiss an experimental result as due to adventitious reinforcement or respondent conditioning without, in fact, having any clear understanding of what has been said. Simply defining everything operationally is of little help in this situation, since a set of definitions is not a theory. And a theory, in the sense of a system of concepts that is internally consistent and coherent, is what is required if we are to be sure, in particular cases, whether we really understand a phenomenon—or are merely substituting one mystery for another, with the assistance of an opaque vocabulary.

What we are proposing is too primitive to be called a theory in this sense. However, it does offer a system of classification that makes it difficult to have the illusion of

understanding a phenomenon if comprehension is really lacking. In the following section, some implications of this scheme are shown in three major areas: acquisition, extinction, and classical conditioning. This is followed by a brief discussion of possible difficulties of this approach. With the aid of this groundwork, it will then be easier to return to a general account of the superstition and related experiments in the concluding section.

### *Acquisition*

The number of trials necessary for learning is one of those perennial problems that seems to defy resolution. Appeal to data is not conclusive because learning curves are sometimes incremental and sometimes step-like. Even in particular cases, theory is not conclusive either, since with sufficient ingenuity, theoretical accounts of both kinds of curve may be constructed on the basis of either one (or a few) trial learning assumptions or incremental assumptions involving thresholds. We turn now to the possibility that the issue is a consequence of the stamping-in view of reinforcement and becomes less urgent once that view is challenged.

A comment by Skinner (1953) on the necessary and sufficient conditions for the development of superstition provides an illustration:

In superstitious operant behavior . . . the process of conditioning has miscarried. Conditioning offers tremendous advantages in equipping the organism with behavior which is effective in a novel environment, but there appears to be no way of preventing the acquisition of non-advantageous behavior through accident. Curiously, this difficulty must have increased as the process of conditioning was accelerated in the course of evolution. If, for example, three reinforcements were always required in order to change the probability of a response, superstitious behavior would be unlikely. It is only because organisms have reached the point at which a single contingency makes a substantial change that they are vulnerable to coincidences [pp. 86-87].

Even within the framework of the stamping-in view, it is clear that the truth of this statement depends on a tacit assumption that responses will not generally occur more than once unless followed by reinforcement.

If a given response can be relied on to occur at least 20 times in succession, even without reinforcement, then 3-trial, or even 10-trial learning might well be sufficient to insure its acquisition under the conditions of the superstition experiment. The assumption that responses will occur only once in the absence of reinforcement is a strong assumption about syntactic constraints, in our terminology. Moreover, it is contradicted by the results of the Williams and Williams study (1969), which show indefinite persistence of pecking in the absence of any contiguous relationship between pecks and reinforcement. There is no reason to suppose that a similar persistence is not characteristic of other behaviors (e.g., position habits), although pecking may be more persistent than most. Thus, the finding of superstitious terminal responses, or of indefinite pecking following just three response-contingent reinforcements (Neuringer, 1970b), need imply nothing about the number of trials necessary for learning.

These considerations suggest, as a minimum, the need to take variation into account in discussions of the "speed of conditioning," since rapid acquisition may either reflect an unpersistent response that is really learned rapidly, or a very persistent one that may be learned quite slowly. No inferences about "speed of conditioning" can be drawn solely on the basis of speed of acquisition without information about the frequency and pattern of a given behavior to be expected in a given situation (which may include predictable delivery of reinforcement) *in the absence of contiguity* between that behavior and reinforcement. In practice, since information of the required sort is rarely, if ever, available, it seems wise to defer the issue of speed of learning until behavioral variation has been much more thoroughly studied.<sup>4</sup>

Thus, the moment-by-moment details concerning the effect of reinforcement remain

<sup>4</sup> Problems of this sort are not solved by referring to a hypothetical "operant level" because (a) this level is often zero in the absence of a history of reinforcement in the situation; (b) it is rarely constant, as the term level implies; and (c) the problem of the origin of this level is thereby simply evaded.

uncertain until much more is known about variation. In the meantime it seems more parsimonious and less likely to lead to fruitless controversies about "speed of conditioning," continuity versus noncontinuity, etc. to assume that the appearance of one behavior, rather than another, at a certain time or place, rather than some other time or place, always requires explanation in terms of principles of variation, with only the *disappearance* of behaviors being attributable to the effects of reinforcement.

This general approach is not novel. It resembles both Harlow's (1959) account of learning-set acquisition in terms of the progressive elimination of error factors, and certain versions of stimulus-sampling theory (Neimark & Estes, 1967). In Harlow's terms, as in ours, one-trial acquisition is a phenomenon that depends on the existence of factors that make the correct behavior much more probable (and persistent) than others (i.e., upon principles of variation). In the learning-set case, these factors are embodied in the prior training procedure, which progressively selects for an initially weak behavior (the "win stay, lose shift" strategy) at the expense of the initially much stronger tendencies to approach particular stimuli. A lengthy process may not be essential, however, for principles of variation involving insight ("compositional transfer," see above) may serve the same function, if they are available to the animal. The important point is the shift of emphasis away from the supposed efficacy of some stamping-in mechanism, the action of which must remain obscure in the absence of knowledge about variation, to the principles of variation that determine the strength of behaviors in advance of contiguity with reinforcement.<sup>5</sup>

#### *Extinction*

Extinction is often used as a test for "what is learned" during a training pro-

<sup>5</sup> Memory has not been separately discussed in this account of acquisition because it is embodied in most of the variational and selective processes we have described. The argument of the present section suggests that a separate account of memory may have to await advances in our knowledge of these processes.

cedure, as in generalization testing (Guttman & Kalish, 1956), and testing for control by temporal factors (Ferster & Skinner, 1957; Staddon, 1970b). Under these conditions, it is assumed that behavior is determined almost entirely by transfer from the base-line condition. Providing the difference between the extinction and training conditions is not too great, either in terms of environmental factors (the stimulus situation is not too different) or temporal factors (the extinction is not prolonged), this assumption can be justified by the reliability and predictability of the behavior usually observed.

When these conditions are not satisfied or when the training preceding extinction has not been protracted, this reliability is not usually found. On the contrary, extinction under these conditions is usually associated with an increase in the variability of behavior (Antonitis, 1951; Millenson & Hurwitz, 1961). This increase in variability is exactly what would be expected if, as we have suggested, reinforcement has a purely selective effect: in these terms, training involves a progressive reduction in variability under the selective action of reinforcement (centripetal selection, see below), so that absence of reinforcement (extinction) represents a relaxation of selection—with an attendant rise in variability. We turn now to a brief account of the effects of changes in the amount and direction of selection in evolution, which may shed some further light on the properties of behavioral extinction.

Darwin (1896) comments on the effect of domestication as follows:

From a remote period to the present day, under climates and circumstances as different as it is possible to conceive, organic beings of all kinds, when domesticated or cultivated, have varied. . . . These facts, and innumerable others which could be added, indicate that a change of almost any kind in the conditions of life suffices to cause variability. . . . [Vol. 2, p. 243].

Although Darwin sometimes (erroneously) interpreted this observation as reflecting a direct effect of changed conditions on the reproductive system, it can be interpreted in modern terms as due to a relaxation of

selection. This is clear from the concept of *centripetal selection* (Haldane, 1959; Mayr, 1963; Simpson, 1953), which refers to the fact that selection under *unchanging* conditions, if long continued, acts to weed out extremes, rather than systematically to shift population characteristics in any particular direction:

When adaptation is keeping up, selection at any one time will be mainly in favor of the existing type. . . . In such cases, the intensity of selection tends to affect not the rate of change but the amount of variation [Simpson, 1953, p. 147].

