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IS THE OPERANT CONTINGENCY ENOUGH FOR A SCIENCE OF PURPOSIVE BEHAVIOR?

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ABSTRACT: The operant contingency remains the most powerful and flexible single technology for the production and control of purposive behavior. The immediate aim of this paper is to examine the conceptual and empirical adequacy of the operant contingency as the basis for a science of purposive behavior. My longer-term goal is to improve the effectiveness of operant contingencies and our understanding of how and why they work. I explore three aspects of the operant contingency: its development as a closed definitional system, its empirical adequacy as a technology, and the appropriateness and usefulness of related theoretical assumptions. I conclude that the efficacy of the operant contingency can be improved further by continued analysis of its implementation, mechanisms, and assumptions and by increasing its links to other approaches and concepts.
Key words: Operant contingency, discriminative stimulus, operant, reinforcer, purposive behavior, definitional system, proper function, niche-related mechanisms, tuning, causal stance

The operant contingency is unquestionably one of the most important technologies for the production and control of behavior to emerge from the twentieth century. B.F. Skinner (1938) developed the operant contingency to produce and define *purposive* behavior (in contrast to phenomena such as reflexes, instinctive behavior, habituation, and reflex conditioning). As Skinner pointed out in an introduction to the reprinting of *The Behavior of Organisms*, "operant behavior is essentially the field of purpose" (Skinner, 1966, p. xi).

Skinner and his students led the way in showing that operant contingencies could be applied in both laboratory and field contexts, to both human and nonhuman animals, and to responses ranging from lever pressing to verbal behavior. With the addition of more complex technologies such as schedules of reinforcement, chaining, and shaping, operant contingencies have been applied in a variety of additional areas, including training of motor and social skills, economic behavior, teaching, and the assessment of drug effects. Most recently, the operant

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contingency has been the centerpiece of attempts to combine operant learning, evolution, and culture in the common framework of selection by consequences (e.g., Hull, Langman, & Glenn, 2001; Skinner, 1966).

What is an operant contingency? It is an experimenter-imposed relation among three codefined concepts (a discriminative stimulus, an operant response, and a reinforcer) that connects them to each other and to an accompanying orderly change in responding. This paper focuses on the conceptual and empirical adequacy of the operant contingency to serve as the basis of a science of purposive behavior. Such a question might seem superfluous given the notable successes of operant contingencies in producing and controlling behavior, but operant conditioning is frequently not as effective as we might expect of a mature technology celebrating its sixty-fifth birthday. It is certainly not as simple to use as textbook descriptions imply or as automatic in its effects as experimenters, practitioners, teachers, and parents would prefer.

Further, the achievement of a successful operant contingency is not the only important goal in the production and control of behavior. Other important aims include increased efficiency of implementing successful contingencies, predicting successful contingencies, understanding the reasons for failure and limitations of operant control, predicting asymptotic performance, and predicting and promoting better conceptual ties between operant conditioning and other approaches to the study of behavior. These goals are not equally facilitated by current procedures and practice.

The long-range purpose of this paper is to improve further our ability to use operant contingencies in the control and analysis of purposive behavior and, as part of this process, to add to our understanding of how these contingencies work at an empirical and theoretical level. My immediate purpose is to consider three aspects of the operant contingency: some ramifications of how Skinner defined it, resultant advantages and limitations on its empirical usefulness, and the importance of considering more carefully the theoretical concepts and stances that have increasingly surrounded its use.

The Operant Contingency as a Definitional System

The operant contingency is central to the field of operant psychology because it serves as the ultimate basis for defining each of the concepts it relates: namely, discriminative stimuli, operants, reinforcers, and reinforcement effects (Skinner, 1938). In other words, the elements of an operant contingency are considered to be present (and can ultimately be identified) only as part of a contingency that produces an orderly change in responding. Thus, the elements of an operant contingency form a closed system in which they are codefined (defined in terms of each other). For example, in the case of a rat in an experimental chamber, a flashing light qualifies as a discriminative stimulus only if it is a part of a contingency in which the presence of the light results in an orderly change in the lever pressing followed by delivery of a food pellet reinforcer. In a similar vein, neither is a lever press an operant nor the food pellet a reinforcer unless they are related by an operant contingency that produces an orderly change in the rate of responding in the presence of the flashing light (Skinner, 1938).

To be sure, most experimenters (including Skinner) have not adhered strictly to the referential limits imposed by this closed definitional system. For example, even before the contingency is implemented, experimenters commonly refer to the flashing light that will be used to signal availability of reward in the experimental chamber as the discriminative stimulus, a food pellet as the reinforcer, and a lever press as the operant. However, strictly speaking this practice is not consistent with Skinner's (1938) codefinitional approach. A more compatible practice would be to label the potential elements of a not-yet-implemented contingency as proto-elements or candidate elements (e.g., a lever press would be a proto-operant, or an operant candidate).

The distinction between proto-elements and contingency-defined elements is important because of the large number of steps separating them. For example, even in the case of a domestic rat in a well-controlled conditioning chamber, not all proto-operants readily become operants. To turn a proto-operant like chewing, grooming, sniffing, or defensive rearing and boxing into a functioning operant often requires a good deal of effort, first in choosing viable response components, then in designing and modifying the apparatus and procedures, and finally in shaping and measuring the operant. I will return to issues of selection and tuning in a subsequent section of this paper that evaluates the empirical adequacy of the operant contingency. In this section I will consider why Skinner may have chosen the codefinitional aspects of the system approach rather than defining stimuli, responses, and reinforcers more independently.

Dewey's Strategy and Some Notes on a Solution

In a review of *The Behavior of Organisms* on the fiftieth anniversary of its publication (Timberlake, 1988), I noted that Skinner's definitional system approach to the operant contingency resembled the technique used by Dewey (1896) to define a reflex. For several reasons Dewey rejected independently specifying the elements of a reflex in favor of treating the reflex as a functional unit, a "coordination" beginning and ending in the environment and formed by evolution and learning. This definition allowed Dewey to emphasize evolution and function in the environment, and it spared him the difficulty of specifying uniquely the elements of a reflex. In retrospect, it appears that Dewey's efforts were more phenomenological than experimental, and he failed to make sufficient contact with either function or evolution to provide a foundation for further research on or conceptual analysis of the reflex.

Because of the formal similarities with Skinner's approach it might be instructive to ask how Dewey might have related reflexes to their control and evolution more effectively. To answer this question let us examine a specific example: the knee-jerk reflex in humans, typically produced by a physician using a rubber hammer to tap the patellar tendon of the crossed leg of a seated patient just below the kneecap. A well-aimed strike typically causes the patient's foot and lower leg to jerk rapidly upward.

There are several ways to clarify control of the knee-jerk reflex. One is to whack away at the knee area of a subject, watching for which strokes produced a reliable, good-sized response. Although this empirical, bootstrap approach is likely to produce results in the hands of a persistent, attentive, and knowledgeable experimenter, we will necessarily be uncertain about the relation of the outcome to potentially important variables such as the type of hammer, the angle, force, and rate of the tap, the knee position, the time of day, the demand characteristics of the test circumstances, and individual differences of the experimenter and patient. An alternative solution of systematic, exhaustive exploration of functional relations between manipulations of potential independent variables and their outcomes is actually very similar. Without more guidance we would need a formidable factorial design that included manipulations such as striking the tendon in different locations and from different angles with differently weighted hammers of different sizes and characteristics that have different force and different interstrike intervals, in addition to social variables such as different experimenter garb, perceived sex, age, socioeconomic status, etc. To add a potential evolutionary dimension to this analysis it would be necessary to repeat the battery of experiments on a variety of other species.

An addition to the techniques of bootstrap empiricism and brute-force factorial exhaustion is to begin by considering the proper function of the reflex (Millikan, 1993). Proper function refers to outcomes generated by mechanisms operating in circumstances similar to those in which they were evolutionarily selected. For example, the proper outcome of an eye-blink reflex is to ward off damage to the cornea (as opposed to, say, to provide a psychologist with a measure of associative conditioning). It is worth noting that the concept of proper function is not limited to reflexes. Consider the case of the Brelands' (1966) raccoon dipping and rubbing a set of coins against the wall of a container before dropping them in. It seems likely that the proper function of the niche-related (evolutionary) mechanisms underlying this "miserly" behavior is not the rubbing of the coins, but washing off the shells of captured crustaceans prior to opening and eating them.

Following a similar line of analysis in the case of the knee-jerk reflex, it seems unlikely that the mechanisms of the reflex evolved to jerk the foot and lower leg rapidly upward when the knee is struck with a rubber hammer. Because of its vigor, reliability, and specificity, it seems more likely that the knee-jerk reflex results from the operation of niche-selected mechanisms relating deformation of the patellar tendon to muscle contractions that facilitate upright locomotion. Thus, when the forward motion of the lower leg is stopped with sufficient abruptness to stimulate the patellar tendon's stretch receptors, they trigger a reflexive extension of the lower leg. These circumstances would likely occur in the niche-related circumstances of stubbing ones toe or running the lower leg into an obstacle while walking briskly or running. Understood in this way, the proper function and niche-related mechanisms associated with the classic knee-jerk reflex are best viewed as related to the suite of righting reflexes exhibited during locomotion in mammals, both bipedal and four-footed.

