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The Experimental Analysis of Human Behavior: Indispensable, Ancillary, or Irrelevant?

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There is growing recognition that psychology as a whole is in an extraordinary state of transition. The changes have far-reaching implications for the experimental analysis of behavior, not only for the theoretical interpretations that guide our research but also for the methods we use to collect and analyze the data.

That methods of behavior analysis should change over the years is not of itself remarkable. History tells us that evolution, if not revolution, is part of the normal course of the development of a science. What is remarkable is that many of these changes are taking place without full acknowledgment or discussion. This has given rise to a lack of correspondence between what is said and what is done. We continue to advocate to our students a set of laboratory procedures, experimental designs, and data-analytic strategies derived from those developed in the animal laboratories from which behavior analysis emerged (Ferster, 1953; Perone, in press; Sidman, 1960; Skinner, 1956). But these methodological prescriptions—the customs and codes that distinguish behavior analysis from the rest of psychology—are increasingly violated, ironically, in favor of the very methods to which the founders of behavior analysis objected (Baron, 1990).

The purpose of our target article (Baron, Perone, & Galizio, 1991) was to stimulate discussion of this troublesome state of affairs. We are honored that such a distinguished group of scientists has seen fit to offer their comments in this issue of *The Behavior Analyst.* Their reasoned arguments will make a significant contribution to the eventual resolution of the issues raised in our article. We regret that the space allotted to us does not permit a response to each and every one of their points. We believe that we can best proceed by trying to integrate our concerns. The outcome, we hope, will be the start of a dialogue that will help us find common ground.

THE PLACE OF THE HUMAN SUBJECT

A good place to begin is with what appears to be a misunderstanding. As indicated by its title, our article asked whether application and behavioristic interpretation can replace laboratory research with humans. Our efforts to answer this question led us to assert that laboratory research is an essential tool in the analysis of the reinforcement process on the human level. We also commented on some rarely acknowledged limitations

This article was prompted by a series of commentaries (Branch, 1991; Buskist, Newland, & Sherburne, 1991; Dinsmoor, 1991; Palmer & Donahoe, 1991; Pierce & Epling, 1991; Shull & Lawrence, 1991; Wanchisen & Tatham, 1991) that were written in response to a previous article of ours (Baron, Perone, & Galizio, 1991). We thank the commentators for their scholarly attention to the issues we raised and Editor Samuel M. Deitz for giving us the opportunity to engage in this stimulating dialogue.

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of application and behavioristic interpretation.

It was not, however, our intention to assert the two alternative propositions that our colleagues rightly take up arms against: that laboratory research with humans can or should *replace* application and interpretation, or that laboratory research with humans can or should *replace* research with animals. To the contrary, we believe that these four approaches—human research, animal research, behavioristic interpretation, and practical application—stand together in a symbiotic relation. Each has a unique role to play. All are needed for full understanding of the principles that guide behavior.

More to the point is the relative weight that should be given each approach. Several commentators caution that serious problems confront the researcher who ventures into the human operant laboratory. Many of their concerns echo those which we have previously described in some detail (Baron & Galizio, 1983; Baron & Perone, 1982; Perone, Galizio, & Baron, 1988). The commentators also express reservations about the ultimate success of any effort to analyze the reinforcement process through experiments with human subjects. Nevertheless, a consensus emerges. In the end, all subscribe to the view that laboratory research with human subjects has a proper, albeit neglected, place in the experimental analysis of behavior. Dinsmoor, in particular, makes it plain that we have misunderstood his position in this regard. We are glad that he has set the record straight.

The commentators vary considerably in their views about the contribution to be made by human research. At one end of the spectrum—the end where we place ourselves—are the positions staked out by Pierce and Epling, by Buskist, Newland, and Sherburne, and by Shull and Lawrence. They see a role for the human subject in the search for *fundamental* principles of behavior. Pierce and Epling are simply tired of waiting for answers about human behavior to come from the pigeon laboratory. In their view, research with humans and animals should proceed in parallel, with each informing the other. In a similar vein, Buskist et al. see discrepancies between experimental outcomes with humans and animals as a challenge, not as a reason for abandoning the laboratory approach. The differences will serve as a spur for clarifying the operation of basic processes in the two cases. Shull and Lawrence also assert that human research is capable of revealing fundamental relations. They have serious reservations, however, about the utility of the traditional methods of the animal laboratory, and they suggest that the full potential of human research cannot be realized without new analytic preparations.