Thus, a *change* in conditions will generally involve a shift *away* from centripetal selection, with its tendency to reduce variability, and will often lead, therefore, to increased variability. The most obvious example of the effects of relaxation of selection in evolution is degenerating or vestigial structures, that are no longer being selected for:

It is so commonly true that degenerating structures are highly variable that this may be advanced as an empirical evolutionary generalization [Simpson, 1953, p. 75].

We have already noted that the onset of variability in extinction is often delayed. A similar delay in the effect of changed conditions is often apparent in evolution, Darwin (1896) notes:

We have good grounds for believing that the influence of changed conditions accumulates, so that no effect is produced on a species until it has been exposed during several generations to continued cultivation or domestication. Universal experience shows us that when new flowers are first introduced into our gardens they do not vary; but ultimately all, with the rarest exceptions, vary to a greater or less extent [Vol. 2, p. 249].

Similar delays have also been reported in experiments on artificial selection (Mayr, 1963). These delays seem to reflect what has been termed "genetic inertia" or "genetic homeostasis" (Mayr, 1963), that is, the tendency for a gene pool which is the result of a long period of consistent selection to resist changes in the direction of selection. A similar mechanism in behavior might account for the dependence of variability in extinction on the duration of the preceding training period, which was referred to earlier: The amount of variability might be ex-

pected to be greater and its onset sooner following a brief training period than after one of longer duration. Genetic homeostasis also seems to be involved in the phenomenon of reversion, to be discussed next.

Not all the variation which occurs either in behavioral extinction, or following a change in the conditions of life in evolution, is wholly novel. A relatively common effect, for example, is the reappearance of what Darwin terms "ancestral types," that is, phenotypes which predominated earlier in phylogeny but which have been selected against more recently. This is the phenomenon of reversion which, because of his ignorance concerning heredity, Darwin (1896) found among the most mysterious of evolutionary processes:

But on the doctrine of reversion . . . the germ [germ plasm] becomes a far more marvellous object, for, besides the visible changes which it undergoes [i.e., phenotypic expressions], we must believe that it is crowded with invisible characters, proper to . . . ancestors separated by hundreds or even thousands of generations from the present time: and these characters, like those written on paper with invisible ink, lie ready to be evolved whenever the organisation is disturbed by certain known or unknown conditions [Vol. 2, pp. 35-36].

Thus, one effect of a relaxation of selection is a more or less transient increase in the relative influence of the distant past at the expense of the immediate past. In behavioral extinction, this should involve the reappearance of old (in the sense of previously extinguished) behavior patterns; that is, transfer from conditions preceding the training condition at the expense of transfer from the training condition.<sup>6</sup> In both cases, evolution and behavior, the effect of the change in conditions may be expected to depend on variables such as the magnitude of the change and the time since the preceding change.

<sup>6</sup>Other than clinical accounts of regression, we have been able to find only one published report of this effect—in an account describing shaping porpoises to show novel behaviors (Pryor, Haag, & O'Reilly, 1969). However, we have frequently observed it while shaping pigeons: if a pigeon has been trained in the past to perform a variety of responses, the increase in variability during extinction of the most recently reinforced response generally includes the reappearance of earlier responses.

The analogy from Darwin suggests that any considerable change in conditions should increase variability, yet a change in reinforcement schedules that includes an *increase* in reinforcement rate is not usually thought of as producing an increase in variability. This apparent contradiction is resolved by noting that an increase in rate of reinforcement, in addition to changing conditions, also increases the rate of selection (since the analogy assumes reinforcement to have a purely selective effect). Thus, variability may be briefly increased, but since the rapidity of selection is also increased, the net effect may be small. An analogous (but impossible) phenomenon in evolution would be to decrease the time between generations at the same time that conditions are changed. This would speed up the attainment of a new equilibrium and minimize the increase in variability generally associated with changed environment.

The increase in variability due to extinction is most directly put to use in the process of shaping by successive approximations. Frequently, following the first few reinforcements delivered during a "shaping" session, the effect is simply an increase in the range and vigor of behavior. This change can be viewed as being due to the interruption of eating (cf. Mandler, 1964), however, rather than any direct strengthening effect of reinforcement (which we are questioning in any case). In terms of the foregoing analysis, the conditions following the first reinforcement should be optimal for an increase in variability: the change is large (from continuous eating to absence of food) and the training procedure is of short duration (the 3-4-second eating bout), so that time since the preceding change is also short. As food continues to be delivered intermittently, selection occurs and variability decreases.

We have been suggesting a purely selective (rather than strengthening, stamping-in, or energizing) role for reinforcement. The present discussion suggests that such an essentially passive role is compatible with a number of phenomena—extinction, the activating effects of isolated reinforcements—that may appear to demand a more active role for reinforcement. This compatibility

was established by drawing attention to similar phenomena in evolution, where the purely selective effect of the conditions of life (analogous to the schedule of reinforcement) is unquestioned. However, no *necessary* identity between the genetic mechanism, which is responsible for the effect of changed conditions on variability in structure, and whatever process is responsible for analogous effects in behavior, is intended. Any process for the production of variation that incorporates some latent memory of past adaptations is likely to show similar effects.

### *Classical Conditioning*

Our scheme has strong implications for the distinction between classical (Pavlovian, respondent) and instrumental (operant) conditioning, to the extent that the distinction goes beyond procedural differences. Classical conditioning is often thought of as a paradigmatic instance of the primary process of learning: "The [learning] process appears to be based entirely on temporal contiguity and to have classical conditioning as its behavioral prototype [Sheffield, 1965, p. 321]." The salivation "reference experiment" can be interpreted as prototypical in at least two ways that are not always kept separate. The first (which has some similarities to our position) is referred to by Sheffield—the notion that learning depends solely on temporal relationships. Guthrie's aphorism that the animal "learns what he does" is a related idea. It is not easy to find a definitive account of this position, but it may perhaps be summarized by saying that reinforcement or reward is simply necessary to ensure that some behavior occurs in a conditioning situation. Principles involving temporal relationships (contiguity) then ensure that whatever occurs will transfer from one occasion to the next.

The second way in which classical conditioning is discussed as prototypical is in terms of the rule that relates the conditioned and unconditioned responses. Pavlov (1927) emphasized stimulus substitution as the distinctive property of the situation: the response originally elicited only by the UCS is later made to the CS. Subsequently, two

kinds of departure from this rule have been pointed out: (a) Even in the salivation experiment, there are other readily identifiable components of the conditioned response that do not fit the stimulus substitution rule. These preparatory responses (Zener, 1937) are largely, but not exclusively, skeletal (rather than autonomic). (b) Even in the case of salivation and other autonomic responses, the CR is rarely identical to the UCR (i.e., a redintegrative response), so that components of the UCR may be missing from the CR. More serious are differences in direction of change between CR and UCR, which may not even be consistent across individuals, as in heart rate and respiratory conditioning (Martin & Levey, 1969; Upton, 1929; Zeaman & Smith, 1965).

Partly because of problems involving preparatory responses, classical conditioning has increasingly been restricted to autonomically mediated responses. There were two bases for this restriction: the apparent difficulty of conditioning skeletal responses by the operations of classical conditioning and according to the stimulus substitution principle (cf. Skinner, 1938, p. 115), and the supposed impossibility of conditioning autonomic responses via the Law of Effect. This is clear from Kimble's (1961) comment:

Obviously the common expression, "*the conditioned response,*" is misleading, and probably in important ways. At the same time it should be recognized that the behavior described by Zener [preparatory responses] was almost certainly instrumentally, rather than classically, conditioned [p. 54].

