THE OPERANT-RESPONDENT DISTINCTION: FUTURE DIRECTIONS

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The operant-respondent distinction has provided a major organizing framework for the data generated through the experimental analysis of behavior. Problems have been encountered, however, in using it as an explanatory concept for such phenomena as avoidance and conditioned suppression. Data now exist that do not fit neatly into the framework. Moreover, the discovery of autoshaping has highlighted difficulties in isolating the two types of behavior and conditioning. Despite these problems, the operant-respondent framework remains the most successful paradigm currently available for organizing behavioral data. Research and theoretical efforts should therefore probably be directed to modifying the framework to account for disparate data.

Key words: operant behavior, respondent behavior, operant-respondent interactions

Many behavioral psychologists accept the proposition that there are two distinct categories of behavior and of conditioning, which we shall refer to in this article as *operant* and *respondent*. Unfortunately, there are no definitions of operant and respondent behavior and operant and respondent conditioning that would be universally accepted by behavioral psychologists. Most definitions that have been proposed, however, bear a high degree of similarity.

Most definitions of operant behavior emphasize that it is a class of responses defined by a particular consequence or effect on the environment, and operant conditioning is defined as resulting from the consequences or effects of operant behavior. For example, Catania (1968) defined an *operant* as a "class of responses the subsequent likelihood of which may be modified by its consequences" (p. 340), and *operant conditioning* as "the modification of operant behavior by reinforcement

or punishment" (p. 341)—that is, consequences that increase or decrease the probability of the behavior. Most definitions of respondent behavior emphasize the importance of prior stimuli in eliciting the behavior, and respondent conditioning is defined as resulting from the pairing of stimuli. For example, Catania defined *respondent* as a "class of responses defined in terms of the stimuli that reliably produce them" (p. 345), and *respondent conditioning* as "the modification of respondent behavior by the pairing of stimuli" (p. 345).

The following questions regarding the operant-respondent distinction should, in our view, be addressed in the future:

1. Are there two classes of behavior that can be distinguished empirically in a manner consistent with the above definitions, or some approximations thereof?
2. Are there two distinct, irreducible types of conditioning corresponding to the above distinction? Note that an affirmative answer to this question necessarily implies an affirmative answer to the first question, although the converse is not the case.
3. If more than one class can be empirically distinguished, how do these classes interact with each other?

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4. Are there more than two fundamental classes of behavior that can be distinguished empirically from each other? If there are, do any of these additional classes have distinct "conditioning" processes associated with them?
5. Would alternative classification schemes organize behavioral data more effectively than can be done by the operant-respondent framework?

In identifying the above issues as the major ones to be addressed in the operant-respondent distinction, we are revealing a bias in our approach in favor of the experimental analysis of behavior. Specifically, we are adopting a largely inductive stance, which will color our subsequent discussion of the history, current status, and future directions of this area.

HISTORY AND CURRENT STATUS OF THE PROBLEM

The distinction between operant and respondent conditioning was first made independently by the Polish physiologists S. Miller and J. Konorski (1928) and by B. F. Skinner (1935). Unlike the former authors, Skinner emphasized the absence of an observable eliciting stimulus in the operant conditioning procedure (Konorski & Miller, 1937a, 1937b; Skinner, 1937). Subsequently, Skinner continued to stress the concept of a fundamental distinction between elicited and emitted behavior, the former being subject to respondent conditioning and the latter to operant conditioning (Skinner, 1953, p. 112).

Many learning psychologists resisted the notion that operant and respondent conditioning, or instrumental and classical conditioning (Hilgard & Marquis, 1940), as they became more popularly called, were fundamentally distinct processes. There was no doubt that the terms denoted different procedures—but the resulting change in behavior might be accounted for by a single process. For example, Guthrie (1935) accounted for both types of conditioning on the basis of

the contiguity between a stimulus and a response closely followed by a change in the stimulus situation. Hull (1943) accounted for both types of conditioning on the basis of the contiguity between a stimulus and a response closely followed by what he called "drive reduction."