By combining the concept of proper function with the procedures of experimental analysis, it should be possible to narrow considerably the range of manipulations and measures need to analyze the niche-related mechanisms involved in an outcome. It also should help integrate behavioral outcomes with the evolution and mechanisms of the control circuitry. In the case of the patellar tendon reflex we should be concerned with the extent to which our triggering stimulus (the rubber hammer) resembled the deformation of the tendon produced by running the lower extremity into an obstacle and whether variables such as speed of movement had any effect on the success of the righting reflex.

In sum, Dewey's focus on the reflex unit rather than its elements was potentially useful in coordinating components of a known reflex with their functional circumstances; however, by itself, his closed definitional approach did not help clarify or narrow the critical physical characteristics of the reflex elements, nor did it adequately support investigation of the evolutionary function of the reflex. To use Dewey's codefinitional approach to the elements of a reflex profitably and to predict details of its occurrence and vigor requires guidance from experimental and/or neurophysiological analyses of mechanisms and the establishment of proper function. Drawing on this analysis, in the next section I consider combining the concept of proper function with analysis of the operant contingency.

Skinner's Strategy

The closed definitional system Skinner applied to the operant reflex can be seen as an improved, experimental version of the approach espoused by Dewey, one that focused on experimental manipulations and results, though still with minimal conceptual analysis based on the use of codefinition. As in the case of the reflex, the major advantage of an experimental approach is the information gained from combining bootstrap empiricism of a working example with planned manipulations of the relations among the elements. Thus, to discover the relation of a discriminative stimulus to other elements of a contingency the experimenter could manipulate the presence, type, or intensity of the proto-discriminative stimulus until orderly changes in behavior occurred. By establishing working examples that could be pointed to and manipulated further, Skinner avoided spending time on abstract specification of contingency elements before the effects of a contingency were established. He also avoided time-consuming and sometimes irresolvable disagreements that such specifications might have invited, and he reduced the space of potential experimental manipulations. It is not surprising that Skinner defined an "orderly" change in responding by pointing to examples in the cumulative record of responding rather than setting some threshold or continuous measure.

Skinner's experimental approach resembled that of biologists studying bodily reflexes from an experimental stance. He first developed conditions for producing a reliable learned response (like the lever press) that then could be used as a "test bed" in establishing functional relations between independent elements and outcomes, e.g., the relation between the size of a fixed-ratio schedule and the pattern and rate of lever pressing (e.g., Ferster & Skinner, 1957). By beginning with the phenomenon of interest, Skinner reduced the space of potential manipulations. A difference, though, is that reflex biologists tended to consider both proper function and the independent definition of the elements in terms of nervous connection and control. Skinner, on the other hand, placed all the weight on orderly outcomes to manipulations.

Because Skinner stuck almost entirely with a codefinitional approach, operant work did not encourage investigation of the characteristics of the contingency and its elements that contribute to its initial success. Thus, although we know a successful contingency when we see one, we lack adequate knowledge of the determinants to predict success ahead of time (or the form it will take). In the absence of more guidance, experimenters typically show a good deal of "trial and error" in exploring the characteristics of the proto-elements and the operant contingency that make it successful. As in the case of establishing a useful picture of the knee-jerk reflex, it seems likely that a useful guide through initial bootstrapping and the matrix of factorial designs is to begin with an analysis of proper function as of the operant contingency and its niche-related mechanisms, focusing on the connection of the contingency to other approaches.

It might be argued that operant experimenters and practitioners are satisfied with random exploration of circumstances to discover the conditions for a successful operant contingency. They do not need to relate their work to proper function, niche-related mechanisms, or the potentially relevant concepts and procedures of other approaches. But, as I will illustrate more extensively in the next section, experienced operant conditioners do not randomly search the space of possible contingency elements based on their knowledge of reinforcement principles. Instead, they use effective shortcuts, guesses, and intuitions to produce successful results. In other words, the efficiency with which successful operant contingencies are established depends at least as much on unanalyzed art as on the random or exhaustive analysis of the contingency. It seems likely to me that much of the art of successful operant contingencies connects to implicit evolutionary function and niche-related mechanisms.

The position of this paper is that the role of science is to analyze and systematize art, strategies, and happenstance so they can be used, added to, and modified by others. In this sense, ignoring proper function and niche-related mechanisms relevant to operant contingencies is like assuming that a squad of zealous physicians can best induce and modify the causation of the knee-jerk reflex by randomly choosing from the space of potential manipulations. It seems likely that their efforts would be more effective in producing and using the reflex if they related the knee-jerk to the niche-related mechanisms underlying it (e.g., they might redesign the hammer or the swing) and to their proper evolutionary function (e.g., they might make the test of the patellar reflex more specific to the maintenance of balance). Similarly, in the case of a rat lever pressing for food we might expect a more efficient gain in knowledge of the determinants of a successful operant contingency if we understood the relation of the lever press to the focal mechanisms of manipulation and exploration and to more general mechanisms of foraging bouts and overall feeding patterns.

Some "Side Effects" of Conceptual Isolation

It seems shortsighted to ignore potentially relevant information available from a consideration of proper function, niche-related mechanisms, and evidence from other approaches. Such a strategy tends to isolate operant concepts from further development and unnecessarily limits forging profitable ties with other approaches. One side effect of such a limitation is a good deal of floundering around when we move out of a familiar, intuitively and empirically honed, operant paradigm into less familiar circumstances. Speaking from experience, there is often a good deal of searching around for the best environment, the right elements of the contingency, and the appropriate schedule terms. It is probably no accident that most of the Brelands' examples of misbehavior appear to be related to developing new operant contingencies without considering carefully that niche-related aspects of their subjects' behavior were likely engaged.

In addition to floundering about in new circumstances, several other potential problems are related to the analytic isolation of Skinner's definitional system approach to the operant contingency. Experimenters often uncritically presume the importance of the entire framework of procedures, apparatus, and measures involved in a successful contingency (I speak here from experience). We often carefully (and even ritually) copy pieces of the framework without fully understanding or testing their contribution to the outcome. I am reminded of the surprise that attended Brown and Jenkins' (1968) demonstration that pigeons predicted grain by illuminating a pigeon key just prior to food delivery. Most of us simply assumed that pigeons had to be carefully shaped to peck a key.

As an aside, I do not believe that operant conditioning has yet fully assimilated the ramifications of Brown and Jenkins' (1968) results. These data (see also Hearst & Jenkins, 1974) support the view that the definitional system approach to the elements that make up the operant contingency is incomplete in several important respects. For one thing, the data indicate that well-organized proto-operants are present before the operant contingency is imposed. Such proto-operants not only speak to the possible importance of evolution in creating relevant niche-related mechanisms but to the importance of the current environment in engaging and supporting those mechanisms.

That we can also use Pavlovian contingencies to produce operant-like responses such as key pecking and lever manipulation strongly indicates that operant and Pavlovian contingencies operate on overlapping niche-related substrates. The operant contingency, like the Pavlovian contingency, induces these proto-operants, and it provides a way of modifying their expression. To return to the point, more than 25 years of pigeon key pecking using the definitional system approach to operant contingencies has not carefully considered the organization of the substrate upon which the operant contingency was working.

A third side effect of the isolation produced by the definitional systems approach is the increased tendency to invoke and maintain relatively simple and intuitive concepts of causation with little serious testing of their usefulness or analysis of their clarity. One such intuitive concept is the assumption that proximal presentation of a reinforcer strengthens an operant that it follows closely and differentially. James Allison and I (Timberlake, 1980, 1984; Timberlake & Allison, 1974) showed that contingent presentation of a previously demonstrated reinforcer could increase, decrease, or leave unchanged an operant response depending on whether the current schedule or presentation produced a disequilibrium condition relative to the baseline levels of the operant and reward responses. Thus, a water reward contingent on wheel running could increase the overall rate of wheel running, decrease the overall rate of wheel running, or leave it unaffected depending on the relation of the schedule terms to the baseline levels. Such results have important relevance to the still-popular "Premack Principle" (Premack, 1965) as well as questioning the appropriateness of the strengthening concept in operant conditioning, a point to which I will return.

A fourth side effect of the isolation of the definitional system approach is the absence of well-developed connections between the concepts of operant conditioning and those underlying other views. This contention might seem odd because operant methods have been widely adopted in a large number of areas ranging from drug ingestion to economics to neurophysiology, but the major basis for the widespread success of operant technology was Skinner's genius in designing two remarkably stable and flexible environments for the study of learning and behavior, the rat lever press apparatus and the pigeon key peck box.

Scientists outside the operant tradition have most often viewed Skinner's carefully constructed contingency concepts as a set of arbitrary labels for physical elements and procedures. Thus, a neurophysiologist believes that an "operant" is simply a label for a lever press, a reinforcer is a food pellet, and a discriminative stimulus is a flashing light. As a result, the extensive proliferation of operant technology into new areas of research often has occurred with little understanding of Skinner's carefully constructed system. Interpretations of the results of operant contingencies reflect the concepts of the new area rather than those of operant analysis, as in the case of behavioral economics, optimal foraging, and drug addiction.