The force of this argument is lost once the susceptibility of autonomic responses such as salivation to operant conditioning is demonstrated.

The foregoing facts are sufficient to show the error of continuing to regard classical conditioning as a unified process, much less as an explanatory element in accounts of operant conditioning. If classical conditioning is a single process, then it must be describable by principles of operation that apply to every instance. As we have seen, even stimulus substitution, the most general such principle, fails to apply in every case.



Many, but not all, the anomalous cases are skeletal responses—which suggested that perhaps the notion of a single process could be preserved by restricting the term to autonomic responses. The only independent basis for this is to segregate skeletal and autonomic responses on the grounds that operant conditioning of autonomic responses is impossible. Since this is now known to be false (Miller, 1969), the only remaining basis for excluding skeletal responses from the class of classically conditionable responses is their failure to conform to the principle of stimulus substitution. But, in addition to being tautologous (classical conditioning simply becomes equivalent to learning via stimulus substitution), this fails because many autonomic responses do not conform to this principle and, based on the work of Brown and Jenkins and of Williams and Williams, at least one skeletal response—pecking in pigeons—does obey it (other possibilities are leg flexion and eyeblink). Thus, the class of classically conditionable responses can be defined neither in terms of the neural mediating system (autonomic vs. skeletal) nor in terms of adherence to a particular principle of learning.

The only remaining feature common to all the situations labeled as classical conditioning is the procedure itself. Research in this area has tended to focus on the properties of the temporal relationship between CS and UCS that are necessary and sufficient for the CS to acquire the power to elicit the conditioned response, and a consensus appears to be emerging that the crucial factor is the extent to which the CS is a predictor of the UCS (Rescorla, 1967). However, the notion of predictiveness does not appear to differ from *relative proximity* (of the CS to the UCS, or of a stimulus to reinforcement) which, as we have seen (Principle of Reinforcement 3, above), is a factor of wide applicability in operant conditioning. Thus, for all practical purposes, classical conditioning may be defined operationally as a class of reinforcement schedules that involve presentation of reinforcement independently of the subject's behavior.

We conclude, therefore, that the division of the field of learning into two classes—

classical and instrumental conditioning—each governed by separate sets of principles, has no basis in fact. As an alternative, we suggest an analysis based on the principles of behavioral variation and reinforcement we have already discussed. In terms of this analysis, all adaptive behavior is subsumed under an expanded version of the Law of Effect, and a given situation is to be understood in terms of two factors: (a) the reinforcement schedule, that is, the rule prescribing the delivery of reinforcement, or, more generally, stimuli, in relation to the behavior of the organism, and (b) the nature of the response under consideration. In terms of such an analysis, the properties normally considered as distinctive of classical conditioning may, once attention is directed to the question, be seen as due in part to a reinforcement schedule that happens to prescribe no correlation between the delivery of reinforcement and the subject's behavior, and in part to the special properties of responses such as salivation (see Implications 2 and 3, below).

*Implications.* Several puzzles become clearer once classical and instrumental conditioning are no longer regarded as separate processes:

1. In the earlier discussion of auto-shaped pecking, Williams and Williams noted that "the directed quality of the induced pecking does not follow naturally from respondent principles." Since we question this framework as a general description of anything outside the salivation experiment, its inability to deal with this particular situation poses no problem. The appearance of key pecking in the auto-shaping and superstition situations is, of course, not fully understood. It may reflect a special susceptibility of consummatory responses to the principle of stimulus substitution, as suggested earlier, or the action of transfer principles in ways that reflect something in common among the past histories of most pigeons. Similarly, little can be said about the directedness of pecking until the conditions under which this response is, and is not, directed have been more fully explored; although the results so far (with pigeons and monkeys) suggest that skeletal responses may always

be directed. Rather than attempt to explain (or explain away) these characteristics, the best course seems to be simply to observe and classify these and other behaviors under a variety of response-independent and response-dependent schedules, in the hope that hypotheses can be devised that have more hope of generality than anything that can be inferred from the scanty data presently available. We return to these issues in the concluding section.

2. There is little doubt that most autonomic responses are more easily conditioned by response-independent schedules than by response-dependent ones. However, this need not imply the existence of two different kinds of conditioning. It can as well be interpreted as reflecting the existence of internal controlling factors that are not amenable to the principles of reinforcement. Because of these fixed factors, these responses are not as free to come under the control of external stimulus factors as are skeletal responses that perform no function in the internal economy of the organism. Moreover, this principle can be extended beyond autonomic responses to deal with any response that is strongly connected to any stimulus, internal or external. Thus, in a recent discussion of operant conditioning of drinking, Black (1970) writes:

This discussion suggests that one dimension along which responses might be classified with respect to operant conditioning is the degree to which they are constrained from being changed by operant reinforcement by the properties of the neural subsystems of which they are a part. The regulatory systems can vary from very simple reflexes, such as the knee-jerk, to complex instinctive ones, such as those involved in courtship. The main point is not the complexity of the subsystems but rather the extent to which they limit the conditions under which operant reinforcement will work [p. 267].

Segal (1970) has made a similar suggestion in a thoughtful discussion dealing with a number of the points raised in the present paper.

As an example, the difficulties associated with demonstrating operant conditioning of heart rate can be viewed as analogous to the problem of acquiring control over an operant response that is already under the

control of other variables, that is, such schedules are really concurrent rather than simple schedules. The heart rate problem is perhaps analogous to training an animal to alter his rate of bar pressing to receive food, while pressing the same bar is also necessary to obtain another reinforcer (such as oxygen or heat). Even without considering the problem of interactions among drives, one would not be surprised to find rather weak control by the food reinforcer.

3. Williams (1965) recorded salivation in dogs while they were bar pressing for food reinforcement on both fixed-interval and fixed-ratio reinforcement schedules. He found that the onset of salivation within each interreinforcement interval approximately coincided with the onset of bar pressing in the fixed-interval case, but began *later* than bar pressing on fixed-ratio. This interesting result is incompatible with an explanation of the operant response in terms of an underlying classical conditioning process. However, it may be understood in terms of the view we have been proposing by assuming that (a) the occurrence of each response (bar pressing or salivation) is separately and independently determined by the conditions of reinforcement peculiar to it, and (b) both responses tend to occur at times of greatest relative proximity to reinforcement (Principle of Reinforcement 3). In the fixed-interval case, these two assumptions predict a similar time of occurrence for both behaviors, because time since the beginning of a trial is, for both responses, the best predictor of reinforcement. However, in the fixed-ratio case, it is apparent that no matter what determines the time of onset of bar pressing, once it has stabilized its onset provides a better predictor of reinforcement than does trial time (since the fixed number of responses making up the fixed ratio take an approximately fixed time). Thus, at asymptote our two assumptions imply that salivation should be reliably delayed with respect to bar pressing on fixed-ratio, but not on fixed-interval, as Williams reports.

However, there is no reason to expect this delay early in training, since the animal is not in a position to learn the cue significance

of bar pressing until it has more or less stabilized. This expectation is also confirmed by Williams (1965), who notes that the delayed onset of salivation in the fixed-ratio case "emerged only after repeated exposure to the schedules [pp. 344-345]."