Branch, Dinsmoor, and Palmer and Donahoe are in a different camp. In their view, the discovery of fundamental principles is the province of the animal laboratory; human research is relegated to an ancillary role. They do not say, however, that human research can make no contribution at all. Human experiments provide tests of principles already developed with animals (Dinsmoor; Palmer & Donahoe), especially when the subjects are children (Branch). Behavioral processes can be studied that may not reach full expression in animals, such as those involved in verbal behavior (Branch; Dinsmoor; Palmer & Donahoe). Finally, human experiments provide adjuncts to animal research on the effects of "layers" or sequences of experiences (Branch).

Wanchisen and Tatham's position is difficult to place. They provide an illuminating review of the challenge posed by historical factors in the study of ongoing behavior. An implicit aspect of their comments is the value of considering human and animal behavior in similar terms. But they seem unduly pessimistic about the likely success of such an effort. Laboratory observations, they tell us, are plagued by historical factors which are exceedingly difficult to identify, much less bring under control. As we pointed out in our article, historical factors cannot be taken lightly; histories can obscure basic processes in humans and animals alike.

Nevertheless, we remain confident that histories (as well as the other extra-experimental variables mentioned in our article) can be accommodated within the traditional methods of the experimental analysis of behavior.

We should also acknowledge another camp within behavior analysis, one that is not represented among the commentators (we are not sure whether we should be relieved or disappointed). We refer to the view that human behavior exemplifies processes and principles that simply have no counterpart in the performances of rats or pigeons. It follows that understanding of exclusively human processes requires equally exclusive reliance on the study of human subjects. In the target article we observed that acceptance of this proposition charts a course away from the other biological sciences. Suffice it to say that this is not the direction that we or the commentators wish to take.

THE GENERALITY OF BEHAVIORAL PRINCIPLES

The commentators' differing views on the value of human research hinge on the question of generality. What steps are needed to substantiate the claim that principles developed in the animal laboratory are fundamental for humans as well as for animals? Palmer and Donahoe anchor one end of the continuum. Although they are not completely disapproving of research designed to show that animal-based principles also govern human behavior, they argue that even successful demonstrations "will not advance our formulation of basic principles." At best, human research "will serve mainly to shore up the applications and interpretations that have taken for granted the generality of basic principles."

We wonder about the wisdom of conferring axiomatic status on the outcomes of experiments with animals. As we see it, generality is an empirical issue, although not necessarily one that needs to be addressed at every stage of the scientific endeavor. Perhaps Palmer and Donahoe are dubious about human experimentation because they see so little hope of collecting conclusive data. If data from humans and animals differ, the limitations of the experiment may be to blame; yet if the data are similar, the underlying processes still may be different. We agree that no pattern of experimental outcomes can provide conclusive (in the sense of deductive) proof for any theoretical proposition. Nevertheless, positive outcomes provide inductive support, and when a sufficient number have accumulated, the proposed principle is established by consensus. The consensus is, of course, provisional. Scientific understanding always is vulnerable to empirical challenge.

Branch and Dinsmoor also look to the animal laboratory, but they see some value in the product of human research. Branch approves of "trying to examine directly the generality of basic conditioning principles that have been isolated with nonhumans." And Dinsmoor argues that "once the basic principles have been worked out with rats and pigeons, I do think it is appropriate to conduct additional tests with human subjects." We take these statements about *examining* and *testing* animal-based principles as evidence that Branch and Dinsmoor do *not* take the principles for granted. By raising questions about generality, human research can contribute to the formulation of the principles, even if only to send researchers back to the animal laboratory to try again.