For many behavior analysts following a strict operational approach, the existence of two distinct procedures was sufficient reason to maintain a theoretical distinction in the absence of proof that a single process accounted for the results of both. Others, however, thought that the distinction could be founded on a firmer basis. We now highlight the history and current status of the major lines of evidence pertaining to the possibility of discovering such a basis.

Topographical Distinctions between Operants and Respondents

Konorski and Miller (1937b) maintained that responses of the autonomic nervous system could not be operantly conditioned, although responses of the skeletal type could be respondently conditioned. Similarly, Skinner (1938) suggested: "It is quite possible on the existing evidence that a strict topographical separation of [operant and respondent conditioning] following the skeletal-autonomic distinction may be made" (p. 112). However, he left the door open to the possibility that autonomic responses could be operantly conditioned and that some skeletal responses could be respondently conditioned.

Interest in possible topographical distinctions between operants and respondents has continued, but no resolution of the issue has occurred. The most supportive evidence that autonomic responses could be operantly conditioned proved not to be as reliable as originally thought (DiCara, 1970; Miller, 1969; Miller & Dworkin, 1974).

Some research on topographical distinctions has focused on operantly conditioning responses typically thought to be purely respondent. For example, Shettleworth (1973) was not able to operantly condition the full grooming sequence in golden ham-

sters. However, such a finding cannot be made the basis of a topographical distinction between operant and respondent conditioning unless it can be shown that the same behavior can be respondently conditioned. Another area in which topographical differences between operants and respondents have been sought is that of autoshaping. Our discussion of autoshaping will be left to a later section.

Avoidance and Conditioned Punishment and Reinforcement

Mowrer (1947) postulated a "two-factor" theory to account for the establishment and maintenance of a conditioned avoidance response. This theory held that a conditioned emotional response (CER) results from the pairing of a neutral stimulus with an aversive event. This is a respondent conditioning process in which the neutral stimulus becomes a conditioned stimulus eliciting the CER. An avoidance response terminates the conditioned stimulus, thereby terminating the CER. This is an operant conditioning process in which the avoidance response is reinforced by the termination of the CER and consequently increases in frequency. Following Mowrer's work, a number of other theorists treated avoidance learning from the perspective of two-factor theory (e.g., McAllister & McAllister, 1971; Miller, 1948; Rescorla & Solomon, 1967; Solomon & Wynne, 1953). In addition to accounting for avoidance behavior, various versions of two-factor theory have been used to account for conditioned punishment—that is, the punishing effect that can be imparted to a neutral stimulus by correlating it with an aversive stimulus (Mowrer & Solomon, 1954). Attempts to provide a similar two-factor theory to explain conditioned reinforcement have also been made (Mowrer, 1960; Staats, 1963, 1975), but the complexities involved in studying conditioned reinforcement as the result of a simple pairing procedure (Gollub, 1977) seem to have discouraged widespread acceptance of this approach.

A simple two-factor theory no longer seems

adequate to account for avoidance learning either. One problem with the theory is that the CER should decrease rapidly during extinction when the aversive stimulus no longer occurs, and thus the avoidance response likewise should decrease rapidly during extinction. In fact, however, avoidance behavior is typically extremely persistent during extinction (Sheffield & Temmer, 1950; Sidman, 1955; Solomon, Kamin, & Wynne, 1953). Another problem is that after avoidance responses have been well conditioned, their latencies are typically shorter than the latencies of indicators of supposed CERs (Solomon & Wynne, 1954). This makes it difficult to explain the maintenance of avoidance behavior with two-factor theory.

Conditioned Suppression and Facilitation

The operant-respondent distinction also gained acceptance from the phenomenon of conditioned suppression, whereby a neutral stimulus correlated with an aversive event acquires the power to disrupt operant behavior (Estes & Skinner, 1941). Subsequently, the range of phenomena discovered by superimposing a conditioned stimulus on an operant baseline was extended. In particular, stimuli correlated with aversive events have been superimposed on a baseline maintained by aversive events, and stimuli correlated with positive reinforcement have been superimposed on baselines maintained by positive reinforcement. Unfortunately, the results of such manipulations are complex. Stimuli correlated with aversive events sometimes increase (e.g., Sidman, Herrnstein, & Conrad, 1957) and sometimes decrease (e.g., Hurwitz & Roberts, 1971) responding on an avoidance baseline, and stimuli correlated with positive reinforcement sometimes increase (e.g., Estes, 1943) and sometimes decrease (e.g., Azrin & Hake, 1969) responding maintained by positive reinforcement. In addition, there may be problems in the use of response rate to measure the effects of the former manipulation (see Hurwitz & Roberts, 1977).