On the basis of a failure to find salivation *preceding* bar pressing on a spaced-responding ("controlled latency") procedure, Williams (1965) concludes that "the hypothesis that the two measures are independent may be rejected [p. 347]," which contradicts Assumption *a*, above. However, since *both* responses are assumed (Assumption *b*) to occur at times of greatest relative proximity to reinforcement, and reinforcement *cannot* occur *before* a bar press if it is always contingent upon bar pressing, there is no reason to expect salivation reliably to precede bar pressing under conditions where the common reinforcement for both responses depends on bar pressing alone. Thus, the apparent asymmetry between salivation and bar pressing observed by Williams may simply reflect an asymmetry between the conditions of reinforcement for each response, and need not imply any fixed internal linkage between them.

4. Recently, considerable attention has been devoted to avoidance of particular foods conditioned by a nauseous experience (induced by insulin or X rays) taking place several hours after ingestion (Garcia, Ervin, & Koelling, 1966; Kalat & Rozin, 1970; Revusky & Bedarf, 1967; Rozin, 1969). Since the CS-UCS interval in these experiments is considerably longer than is customary in classical conditioning experiments, these data are even less congenial to a Pavlovian analysis than the auto-shaping results. They hint at the existence of a number of unsuspected built-in linkages between response systems and various salient stimuli. Such linkages are not unexpected from a broad evolutionary point of view that sees principles of variation and reinforcement as behavioral characteristics that are separately selected for, and bear as much (or as little) relationship to one another as do morphological characteristics.

### *Difficulties of the Proposed Classification*

Science is conservative and, quite correctly, resists most attempts to alter an established theoretical framework. We have already tried to show that the number of anomalies facing current learning theories is sufficient to justify a search for alternatives. Nevertheless, the radical appearance of the scheme we suggest is a substantial obstacle to its consideration. It is important, therefore, to point out that it is little more than an extension and reorganization of familiar concepts, that is, reinforcement, S-R behavior units, learning principles such as transfer, and ethological observations on species-related behaviors. The difference is therefore largely one of emphasis and selection rather than the introduction of wholly novel ideas.

Any discussion of evolution and learning naturally brings to mind the learning-instinct issue. There is no simple parallel between this dichotomy and anything in the scheme we propose. The origin of every behavior is supposed traceable to principles of variation; if, for example, in a particular case a principle of transfer is involved, one might want to say that the behavior is learned. However, the question must then simply be asked again about the previous situation from which transfer has supposedly occurred. In this way, almost any question about the relative roles of heredity and environment will involve unraveling the whole of ontogeny. This conclusion will not be unfamiliar to ethologists (cf. Beach, 1955).

It is also important to emphasize that we have not been directly concerned with the evolution of the capacity to learn; although it may be that increasing knowledge of variation will shed light on this issue.

One objection that may be raised to the proposed scheme is that it is derived from and deals explicitly only with positive reinforcement. However, a recent account of behavior sustained by negative reinforcement (Bolles, 1970) is in perfect agreement with our position. Bolles points out that some activities are much more easily conditioned than others in avoidance situations, and these are the unconditioned activities

that normally occur in a variety of potentially dangerous circumstances. These species-specific defense reactions, in Bolles' terminology, occur in advance of reinforcement (i.e., the avoidance of electric shock)—in our terms, they are determined initially by one of the principles of behavioral variation. The lack of arbitrariness of the response is perhaps more obvious in avoidance than in any other situation because of the complexity of the schedules involved: the animal must usually learn something about the pattern of occurrence of an intermittent aversive event, in the absence of responding, before he is in a position to detect alterations in that pattern correlated with his own behavior. Although a similar situation prevails in all reinforcement schedules, the change to be discriminated seems considerably easier both in appetitive conditioning, where the shift is from zero reinforcement in the absence of responding to reinforcement following every response, and in escape, where it is from continuous presence of the aversive stimulus in the absence of responding to complete absence following each response. Bolles suggests other reasons, related to the limited opportunities for avoidance (in the schedule sense) in the wild life of small mammals, and thus the limited opportunities for the capacity to avoid to be selected in phylogeny.

The strongest point in favor of our proposal is its promise of parsimony. Consequently, the most damaging criticism that can be directed against it is the absence of firm specification of the principles of reinforcement and variation. This appears to allow the creation of such principles at will, enabling us to explain everything—and nothing. There are two defenses against this criticism. First, we again emphasize the tentative nature of the principles we have suggested. The overlap among the principles of variation, particularly, suggests that our list is provisional. Second, there is the strong possibility that clear recognition of the distinction between variation and reinforcement may be essential to further advance. In defense of this proposition, we first briefly discuss some examples from synoptic accounts of current learning

theory, which show it to incorporate few safeguards against multiple explanations for phenomena. Since our scheme of classification is at least internally consistent and forces one to relate each new principle of variation to others that already exist, it has some advantages in this respect. Second, we discuss the controversy between cognitive and behavioristic theorists regarding the role of structure in behavior, in relation to a similar controversy in the history of evolutionary thought. The persistence of this controversy and its amenability to analysis in terms of variation and reinforcement suggest that our classification may be of some value despite its incompleteness.

1. Hilgard and Marquis (1940) list as principles of reinforcement: stimulus substitution, expectancy, and the principle (law) of effect. It should be apparent from the earlier arguments that the Law of Effect is the result of the combined effect of both variation and reinforcement, stimulus substitution is a principle of variation, and expectancy refers to a general characteristic which can be imputed to most learning. Consequently this set of terms allows for considerable uncertainty in application to particular situations. For example, our analysis of the Williams and Williams experiment (see p. 33) makes use of both stimulus substitution and a principle of reinforcement analogous to what Hilgard and Marquis mean by the Law of Effect. Yet the same situation could also be analyzed in terms of expectancy; and it appears to be incompatible with the Law of Effect as traditionally understood. Progress since 1940 has not been dramatic, as illustrated by a list of "elementary conditioning processes" inventoried by Jenkins (1970) in connection with his work on cyclic reinforcement schedules: generalization, delay of reinforcement, conditioned reinforcement, unconditioned effects of eating, frustration effects, effects related to "behavioral contrast." Despite the number of these processes and the lack of any obvious relationship among them, Jenkins finds that they are unable to account for some rather simple features of his data, which require a description in terms of the relative proximity

of a stimulus or a response to reinforcement as a major determiner (see Principles of Reinforcement, above).

2. There is a history of fruitless controversy between many behaviorists, who place little emphasis on the structural properties of behavior, and students of cognitive processes, who see structure as the most interesting and important behavioral attribute (cf. Staddon, 1967, 1969, for a discussion in relation to operant conditioning). The distinction between variation and reinforcement can shed some light on this issue, which can be illustrated by briefly considering the contrasting views of Skinner and Chomsky (Chomsky, 1959; MacCorquodale, 1969) on the causation of learned behavior.

Chomsky's major concern is with principles of variation, in our terms, as is clear from his emphasis on the rule-governed nature of language (see Principle of Behavioral Variation 4, above).

Skinner's position is less obvious, but becomes clear from his account of the shaping of behavior (Skinner, 1953); he writes:

Operant conditioning shapes behavior as a sculptor shapes a lump of clay. Although at some point the sculptor seems to have produced an entirely novel object, we can always follow the process back to the original undifferentiated lump, and we can make the successive stages by which we return to this condition as small as we wish. At no point does anything emerge which is very different from what preceded it. The final product seems to have a special unity or integrity of design, but we cannot find a point at which this suddenly appears. In the same sense, an operant is not something which appears full grown in the behavior of the organism. It is the result of a continuous shaping process [p. 91].

