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The Place of the Human Subject in the Operant Laboratory

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Although laboratory study of human behavior seems an obvious vehicle for strengthening the scientific base of behavior analysis, the place of the human subject within the operant laboratory remains problematic. The prevailing research strategy has been to link principles developed with animals to human affairs, either through interpretation of naturally occurring human behaviors or through application of the principles to the solution of human problems. The paucity of laboratory research on human operant behavior derives from several misconceptions: the possibility that experimental demand characteristics and pre-experimental behavioral dispositions of human subjects contaminate the results; that ethical considerations place undue constraint on research topics and experimental designs; and that uncontrollable variation in subjects' histories and other relevant personal characteristics prevents observation of reliable functional relations. We argue that these problems do not pose insurmountable obstacles to the experimental analysis of human behavior; that adequate methods of control and analysis are available; and that operant techniques, by emphasizing experimentally imposed contingencies, are well suited for the laboratory study of human behavior.

Behavior analysis originated in the laboratory and discussions of the research methods developed there naturally emphasized experimental work with animals, usually rats and pigeons (Ferster, 1953; Sidman, 1960; Skinner, 1938). Descriptions of specific apparatuses were accompanied by explanations and justifications of the rationale of the characteristic methods, including free-operant procedures rather than instrumental ones, close experimental control rather than statistical analysis, and extended observations of individual subjects rather than brief study of large numbers of subjects.

Subsequently, attention turned toward extending these methods to the analysis of human behavior, in particular the socially important behaviors involved in retardation, crime, mental illness, and education (Baer, Wolfe, & Risley, 1968; Hersen & Barlow, 1976; Sidman, 1962). Many new issues are encountered when research is conducted in clinical and other settings where primary concern is with applied rather than basic aspects of behavior. Special requirements include the study of environments and behaviors selected by societal rather than scientific considerations, the production of changes sufficiently large and general to be of practical value, and the demonstration of the effectiveness of the procedures under circumstances where rigorous experimental control is not possible.

In these discussions of the methods of the experimental and applied analysis of behavior, not enough attention has been paid to the status of the laboratory analysis of human behavior in the development of basic principles. This has created the impression that basic research must focus on the behavior of infrahumans—only when research is directed toward applied questions does human behavior become the central issue. The state of affairs with regard to methodological prescription is paralleled by the state of the research literature. Examples of the use of human subjects in basic laboratory research may be found throughout the history of the experimental analysis of behavior, but such efforts remain infrequent by comparison with applied analyses of human behavior or with laboratory research with infrahumans.

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Although laboratory study of human behavior seems an obvious way to strengthen the scientific base of behavior analysis, the place of the human subject within the operant laboratory remains problematic. In the discussion that follows, we address three pertinent issues. First, we describe and criticize what we take to be the modal view of the relative roles of human and animal research. Next, we discuss some of the special methods, procedures, and problems associated with the laboratory study of humans. Finally, we present data from our own laboratory to illustrate how variation among individuals, perhaps the most serious obstacle to the basic study of human behavior, can be approached as a suitable object of an operant analysis.

THE PREVAILING STRATEGY

The tendency to exclude human subjects from the operant laboratory is noteworthy for several reasons. Certainly the practice runs counter to longstanding traditions of experimental psychology, where there has been heavy reliance on data from human subjects in such areas as perception, learning, memory, and cognitive processes. More importantly, a division is created that seems to be at odds with the goals of behavior analysis. The development of behavior analysis reflects increasing concern with the broad application of behavioral principles to the solution of the social problems of our time. Its promise of success is said to be linked to the validity of the scientific account of behavior that serves as its foundation. However, that account has been sought almost exclusively in experiments on laboratory animals. The justification for this practice is well-known—research with animals allows control of conditions in ways that are not possible with humans. The prevailing strategy has been to link animal-based principles to the broad domain of human behavior in two ways: through interpretations of instances of complex human behavior as they may occur in natural environments, and through application of the principles in efforts to solve human problems.

Legitimate questions can be raised about whether this strategy will lead to a comprehensive understanding of human behavior. A criticism voiced with increasing frequency from within as well as without the field is that behavior principles derived from infrahuman organisms are overly simplistic and cannot deal with the complexities of human conduct (e.g., Chomsky, 1959; Schwartz, Schuldenfrei, & Lacey, 1978). Nativistic and ethological conceptions of behavior frequently are cited to support the argument that straightforward extrapolations from animal to human behavior are not possible. Animals at different phylogenetic levels are said to possess innate mechanisms that prepare them in different ways to associate stimuli or to be influenced by response-contingent events (Bolles, 1970; Seligman, 1970). Thus, the particular stimuli and responses chosen for laboratory study and their interactions with the subject's phyletic characteristics become critical limitations of whatever principles may emerge. The conclusion follows that the human organism may possess its own unique versions of preparatory and species-typical mechanisms, and that only through direct study can information be gained about the mechanisms governing human behavior.