Pierce and Epling take the argument a step further in their objections to the dominant status of animal experiments. They express considerable impatience with the "simple-to-complex research strategy," an approach that demands analysis of "the behavior of simple organisms in simple environments" as the prerequisite for the initiation of work at the human level. In their view, scientific understanding of human behavior can no longer await the discovery of order in the animal laboratory. The time has come for a "two-pronged approach" that will give human research its due, one that involves simultaneous and complementary efforts at both levels.

There is plenty of room within the experimental analysis of behavior for these differing emphases. The important point is that the commentators agree that human research is logically continuous with animal research, and therefore, an integral part of the basis for a general theory of behavior (cf. Baron & Perone, 1982).

The search for fundamental relations has led researchers to examine human performances under schedules of reinforcement. Shull and Lawrence ask, "Do the patterns of behavior engendered by different reinforcement schedules exemplify fundamental relationships?" They comment that "Sometimes Baron et al. write as if they think so," and add that "relationships involving patterns are not the foundation of any system that we are aware of." These commentators then make the case that the patterns in question are secondary, that they "are usually conceptualized as the result of combinations of more fundamental variables." Of course they are! Our view on this issue (Baron et al., 1991; Perone et al., 1988) is no different from the one shared by most behavioral analysts (cf. Zeiler, 1977), including Shull and Lawrence. (But the notion mistakenly attributed to us—that schedule-controlled response patterns might be fundamental—is not as far-fetched as Shull and Lawrence apparently believe; see Morse & Kelleher, 1977, and Zeiler, 1984.)

Having raised the issue of whether response patterns are fundamental, Shull and Lawrence might have gone on to explain that these patterns, although secondary, represent an important way of elucidating the more basic processes—for example, those related to the temporal contingency of the fixed-interval schedule. Observation of the theoretically predicted temporal patterning gives us increased confidence in our understanding of the processes that generate such an outcome. Discrepancies, such as the absence of temporal patterning in humans, are, therefore, more than curiosities. They raise doubts about our level of theoretical understanding.

Although human-animal discrepancies must be reckoned with, in our view they have generated unnecessary theoretical contention. We argued elsewhere that the evidence purporting to show the existence of discrepancies leaves much to be desired (Perone et al., 1988). In many cases, discrepancies are identified by comparing limited samples of human behavior against vague characterizations of "typical" patterns in animals. Our review of the literature on the so-called "fixed-interval scallop" suggested that commonly-held views about characteristic performances are simply mistaken; there is too much variability in the fixed-interval performances of either animals or humans to permit general conclusions at this stage of knowledge.

INTERPRETATION AND APPLICATION

Although behavioristic interpretation and practical application cannot replace laboratory research, they can help extend the generality of the basic principles discovered in the laboratory. Palmer and Donahoe remind us how vital interpretation is to the scientific enterprise. "We engage in experimental analysis so that we can interpret the world. . . . Most of our scientific understanding of the world is interpretation: No one has done an experimental analysis of the tides or of the orbit of the planets. . . ." Interpretation *is* important; indeed, Palmer and Donahoe's comments make it plain that it is not only important but indispensable.

We must, however, inquire further of Palmer and Donahoe about the domains they would have behavior analysts interpret. For the physical scientist, interpretations of such matters as the orbits of the planets are based on meticulously collected observations. Unfortunately, this is not the way things work in behavioristic interpretations, as, for example, when the action of a fixed-interval schedule of reinforcement is detected in the behavior of students who put off studying for exams until the last minute. More often than not, the object of behavioristic interpretation is best described as "anecdotal"—descriptions and illustrations of behavior of unspecified origin that may be collected without sufficient attention to errors of measurement and reported selectively. Moreover, the interpretation may seriously misrepresent the behavioral principle that the interpretation is supposed to support (Michael [1980] referred to the student example above as "superficial nonsense"). Behavior analysis lacks a set of agreed-upon observational procedures that might provide acceptable descriptive data about naturally occurring behaviors (Baron & Perone, 1982; see also Bijou, Peterson, & Ault, 1968). Because so little progress has been made in this regard, the kind of interpretation envisioned by the commentators is rare.