In sum, Skinner's definitional system approach to the operant contingency has related its conceptual elements to the world primarily through manipulation of the contingency elements of specific successful examples. This manipulation-centered approach has produced highly successful operant contingencies in a variety of subject areas and circumstances. On the other hand, operant contingencies are not always successful, and new applications often fail initially. The absence of an analysis of the characteristics of successful contingencies and their relation to concepts and approaches such as proper function and niche-related mechanisms makes it difficult to know why.

It can certainly be argued that operant contingencies are successful enough without the extra baggage of analysis and connections that I am suggesting, but in the remainder of this paper I will point out that operant contingencies nearly always involve more than the simple elements of the contingency. Experimenters always bring extra baggage to bear in selecting the elements of a schedule, relating them, and measuring the results. The major issue here is the extent to which this baggage is analyzed and integrated clearly or obscured by a lack of conceptual analysis combined with empirical bootstrapping and functional relations. As I said in the introduction, the question is not whether there are successful examples of operant contingencies—there are. The question is whether we can improve the effectiveness with which we implement such contingencies.

For all its advantages and popularity, the closed definitional system approach to the operant contingency has tended to isolate operant analyses from other approaches and from the systematic development of causal explanation. Skinner's (1938) complaint about scientists more interested in theories than behavior added to that isolation. So did his *Psychological Review* article (Skinner, 1950), "Are Theories of Learning Necessary?," a query he answered soundly in the negative. Although he later relented somewhat (Skinner, 1969, pp. vii-xii) and (as we shall see) a surprising assortment of mini-theories and theoretical stances have sprung up in operant conditioning, there is still a tendency toward mistrust of all but the familiar and intuitively appealing concepts.

Three aspects of the operant contingency appear to have substituted for and inhibited development of theoretical analyses. The first is the largely implicit and untested assumption that the effects of a contingency are completely caused by its elements. This aspect is supported by the correlational evidence that the elements of the contingency are present (by definition) when there is a successful contingency and absent when the contingency is unsuccessful; however, it is worth pointing out that the closed definitional system is actually discretely open and incomplete in that the causal aspects of circumstances that are not captured by the contingency elements are added in the form of setting conditions.

The second aspect of the operant contingency that inhibits the development of theoretical analyses is that the technology can be used as a tool to answer questions posed by other fields of research such as neurophysiology or economics. The drawback to this use of the operant contingency is that it has become less the cornerstone of an analysis of purposive behavior than a tool to be used in the service of the questions of other scientists. If Skinner's vision of a universal approach is to be even partially reclaimed, it is important to develop causal explanation in operant conditioning systematically and seek out and clarify conceptual connections with other approaches to behavior.

Finally, it is possible to argue that the success of operant conditioning in its present form belies the need for any change in Skinner's approach, especially if such a change might involve concepts as amorphous as niche-related mechanisms and proper evolutionary function. However, the success of the operant contingency has not been as rapid, pervasive, or predictable as was expected. There is no question that its precision and control is a marked improvement over folk-tales and common sense, but it has not proved to be the simple empirical and atheoretical approach that it appeared to be. In the next section I will argue that the enduring empirical success of operant techniques actually owes a large (and not properly acknowledged) debt to the skill of operant psychologists in choosing and refining their apparatus and procedures to fit their subjects. In the final section I will consider the equally unacknowledged contribution of theoretical concepts and stances in the use of operant conditioning. By clarifying and consolidating these debts we should be able to provide a more effective approach to operant conditioning.

The Empirical Adequacy of Operant Contingencies

The clear success and apparent empiricism of operant contingencies is a major positive point supporting their use and generality. Without considering intervening variables explicitly, the clever experimenter or practitioner gains control of behavior simply by manipulating the proto-elements of an operant contingency until success is achieved. However, the flexibility of operant contingencies in the hands of a knowledgeable and talented experimenter distracts attention from the difficulties encountered in creating a successful operant contingency in the first place and from the subsequent limitations on the functional relations a contingency can support.

Anyone who believes in the simple empirical adequacy of an operant contingency needs to experience or re-experience the initial difficulties encountered in applying operant procedures in working with a new species, an unfamiliar reward, or a newly designed apparatus. It is apparent that extensive and specific experimenter experience with particular species and proto-elements, along with specific apparatuses and procedures, contributes critically to success in implementing a contingency. The importance of experience can be seen by placing subjects in the hands of an experienced versus a novice trainer, with each trainer equally conversant with textbook information about operant conditioning. The success of the contingency will nearly always be much greater in the hands of the experienced trainer. Similarly, if you take two experienced trainers and present them with a species that is familiar to one and not to the other, the trainer familiar with the species generally will be better at controlling behavior with a contingency. The differences are often most apparent in the experienced trainer's ability to shape or "scaffold" complex behavior, but they also apply to the procedures of simply rewarding a subject with food for a simple operant.

A related variable contributing to establishing successful operant contingencies is individual differences in experimenter talent. A cursory look at the beginnings of operant psychology immediately suggests two clear examples of exceptional talent, Professor Skinner and the Breland-Bailey group. Skinner was remarkable. He repeatedly dealt with new species and new tasks in his work, persisting with amazing success. He trained rats to press magazine flaps and horizontal levers and carry and deposit ball bearings down chimneys. He trained pigeons to peck keys, drive dots across screens, bowl, play ping-pong, and use a mirror to see hidden sections of their bodies. In an analogue of a "cold read" of an unknown person at a carnival, in a single brief session Skinner trained an unfamiliar dog brought by the photographer to jump high against a wall. He even incorporated the photographer's flashbulb going off as a discriminative stimulus (Peterson, 2001). The Breland-Bailey group was similarly successful in custom training animals for commercial exhibits (e.g., Breland & Breland, 1966). For example, they trained pigs to vacuum and deposit tokens, raccoons to play basketball games and pianos and hunt for eggs or crawfish, and chickens to dance, play baseball, and eliminate defective drug capsules.

So how do experience and individual talent contribute to successful applications of operant contingencies? In his well-known essay on doing science, Skinner (1956) credited his successes in training to personal laziness. In contrast, the Breland-Bailey group, perhaps especially in dealing with unsuccessful contingencies (Breland & Breland, 1961, 1966), paid explicit attention to how preorganized (instinctive) elements of behavior contributed to the success and failure of an imposed contingency. I think it is likely that Skinner inductively paid attention to the same elements in training. What we can conclude with certainty is that both Skinner and the Breland-Bailey group spent considerable time selecting behaviors and designing and tuning their apparatuses and procedures to establish successful contingencies (see Timberlake, 1990, 1997, 2001, 2002). I will consider these types of contribution below.

Tuning of Apparatus

A close reading of Skinner (1938) supplemented with other accounts (Skinner, 1956, 1958, 1961, 1977) reveals an investigator deeply sensitive to the behavior of his subjects. On close inspection, Skinner was enormously productive as an apparatus designer, his output seemingly on par with the outpouring of inventors like Alexander Graham Bell. It is not mere productivity that is the key, but the cleverness with which he carefully altered the apparatus to produce and control the operant response. In describing the laboratory devices he invented, Skinner says, "But note that the apparatus was designed by the organism we study, for it was the organism which led us to choose a particular manipulandum, particular categories of stimulation, particular modes of reinforcement. . .and to record particular aspects of its behavior" (1961, p. 543).

For example, to increase the likelihood of getting a rat to press the bar, Skinner modified the location of the bar, its height, shape, and insertion point. He even placed screening above the lever to discourage rearing above the bar. In another instance Skinner developed an apparatus for the rat Pliny to demonstrate its capability to press a lever for tokens (secondary reinforcers) that it would then exchange for food. Pliny did well in pressing for the token (a marble) and carrying it to a floor chute to deposit, but he ran into considerable difficulty in releasing the marble cleanly, without attempting to retrieve it again. Skinner solved the problem by building a chimney over the chute to prevent Pliny from visually tracking and lunging after the marble as it disappeared.

Another example of tuning, this one away from rather than toward preorganized perceptual-motor organization, occurred in my own research using periodic presentations of rolling ball bearings that were followed by food when the bearing exited the chamber (Timberlake, Wahl, & King, 1982). Rats displayed a baseline interest in interacting with the rolling bearing, but in our initial attempts to use the bearing as a signal for the delivery of food pellets we subverted this interaction by rolling the bearing away from the tray where food was delivered. When we reversed the direction by rolling the bearings toward the food tray, the animals greatly increased their attention when the bearing predicted food.

Finally, a major aspect of tuning involves creating environmental support for the behavior of interest (e.g., Tolman, 1932, p. 85). For example, illuminating the raised grain hopper is important in maintaining effective food reward in pigeons. Even a well-trained and comfortable pigeon hesitates to peck into a dark hole. In contrast, suddenly illuminating a food well for a rat is much less helpful in encouraging them to find the pellet. If anything, they are less inclined to search out food than if the food tray remains dark.