There are, of course, alternative views of the contribution of innate factors to complex human behavior. We prefer the position that regardless of the importance of instinctive mechanisms for the operant behavior of animals, the human lacks such mechanisms or at least they do not play important roles. But whatever the role ascribed to innate factors, insofar as they limit the behavior of laboratory animals, the value of such research as a way of explicating human behavior is correspondingly limited.

Differences of opinion about animal research reflect the lack of a consensus about the sort of evidence needed to establish the generality of animal-based principles. One tactic which antedates operant psychology (e.g., Guthrie, 1935) is that of noting the appropriateness of the principle for instances of naturally occurring human behavior, what Verplanck

(1955) called "extrapolation by analogy." Thus, instances may be found on the human level that show relationships among responses, reinforcers, and stimuli paralleling those studied under laboratory conditions. This approach is easy to criticize. For example, one reviewer of Holland and Skinner's (1961) programmed text took the authors to task for adopting what he termed "anecdotalism," the discredited practice of basing conclusions on reported instances not observed under adequate conditions of control and recording (Thouless, 1963). A frame of the program described the case of a man who brought candy to his wife to end an argument with the consequence that she became even more argumentative. The reviewer suggested that this might be an expected outcome from laboratory studies in which pigeons' keypecks have been reinforced by food, but empirical research is needed to determine whether wives react as simply and as predictably to gifts of candy. Skinner (1963) responded that he intended no more than to illustrate the possibility of a behavioral analysis of complex events. Presumably the further questions of whether the principle is effective calls for more elaborate procedures.

In a different context, a political scientist characterized Skinner's analysis of social institutions as reductionistic and analogical rather than empirical (Watts, 1975). In response, Skinner (1975) compared his efforts to those of modern astronomy, where observations of "events occurring in outer space" under conditions "beyond any hope of experimental control" are subsequently interpreted in light of "facts about gravity, radiation, pressure, temperature, and so on, obtained under the controlled conditions of the laboratory" (p. 228). Skinner did not consider, however, the extent to which behavior analysis actually has access to data equivalent to those collected in the astronomical observatory, in other words information about naturally occurring behavior based on systematic and agreed-upon methods. Problems in this regard were discussed by Bijou, Peterson, and Ault (1968) who concluded that in the field of child development, at least, acceptable data are sadly lacking. While it is the case that various disciplines within the social sciences routinely study human behavior as it ordinarily occurs (using such field techniques as surveys, interviews, and direct observation), the results typically are interpretations about behavior rather than straightforward descriptions, and they usually are expressed within a theoretical framework at odds with that of behavior analysis. Bijou et al, went on to propose behavioral methods that might lead to acceptable descriptive data comparable to those obtained in the laboratory, but use of their methods is infrequent.

A different way of testing the generality of animal-based principles is to apply procedures suggested by the principles to the solution of practical problems. Thus, operant conditioning principles may serve as the basis of programs designed to modify the behavior of pupils in the classroom or patients in the psychiatric hospital. The success of the program then is taken as support for the validity and generality of the principles in question.

Successful development of such a behavioral technology and its expanding use in a range of applied settings provides considerable evidence in support of animal models of behavior. However, success in this regard should not be taken to mean that applied research simply can be substituted for laboratory analysis in the development of basic principles. As emphasized by Baer et al. (1968), the purposes of applied research create serious obstacles for the investigation of basic questions. Of necessity, applications of behavioral principles concentrate on amelioration of behaviors viewed as maladaptive, and research in applied settings must be conducted in ways consistent with the therapeutic goals of social agencies. By comparison, thorough tests of the implications of animal-based principles require freedom to investigate a range of behaviors, environments, and variables selected strictly in terms of theoretical considerations.

CONCERNS ABOUT THE LABORATORY STUDY OF HUMAN BEHAVIOR

Study of human behavior within the operant laboratory is an obvious link between the scientific base of behavior analysis and its extension to the domain of complex human behavior. Unlike interpretations and descriptions of behavior in natural environments, laboratory research creates the possibility of manipulating and controlling variables of interest. And unlike applied research, laboratory research allows investigations that are unencumbered by therapeutic considerations.