Applied behavior analysis provides the other source of naturally occurring human behavior against which basic principles might be gauged. Branch sees many successes when behavioral principles are directed toward the solution of practical problems, and he finds it hard to believe that the positive outcomes could be epiphenomenal. We don't think these results are an accident either. We must reiterate, however, that the bearing of the results of practical application on the theoretical concerns of the laboratory is limited at best. Applied research may help establish that reinforcement works, that shaping works, that stimulus control works. But the theoretical concerns of experimental analysis run deeper. We doubt that applied research can tell us much that will help us identify the origins of the fixed-ratio pause, assess the adequacy of the delay-reduction hypothesis of conditioned reinforcement, or decide among the various matching, melioration, and maximization accounts of choice. Applied behavior analysis has an important mission, but it is more in connection with solving human social problems than in clarifying the theoretical issues currently pursued in the laboratory.

TRADITIONAL METHODS

A number of the commentators take us to task for advocating that research with humans should emulate the methods that have been used so successfully in the animal laboratory. Branch is "skeptical about the utility of employing (at least as they have been employed, for the most part, so far) direct analogues of laboratory-conditioning procedures with adult humans." Shull and Lawrence admonish us that "nothing inherent in operant principles nor in traditional behavior-analytic research methods requires that the procedure mimic features of the rat's lever-box." Palmer and Donahoe caution against research that "displays only the superficial trappings of the animal laboratory," and Buskist et al. alert us to the dangers of "an infatuation with the operant chamber."

We wonder if the commentators are confronting us with a straw person here. In considering the wisdom of "mimicking" (or developing a "direct analogue") of an apparatus customarily used in animal research, it is essential to keep separate the *form* of the apparatus (such as the lever used to detect the response or the lights and sounds that serve as discriminative stimuli) and its *substance* (the principles of observation and measurement that enter into the design of the apparatus). Of course it is not essential that human subjects be studied in structures that resemble the rat's lever-box. What is essential—if one wants to compare the processes of operant conditioning in humans and animals—is that the methods meet the requirements for the study of steady-state free-operant behavior in the individual organism. (We trust that there is agreement that this is and should continue to be a major focus of the experimental analysis of behavior.)

As things turn out, the lever-pressing apparatus developed by Skinner does a pretty good job of incorporating the "tactics" needed for a steady-state analysis (Sidman, 1960). The rat is confined in an environment that permits objective measurement of a clearly defined response; contingencies involving reinforcing and discriminative stimuli are precisely controlled; and behavior is shielded from unwanted influences. The commentators surely regard these experimental tactics as no less important when human behavior is the object of study. They should not, therefore, find it surprising or objectionable that precision and control in human research might require physical arrangements analogous to those used with rats. In our laboratories, the human subject works in an isolated environment (sound-shielded chamber) in which reinforcing events (money) are contingent on a specified response (pressing a key; operating one or more switches) in the presence of stimuli (geometric forms; words; sentences) displayed on a computer monitor. All these features, in conjunction with long-term study of the individual subject, play essential roles in the steady-state analysis of human performances.

Even a cursory review of the human operant literature will uncover obvious methodological shortcomings. By comparison with animal research, human research is less likely to involve significant degrees of exposure to the experimental conditions, control procedures such as changeover delays, objective criteria for identifying steady states, or within-subject replications (e.g., by way of reversal designs). Equally troublesome is the insufficient attention paid to the consequences that are supposed to function as the reinforcers of human operant behavior. We share Shull and Lawrence's concerns about use of "points." In the animal laboratory, a standard set of procedures underlies the use of food as an effective reinforcer (e.g., establishing the stimulus as a reinforcer by arranging appropriate levels of deprivation, delivering the reinforcer for consumption during the session, and preventing satiation). Such matters need to be given more careful consideration in human research; as things stand, a wide variety of reinforcement procedures is used with human subjects, and the effectiveness of many of them has yet to be established (Galizio & Buskist, 1988).