For Skinner, apparently, moment-to-moment variation in behavior is small in magnitude, and essentially random (in the sense that it is unrelated to the final goal) in direction. Behavior is the result of the "accumulation . . . of indefinite variations which have proved serviceable" in Darwin's phrase. The similarity to natural selection is further emphasized by Darwin's (1951) account of the evolution of complex structures:

If it could be demonstrated that any complex organ existed, which could not possibly have been formed by numerous, successive, slight modifica-

tions, my theory would absolutely break down [p. 191].

In the history of evolution after Darwin, the rediscovery of Mendel's laws led to a retreat from gradualism in favor of a saltationism that traced evolutionary progress (especially evolutionary novelty) to large changes (mutations) of a more purposive sort (cf. Mayr, 1960). This position is closer to the view of Chomsky and other cognitive theorists, who tend to stress the importance of insight and other rules of composition that can produce sudden jumps in behavior.

The history of evolution has not supported the saltationist view. Fisher (1930) showed that large changes are much less likely to be adaptive than small ones, and Haldane and others have shown by a variety of arguments that the time available for evolution by the selection of small variations is more than sufficient to account for the observed differences among taxa: "The saltationism of the early Mendelians has been refuted in all its aspects [Mayr, 1960, p. 350]."

At a superficial level, therefore, these comparisons might appear to favor Skinner's gradualism and emphasis on reinforcement (selection), to the detriment of the cognitive position. This is probably unjustifiable for two main reasons. First, detailed analysis of complex problem solving clearly indicates the insufficiency of random variation as an account of the process (e.g., Neisser, 1967). The heuristics that are employed may, of course, be attributed to past learning based entirely on random variation. However, this suggestion meets with quantitative difficulties when applied to the development of language—the best-studied example of rule-governed behavior. Although calculations in this area are of limited validity in the absence of established principles of variation (analogous to Mendelian genetics), the attempts that have been made seem to indicate that the time available in ontogeny for the development of language is incompatible with any kind of learning by random variation (Chomsky, 1962; McNeill, 1968). This negative result is the opposite of Haldane's affirmative conclusion on the sufficiency of small mutations as a basis for phylogenetic

changes. It suggests that neither the heuristics employed in complex problem solving, nor the rules of syntax, need to be built up entirely *de novo* during ontogeny.

Second, the relationship between evolution and learning is such as to allow greater flexibility to learning. This is because natural selection can only be a response to small differences in "fitness": the most fit genotype will tend to prevail, although the species as a whole may thereby be led into an evolutionary blind alley. In terms of contemporary accounts of goal-directed mechanisms, natural selection represents a hill-climbing process (Minsky, 1961) and has no provision for prediction. This is clear in a familiar analogy due to Sewall Wright (1931) which shows the relationships among selection, structure, and variation. He pictures the field of possible structural variation as a landscape with hills and valleys. The range of variation present in a population of organisms is represented by a closed area on this landscape. Selection pressure is represented by the gradient (upward slope) of the landscape, so that each *peak* is an adaptive optimum for a given constellation of characters; valleys represent unstable equilibria yielding so-called centrifugal selection. If the area representing a given species includes a single adaptive peak, selection will be centripetal, so that the species will tend to cluster more and more closely around the peak. Thus, small mutations are more likely to lead to improvements in fitness than large ones (with a limiting probability of .5, as Fisher, 1930, has shown), and the consequent predominant role of such small differences in fitness in the evolutionary process becomes obvious.

Learning is not so limited, however, because the principles of variation can be weighted to take account of regularities in the past history of the species (these are Skinner's, 1966b, "phylogenetic contingencies"); that is, behavior need not occur at random in advance of reinforcement, but can reflect *a priori* probabilities that have been selected for during phylogeny. Other more complex strategies of this sort may also be built up by natural selection, giving learning a predictive capacity largely denied to

evolution itself. Thus, although learned behavior reflects differences in reinforcement rate, just as evolution reflects selection on the basis of relative fitness, it need *not* be generally true either that small changes in behavior are more likely to be adaptive than large ones, or that the direction of change is unrelated to the final goal—as Skinner's account implies, and as is usually (although not invariably) the case in evolution. However, since the more elaborate principles of variation must themselves be built up step-by-step by natural selection, it is to be expected that the pattern and range of behavioral variation must bear some relationship to the phylogenetic status of the organism: "higher" organisms, such as man, are likely to have developed more complex principles of variation than "lower" organisms, such as the pigeon.

Thus, the major focus of the argument between Skinner and Chomsky is not on the importance of reinforcement, but about the complexity of the principles of variation that determine the nature of behavior in advance of reinforcement. Since Skinner derives his ideas from work on rats and pigeons, and Chomsky from the study of human language, there are considerable grounds for a disagreement, especially if its basis is not clearly perceived by either party. Clear conceptual separation of variation from reinforcement makes this kind of confusion much less likely.

#### CONCLUSION

The argument so far has served to draw attention to a number of generalizations about steady-state conditioning situations:

1. Most such situations involve some times and stimuli associated with relatively high reinforcement probability (e.g., the period at the end of the interval on fixed-interval schedules), and others associated with relatively low reinforcement probability (e.g., the period at the beginning of the interval).

2. The *terminal response* (a discriminated operant in Skinner's terminology) is restricted to periods of relatively high reinforcement probability. This distribution of the terminal response with respect to time

and stimuli corresponds to a principle of reinforcement that relates the strength of a response to the relative frequency, density, or proximity of reinforcement associated with that response (Catania & Reynolds, 1968; Herrnstein, 1970; Jenkins, 1970).

3. The type, as opposed to the temporal and stimulus location, of the terminal response in situations involving both response-dependent and response-independent reinforcement is determined by the interaction between principles of variation (e.g., transfer, stimulus substitution) that describe the occurrence of the response, in advance of reinforcement, and principles of reinforcement that determine whether it will persist or not (selective function of reinforcement).

4. Periods of low reinforcement probability are generally associated with *interim activities*, resembling appetitive behavior. If appropriate stimuli (goal objects) are provided, stereotyped *adjunctive behavior* (e.g., polydipsia, pica; Falk, 1969) takes the place of the more variable and relatively undirected interim activities.

5. Both terminal response and interim activities are more correctly labeled as predisposing conditions or *states* rather than behaviors, since in the absence of response dependency, the type of activity falling into these categories is not fixed. Thus, drinking, wheel running, fighting, pecking, and a number of other activities may be either terminal or adjunctive behaviors, depending on historical and stimulus factors (Segal, 1969b; Skinner, 1959; Skinner & Morse, 1958). Directing factors for adjunctive behavior are the availability of appropriate goal objects (see above), and factors related to reinforcement that render some kinds of activity more probable than others: for example, polydipsia appears to partially displace both adjunctive wheel-running (Segal 1969a) and chewing-manipulatory behavior (Freed & Hymowitz, 1969) in rats (in situations with food as terminal reinforcer), even when both supporting stimuli are concurrently available.