But, as noted initially, laboratory studies of human behavior are infrequent. For some researchers, the issue may not be whether laboratory study with humans is desirable but whether it is possible. In contemplating such an effort, procedural and analytic questions arise which do not appear to have clear counterparts in either the animal laboratory or the clinic. Particularly for researchers accustomed to working with animals, the problems—real and anticipated—may seem formidable. The history of the human subject prior to the experiment cannot be ascertained directly and the environments the subject inhabits between experimental sessions usually are outside the limits of experimental control. In selecting subjects for such research, decisions must be made about personal characteristics such as the subject's age, educational background, or gender. Complicated issues are created by the verbal capabilities of humans and the need to provide them with justification for participating in the research. All of these problems may be exacerbated by social and ethical considerations, including the need to comply with Federal standards for the conduct of research with human participants.

While these are legitimate concerns, our contention is that they should not create undue pessimism about the experimental analysis of human behavior. One response is to note that equally serious problems of control and analysis potentially are present within the animal laboratory. There, the novice researcher is prone to a variety of errors in arranging deprivation levels, shaping responses in the experimental chamber, controlling extraneous variables, and so forth. The important difference from the state of affairs when the subject is human is the availability of solutions to these problems, solutions developed during the history of the experimental analysis of animal behavior that now form part of standard laboratory practice. A second response is that satisfactory degrees of experimental control, in fact, can be accomplished in research with human subjects. A variety of examples can be found in the published literature where the standards of control were comparable to those of the animal laboratory. Finally, concerns about the rigor and precision of work with human subjects are misdirected if research ostensibly conducted within an operant framework does not actually employ the procedures needed for an operant analysis. Consequently, interpretive problems are inevitable if observations are terminated before performances are stable or if behavior is studied in too few conditions to allow functional relationships to be discerned. (Of course, laboratory studies with animal subjects are not immune to this last criticism.)

The conceptual and methodological issues in human laboratory research undoubtedly are complex, and they lead to a variety of interrelated questions about the suitability of humans as subjects. Not all of these issues can easily be laid to rest. However, it is worthwhile to consider some of the more obvious concerns and to suggest some remedies based upon our experiences.

"Demand" Characteristics

A pervasive concern is that special reactions of humans to the laboratory environment—the so-called "demand" characteristics of the experiment—interfere seriously with the systematic analysis of experimental variables. In this regard, some social psychologists have characterized laboratory research as a complex interaction between subject and experimenter in which the subject's expectations, attitudes, and beliefs about the research may influence the results in important ways (e.g., Orne, 1962). Subjects are described as playing various roles such as that of the "cooperative subject" who behaves in ways designed to corroborate what he views as the experimenter's hypothesis, or the "faithful subject" who, in the interests of producing honest results, leans over backwards not to produce the hypothesis-implied behavior. Such reactions are viewed as a serious threat to the experimental study of human behavior, to the extent that some social psychologists have proposed that laboratory work be abandoned or, at the least, modified in major ways (e.g., Kelman, 1967).

Are such concerns appropriate when human operant behavior is analyzed in the laboratory? Although we know of no operant research explicitly directed toward this issue, it is not unusual to observe the sorts of behaviors that have attracted the attention of social psychologists. For example, subjects in our laboratory may seek unauthorized information about the procedures, either through direct questions (e.g., "What am I supposed to do?") or through expressions of hypotheses about the contingencies to which they have been exposed (e.g., "I think I can make the light come on by making three long taps and one short tap on the key"). Such reactions are an expected consequence of the complex history that the subject brings to the laboratory. Although this history undoubtedly can complicate an operant analysis, three interrelated features of operant methods serve to counteract its influence.

First, the extended series of experimental sessions usually required by a steady-state analysis provides an opportunity for pre-existing reactions to extinguish. Our experience has been that comments and questions of the sort mentioned above are infrequent following early sessions. Progressive weakening of these and similar reactions allows control to be assumed by the contingencies and other forms of controlled stimulation introduced into experimental environments.

Second, free-operant methods reduce the opportunity for inadvertent reinforcement of pre-existing reactions. Because direct social contact between subject and experimenter is not an essential part of the procedure, experimental sessions can be held in an isolated chamber with remote programming of experimental variables and recording of responses. Within the limits of courteous and considerate treatment, unnecessary conversations can be minimized during rest periods outside the chamber and when subjects are entering and leaving the laboratory.

Third, the behaviors studied in an operant analysis can effectively compete with pre-existing reactions because of the explicit contingencies that are manipulated in the research. Many aspects of natural work situations can be approximated in the laboratory, particularly when subjects are paid on the basis of their performance. In both cases, responses are performed repetitively, involve varying degrees of skill, and produce immediate consequences—such as response feedback or completion of one piece of work and advancement to the next—and more remote consequences, of which the most important is the payment the worker or experimental participant receives.