The apparatus and methods developed by Skinner for studying operant conditioning in the rat have remained more-or-less unchanged for 50 years. Their wide use in animal research by behavioral and biomedical scientists serves as a testimonial to Skinner's wisdom. By comparison, human research has not done nearly as well. In our view, the problem is not that the methods of the animal laboratory have had too much influence on the experimental analysis of human behavior, but rather that they have not had enough.

DEFINING BEHAVIORAL UNITS

Some of the commentators voice doubts about using button pressing as the response of interest. Branch asks whether button pressing has the status of an operant. "What evidence is there that a single push of a button by a human in a laboratory is a functional unit of behavior?" Shull and Lawrence are more concerned about the complex behavioral relations into which button pressing may enter.

> A human's button press seems likely to be part of many different behavior classes, including such complex, higher-order classes as "strategies" for interacting with games, complex chains prompted by and prompting verbal behavior, and "test-taking" repertoires reinforced and motivated by signs of social approval and success.

Perhaps Shull and Lawrence's comments will help convince Branch that the human's button press, like the rat's lever press and the pigeon's key peck, provides a legitimate vehicle for studying a range of behavioral processes. After all, we press buttons for a variety of reasons: to tune a radio, to withdraw cash from an automatic teller machine, and to phone our friends. (Indeed, there are honored professions whose members appear to do little else but engage in such movements, including telegraphers, computer programmers, and concert pianists.)

Branch and Shull and Lawrence do agree about what they take to be a fundamental difference between button and lever pressing. Unlike the human's button press, the rat's lever press is said to be "arbitrary" (Branch) or "unconstrained" (Shull & Lawrence). As Branch puts it, "rats were not selected genetically in environments in which levers existed and presumably they have not experienced such levers in their lives before they get to the experiment." The commentators may be missing an important point. Behavior analysts now recognize that the responses chosen for study in the operant conditioning laboratory are hardly arbitrary. We should have realized long ago that their successful use in experiments depends in large part upon their status within the animal's natural repertoire. Although the particular devices used in the laboratory to detect the responses (levers, keys) may not have been encountered previously, the behaviors that activate these devices have occurred repeatedly (rearing and grasping by the rat, pecking by the pigeon). Quite relevant here is Schoenfeld, Antonitis, and Bersh's (1950) suggestion concerning the source of the "unconditioned strength" of the lever-press response. They attributed responding prior to explicit reinforcement in the experiment to the generalized extinction of responses of a similar character that had been conditioned in the history of the organism.

A theme of our article was that the sorts of concerns that alarm the commentators frequently have counterparts in the animal laboratory. We can only remind them that previous concerns about the "prepared" nature of the operant responses of rats and pigeons (e.g., Bolles, 1970; Seligman, 1970) have yielded to solutions. The pigeon's key peck, in particular, has withstood challenges from those who have argued that our knowledge from studies of key pecking on operant schedules—studies which constitute the majority published in the *Journal of the Experimental Analysis of Behavior* (Grossett, Roy, Sharenow, & Poling, 1982)—is flawed by the peck's susceptibility to non-operant influences (e.g., Williams & Williams, 1969). No doubt, complex determinants—experiential as well as biological—enter into a human's, a rat's, or a pigeon's execution of the complex movements needed to operate a button, lever, or key (cf. Schwartz, 1974). But this recognition does not seem a very good basis for discarding the results of experiments that involve such responses.

What about concerns that the button press may not represent a "functional unit" of behavior—that it may not mark a "natural line of fracture" (Branch; Palmer & Donahoe)? It is important to remember that functional units are not defined structurally, as an emphasis on *the button press* implies. Further, there is disagreement about whether functional units should be identified on the basis of order in the behavioral patterns to which a given response contributes, or in terms of the contingency that relates the response to reinforcement (cf. Schick, 1971). According to the latter, more commonly accepted view, functional units have more to do with the schedules arranged by the experimenter than with the engineering of the operandum. With a fixed-ratio 50 schedule, for example, the functional unit may be the run of 50 responses rather than the individual key peck, lever press, or button press.