### *The Terminal Response*

We have already discussed the probable role of principles of variation such as stim-

ulus substitution in describing the origin of terminal responses such as Pecking and Head in magazine in the superstition situation. Before turning to the more complex matter of the interim activities, a word should be said about the paradoxical results of the Williams and Williams (1969) study, in which they found persistent pecking at a brief stimulus, ending in reinforcement, despite the fact that a key peck terminated the stimulus and thus prevented reinforcement. In terms of our analysis, this situation pits reinforcement and variation against one another; thus, (a) the predictable delivery of food at the end of each presentation of a brief key stimulus may be a sufficient condition for the occurrence of key pecking in that stimulus, by the principle of stimulus substitution. But, (b) because of the response-contingency, the occurrence of a peck turns off the stimulus, omitting reinforcement on that occasion, and thus reducing the overall reinforcement rate. In turn, this reduction in reinforcement rate will, via Principle of Reinforcement 3, tend to reduce the tendency to make any terminal response, including pecking, in that situation. This process will continue until the tendency to peck has been sufficiently reduced to allow the key stimulus to continue unpecked until the delivery of reinforcement, which will again provide the occasion for the operation of the stimulus substitution principle, making pecking likely once again. Thus, an equilibrium will be established at a rate of pecking higher than zero, but less than the rate which would obtain if pecking had no effect on reinforcement rate. Extinction of pecking takes place if key pecking prevents reinforcement, but does not turn off the key stimulus, because the predictability of reinforcement, and thus the necessary condition for the operation of stimulus substitution, is thereby destroyed. Under these conditions, variation and reinforcement combine to weaken the tendency to peck, which therefore declines relatively rapidly.

The data on "instinctive drift" reported by Breland and Breland (1961, 1966) are

also compatible with this kind of analysis.<sup>7</sup> However, their results strongly suggest that too much stress should not be laid on the apparent identity we (and others, e.g., Brown & Jenkins, 1968; Williams & Williams, 1969; Wolin, 1968) have found between the topography of the terminal response and the (unconditioned) response made to the terminal reinforcer. As in the case of adjunctive behavior (discussed below) and behavior elicited by central stimulation in reinforcing brain areas (e.g., Glickman & Schiff, 1967; von Holst & von Saint Paul, 1963), the effect of the experimental procedures we have described appears to be the induction of a *state* (in our terminology) or "mood" (von Holst & von Saint Paul) which makes some kinds of activity much more likely than others but preserves some flexibility in the animal's mode of response to the stimulating environment.

For example, Breland and Breland (1961) describe two situations, both showing "instinctive drift" in chickens, but only one of which conforms to the stimulus substitution principle. In the first case, the chicken pecked a ball which he had learned to project (via a remote firing mechanism) at a target—a hit being immediately followed by food. The history of contiguity between the moving ball and reinforcement and the similarity of the responses to both ball and food (i.e., pecking) fit easily into the stimulus substitution paradigm. In the second case, however, the chicken was reinforced for a chain of responses, the last of which involved standing on a platform for 15 seconds. After training, the chicken showed vigorous ground scratching while standing

<sup>7</sup> The delayed appearance of the food-related behaviors in the Breland and Breland situations (as compared to the relatively rapid emergence of pecking in auto-shaping experiments) reflects the fact that food delivery could not become predictable (the necessary condition for the operation of the stimulus substitution principle) until after the animals had learned to produce it by making the required "arbitrary" response. However, once this initial response was learned (via principles of variation other than stimulus substitution), the stage was set for the operation of stimulus substitution, which could then override the original learning.

on the platform. This response has no topographic resemblance to pecking food, but is, of course, a universal food-getting behavior in chickens and typically occurs in the vicinity of food.

These examples, and others discussed by Breland and Breland (e.g., raccoons "washing" poker chips, porpoises swallowing manipulanda, pigs "rooting" tokens, etc.), as well as exceptions to stimulus substitution in the auto-shaping literature (e.g., Rachlin, 1969; Sidman & Fletcher, 1968), are compatible with a more general notion, to the effect that the stimulus (temporal or exteroceptive) most predictive of reinforcement comes to control a state or mood (the terminal state) appropriate to that reinforcer. The particular activity which occurs during the terminal period will then depend on principles of variation, which take into account *both* its motivational properties (e.g., food-related activities become more likely if the terminal reinforcer is food), *and* the nature of the stimulating environment; that is, the nature of the terminal state determines what stimuli will be effective in eliciting what behavior. Thus, the Breland and Breland chickens pecked when the stimulus defining the terminal state was appropriate (in some sense) for pecking, but scratched when it was not; pigeons peck the key in auto-shaping experiments, but (based on our results) are slower to peck in the super-stition situation—presumably because an appropriate target is not provided. Similarly, Glickman and Schiff (1967) reviewed a large number of studies of behavior induced by direct brain stimulation which suggested that the effect of stimulation at a particular site is to induce a predisposing motivational condition or state which may lead to a variety of behaviors depending on the presence or absence of appropriate supporting stimuli. More recent work (e.g., Valenstein, Cox, & Kakolewski, 1970) further emphasizes the similarity between the terminal state and behavior induced by central stimulation:

Hypothalamic stimulation does not activate only one specific behavior pattern. The stimulation seems to excite the substrate for a group of responses that in a given species are related to a common state [Valenstein et al., 1970, p. 30].



Within this more general framework, stimulus substitution becomes a special case, which simply reflects the fact that the response normally made to the terminal reinforcer often becomes highly probable when the animal is in a terminal state corresponding to that reinforcer, and will occur if minimal environmental support is provided.

#### *Interim Activities and Adjunctive Behavior*

The principles of variation and reinforcement so far discussed refer to the origin and maintenance of the terminal response. The interim activities (including adjunctive behavior) require a separate although complementary account, to which we now turn.

There is as yet no general agreement on the causal factors underlying adjunctive behavior (i.e., behavior occurring during the interim period on a variety of intermittent reinforcement schedules). As we have already noted (p. 13), data presently available appear to rule out simple physiological interpretations (Falk, 1969), although they do not clearly point to any alternative. Some help is offered by the similarities between adjunctive behavior and displacement activities: the same explanation should be adequate for both. Possibilities are also somewhat restricted by general functional considerations. In the present section, we state a tentative general hypothesis and its empirical basis in what is presently known of adjunctive behavior. The relationship of this hypothesis to accounts of displacement behavior and to general adaptiveness is discussed in the following section.

The hypothesis may be stated in the form of three propositions:

1. The interim and terminal periods correspond to states, in the sense described earlier, defined by (a) the class of reinforcer or reinforcers that are effective at that time, and (b) the applicability of principles of variation appropriate to that class of reinforcer (e.g., food-related behaviors are likely to occur during the food state, defense reactions during the fear state, etc.).

2. The terminal state corresponds to the terminal reinforcer; the state during the interim period corresponds to *all other reinforcers*, although all need not be equally

effective. The linkage between terminal and interim states is assumed to be direct and reciprocal, so that the strength (defined below in terms of *rate*) of activities during the interim period is *directly related* to the strength of the terminal response.

3. The strength of the terminal response is directly related to the "value" of the reinforcement schedule; that is, to relative rate and amount of reinforcement and to motivational factors (e.g., deprivation).

As a consequence of the reciprocal interaction between the terminal and interim states (Proposition 2) and the dependence of the terminal response on the value of the reinforcement schedule (Proposition 3), the strength of behaviors associated with the interim period will be determined both by the value of the terminal reinforcement schedule, as well as by the value of the reinforcers proper to them.

The notion of the interim and terminal periods as states (Proposition 1) has already been discussed. The dependence of the strength of the terminal response on variables related to the value of the reinforcement schedule (Proposition 3) should also encounter no opposition. It remains to show, first, that reinforcers other than the terminal reinforcer are effective during the interim period; second, that a number of different reinforcers may be effective at this time; and third, that the direct relation between the strength of the terminal response and the strength of adjunctive behavior implied by Propositions 2 and 3 has some basis in fact. Evidence for the effectiveness of reinforcers other than the terminal reinforcer, during the interim period, comes largely from studies of polydipsia (excessive drinking), induced by intermittent schedules of food reinforcement, as follows: (a) Consummatory behavior (e.g., drinking) occurs in the presence of the appropriate goal object (water) during the interim period on a variety of schedules. (b) This goal object can reinforce operant behavior:

If water is not freely available . . . concurrently with a food schedule, but is available in small portions contingent upon the completion of a fixed-ratio schedule, polydipsia is acquired and will sustain large fixed-ratios [Falk, 1970, p. 297].