Tuning of Procedures

Tuning a procedure is similar to tuning an apparatus. It involves changing aspects of the operant contingency to produce better control of the target behavior. A good example of procedural tuning occurred in the study of the "matching law," in which pigeons match the ratio of response rates to obtained reinforcement rates on two concurrently available VI (variable–interval) schedules. Each schedule was associated with pecking one of two "keys" (manipulanda), one on either side of a food hopper. To obtain reliable matching it is important to insert a "changeover delay" in the availability of grain following a shift between pecking the two keys.

If the changeover delay is too short, the pigeons tend to "undermatch" by simply alternating pecking the two keys independently of the schedule values. If the changeover delay is too long, the pigeons tend to "overmatch" by spending a disproportionate time on the side with the highest rate of food delivery (e.g., Dreyfus, DePorto-Callan, & Pseillo, 1993). Thus, if experimenters want to study the matching of response to reinforcer rates, they must tune the changeover delay to allow that balance to occur. However, notice that doing this tuning isolates a single portion of what appears to be a more continuous functional relation ranging from simply alternating proximate patches to staying longer in the "richer" of more distant patches, a relation shown in a variety of foraging circumstances (Stephens & Krebs, 1986). It would be interesting to know what the requisite changeover delay thresholds were to get matching in birds that have different foraging patterns.

Tuning a procedure often requires at least implicit knowledge of how regulatory processes vary over time. Treating a food deprivation schedule as a setting condition is acceptable if the rate and cost of food availability stays within a portion of the regulatory curve in which the trade-off between the cost of lever pressing and the benefit of the food obtained is relatively constant. However, if the experimenter further manipulates variables such as the requirements of the schedule, the amount of each reinforcer, and the length of the session, knowledge of the regulatory curves relating lever pressing and eating becomes important in predicting the success of a particular contingency and the functional relations it shows with changes in the value of one or more of its elements. Similarly, if the experimenter focuses on measures that highlight local changes in the response rate, imposes a closed economy, or uses a species that has too high a metabolic rate to be limited to a single daily feeding, an understanding of the regulatory issues involved is again important in controlling and accounting for the results (e.g., see McSweeney & Swindell, 1999; Staddon, 2001; Timberlake, 1984).

To clarify further the complexity of the motivational processes present in an operant contingency, note that the regulatory effects of food ingestion can vary with the time of day. For example, Jan Strubbe and his coworkers (Strubbe et al., 1986) showed that the effects on lever pressing for food pellets produced by slowly infusing liquid food into a rat's stomach varied with the stage of the rat's active period. During the first half of the night, the animal compensated for the infused food by proportionally decreasing the amount of lever pressing for pellets. However, in the second half of the night, the infusion of food had no effect on lever pressing. Apparently, the regulation of feeding differs as rats approach their long daytime fast.

Shaping

Shaping (the rewarding of successively closer approximations to an operant criterion) marks an important addition to tuning the apparatus and procedure. Many of us have treated the technique of shaping successive approximations to a target response as present from the beginning of operant conditioning. In fact, the procedure of shaping response approximations did not arrive on the scene until 1943 when Skinner and his Minnesota group began working on Project Pelican (Skinner, 1958). Gail Peterson (2000) has written a delightful article clarifying the transition of Skinner's approach from his initial focus on controlling behavior by means of tuning the apparatus and procedure to the controlling of a more-or-less freely behaving organism by delivering food (or cues that predict food) contingent on a topographic or environmental criterion (Skinner, 1958).

Skinner, along with graduate students Norman Guttman and Keller Breland, first implemented shaping by training a pigeon to "bowl" by using a beak swipe to send a small ball down a miniature alley toward a set of miniature pins. Several years later Skinner unveiled shaping to the world in a one-person press conference for a *LOOK* magazine photographer. During the press conference he trained a dog belonging to the photographer to jump high against a wall by rewarding it for successively putting its nose above a series of horizontal lines taped on the wall (Peterson, 2001). Peterson's account aptly emphasized the clever things Skinner did to allow a photographic session and shaping to coexist, one being to establish the flashbulb going off as a secondary reinforcer, and another, the perhaps inadvertent creation of a target for the dog in the form of the tape lines he placed on the wall to allow him to impose successively more difficult response criteria.

The Brelands participated significantly in the development of applications of operant conditioning and rapidly saw the potential of shaping for training animals for commercial purposes. They founded a company to train animals for exhibits using flexible portable targets and secondary reinforcers. Subsequently, talented animal trainers such as Karen Pryor (1975) have used a variety of creative shaping procedures to train animals in captive settings.

Proto-Elements and Shaping Pathways

The combined power of tuning apparatuses and procedures and shaping responding is marvelous, strongly supporting the view that operant technology is extremely flexible. However, even a cursory consideration of history calls attention to a much-overlooked and critical contributor to the success of an operant contingency, namely the choice of candidate elements—proto-operants, proto-discriminative stimuli, and proto-reinforcers, that eventually, after appropriate tuning and shaping, constitute the elements of a successful operant contingency. In other words, we are missing the rules for choosing the proto-elements with which to begin.

We are also missing rules for efficient shaping. Many experimenters have worked out their own ways to shape lever pressing and/or key pecking, but although there is more than one shaping pathway to produce a response, not all pathways work equally well. Even after an experimenter works out environmental factors that influence and support particular proto-elements of the contingency, there are still choices of postures and response sequences that make shaping easier. For example, in shaping a chain of responses there is no generally correct direction. The experimenter can start with the final link or the first link; the experimenter can assemble units and repeat them or "glue" them together. Some choices work better in a forward direction, some in a backward direction.

B.F. Skinner was clearly a past master at selecting proto-elements for an operant contingency and figuring out shaping pathways for assembling them. His abilities were demonstrated throughout his career. Perhaps nowhere was it more apparent than in his selection and tuning of lever pressing in rats and key pecking in pigeons, the two completely dominant paradigms for operant research in the twentieth century. His demonstrations of how to shape pigeons to bowl, play ping-pong, guide missiles, "communicate," and inspect themselves in a mirror are in the same category. Inexplicably, for a man of many words, and even when asked point blank, Skinner never went beyond his alleged laziness in explaining the reasons for his choices of the proto-elements of a contingency, nor his methods of shaping (Skinner, 1956; personal communication, November, 1976; remarks at APA symposium on *The Behavior of Organisms at Fifty*, Atlanta, GA, 1988). As a result of his reticence, we missed what would have been an illuminating account of the attentional and interactive skills used by one of the great practitioners of shaping. We are regrettably left on our own to infer and deduce many aspects of his methods.

As noted above, we know from Skinner's writings that he paid careful, continued attention to how the design of the apparatus, the selection of the operant manipulandum, and his experimental procedures fit with his subjects and the results he wanted. I think we can infer that one important criterion was to choose proto-elements that reflected the preorganized repertoire of the animal relevant to the reward he was using. I do not mean this in the trivial sense of not asking a rat to fly, but in the sense of asking a rat to manipulate a moveable target in close proximity, temporally and spatially, to the location of food. The assumption is that a food-deprived animal will tend to treat an operant contingency involving food reward as a foraging opportunity, meaning that the repertoire of stimulus sensitivities and response components primed by food deprivation will most likely relate to appetitive feeding behaviors organized by evolution and experience.

By developing an awareness of such perceptual–motor organization through a combination of observation of foraging animals and the inductive noting of the reactivity of the animal to stimuli in the experimental situation, it would be possible for a person of Skinner's talent to select and support proto-elements that would be easy to condition (see Timberlake & Silva, 1994, for a more complete description of how one might use observation). In essence, he would be inferring the niche-related mechanisms available for controlling behavior. Several examples in the literature point to the importance of such preorganization in conditioning responses; for example, Shettleworth's (1975) work food-rewarding different responses in hamsters, and the Neuringers' (1974) training of hand-reared squab to peck a disk for food by demonstrating "pecking" at the disk with a finger. Similarly, Stokes and Balsam (1991), in painstaking studies of the origins of rat bar-pressing, documented that shaping changed the frequency of the components of bar interaction, but the basic components were present prior to shaping.

It is worth noting again that failure to recognize the importance of such niche-related mechanisms and the cues that control them most likely underlies the phenomenon of "misbehavior" (Breland & Breland, 1961, 1966). Misbehavior refers to a response that unexpectedly emerges in the midst of training another response that typically supercedes the response that had been so carefully shaped. For example, in training a pig to deposit tokens in a "bank," the Brelands began to lengthen the schedule. The pig responded by repeatedly dropping, picking up, dropping, and rooting the coin in a length cycle.

Both the Brelands (1966) and Skinner (1977) recognized misbehavior as related to the subject's "instinctive repertoire"; in the case of pigs, rooting is normally employed in the search for proximate food. To the detriment of the field, misbehavior has been treated primarily as an oddity rather than as a reflection of the same basic processes that underlie the choice of good proto-operants matched to the contingency by tuning of the apparatuses and procedures. Consider that rather than requiring their pig to pick up and deposit a token for food the Brelands could have taken advantage of the preorganized perceptual–motor organization of the pig and imposed a contingency that required the pig to root the token. I strongly suspect this training could have gone rapidly to stable behavior.