Two major categories of hypotheses to account for the Estes-Skinner conditioned sup-

pression effect have recently received a good deal of empirical attention. First, the interference hypotheses state that operant behavior is disrupted by competing operants or respondents conditioned during the Pavlovian pairings. However, there are difficulties with this account. In addition to the question of identifying whether responses produced by the superimposed stimulus are operant or respondent, there is also the difficulty of proving that such responses interfere with the ongoing operant rather than simply occurring because the operant has been suppressed. Moreover, complications with a strictly respondent account of conditioned suppression arise from the fact that presumably competing respondents (physiological responses) do not appear to covary in predicted ways with changes in the operant response (see Blackman, 1977).

Second, motivational hypotheses attribute changes in operant behavior during the superimposed stimulus to the interaction of motivational or other hypothesized mediational states (e.g., Estes & Skinner, 1941; Overmier & Lawry, 1979; Rescorla & Solomon, 1967). However, the data relating to this account are inconsistent. In particular, facilitation of operant behavior is not invariably obtained when a stimulus paired with an aversive event is superimposed on avoidance (e.g., Hurwitz & Roberts, 1977) or when a stimulus paired with positive reinforcement is superimposed on a baseline maintained by positive reinforcement (e.g., Azrin & Hake, 1969), contrary to what motivational hypotheses would predict. In addition, motivational hypotheses can be criticized on the basis that "motivation" and other postulated internal states are poorly defined concepts, and as such are unlikely to provide any real explanatory power.

Concurrent Measurement of Operants and Respondents

Skinner (e.g., 1953, pp. 126-128, 266) implied that operant and respondent behavior occur together and frequently interact. Concomitant with this type of speculation were studies of the covariation of operants and

respondents in the same experimental paradigm. For example, it was found that lever pressing and salivation by dogs tended to vary together within a fixed-interval schedule (Shapiro, 1960), but that bursts of salivation preceded pressing on a differential-reinforcement-of-low-rates schedule (Shapiro, 1962) and that pressing tended to begin well before salivation on fixed-ratio schedules (Williams, 1965). In addition, a number of studies focused on the joint occurrence of operant avoidance or escape responses and elicited attack generated by the aversive stimuli on which the avoidance or escape behavior was based (e.g., Azrin, Hutchinson, & Hake, 1967). Difficulties in clearly identifying operants and respondents in various experimental procedures, however, seem to have derailed this type of approach. These difficulties will be examined when we discuss autoshaping.

Constraints on Conditioning Principles

The issue of constraints on conditioning principles was raised by the documentation of apparent failures of general conditioning principles (e.g., Hinde & Stevenson-Hinde, 1973). For example, the discovery of conditioned food aversions seemed at first to challenge a general conditioning interpretation of avoidance learning in terms of the effective interval between the conditioned and unconditioned stimuli, the number of pairings required for conditioning to occur, and the differential conditionability of different "neutral" stimuli (Garcia & Koelling, 1966; Garcia, McGowan, Ervin, & Koelling, 1968). Another important example is the finding of Breland and Breland (1961) that operant conditioning was often disrupted by competing patterns of behavior with apparent phylogenetic origins. Their conclusion that their results "represent a clear and utter failure of conditioning theory" (p. 683) set the tone for subsequent attacks on the operant-respondent framework.