Button pressing in the laboratory can enter into functional units of varying degrees of complexity. It is instructive to sample the range of theoretical issues that have been studied with this response. The human subject may press a button to see a meter (*vigilance*; Holland, 1958), to identify a stimulus that resembled one previously presented (*matching-to-sample*; Sidman, 1969), to prevent the occurrence of a signal correlated with the loss of money (*avoidance*; Baron & Kaufman, 1966), to produce stimuli correlated with the components of a compound schedule of positive reinforcement (*observing*; Perone & Baron, 1980), to review scores reflecting a fellow subject's performance (*auditing*; Hake, Vukelich, & Kaplan, 1973), or to answer "yes" or "no" to questions about recent behavior on a conditional discrimination task (*verbal self-reports*; Critchfield & Perone, 1990). Each of these behaviors is functionally distinct. Little purpose is served by lumping them together into a single class simply because the operandum happens to be the same.

HISTORY AND CONTEXT

Research is conducted in laboratories to isolate relations between the independent and dependent variables. The isolation is necessarily imperfect in the analysis of behavior; as Skinner often pointed out, the outcome of an experiment depends on the behavior the organism brings to it as well as on the conditions imposed by the experimenter (e.g., Skinner, 1958). A major theme of our article was that the human organism, in particular, arrives at the laboratory equipped with a complex repertoire, one that reflects the effects of diverse extra-experimental factors, both historical and contemporary.

Not surprisingly, our colleagues also are concerned about the complications introduced by these factors, and several take pains to remind us that it is much easier to control histories in animals than in humans (Branch; Dinsmoor; Palmer & Donahoe; Wanchisen & Tatham). It is, of course, easier to control almost everything in animals. Nevertheless, control of history is not routine practice in the animal laboratory; researchers commonly report without apology that their subjects have had previous experience with a variety of procedures (particularly if the subjects are of long-lived species such as pigeons and monkeys). This casual approach should not be judged too harshly. In their review of reinforcement and punishment, Morse and Kelleher (1977) pointed out that historical variables can be overridden by experimental procedures that are "especially forcing"—in the case of animals, food following deprivation or a painful shock. Procedures with humans usually suffer by comparison (Buskist et al.'s commentary provides some interesting lore on this point), thus making laboratory studies of human behavior more vulnerable to history effects. The obvious antidote is to seek stronger forms of experimental control that can be imposed in the human operant laboratory. Although the human researcher is constrained by ethical considerations (Dinsmoor), the remedy often is within reach—money rather than points as the reinforcer, for example.

A different way of dealing with history effects is through long-term exposure to the experimental conditions. A subject's behavior at the beginning of an experiment is governed largely by historical variables (what else could be exerting control?), and significant contact with the experimental variables is needed before their effects take hold. This logic is at the very foundation of the steady-state approach to operant conditioning. While our colleagues seem to share our enthusiasm for steady-state research, they express considerable doubt about its utility in overcoming history effects (Branch; Dinsmoor; Shull & Lawrence; Wanchisen & Tatham). Wanchisen and Tatham put it this way: "Whether history effects eventually diminish as a correlate of extended exposure to the conditions under which the history effect is revealed is essentially an open question within both the human and non-human behavioral history literature."

More research will be needed to close the matter, but the available data lead us to be optimistic that history effects will yield to steady-state experimental designs. In an instructive study, Todorov and his colleagues (Todorov, de Oliveira Castro, Hanna, de Sa, & de Queiroz Barreto, 1983) found that sensitivity to concurrent schedules diminished as pigeons gained experience with successive pairs of schedules. But when the schedule conditions were sufficiently extended—55 rather than 30 sessions—sensitivity returned. In a similar vein, Freeman and Lattal (in press) gave pigeons a history that engendered high and low rates in the components of a multiple schedule and then exposed the birds to a common test schedule in the presence of the component stimuli. Although response rates differed at the outset of the test, in 5 of 6 birds they converged within about 40 sessions. These two sets of data show nicely that variability caused by different histories can be reduced by sufficient exposure to the conditions of current interest. As common observation tells us, the effects of the past usually diminish as the past becomes more remote.