As a perhaps less obvious example, consider the difficulty of imposing an omission contingency on autoshaped key pecking in pigeons (under an omission contingency, reward is omitted rather than delivered contingent on the occurrence of an operant response). Instead of tracking the operant contingency and avoiding pecking, the pigeon misbehaves by continuing to peck the lighted key, thereby losing considerable rewards. This outcome occurs even if the omission contingency on pecking is imposed from the beginning of training (Hearst & Jenkins, 1974).

There is a tendency to view the omission effect as the result of conflict between two different conditioning contingencies, the respondent contingency (the pairing of the key light with food), and the operant contingency (omitting food during any key peck during the light). This is certainly one way to look at it, but consider that the conflict is because of the animals' preorganized repertoire, not the experimenter's procedures per se. The experimenter could present a white noise and a key light for 10 seconds simultaneously and reward the animal for turning in the presence of the tone as long as it did not peck the key. This would be a difficult contingency to learn or to continue to perform, even if the turning was trained first. In contrast, the experimenter could use the same stimulus presentation schedule but reward the pigeon for pecking the key in the presence of the keylight as long as the animal did not turn in the presence of the tone. This would likely be much easier. The point to be drawn from this thought experiment is that the difficulty is at the level of preorganization, not at the level of respondent versus operant contingencies.

Similar difficulties arise in gaining operant control of responding under contingencies such as differential reinforcement of other responses (DRO) and differential reinforcement of low-rate responding (DRL). Neither of these schedules is compatible with the tendency of rats and pigeons manipulate target objects more frequently as the delivery of food becomes temporally proximate or more frequent overall. Viewed in this way, the difference between misbehavior and effective operant responding appears to lie in the experimenter's chosen combination of proto-operant and contingency relation. Such a view also suggests that a common set of niche-related mechanisms underlie both successful and unsuccessful operant contingencies.

In short, at an empirical level, an operant contingency can be a practical guide and a powerful tool for the control of behavior, but simply imposing a contingency relation among possible discriminative stimuli, operants, and reinforcers is not enough to guarantee successful operant control. Tuning of the apparatus and procedure, the choice of proto-elements, the shaping of behavior, and the selection of contingency terms are critical contributors to producing reliable orderly change. Further, neither the choice of proto-elements, tuning, or shaping represent a random search of the space of possible apparatus configurations, procedures, and elements. The key to tuning is to focus on the apparatus configurations and procedures that support and promote niche-related proto-elements that are compatible with the motivational state, perceptual–motor organization, environmental support, and schedule. Effective shaping requires the same focus on choice of proto-elements, environmental support, and pathways of shaping.

In all cases, the more the experimenter knows about the subject's niche-related perceptual–motor organization and motivational processes, the more rapidly and effectively the operant contingency can be implemented. Modifying apparatuses and procedures and shaping behavior represent a two-way interaction between the experimenter and the subject, a sort of negotiation between the experimenter's support and criteria and the organism's repertoire of mechanisms. As in any negotiation, the experimenter often finds it important to alter his or her initial position to produce a successful outcome.

Some readers will find this commonsensical, if not trivial, although such a reader would be one up on many of us who came to such conclusions only after frustrating interludes of trying to explain outcomes that did not appear to make sense until we ventured an explicit analysis of the subject's niche-related repertoire and the environmental support available for it. The important question addressed here, though, is not whether everyone has come to the knowledge briefly reviewed above but whether it is possible to facilitate use of the operant contingency by explicitly recognizing and formalizing our knowledge of the subject's characteristics and evolutionary niche. If that answer is yes, then it is appropriate to accumulate and integrate this information and formalize its critical contribution to the operant contingency in some sort of model or general approach.

Mini-Theories, Stances, and Operant Conditioning

Given the apparent atheoretical nature of operant conditioning, it might seem paradoxical to speak of theoretical concepts involved in operant conditioning. However, in science, as in life, both reliable successes and unexpected failures tend to attract attempts at explanation. In operant conditioning, the tension between the limitations imposed by the definitional system approach and the desire for causal explanations of the success and failure of operant contingencies has led to the use of modest "mini"-theories that explain a particular outcome and to more general theoretical "stances" that are used to explain and integrate a variety of results. I will consider the mini-theories first.

Mini-Theories

As a rule, the mini-theories sprinkled throughout operant psychology tend to be viewed as descriptive labels and/or simple common sense. In this way they have passed largely unchallenged despite a resistance to theories in operant conditioning beginning with Skinner taking to task scientists who were more interested in theories than data. Examples of common-sense descriptive labels include ratio "strain" used by Ferster and Skinner (1957) to describe and intuitively account for why less orderly behavior appears when a fixed ratio requirement is increased rapidly beyond some point. Although we can identify with common-sense notions the "strain" of responding at a high rate, I assume the difficulties are evident in using such descriptive intuitions as a form of explanation. If no causal explanation is intended, the effects should be described with more neutral terms such as "increased variability in response rate" or the like.

The implicit use of mini-theories extends to food deprivation as a setting condition or "establishing operation" (Michael, 1982) that is required for an operant contingency to succeed. The setting condition of deprivation is not codefined with the contingency elements of stimulus, operant, and reinforcer; instead it is defined in terms of hours without food or percentage of free-feeding body weight. The mini-theory reason typically given for the importance of deprivation is that it increases the attractiveness or "value" of the reinforcer. The intuitive reasoning behind this assumption is that attractiveness of a reward is a necessary condition for a learned change in behavior. This latter premise creates difficulties in dealing with permanent changes in learned performance in the absence of obvious rewards. For example, Timberlake and White (1990) found that rats would increase their efficiency and rate of traversing all the arms of a radial arm maze in the absence of food on the maze.

In addition, in this mini deprivation-value theory, the attractiveness of a reinforcer is usually assumed to increase monotonically with deprivation. The difficulties with this assumption can be seen by examining the behavior of an animal living in a 24-hour closed economy. Neither hours of deprivation nor percentage body weight are good predictors of when and how much the animal will eat under free access. For one reason, the day/night cycle typically controls the time of feeding to a remarkable extent. For another, the relation between amount eaten and hours of self-imposed deprivation preceding or following a meal is ambiguous (e.g., Collier, 1983). Further, a single large meal can entrain a burst of general activity 24 hours later that is largely independent of the rest–activity cycle or the possibility of temporal conditioning (e.g., Bolles, 1975; Mistlberger, 1994; White & Timberlake, 1999).

These complexities suggest the importance of prediction for developing the local deprivation theory into a full-fledged empirical and conceptual account of the response evoking and regulatory processes involved in feeding. The deprivation manipulations of the experimenter could be placed in the same framework as within session changes in response rate and choice, response-evoking qualities of sweet and salty substances, and potential differences in the effects of open and closed economies (e.g., Collier, 1983; Timberlake & Peden, 1987). It also seems appropriate to integrate such an account of deprivation effects with foraging models (e.g., Collier, 1983; Staddon, 2001; Stephens & Krebs, 1986).

Several mini-theories related to operant contingencies seem well on their way to theory status and would best be recognized as such to encourage their testing. The concept of reinforcement value introduced above to explain the necessity of food deprivation as a setting condition has been invoked to deal with the effects of amount and delay of reward. The basic assumption is that choice between two alternative operant contingencies is a function of the value of the reinforcer obtained for each choice. Value is an increasing function of the amount of reward discounted by a hyperbolic function of the delay to reward (e.g., Ainslie &

Haslam, 1992; Rachlin, Brown, & Cross, 2000). Based on this assumption, it is possible to predict that a small, proximate reinforcer will be chosen over a large, distant reinforcer. Further, this preference can be reversed by adding a sufficient amount of time equally to the two choices or by inserting a commitment procedure, in which the subject "locks" in its choice at a temporal distance from the initiation of the schedule. It is worth noting that the term "commitment" has mini-theoretic qualities in that it serves to explain the result as well as label the procedure.

A second mini-theory that has moved toward theory status involves the concept of behavioral momentum (Nevin & Grace, 2000). This mini-theory was originally developed to deal with instances in which levels of operant responding under different schedules are not monotonically related to the outcome of changing that schedule by manipulations such as satiating the subject or changing the contingency. Although the concept is empirically specified in terms of the ratio of changes in response rate to changes in reinforcement rate, the notion of momentum implies a causal variable potentially viewed as at least a partial alternative to the reflex reserve concept that Skinner introduced and abandoned early in his career (Skinner, 1938; Timberlake, 1988). In a sense, behavioral momentum attempts to account for circumstances in which the reflex reserve concept does not apply, namely when the level of responding after a schedule change does not reflect the level of responding before the change.

In short, there are increasing numbers of general mini-theories and intervening variables spread throughout operant conditioning. I think the presence of mini-theories should be welcome. The time seems ripe to acknowledge and test the conceptual underpinnings of these mini-theories so that experimenters can make more rapid progress in their analyses. Given the widespread use of value as a causal variable, it seems particularly appropriate and worthwhile to further develop and analyze the concepts of reinforcer value and behavioral momentum, perhaps by relating them to regulatory theories such as response disequilibrium and arousal/memory theories such as mathematical principles of reinforcement (Killeen, 1994).