Azrin (1964, reported in Falk, 1970) has found similar schedule control, in pigeons, by a bird provided as a target for schedule-induced aggression. (c) Polydipsic drinking can be both increased and decreased by appropriate alteration of the palatability of the liquid available (Falk, 1964, 1966). (d) Polydipsia is usually reduced by using liquid terminal reinforcers. In the cases where the liquid used is at least as reinforcing to a hungry rat as dry food pellets (e.g., liquid Metrecal, condensed milk, liquid monkey diet; Falk, 1964, 1966; Hawkins, Everett, Githens, & Schrot, 1970; Stein, 1964), this decrease may be attributed to a direct effect on the thirst system (state), due to the water content, as in water preloading (see below). Wesson oil as terminal reinforcer is also less effective than food pellets in producing polydipsia (Stricker & Adair, 1966), although it contains no water, but is probably also less reinforcing and may reduce polydipsia for this reason (see Propositions 2 and 3). (e) Acquisition of polydipsia can be prevented by pre-session stomach loads of water (Chapman, 1969, reported in Falk, 1970), although established polydipsia is little affected. This kind of effect is also found with food-motivated terminal responses which, once established, will continue to occur at a somewhat reduced rate even in the presence of ad lib food (Neuringer, 1970a).

That more than one reinforcer is effective during the interim period is suggested by the facts that (a) the interim activities (which occur in the absence of appropriate goal objects) have no consistent direction and are not obviously related to any particular reinforcer; (b) a variety of goal objects—water, wood shavings, another animal—are sufficient to elicit appropriate consummatory reactions (drinking, chewing or eating, aggression); (c) physiological data, suggesting short-term reciprocal interactions between hunger and thirst drives which were induced centrally by electrical or chemical stimulation (Grossman, 1962; von Holst & von Saint Paul, 1963), indicate a possible mechanism for the simultaneous effectiveness of reinforcers other than food at nonfood times (interim periods) and food at food

times (terminal periods). While it is as well to be cautious in generalizing both across species and from physiology to behavior, these hypothalamic mechanisms are evidently quite similar in birds and mammals (cf. Åkerman, Andersson, Fabricius, & Svensson, 1960), and recent work supports the similarity between schedule-induced drinking and drinking induced by direct hypothalamic stimulation implied by these comparisons (Burks & Fisher, 1970). This kind of reciprocal interaction suggests that at a time when activity motivated by hunger is suppressed (e.g., during the period of interim activities on a food schedule), activities motivated by thirst might be facilitated.

That the effective reinforcers during the interim period are other than the terminal reinforcer is suggested both by the nonoccurrence of the terminal response at that time and by data reported by Segal (1969b) showing a shift in the status of drinking—from an adjunctive behavior, occurring early in the interval, to a terminal response, occurring largely at the end. The changeover took several experimental sessions, but it suggests that in the steady state, the same activity is unlikely to occur during both terminal and interim periods, as might be expected if these periods are associated with the action of different reinforcers.

Propositions 2 and 3 in combination imply that the strength of adjunctive behaviors, like that of the terminal response, should be directly related to the "value" of the reinforcement schedule, as indexed by motivational and reinforcement variables. The evidence in favor of this deduction is as follows: (a) With the interreinforcement interval held constant, the amount of polydipsic drinking in a session of fixed length is inversely related to body weight (Falk, 1969). Similar results have been reported for schedule-induced attack and air licking (Falk, 1970). (b) Postpellet pause (until the onset of polydipsic drinking) increases as a function of the interval duration (Segal et al., 1965). (c) Rate of licking within a drink bout tends to decrease as a function of interval length (Segal et al., 1965). (d) Rate of polydipsic drinking is increased by increasing the size of food reinforcement

(Hawkins et al., 1970). Hawkins et al. attribute a contrary result by Falk (1967) to session length differences between the one- and two-pellet conditions of Falk's experiment. (e) Polydipsic drinking falls off drastically at fixed-interval values longer than about 3 minutes (Falk, 1966; Segal et al., 1965). Falk (1966) also reports a *direct* relationship between polydipsia (measured as total amount drunk per session) and fixed-interval length over the range 2–180 seconds. However, based on the results of experiments indicating the relative constancy of ingestion rate within a drinking bout in rats (Davis & Keehn, 1959; Schaeffer & Premack, 1961; Stellar & Hill, 1952), and the different times available for polydipsic drinking under different fixed-interval values, Falk's finding of a direct relationship between fixed-interval value and total amount drunk, over part of the range, is compatible with an overall *inverse* relationship in terms of *rate* of drinking. This inference is confirmed by recent data reported by Hawkins et al. (1970) which show a monotonically decreasing ingestion rate as a function of fixed-interval length, over the range 1–5 minutes. Given that overall ingestion rate is probably better than total amount drunk as a measure of the tendency to drink, this finding is both consistent with the deduction from Propositions 2 and 3 and more easily reconciled with other measures that indicate an inverse relationship between tendency to drink and frequency of reinforcement. Falk's finding that total amount drunk is maximal at intermediate interval values may reflect, therefore, an optimal balance between two factors: tendency to drink, which decreases as interval value increases, and time available for drinking, which increases with interval value.

The above account, in terms of interaction between motivational systems (states), seems to be the simplest that can presently be given of both adjunctive behavior and displacement activities. Its relationships to these activities, to the ethological interpretation of them, and to general functional considerations are discussed in the next section.

### *Adjunctive Behavior and Displacement Activities*

Most behavioral and morphological characteristics are adaptive, in the sense that they can be directly related to the reproductive fitness of the organism in its natural environment. Morphological exceptions to this rule are either the result of "correlated variation," in Darwin's phrase, or are vestigial characters, in the process of being lost. In neither case do they show the ubiquity and reliability that distinguish adjunctive and displacement activities. It is very likely, therefore, that these behaviors reflect a function, or functions, of considerable adaptive value to animals in the wild. What might this function be?

Learning theories generally consider the organism to be motivated by one thing at a time, for example, hunger, thirst, exploratory drive, etc. In like spirit, most learning experiments are designed to ensure the predominance of one kind of motivation at the expense of all others. In the wild, however, animals must allocate their time among a variety of activities so as to both satisfy current wants and anticipate future ones. It is reasonable to assume that there has been considerable selection pressure favoring an optimal balance among the various possibilities.

We have already noted the fact, related to stimulus discrimination, that animals tend to make the terminal response only at times when reinforcement is likely. This fact, and the principle of reinforcement based on it, might seem to reflect some kind of Law of Effort, since responding at times or places when reinforcement never occurs is obviously wasteful. However, a considerable weight of evidence suggests that the Law of Effort is not a major psychological principle, since it is easy to devise situations in which animals make many more responses than necessary (cf. Ferster & Skinner, 1957). A Law of Effort principle would also not explain active *avoidance* of situations associated with nonreinforcement.