In fairness, it should perhaps be pointed out that the implied charge that early learning theorists had not given proper considera-

tion to constraints on learning may have been overstated by those who argued against general laws of learning. This is indicated by Skinner's (1966) response to Breland and Breland (1961):

No reputable student of animal behavior has ever taken the position "that the animal comes to the laboratory as a virtual *tabula rasa*, that species differences are insignificant, and that all responses are about equally conditionable to all stimuli." (p. 1205)

In a similar vein, Dinsmoor (1977) recalled:

We were, of course, aware from the beginning of the possibility that unlearned behavior might be involved. . . . But in those days it was not the fashion, as it is today, to take innate behavior as the target of investigation in a learning experiment. Matters of this kind were treated merely as difficulties to be circumvented in order to study more general laws, those governing learned behavior. (p. 83)

Despite the current interest in constraints on conditioning principles, the issue does not seem to have had as great an impact on the field as many thought it would. Domjan and Galef (1983) argue cogently that this approach has not led to a systematic plan of research or a systematic body of data, but rather to a catalogue of anomalies. Many of these anomalies, they argue, can be handled without major modification to current operant or respondent formulations.

Schedule-Induced Behavior

The history of schedule-induced behavior began with Falk's (1961) finding that rats on a variable-interval schedule of food reinforcement with water freely available drank after each reinforcement and, in the process, consumed excessive amounts of water. Although the excessive drinking was generated by the reinforcement schedule, subsequent research seemed to rule out any possibility that the behavior was maintained by the food-reinforcement contingency (Falk, 1966). In addition, it did not appear to be

elicited because it required several sessions to develop, it was subject to environmental circumstances, and it could withstand temporal delays between the food presentation and the availability of water (Falk, 1971; Flory & O'Boyle, 1972).

Staddon and Simmelhag's (1971) attempted replication of Skinner's (1948) superstition experiment also raised the issue of schedule-induced behavior as a challenge to the operant-respondent formulation. It implied that much, if not all, of what Skinner had interpreted as adventitiously reinforced behavior was in fact schedule-induced, thus implicating a possible third class of behavior. Moreover, it called into question the notion that reinforcement acts in an automatic manner in strengthening whatever response it follows, which is an important assumption in the operant-respondent formulation. The question of whether schedule-induced behavior constitutes a third class of behavior has been discussed recently in the literature (Herrnstein, 1977a, 1977b; Wetherington, 1982), but no answer has evolved.

Autoshaping

The discovery of autoshaping by Brown and Jenkins (1968), whereby the pairing of a keylight with food presentations produced responding on the key by pigeons, led to a problem for the operant-respondent framework. At first the phenomenon was thought to be purely operant, but a study by Williams and Williams (1969) revealed that autoshaped responses could be maintained when responses canceled the reinforcer. Moreover, Jenkins and Moore (1973) showed that the topography of the pigeon's autoshaped key peck is similar to the topography of responses directed at the reinforcer. These two findings suggested that autoshaped behavior was respondent. Subsequently, a number of researchers arrived at the view that autoshaped responses are neither exclusively operant nor respondent, but represent an interaction of response-reinforcer and stimulus-reinforcer contingencies (Jenkins, 1977; Williams, 1981; Woodruff,

Conner, Gamzu, & Williams, 1977). The fact of autoshaping has caused others to see operant and respondent processes as hopelessly intertwined, thus leading them to reject the operant-respondent framework.

A good deal of research related to autoshaping attempted to discover if respondent-controlled key pecks were present in standard operant contingencies. Schwartz and Williams (1972) discovered two types of key pecks, distinguished on the basis of duration, that appeared to be differentially affected by contingencies of reinforcement. They suggested that only key pecks of relatively long duration were controlled by operant contingencies. Other research suggested that the behavioral contrast phenomenon is produced by the contribution of respondently conditioned key pecks in an operant discrimination (Rachlin, 1973). More recent work, however, has seriously questioned this interpretation of behavioral contrast (Williams, 1983).

PROPOSED SOLUTIONS AND FUTURE DIRECTIONS

With regard to the questions listed in the introduction to this article, we see the second—Are there two distinct types of conditioning corresponding to the distinction between operants and respondents?—as being the critically important one.