Theoretical Stances

In addition to the mini-theories that have made their way into interpretations and predictions of the results of operant contingencies, several general theoretical orientations have also exerted considerable influence on procedures and interpretations. Following Dennett's (1977) usage, I will refer to these theoretical orientations as "stances." They include *Predictive (Bayesian) Empiricism*, *Causal Strengthening*, *Selection by Consequences*, and *Reinforcement History*. While not explicitly developed as theories, they provide at least intuitive grounds for the causal basis of a successful operant contingency and speak at least loosely to their effects. As such, they have on occasion tended to block further theoretical development of causal explanations of the operant contingency.

Predictive (Bayesian) Empiricism. This stance refers to the role that a researcher's expectations play in the performance and outcome of research. These expectations guide and direct the design and tuning of the apparatuses and procedures as well as influence how the resultant data are analyzed and interpreted. When expectations are confirmed, experimenters increase their certainty (the weight of their prior odds on a particular outcome). When expectations are not confirmed, the prior probabilities are usually revised a bit, but their revision is often accompanied by considerable effort put into redesigning and re-tuning apparatuses and procedures before running the experiment again.

The effects of Predictive Empiricism are clearly a mixed blessing. It promotes persistence, which might pay off in good data, but, much like a theory, it tends to distort the performance and interpretation of research in the direction of our prior expectations. There is a special difficulty in operant contingencies because there is a tendency to overlook the codefinitional aspects of the contingency elements. As a result, we often expect each element of a successful contingency to independently produce the same outcome again. For example, a light that controls a treadle press response for food in pigeons is expected to do equally well in the case of controlling a shock avoidance response (Foree & LoLordo, 1973). Likewise, a food reinforcer for lever pressing should reinforce other responses such as mounting a model female, retrieving pups, or grooming, and its effects should be independent of the presence of a disequilibrium schedule (Timberlake, 1980).

That many expectations about contingencies fail, and others require artful tuning and shaping for the contingency to succeed, casts doubt on the adequacy of a Predictive Empiricism stance to produce consistently effective operant contingencies. Unquestionably, information about the organism is contained in a successful contingency, and barring further knowledge and the absence of more explicit theories, a predictive empirical stance is a common-sense place to start. However, a straight element-based stance of Predictive Empiricism is arguably not the most effective means of discovering the useful information present in a successful contingency. More information can usually be gained by a careful analysis of the necessary and sufficient conditions that underlie a successful operant contingency. The operant contingency is sufficiently complex that the individual elements of a successful contingency cannot be expected to independently produce further successful outcomes.

Causal Strengthening. Since at least the time of Thorndike (1911), the stance that learning is a form of strengthening that causes subsequent behavior has pervaded thinking and experiments in the study of behavior. Despite careful adherence to the neutral language of probability in definitions of reinforcers (e.g., “A positive reinforcer is defined as any consequence that increases the probability of the operant that produced it.” [Pierce & Epling, 1999, p. 92]), operant conditioning has not escaped the influence of a strengthening stance (Timberlake & Allison, 1974; Moxley, 1999). If a behavior followed by a reinforcer increases in frequency or rate, the assumption that the reinforcer strengthened the behavior is common in subject areas ranging from the study of lever pressing in rats to skill learning and token economies in humans.

The line between the relatively neutral assertion that an operant contingency changes the probability of an operant followed by a reinforcer in the presence of a setting stimulus (or the more recent view that an operant contingency "selects" the operant) and the theoretical stance that a reinforcer *strengthens* the operant is a subtle one that is crossed, recrossed, and often obscured in the same article. Moxley (1999) reviewed considerable evidence from Skinner's writings across the years that Skinner espoused such a strengthening (necessitarian) stance, primarily (but certainly not exclusively) in the first part of his career.

It is important to note that a Causal Strengthening stance abandons the codefinitional approach to the operant contingency in favor of the assumption that a previously demonstrated reinforcer necessarily strengthens an operant it closely follows. An advantage of this view is that the proto-elements of the contingency and the contingency relation do not have to be chosen ahead of time. In the simplest form, one can simply assume that changes in responses that emerge from a contingency are the result of strengthening (except for the standard disavowals of motivational effects). The complementary disadvantage, of course, is that the Causal Strengthening stance has fewer specified elements, and it is thus even less constrained and potentially more difficult to test. It also shares with the Predictive Empirical stance an inability to predict or explain why some operant contingencies are successful and others are not, or why individual reinforcers (strengtheners) are inconsistent in their effects.

A particularly clear use of the Causal Strengthening stance is Skinner's (1948) explanation of the "superstitious" behavior in pigeons that emerges when food is repeatedly presented every 15 seconds independent of responding. Because there is no experimenter-designated operant contingency, and, with the exception of the candidate reinforcer, no proto-elements in the contingency, there is no way to use Skinner's definitional system approach to codefine the elements of the contingency. Skinner (1948) surmounted this difficulty by changing his focus to temporal contiguity of the reward and providing an intuitive explanation by labeling the resulting behavior as "superstitious," thereby appealing in essence to an intervening causal "strengthening" variable.

Briefly, Skinner's (1948) account is that superstitious behavior begins with an accidental temporal juxtaposition of food with the perhaps-to-be-superstitious-behavior, a juxtaposition that increases the momentary probability of that response. This increased probability is assumed to persist long enough to increase the likelihood that a subsequent delivery of food will also occur in juxtaposition with the developing superstitious behavior, thereby further increasing its probability and making it even more likely that a subsequent food delivery will occur in conjunction with it, and so on. Once started, this feed-forward cycle in which the accidental increase in probability of a response occurring through temporal proximity of the reinforcer increases its likelihood of a further increasing in probability by additional accidental pairings is assumed to continue. The outcome should be a high asymptotic level of the "superstitious" response.

This account is clearly theoretical and not in keeping with Skinner's original codefinitional approach. Skinner specifies the reinforcer ahead of time and presumes that its effects are attributable solely to proximity and frequency. This is the province of standard learning theories. Although for the most part Skinner preserved his verbal behavior of referring to the change in a response only as an increase in its probability (as opposed to calling it strengthening), it is almost impossible not to view his hypothesized bursts and extinctions of responding related to the proximity and frequency of the reinforcer as strengthening in the tradition of Hull or Thorndike.

Interestingly, in the more than fifty years since Skinner (1948) proposed his theory of superstitious behavior there has been no direct observational test of his account and only a few indirect tests (e.g., see Timberlake & Lucas, 1985). Given a strengthening explanation of superstitious behavior, it is important to show that the eventually dominant superstitious response was the one most frequently "contacted" by the reinforcer. This test has yet to be made, but a positive outcome appears to be unlikely based on the considerable evidence that the emergence of reliable food-related responses occurred across the interval between food deliveries.

Both Staddon and Simmelhag (1971) and Timberlake and Lucas (1985) reported that reliable and functionally interpretable (rather than arbitrary) behaviors appeared under the superstition procedure in pigeons. Further, Timberlake and Lucas (1985) provided examples in which increasing the probability of an operant prior to transition to the response-independent delivery of food had little effect on the behavior that subsequently emerged. The failure of the initial probability of an important proto-operant to affect its likelihood of occurrence in the presence of food casts doubt on the probability-dependent explanation offered by Skinner. A more direct test would be preferable.

An interesting question is why Skinner was willing to invoke the intervening variable of superstitious strengthening, an account that seems to go against his previous carefully codefinitional approach to the effects of contingencies. A possible explanation is that this step toward a causal theory was supported by his experience with shaping. In the phenomenon of shaping, the behavior of the animal often appears to be strongly affected by the proximity and frequency of reward. The idea of reinforcing successive approximations to a target response class requires designation of the reinforcer and the operant ahead of time; they are no longer proto-elements of a response contingency. They must be designated ahead of time, even if the result of the operant contingency is not successful. After this step toward independent definition (and away from codefinition) it was probably a smaller step to scrap the experimenter-controlled contingency and go with the independently defined reinforcer and its causal effect.

A reviewer of this paper argued that the superstition account actually marked a complete intentional shift in Skinner's conception of the operant contingency from the closed definitional system approach to a strengthening approach based on temporal proximity to a reward. I agree that a shift occurred in Skinner's willingness to infer the existence of controlling contingencies in the behavior of human and nonhuman animals, as opposed to actually imposing them or providing independent evidence of their existence. I do not think, however, that Skinner changed his view of the laboratory-imposed operant contingency to one of strengthening based only on proximity and frequency of reinforcer. This not only would have aligned him with the traditional theories of learning he rejected in 1938 and even more explicitly in 1950, including Hull and Thorndike (Skinner, 1938, 1950), but he no longer would have had a reason to avoid developing a more traditional causal intervening variable approach to the study of learning. Perhaps what Skinner did instead was move through a Causal Strengthening view of superstition toward a more inclusive theory of Selection by Consequences.