A more plausible alternative is that these facts are related to animals' need to budget their time effectively. In these terms, a time, or stimulus, reliably associated with

the absence of a given reinforcer provides information just as useful as a time perfectly correlated with the delivery of that reinforcer, since it permits the animal to attend to present and future needs other than the one associated with the absent reinforcer. However, other potentialities of the environment cannot usually be sampled as long as the animal remains in the vicinity of the unavailable reinforcer. One might expect, therefore, that natural selection will have fostered the development of a mechanism to ensure that animals avoid places at times when, on the basis of past experience, they have learned that reinforcement is not forthcoming.

Evolution is notoriously opportunistic in the sense that adaptation is achieved by whatever structural or functional means happen to be available. In the present case, we suggest that the means for ensuring that animals will not linger in the vicinity of food (or other reinforcers) at times when it is not available may be provided by the facilitation of drives other than the blocked one (Propositions 2 and 3); that is, that the relative aversiveness of the stimuli in the vicinity of food, during the interim period, may be a *direct effect* of the simultaneous suppression of the food state and facilitation of states associated with other reinforcers. In the wild, such facilitation will usually ensure that the animal leaves the situation to seek other reinforcers. Moreover, once the animal has left the situation, generalization decrement will ensure that the effect of factors acting to facilitate these other drives is reduced, restoring the animal to a state appropriate to his condition of deprivation and allowing him to take advantage of new opportunities to satisfy the previously blocked drive.

In experimental situations showing adjunctive behavior, however, the animal is *kept in the vicinity of the withheld reinforcer*, both by the physical restraint of the enclosure and, perhaps more importantly, by the properties of the reinforcement schedule. Since enclosure size has not been explicitly investigated, one cannot be sure of the relative importance of these two factors. In our laboratory, we have often observed animals

on time-based schedules and noticed that pigeons tend to turn away from the key during the no-pecking phase of schedules such as the fixed-interval, which yield a period of no pecking followed by pecking. Figure 1, which shows the birds' orientation ( $R_1$ ) as a function of postfood time, is a quantitative record of this effect. However, the avoidance of the key is much more complete on schedules which require pecking followed by no pecking (temporal go-no go schedules, Staddon, 1970a, 1970b), presumably because the no-pecking period is terminated by an external event (delivery of reinforcement) rather than by the bird returning to the key to peck. These observations provide some evidence both for the tendency of pigeons to avoid the key at times when key pecking is not reinforced and for the restriction placed on this tendency by fixed-interval schedules.

In situations involving external (rather than temporal) stimuli, there is also considerable evidence for the aversive character of stimuli associated with nonreinforcement when they occur in a context associated with reinforcement (e.g., multiple and concurrent reinforcement schedules; cf. Beale & Winton, 1970; Catania, 1969; Terrace, 1966).

Thus, the temporal locus of adjunctive (and interim) behavior coincides with a period when, by other measures, the situation is aversive to the animal so that he will withdraw from it if he can. Studies of schedule-induced escape have shown that animals will learn to make a response during the interim period on fixed-ratio schedules that has the effect of removing them from the situation, even though the frequency of the terminal reinforcement may thereby be reduced (e.g., Azrin, 1961; Thompson, 1964). The present argument suggests that these data may reflect a general property of interim periods, although experimental results with other schedules are presently lacking.

Falk (1969, 1970) has ably summarized the similarities between adjunctive behavior and displacement activities, which include the apparent "irrelevance" of both kinds of activity, their association with situations in which a strong drive is blocked, and their

modifiability by available stimuli and conditions of deprivation other than the major drive (cf. Morris, 1954; Rowell, 1961; Sevenster, 1961). A study by McFarland (1965) concerning displacement pecking induced by preventing drinking in thirsty doves further emphasizes the "state" property of displacement behavior:

The evidence for the view that this pecking belongs to the feeding system . . . [is] as follows: 1. Total time spent pecking is increased by the presence of grain. 2. Time spent pecking is partly replaced by time spent at a specific food getting activity, when the birds have previously been trained to obtain food in this way. 3. Food deprivation . . . increases the time spent pecking when grain is present, and this effect is counteracted by [prefeeding] [p. 298].

The interpretation of adjunctive behavior that we have presented is very similar to the disinhibition hypothesis concerning displacement activities first suggested by Andrew (1956). Hinde (1966) summarizes this view as follows:

When mutual incompatibility prevents the appearance of those types of behaviour which would otherwise have the highest priority, patterns which would otherwise have been suppressed are permitted to appear [p. 279].

Displacement behavior is usually exhibited in approach-avoidance conflict situations (e.g., territory defense, birds returning to the nest after an alarm, etc.) when the animal is consequently *prevented from leaving the situation*. We have already seen that during the interim period on intermittent reinforcement schedules, animals are also restrained in the situation both by the enclosure and, probably, by the properties of the schedule. Both situations therefore meet the conditions necessary and sufficient, by our hypothesis, for the *elevation* (rather than merely disinhibition) of motivational states other than the blocked one, leading in the schedule case to adjunctive behavior, and in the approach-avoidance case to displacement, redirection, or vacuum activities—the particular activities being a function of the strength and nature of the blocked response, the proximity to the goal, the stimuli available, the past history of the animal, and the duration of the blocking. This modifica-

tion causes no difficulty in application to displacement behavior and meets theoretical objections to certain forms of the disinhibition hypothesis raised by McFarland (1966).

Finally, our account of the adaptive significance of adjunctive behavior, as a reflection of the integrative capacities of the organism which enable it to strike an efficient balance among a number of activities, finds a counterpart in McFarland's (1966) account of the significance of displacement behavior:

Thus it is suggested that the functional significance of displacement activities is that they are the by-product of a mechanism which enables animals to break away from a specific course of action, when progress in that course of action comes to a standstill [p. 231].

We conclude, therefore, that interim, adjunctive, and displacement behaviors may be grouped together on the basis of similar functional properties, similar probable causal factors, and similar adaptive role.

In summary, the argument relating to the interim period is as follows: (a) Extant data on adjunctive behavior are consistent with a tentative general interpretation in terms of interactions among motivational systems (states). (b) On the basis of general adaptive considerations, we have suggested the probable existence of a mechanism which enables animals to budget their time efficiently (e.g., by giving up temporarily ineffective activities). (c) McFarland has suggested that displacement and other "irrelevant" activities may reflect the action of such a mechanism. (d) Falk has pointed out the extensive similarities between adjunctive and displacement activities. (e) McFarland's suggestion may therefore be extended to interim and adjunctive behaviors, both on the basis of their resemblance to displacement activities and their restriction to the aversive interim period. Thus, the general interpretation of adjunctive behavior offered earlier gains additional support from the resemblances between adjunctive and displacement activities, from its similarity to the disinhibition hypothesis for displacement activities, and from its adequacy as a mechanism for enabling the animals to budget their time efficiently.

## EPILOGUE

This ends our outline of conditioning. We have dealt with both terminal periods—which, we suggest, reflect a Law of Effect process that can best be understood by analogy with evolution by means of natural selection—and interim periods—which may reflect a mechanism enabling animals to allocate their activities efficiently. Learned behavior, under the relatively simple conditions of reinforcement schedules at least, is viewed as reflecting the sequencing, with respect to time and stimuli, of terminal and interim periods; and the scheme is therefore potentially comprehensive, although necessarily incomplete as to details.

Our proposal is founded on the belief that the most distinctive thing about living creatures is the balance they maintain among a number of tendencies to action, each one adaptive, yet each destructive if pursued to the exclusion of others. This emphasis on the *integration* of behavior has required that the scheme attempt to be comprehensive and that it relate in a natural way to biological and physiological considerations. Such merits as it possesses lie not in formal elegance or precision, but in an ability to organize otherwise unrelated facts and to suggest gaps where others may possibly be found.

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