There are two distinct general procedures for conditioning responses. Basically, the operant procedure involves pairing a response with a particular stimulus; the respondent procedure involves pairing two stimuli. In order to show that the effects of the two procedures are manifestations of the same underlying process, it will be necessary to demonstrate an isomorphism between the two procedures. Conversely, in order to show that the two effects are manifestations of different processes, it will be necessary to show that such an isomorphism does not exist. This latter would, of course, require the empirical establishment of a set of principles that would logically rule out such an isomorphism. Neither of these demonstrations has

been accomplished. The following sections discuss approaches that, separately or in combination, may lead to either type of demonstration.

Cognitive Versus Radical Behavioristic Approaches

An increasing number of writers have advocated a return to cognitive psychology as a way of solving difficulties such as those under discussion. The following statement by Kimble (1971) summarizes this position:

As the evidence accumulates, it begins to appear that even two-factor theory is an unrealistic oversimplification. Classical and instrumental conditioning are both much more complicated than . . . two-process theory treated them as being. A strong argument can be made that cognitions and stimulus-response connections are both formed in any learning situation and that the results of learning experiments depend on contributions of both types. (p. 75)

Although the possible future success of a cognitive approach cannot be dismissed out of hand, those adopting it would be well advised to consider the numerous and eloquent philosophical arguments that have been raised against it (e.g., Branch & Malagodi, 1980; Moore, 1975, 1981; Morris, Higgins, & Bickel, 1982; Skinner, 1977).

One alternative to a cognitive approach is a strict operational approach. This involves simply classifying behavior on the basis of the operations involved in producing it, without regard to presumed underlying processes. For example, rather than talking about a CER being responsible for conditioned suppression, one might take the conditioned suppression effect purely as a functional relationship (e.g., Lyon, 1963, 1968). In this approach, stimuli may acquire certain effects through a respondent conditioning procedure in addition to the tendency to elicit certain responses. Essentially, this is the approach adopted by Skinner in describing avoidance as being conditioned by the negative reinforcing effect of the removal of

a conditioned aversive stimulus (e.g., 1953, p. 176). Inasmuch as the concepts in a valid theory must be clearly defined by empirical operations, it can be argued that such an approach has at least as good a chance of leading to a comprehensive theory of behavior as does a cognitive approach.

Eliminating Versus Modifying the Operant-Respondent Framework

As we have indicated, there is dissatisfaction with the operant-respondent framework for organizing behavioral data. Arguments for modifying or altogether eliminating these two categories have abounded in the literature. For example, Hearst (1975) stated:

It would seem a good idea to shed, as far as possible, some of our preconceptions and biases about behavioral categories and take a new look at how one might study and interpret the phenomena of learning and behavioral change. (p. 218)

Similarly, Wetherington (1982) recommended:

Given the growing difficulties in maintaining an operant-respondent dichotomy . . . research efforts should de-emphasize preconceived and outdated notions about categories of behavior and how such behaviors should behave. (p. 329)

Likewise, addressing the operant part of the framework, Timberlake (1984) stated:

I think we have pushed reinforcement-based explanations beyond their appropriate limits, beyond what is justified by the data or our understanding of the underlying processes. Reinforcement explanations simply do not consider a sufficient number of the determinants of behavior. (p. 385)

The problem with getting rid of the operant-respondent framework is that it has worked reasonably well in the sense that most data in the basic learning literature have been usefully organized around it. Certainly this is the case for many of the data appearing in this journal. To justify getting

rid of the framework, an alternative with accompanying operationally well defined concepts is necessary. If the operant-respondent framework can be considered a "paradigm" in the sense of Kuhn (1970), then such a stance seems supported by the history of science. For example, basing his arguments on the history of the physical sciences, Kuhn stated:

Once a first paradigm through which to view nature has been found, there is no such thing as research in the absence of any paradigm. To reject one paradigm without simultaneously substituting another is to reject science itself. (p.79)

To the question of the scientific response to anomalies in the absence of an alternative paradigm (cf. our earlier discussion of constraints on conditioning principles), Kuhn commented:

There are always difficulties somewhere in the paradigm-nature fit; most of them are set right sooner or later, often by processes that could not have been foreseen. The scientist who pauses to examine every anomaly he notes will seldom get significant work done. (p. 82)

Several alternatives to the operant-respondent framework have been proposed, but there is no indication that any of these currently has comparable organizing power. Until such a paradigm is put forth, therefore, we see modification of the operant-respondent framework, rather than its elimination, as the more fruitful approach.