Selection by Consequences. The Selection by Consequences stance is represented in Skinner's (1966) attempt to integrate operant conditioning with the reproduction-based evolution of organisms and the importance of culture. According to Skinner (1966, 1975), all three phenomena share a unique universal causal "mode," namely modification of the future expression of a "response" by a consequence that follows it. The response is either a gene-based characteristic in the case of evolution, an operant in the case of conditioning, or a belief or set of mores in the case of culture. Similarly, the consequence in the case of evolution is the survival and reproduction of individuals, in operant contingencies it is the occurrence of a reinforcer, and in the case of culture it is the survival of beliefs and mores.

I think that unification of such disparate and important phenomena based on the action of the single mechanism of selection by consequences is appealing on the grounds that it reduces the number of mechanisms and variables in our causal universe. I agree with Moxley (1999) that Selection by Consequences is a more probabilistic and flexible explanation of operant conditioning than the necessitarian strengthening view or Predictive Empiricism. It is also of interest that Skinner added evolution to the domain of learning, a domain that had already been expanded to include culture at Yale's Institute for Human Relations. However, I think the jury is still out on several issues concerning the usefulness of Skinner's universal causal mode of Selection by Consequences.

First, the relation between these phenomena is made at the level of analogy rather than on the basis of a specific identical biological mechanism operating at each level (e.g., there is no conditioning level mechanism at the level of genes or even in the behavior of sexual reproduction for most species). Second, how powerful and useful the analogy is remains to be seen. Hull, Langman, and Glenn (2001) took a cautious approach in evaluating whether the same terms could be applied across this range of phenomena. Their conclusion is close to mine in that they were uncertain regarding how far the analogy could be pressed usefully, especially because it was not clear how related the underlying processes were.

For example, it might be easy to see an analogy between operant shaping of a lever press and hypothesized selection on the migration of generations of sea turtles based on their ability to swim the incrementally increasing distance to their breeding grounds as the underlying tectonic plates separated (Skinner, 1975), but such an easy parallel to shaping is the exception rather than the rule in evolution. Many evolutionary changes are more sudden and/or difficult to see as slow and systematic (e.g., the appearance of the three-chambered heart, the "explosion" of body plans in the Cambrian). Further, trying to extend the analogy of selection to account for reliable behavioral phenomena such as contrast, secondary reinforcement, schedule effects, or misbehavior appears awkward. Temporal and spatial distance are basic dimensions in operant conditioning, whereas genes are critical in evolution and social interaction is critical in culture.

All in all, there appears to be insufficient agreement about the parallels between operant contingencies, evolution, and culture to grant the analogy a central position in behavioral theory. Such causal analogies seem by nature much easier to make than to evaluate. The lowest level of evaluation is whether the analogy is productive. Is an operant theory of selection by consequences an improvement over the previous empirical and strengthening stances? On the positive side, selection by consequences does add a causal mechanism to the Predictive Empiricism stance and it substitutes explicit terms describing the result of an operant contingency for the implicit presumption of Causal Strengthening. This can be heuristic in suggesting experiments that use reinforcement schedules to select for different frequencies of response sequences or inter-response times (e.g., Blough, 1963; Machado, 1992).

However, even at the heuristic level it remains unclear how Selection by Consequences improves our ability to predict what components of behavior we can and cannot alter easily with a contingency or (at least as yet) the circumstances under which phenomena such as contrast, schedule effects, adjunctive behavior, and misbehavior will occur. The heuristic value of operant conditioning phenomena or paradigms for evolutionary effects appears to be even less clear. Do contrast and misbehavior have meaningful analogues in evolution? Further, the generation-dependent selection by consequence "loops" that occur in evolution, although much more extended in time than the tight temporal selection loops pointed to in operant conditioning, are surprisingly clearer and better differentiated. In the reverse direction, the analogy provides operant experimenters with the intuitive notion of populations of potential operant components, but it does not help us understand or predict what they are or to what extent they will be incorporated in successful operant contingencies.

In short, the analogy of Selection by Consequences supports the idea that behavior can be shaped from a repertoire of elements, but it does not explain or predict interactions among discriminative stimuli, reinforcers and operants, nor does it speak directly to critical issues, like the setting conditions related to regulation. Selection by Consequences falls short in predicting ahead of time what constitutes a consequence and the mechanism by which it acts. Selection by Consequences does not yet appear to add anything to the inability of *Predictive Empiricism* to account for superstitious or, for that matter, adjunctive behavior (e.g., Timberlake & Lucas, 1991). Finally, in most ways Selection by Consequences is even more abstract and less grounded than the Causal Strengthening stance.

Reinforcement History. The final theoretical stance related to causation in operant contingencies is Reinforcement History. This stance is often invoked to account for differences in the effects of operant contingencies, but its application is guided by few rules. As a result, Reinforcement History appears in different instances to be varying combinations of the other three stances. It is similar to

Predictive Empiricism in that it anticipates that the elements of an operant contingency that worked previously will continue to work to similarly influence behavior in other circumstances. The Causal Strengthening stance comes into play in that when behavior has been organized by a reinforcer (whether within an operant contingency or based simply on results attributed to a putative reinforcer, as in superstitious behavior), the organization is presumed to remain influential in determining subsequent behavior. Finally, like Selection by Consequences, Reinforcement History is presumed to unite evolution, development, and previous operant learning in a common causal framework. They are all assumed to reflect the reinforcement history of the organism.

It seems to me that the use of Reinforcement History, like the case of the other stances, reflects our tendency to overgeneralize what we think we know based on the current success of an operant contingency. The direction of generalization for Predictive Empiricism is to the future; in contrast, the direction of generalization of Causal Strengthening, Selection by Consequences, and Reinforcement History tends to focus more on the past. Thus, what we see currently in the way of perceived or inferred change in behavior is attributed to a presumed contingency in the past involving a candidate reinforcer. The difficulty with this approach is that we cannot manipulate or verify the presumed causal relation without going toward without going toward an intervening variable approach.

For example, in the case of superstition we would need to argue for the existence of a momentary strengthening effect of proximity between a motor movement and a known reinforcer, an effect that dissipated unless the juxtaposition was renewed within a limited period of time. All of these contentions can and should be tested if we are to use the strengthening argument. In the case of sea turtles evolving to long oceanic migrations the test is more difficult, but converging evidence is possible. For example, we could argue that the assumptions of evolution argued for a distribution of individual turtle migrating abilities in terms of accuracy, endurance, and predator avoidance, then we could test for the distribution of similar distributions of abilities in modern turtles.

In short, procedures and concepts based on a stance of Predictive Empiricism, frequently combined with an intuitive concept of Response Strengthening, appear to be useful but are too simple in their present forms to deal with the influence of phenomena such as regulation or effects of operant repertoire on the success of operant contingencies. As a result, these approaches tend to make predictions that are not sufficiently nuanced to deal with the complexities of operant behavior. The stance of selection by consequences has heuristic benefits in suggesting experiments concerned with the differential reinforcement (selection) of particular distributions of complex timing or sequential responses from a larger population of possible responses. However, selection by consequences does not yet clearly distinguish outcomes as a function of different form of consequences, thus, for one thing, it is not explicit enough to predict successful operant contingencies. Finally, the stance of Reinforcement History appears to provide an almost dizzying array of possible causes to point to without clarifying the rules for when each case should apply.

One could argue that the above criticisms of operant causal stances are misplaced. Causal stances were not meant to be evaluated, so they have not been articulated in a way that allows them to be tested experimentally or clarified by connections to other approaches. The difficulty, though, is that in the absence of a general theory of operant conditioning, theoretical stances wind up guiding the behavior of scientists and explaining (at least intuitively) the effects of operant conditioning. As a result, their influence continues unchecked and certainly not tested explicitly. To provide a more testable alternative to the present stances I would like to suggest an Evolutionary Stance.

An Evolutionary Stance. Given that invocation of the past seems to be critical to analyzing the predictions of most stances, it seems a minor step to add another causal stance that depends explicitly on what we know and can infer about evolution and behavior in niche-related environments, a more predictive and testable stance that depends on the concepts of proper function, niche-related mechanisms, and the resultant organization animals bring to the operant contingency.

The concept of proper (evolutionary) function encourages awareness of the ecological function of mechanisms and behavior selected in evolutionary time for their contribution to differential survival and reproduction. The concept of niche-related mechanisms provides a means of mediating between the selection environment and a current environment. Niche-related mechanisms were selected for in evolutionary time, but they can be engaged in present stimulus environments, and in ways that might deviate from their ecological operation and effects. In other words, they are mechanisms that do not necessarily have limiting specific "knowledge" of how they were evolved to work, so they can be engaged in new circumstances. Finally, the organization of mechanisms into systems and motivational processes calls attention to hierarchical and sequential structures that can facilitate or impede the operation of a specific operant contingency (e.g., Timberlake & Lucas, 1989).

As an example of the possibility of prediction and understanding based on an Evolutionary Stance, consider that phenomena that are distinguished based on proper evolutionary function, and niche-related mechanisms are attributed to a single causal source by other stances. Thus, adjunctive behavior and fixed-ratio lever pressing in rats are both accounted for as simple products of operant contingencies in the causal stances of Predictive Empiricism, Causal Strengthening, Selection by Consequences, and Reinforcement History. A reinforcer is present, and responses that increase over time are assumed to do so as a function of the presentation of that reinforcer.