Do studies such as the ones cited above establish that we can escape the past? Branch points out that once established, a history can never be eliminated. But can its *effects* be eliminated? Wanchisen and Tatham argue that even when history-related differences have disappeared in the face of extended training, latent effects may be lurking below the surface. The task here is to identify the conditions under which the latent becomes manifest. We agree with Wanchisen and Tatham (cf. Wanchisen, 1990) that the focus of operant research and theory on contemporary influences has led to neglect of the possible influences of remote historical factors (including their sequential effects; Branch). We certainly encourage greater attention to such factors, as an area of study in its own right as well as a potential source of confounding variables. But we hasten to add that the traditional methods of the animal laboratory have already proven to be up to the task, not only with animal subjects, but also with humans (e.g., Weiner, 1969).

Buskist et al.'s concerns with extra-experimental factors go beyond history. They note that an experiment takes place within a broader environmental context that can have decided effects on what happens during the session. Perhaps many context effects, like those of history, can be overriden by experimental manipulations that are sufficiently "forcing." But unlike historical factors, which are relatively remote, contextual factors are relatively contemporaneous, and they may be more potent contributors to laboratory behavior. We agree with Buskist et al. that the manipulation of contextual variables may pay dividends in the experimental analysis of human behavior. And, again, we look to the animal laboratory for illustrations of how traditional methods can be put to this use, for example, in the study of open versus closed economies (e.g., Hursh, 1984).

CLOSING REMARKS

Our target article pointed to apparent discrepancies in the outcomes of experiments with humans and animals as provoking somewhat of a crisis in behavior analysis. The range of viewpoints expressed by our colleagues, and the fervor with which they hold them, bear out the need for continuing dialogue on this issue. Although there appears to be consensus that attention must be paid to discrepancies, there is considerable disagreement about the interpretation to be placed on them.

It is important to recognize that the results of different experiments are identified as "discrepant" not merely because they differ, but also because there is no principled explanation for the difference. For example, the fact that a given drug increases the rate of food-reinforced lever pressing in one study but decreases it in another represents a discrepancy only until we have the principle of rate dependency to explain the difference.

Discrepancies, then, are valuable because they reveal our ignorance about principles. In seeking a common explanation of human and animal behavior, it seems reasonable that research using analogous methods will identify discrepancies—gaps in our understanding—more clearly than applied behavior analysis or behavioristic interpretation.

But identifying discrepancies is the easy part. The challenge is in their resolution. And that is where we may part company with some of our colleagues.

Should we be satisfied by saying that discrepancies in human and animal performances would not have emerged if we had viewed the problem at the proper level of analysis? If we had managed to measure the behavior along its natural lines of fracture? If we had controlled the context or history better? If we had been able to simplify the subject's repertoire to eliminate hypothesis-testing, rule-following, or attempts to please or confound the experimenter? If we had used more potent reinforcers? The list of possibilities may be endless. And as plausible as such accounts sound, they are not *principled* explanations of the differences that might be observed in human and animal behavior. If we stop at such accounts, we have accomplished nothing more than asserting our faith that a principled explanation eventually will emerge. A much greater effort is required to derive a precise, verifiable explanation through experimental analysis. And, because the experimental answer must be rooted in data rather than logic, the answer will be forever tentative.

Is the effort worthwhile? Our colleagues acknowledge that at least some human research is needed in behavior analysis. Indeed, some advise us to exert greater energies in this regard (or, perhaps, put them to better use), in the direction of studies on complex forms of human behavior such as handwriting, social behavior, information processing, and, of course, verbal behavior in general. These are worthwhile avenues for future research. Our concern at present, however, is with fundamental principles of reinforcement, and we find that a number of us are satisfied to use animal data as the benchmarks. In such a case, human "demonstrations" of principles that have already been agreed upon are luxuries that we might be able to do without—an ancillary effort at best. But will this position satisfy those who are not impressed by expressions of faith and demand instead firm empirical support for claims about the relevance of animal-based principles to human behavior? And if one believes, as we do, that human data can contribute to the formulation of fundamental principles, then the experimental analysis of human behavior is indispensable.

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