If the two categories are to be retained for the foreseeable future, with appropriate modifications as dictated by data, the question arises as to whether a third category is required for data that do not fit neatly into either of the categories. A likely candidate for a third category is schedule-induced behavior. Wetherington (1982) made a strong case for numerous similarities between this and respondent behavior, but concluded that it would be inappropriate to classify schedule-induced as respondent behavior. However, given the above com-

ments, Wetherington's points may provide a useful prescription for modifying the respondent category so that it satisfactorily accommodates the data. For example, one might modify the concept of an unconditioned respondent to permit (in certain specified cases) long delays between the unconditioned stimulus and the unconditioned response when the opportunity for the latter to occur is restricted.

A different approach was advocated by Herrnstein (1977a, 1977b), who suggested modifying the concept of the operant to include schedule-induced and other types of phylogenetic behavior. Essentially, the modification he proposed is the assumption that phylogenetic behavior is self-reinforcing. This may seem to be an extreme move to preserve the operant-respondent framework, but Herrnstein considers it preferable to abandoning the framework prematurely. He stated:

My suggestion for avoiding the theoretical burdens of postulating a third category of behavior is: Don't do it. At least, not yet. At this point, the evidence does not require so rash a step. (1977b, p. 1014)

The problem this approach faces is that it requires a redefinition of the concept "reinforcement." It is not clear how Herrnstein's "self-reinforcement" fits the definition of reinforcement given in the introduction of this article.

Before modifying the concepts of operant and respondent, it might be better to determine whether simply adding assumptions would be a more effective way of incorporating schedule-induced behavior into the operant-respondent framework. An example of this approach is provided by Keehn and Stoyanov (1983). After showing that schedule-induced drinking can be modified by drinking-contingent food delays, they speculated that rather than drinking being schedule-induced, it may be a condition that is alleviated by drinking that is schedule-induced. More precisely, such a condition may be an aversive state that is reduced by the polydipsic behavior. In support of this

assumption, they cited the fact that drinking has been shown to increase in the presence of a conditioned aversive stimulus (Gillen & Keehn, 1983).

Topographical Analysis

As indicated earlier, many theorists have abandoned the notion of distinct response systems underlying the two types of conditioning. However, it is possible that there exist fine-grained topographical differences between the two categories of behavior. If this turns out to be the case, classification of a particular type of behavior into a particular category would simply be a matter of making the appropriate topographical measurements of the behavior. Such considerations have motivated the search for topographical differences between operant and respondent behavior (e.g., Schwartz & Williams, 1972), and may continue to do so for some time.

Recent advances in microelectronic technology have greatly expanded aspects of topography that can be examined and quantified. For example, in our laboratory we have developed a behavior tracking system that continuously records the position of an arbitrary point on a pigeon's head in three-dimensional space (Pear, Rector, & Legris, 1982; Pear, in press). Figure 1 shows a block diagram of the system. Figure 2 compares data generated by an autoshaping procedure and by a variable-interval (VI) schedule alternated with another VI schedule and with extinction (EXT). The left column of the figure shows data obtained from autoshaping sessions with two pigeons (Birds 1134 and 5052). These data indicate the distance of the bird's head from the key over time. Note that each bird approached the key rapidly at the onset of the conditioned stimulus (CS), remained close to the key throughout the duration of the CS, and then left the region of the key (and the feeder) after the termination of the feeder interval. The right column of the figure shows data for a bird on multiple VI 60-s VI 60-s and the same bird on multiple VI 60-s EXT. Note that the head movements during multi-

Fig. 1. The behavioral tracking system. Two video cameras are connected to a video-acquisition module that computes the spatial coordinates (x, y, z) of a hypothetical dark point on a pigeon's head as the bird engages in key pecking and other activities in an operant conditioning chamber. The bird's head is painted black so that it can be detected by the video-acquisition module. The coordinate values are obtained at 1/30-s intervals and then used to compute the absolute distance of the bird's head from the key. The resulting data are averaged in groups of three and stored on magnetic disks for later analysis.

ple VI 60-s VI 60-s were quite regular and that the bird moved its head farther from the key than did the birds on autoshaping. However, note that the bird's head was considerably closer to the key during the reinforcement component of multiple VI 60-s EXT. The similarity of these head movements to the autoshaped head movements suggests a possible connection between autoshaping and behavioral contrast. More conclusive evidence of a topographical similarity between autoshaped responses and responses during behavioral contrast might be obtained by increasing the precision of the measurement system.