In contrast, proper function and niche-related mechanisms suggest significant differences between these two response classes and exemplars. Lever pressing appears to be related to mechanisms appropriate to the imminent discovery of food through manipulation responses. The adjunctive behavior of wheel running seems to be mediated by mechanisms related to locomotor search for food in a hungry animal in the absence of cues for temporally proximal food, whereas adjunctive drinking seems to be triggered by meal-ending mechanisms that come into play after a pause in eating of greater than one minute. Whether correct or not, these easily generated accounts represent hypotheses it is possible to test. No matter what the outcome, more information is provided about niche-related mechanisms. In comparison, the overgeneralized aspects of other stances do not allow such diverse predictions, fail to encourage their test, and in general tend to interfere with connections to other explanatory systems that might improve our understanding of purposive behavior.

Summing Up

The aim of this paper was, first, to evaluate whether the operant contingency is sufficient to serve as a foundation for a science of purposive behavior, and, second, in the event systematic improvement was possible, to discover ways to increase success in implementing operant contingencies by clarifying how they work. To this end I evaluated the adequacy of the operant contingency as a definitional system, an empirical procedure, and in terms of its relation to intuitive theoretical concepts and stances.

The definitional system approach to operant contingencies has proved empirically usable, but it lacks independent definition of the elements of the contingency, and, in practice, it depends on intuitively specified setting conditions that might well be seen as mini-theories. As a result, the definitional system approach is unable to generate predictions of successful contingencies or facilitate conceptual refinements of the elements contributing to their success. There is no question that a successful operant contingency is an important phenomenon, yet I believe it could be even more impressive if it made sufficiently clear connections with other conceptual approaches and with niche-related mechanisms and evolutionary function, thereby suggesting how to improve our ability to produce and take advantage of it.

An inductive examination of the empirical techniques used to produce successful operant contingencies suggests the importance of experimenter sensitivity and expertise in carefully and systematically tuning apparatuses and procedures and in making appropriate choices of potential contingency elements and shaping techniques. In contrast, a contingency that produces misbehavior can be viewed as the obverse of a successful contingency. Misbehavior results from inadvertent tuning of the apparatus and procedure to support niche-related mechanisms related to alternative behavior, combined with an inappropriate choice of potential elements of the contingency and a lack of appreciation of the niche-related mechanisms and functional repertoire of the organism. Establishment of the subject's repertoire of motivational processes and states, stimulus sensitivities, and response tendencies should facilitate the implementation of successful contingencies in both cases (Timberlake, 1999).

Many experimenters agree that a subject brings to an operant contingency organized sensory–motor and motivational aspects of behavior that likely affect the outcome. Some experimenters might also agree that such organization might be what experimenters tune toward in creating successful contingencies. However, most experimenters still explain the results of the operant contingency using only elements of the successful contingency. The contribution of the animal to the outcome of an operant contingency is not considered, whether it is directly observable (e.g., Stokes & Balsam, 1991) or only inferred. An example here is the reply of a noted operant conditioner to my graduate-student question about what timing mechanisms might facilitate pigeons emitting near-exponential distributions of inter-response intervals (as in Blough, 1963). His answer, after a pause, was, "The data are the data"; by this he meant that the nature of the contingency elements was sufficient explanation. The interesting questions to me, concerning what mechanisms interacted with the contingency to produce the results and how generalizable they were to other distributions, were not relevant.

To recapitulate: there should be no difficulty in admiring the empirical power of operant contingencies, especially when manipulated by an experienced conditioner with a good sense of how the subject functions. It is easy to appreciate the apparent conceptual simplicity of operant contingencies and the advantages of not dealing with more complex theories separated from empirical phenomena by several steps. It is also easy to appreciate the unwillingness of experimenters to alter what works, both in the effects of contingencies on their subjects' behavior and in publishing contingencies on their own behavior.

However—and it is a large "however"—the codefinitional approach used by Skinner in dealing with operant contingencies is not readily testable as it stands. Perhaps most importantly, there are limits to avoid and facilitating circumstances that are needed for an operant contingency to work efficiently. These limits and circumstances are not at the level of the organism being unable to see the discriminative stimulus light (although these are important issues to keep in mind); rather, they form around evolutionary functions of behavior, the operation of niche-related mechanisms, and how these mechanisms are organized in systems of motivational processes engaged and supported by the experimental environment (Timberlake, 1999).

Finally, in moving beyond the experimental chamber, Skinner and others began to use mini-theories and theoretically-tinged causal stances in guiding and interpreting research, including Predictive (Bayesian) Empiricism, Causal Strengthening, Selection by Consequences, and Reinforcement History. Despite their obvious influence, these theoretical concepts and causal stances lack specificity and clear linkage to other conceptual approaches. They could profit from more explicit recognition of the extent of their influence and a clarification of their assumptions.

A key factor in the success of the operant contingency lies in the interaction of the experimenter and the organism. That interaction has been highlighted in popular cartoons in which one rat confides to another how well the experimenter is trained (e.g., "Watch this! Every time I press this lever, the guy in the white coat gives me a pellet!"). The ego-deflating aspect of the humor is funny, but I believe these cartoons miss a more important point of the experimenter–organism interaction. The rat (or any subject for that matter) does participate in controlling the experimenter's behavior in a nontrivial way, but pressing the lever is only a final manifestation of the process. The key is how the subject participates in the experimenter's inductive and deductive perception of the niche-related mechanisms underlying the subject's behavior.

The tuning of apparatuses and procedures and the shaping of behavior reflect the experimenter's implicit appreciation of the working characteristics of the subject, the perceptual–motor organization and regulatory processes it brings to the situation. Thus, at bottom, any "empirical" procedure the experimenter uses in producing successful operant contingencies necessarily requires close attention to the nature of the subject. Because most effective tuning of apparatuses and procedures appear to be toward or away from engagement of the niche-related perceptual–motor organization and motivational processes of the subject (Timberlake & Lucas, 1989; Timberlake, 2001), beginning with this structure of the organism should facilitate an operant approach to behavior and the empirical adequacy of the operant contingency.

A start on uncovering the relevant mechanisms and environmental supports producing and controlling behavior can be made by combining observations of motivated behavior in freely behaving organisms with the use of the analytic tools of operant and Pavlovian contingencies (Timberlake, 1999). Seeking to ground operant contingencies in evolutionary function and niche-related mechanisms will often provide insights into what is occurring. At the same time it is appropriate to be cautious about positing normative evolutionary functions for behavior and mechanisms contributing to it. It is plain that the evolutionary function of even a simple survival mechanism such as a righting reflex can be co-opted for other purposes by evolutionary processes or by human experimenters (as by a physician with a rubber hammer seeking information about a patient's health from the knee-jerk reflex).

Conclusion

Operant psychology, based primarily on its empirical focus on the application of the operant contingency to behavior, is arguably still the most widespread, powerful, flexible, and generative approach to the control and analysis of purposive behavior. Through its emphasis on the control of three interrelated concepts (discriminative stimuli, operants, and reinforcers) and appropriate setting conditions, the operant contingency has proved flexible in its applications and generative in its development of techniques of scheduling, analysis of choice, and behavioral control. Probably the most diagnostic characteristic of the operant approach is the widespread application of its apparatus and procedures in areas ranging from the study of neurophysiological mechanisms to motivation and behavioral ecology to cognitive processes such as counting, categorizing, and remembering to teaching, training of social behaviors, and implementation of skill learning.

Perhaps unexpectedly, the second most diagnostic characteristic of the operant approach has been the absence of a well-developed conceptual and theoretical framework with good connections between operant conditioning and other approaches. As a result, despite its breadth of application and ideal placement as a technique for clarifying mechanisms and testing theories, the operant approach has remained conceptually isolated from other approaches. As a result, it has often been more a technical assistant than a collaborating scientist.

Many experimenters and practitioners have emphasized the simplicity of the classic operant contingency as a major strength. Much like a law of mechanics, it specifies the relations between pure concepts untarnished by such things as friction, variation in the gravitational field, or the history of the objects. Unfortunately, there is no ideal conceptual world in which operant contingencies can be applied; instead there are individual species and organisms and specific environments. Important contributions to the outcome of operant contingencies are made by previous learning, evolutionary history, and niche-related mechanisms. Simplicity is when the operant contingency fits with the environment and these other determinants.

A basic conclusion of this paper is that if Skinner's vision of the operant contingency as a cornerstone of the analysis of purposive behavior is to be fulfilled, the operant contingency must be clarified further and integrated with other approaches. The clarification should be based on finding the grounds for better predicting and understanding the success of some operant contingencies and procedures, and the failures of others. In my opinion this clarification would profit from considering the evolutionary function of behavior and the niche-related mechanisms involved in it. Such a clarification will be important not only in improving our control and understanding of behavior but also in relating the results of operant contingencies to other important approaches including genetics, neurophysiology, and development.

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