The topographical approach just described may also be useful in studying schedule-

induced behavior. A possible example can be seen in the portions of the intertrial intervals during autoshaping shown in Figure 2. More complete samples of this behavior are presented in Figure 3 in which the left column shows head movements in terms of distance-from-key over time. The movements during the intertrial intervals were very regular for Bird 1134 and somewhat regular for Bird 5052. The right column of the figure traces the movements of the birds from an overhead perspective during the time periods shown between *a* and *b* in the graphs on the left. As indicated in the figure, both birds typically paced back and forth throughout the intertrial intervals. Bird 5052 occasionally made a loop pattern as well. These patterns are similar to those reported on the basis of direct visual observations by Innis, Simmelhag-Grant, and Staddon (1983).

Further developments of the tracking technology should also permit any specified topography to be shaped with a high degree of precision. This would be useful in confirming or disconfirming data that seem to indicate that certain presumed respondents (e.g., grooming) cannot be operantly conditioned (Shettleworth, 1973). Strong evidence confirming that these topographies are not operantly conditionable may provide a topographical basis for distinguishing respondents from operants—provided, of course, the same patterns can be shown to be respondently conditionable.

Functional Analysis

Even if it proves to be impossible to distinguish operants and respondents on the basis of a topographical analysis, it may still be possible to do so functionally. That is, it may be possible to show that behavior under the control of one type of procedure is functionally different from behavior under the control of the other type of procedure, even though both types of behavior are identical in all other respects.

There are two approaches to a functional analysis of the two types of conditioning. One is to seek different functional relationships, or different laws, relating the two

Fig. 2. Distance from the key over time during autoshaping, multiple VI VI, and multiple VI EXT. The band at the bottom of the autoshaping sessions indicates the CS. The short vertical marks indicate key pecks. The band at the bottom of the multiple-schedule sessions indicates the reinforcement components of multiple VI EXT.

types of behavior to environmental manipulations. Thus far, no obvious differences have been clearly demonstrated between the two types of behavior with regard to such processes as extinction, spontaneous recovery, and stimulus generalization. There is some evidence that partial reinforcement may affect respondents differently from operants (Kimble, 1961, p. 103; Spence, 1966). In addition, Weiss and Dacanay (1982) have presented evidence that the peak-shift phenomenon does not occur with respondents (assuming that auto-shaped key pecks are respondents). However, a systematic parametric comparison of phenomena associated with the two types of conditioning would seem to be an important future undertaking.

The other approach to a functional analysis is to study the joint control of operant and respondent procedures on the same behavior. Impressive starts in this direction

using key pecking by pigeons have been made by Jenkins (1977), Woodruff et al. (1977), and Williams (1981). In this approach, the response-reinforcer and stimulus-reinforcer relationships are manipulated independently. For example, Jenkins manipulated the response-reinforcer contingency by varying the probability of the reinforcer following a response and the stimulus-reinforcer contingency by varying the probability of the reinforcer in the absence of the stimulus. Woodruff et al. manipulated the response-reinforcer contingency by varying the percentage of delayed reinforcers and the stimulus-reinforcer contingency by varying the relative duration of two components of a multiple schedule. Williams manipulated the response-reinforcer contingency by manipulating the contingency between key pecks and the reinforcer, and both contingencies jointly by manipulating the con-

BIRD 1134 AUTOSHAPING

BIRD 5052 AUTOSHAPING

Fig. 3. Behavior patterns during intertrial intervals of autoshaping. The graphs in the left column plot distance of the bird's head from the key over time. The band below each graph indicates the CS. The graphs in the right column are overhead plots of the behavior between *a* and *b* in the graphs on the left. The dotted lines indicate regions of the chamber that were not visible to the cameras for technical reasons.

tendency between key pecks and the duration of the conditioned stimulus. In all three studies, the results indicated that the two types of contingencies have separate effects in maintaining behavior. This implies that the two types of contingencies are distinct, but it is not clear how they interact in determining behavior in standard conditioning situations.

The difficulty of determining the joint contribution of the two contingencies in many situations has caused some writers to

express pessimism regarding the possibility of a complete understanding of how the contingencies interact (e.g., Jenkins, 1977, pp. 47, 60-61). Such pessimism may not be justified. As its data base expands, a science often develops powerful though unforeseen techniques of inference where direct measurement is not possible, and such may be the case here.

Theoretical Unification

Theoretical efforts to unify operant and

respondent conditioning have been present since the distinction was formulated (Gormezano & Tait, 1976). As mentioned, a successful theoretical integration will have to demonstrate a clear isomorphism between the two types of conditioning. In addition, of course, it must correctly predict the phenomena identified with the two types of conditioning (e.g., schedule effects). To date, few theories have been formulated clearly enough to begin to meet either of these criteria for success.

One way in which the two conditioning paradigms might be unified is by showing that one can be "reduced" to the other (i.e., expressed in terms of the other). For example, it might be shown that respondents are in fact operants because they are established and maintained by events that follow them (i.e., reinforcement). This approach has been attempted and has generated a body of literature, but the data are not particularly supportive (Gormezano & Kehoe, 1975). In particular, explicit reinforcement contingencies in certain respondent-conditioning situations do not seem to increase the degree of conditioning, and may decrease it (e.g., Coleman, 1975). However, further formulations of the theory may overcome this and other difficulties.

The converse approach is to show that operants reduce to respondents. This approach must reconcile the occurrence of the reinforcer following the target response in operant conditioning with the occurrence of the unconditioned stimulus prior to the unconditioned response in respondent conditioning. In other words, it must deal with the fact that the unconditioned stimulus/reinforcer appears to occur in radically different temporal relationships with the unconditioned response in the two types of conditioning. An ingenious device for doing this has been suggested by Commons and Armstrong-Roche (1984). They propose that operant conditioning is the result of two instances of respondent conditioning. The first begins when a covert unconditioned stimulus for a response occurs. If this response produces reinforcement, the covert stimulus

is associated with the reinforcer and so becomes a conditioned stimulus. This renders the covert stimulus "salient," thus permitting the occurrence of the second instance of conditioning whereby the response elicited by the covert stimulus is conditioned to the stimulus situation. It is too early to predict how successful this theory will be, but it may well give rise to some interesting research and discussion.

Another approach is to unify the two types of conditioning in a single theory that retains the critical elements of both. An example of this approach is provided by Donahoe, Crowley, Millard, and Stickney (1982), who extended the respondent conditioning model of Rescorla and Wagner (1972) to operant conditioning in an attempt to develop a "unified principle of reinforcement." Again, it is too early to predict how successful this theory will be.

CONCLUSIONS

After a promising historical start, the operant-respondent distinction has not lived up to the high expectations that many psychologists held for it. Clearly, behavior turned out to be more complex than the simple formulations that were made initially. This, however, should not be taken as a failure of the operant-respondent framework. Many powerful scientific concepts started out as oversimplifications of nature, and later were effectively modified to accommodate the complexities revealed by subsequent data.

In our view, it would be premature to abandon the operant-respondent framework. Despite its weaknesses, it has been remarkably successful in organizing a large quantity of data—and no viable competing paradigm has yet been proposed. Our opinion is that research efforts should probably be directed toward modifying the two concepts to account for disparate data. In this endeavor both topographical and functional approaches should prove useful in confirming or disconfirming possible differences and interactions between the two types of behav-

ior and conditioning. Finally, although previous theoretical attempts to unify operants and respondents have lacked precision, largely because their concepts were not well operationalized, future efforts in this direction may prove more satisfactory.

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