Thus, a case can be made that operant procedures, although developed originally for the study of laboratory animals, are ideally suited for the study of human behavior. The procedures appear to minimize several of the problems associated with traditional methods, whose unclear contingencies and brief durations may provoke behaviors that obscure the variables of interest.

Ethical Questions

A different concern is that ethical constraints may restrict research procedures too severely. Certainly ethical as well as legal considerations severly limit use of procedures that might expose subjects to harm. Subjects must be told about potential risks so that they may give "informed consent" to participate in the research, and they must be allowed to withdraw their participation at any time. While these requirements complicate laboratory study in various ways, it is a mistake to conclude that they pose insurmountable obstacles for the operant researcher.

The most difficult problem, perhaps, pertains to the informed consent requirement. What information should one or must one give to the subject? Informed consent has been a particularly thorny problem for traditional researchers in such areas as personality and social psychology because of their use of procedures involving various degrees of deception. For the purposes of the research a subject might be told that he has failed badly on a test (when he actually has not) or that others rated him as singularly unattractive (when this was not, in fact, the case). By comparison, deception usually is not an essential aspect of operant procedures. As a rule, there is no reason why subjects cannot be given full information about the events to which they will be exposed, including those which might lead to discomfort—for example, that electric shock will be delivered or that there will be periods when monetary payment will be suspended.

It is important to emphasize that the informed consent requirement pertains only to aspects of the procedure that create risks for the subjects. Thus, the operant researcher need not describe essential details of the procedure, such as the specific contingencies of reinforcement, the sequences of discriminative stimuli, and so on. Furthermore, it is quite appropriate to use procedures involving misinformation, such as giving subjects inaccurate information about the reinforcement schedule (e.g., Galizio, 1979), as long as such manipulations do not create new risks beyond those already within the procedures.

Aversive Control

Ethical questions are most acute when aversive control is the topic of investigation, and the reasons why such control is studied most easily with animals or through naturalistic observation do not require extensive comment. These questions should not, however, exclude laboratory study with humans as a vehicle for clarifying relationships established with other methods. We will pay special attention to aversive control because of the important role it plays in a range of complex behavioral processes related to social behavior, child development, psychosomatic disorders, self-destructive behavior, and the like.

Given the potential ethical problems, the number of laboratory studies of aversive control with humans is, perhaps, surprising. Less surprising is that very few of these have investigated control by noxious events such as loud noise (e.g., Azrin, 1958) or electric shock (e.g., Ader & Tatum, 1961). More commonly, the aversive events have involved loss of, or timeout from, positive reinforcement, for example, monetary loss (e.g., Stone, 1961), timeout from schedules of monetary reinforcement (e.g., Zimmerman & Baydan, 1963), and loss of points (e.g., Weiner, 1962). Other aversive events have included termination of room illumination (Shipley, Baron, & Kaufman, 1972), timeout from viewing a cartoon movie (Baer, 1960), and increased work (Miller, 1970). All of the usual schedules of aversive control have been investigated, at least in a preliminary way, including escape (e.g., Azrin, 1958), avoidance (e.g., Stone, 1961), punishment (e.g., Zimmerman & Baydan, 1963), and the conditioned emotional response paradigm (e.g., Remington & Strongman, 1970).

Many of the ethical and social problems created when noxious stimuli are the aversive events are circumvented when control is by timeout from, or loss of, positive reinforcement. Our assessment of studies using these procedures is that timeout and loss are at least the equal of noxious events in the aversive control of behavior, insofar as subjects respond in a sustained manner to avoid timeout from a schedule of positive reinforcement or show substantial response suppression when responding results in loss of reinforcement. However, some writers have raised the question of whether it is proper to term such control "aversive." Leitenberg (1965) argued that control of behavior by schedules of timeout or loss usually is accompanied by an increase in the frequency of positive reinforcement. If control can be explained in terms of established principles of positive reinforcement, explanations in terms of aversiveness are seen as superfluous.

The various ramifications of the argument that timeout and loss are not aversive events, even when incorporated into schedules of aversive control that maintain responding, cannot be discussed in detail here. However, note should be taken of some justifications for their continued study: (a) There is a certain abitrariness in advocating that behavior should be accounted for in terms of positive reinforcement in preference to accounts in terms of negative reinforcement. Some reinforcement theorists have proposed the reverse (cf. Hull, 1943), and Michael (1975) has proposed that the distinction be abandoned. (b) Accounts emphasizing relative differences in reinforcement frequency assume the existence of complex discriminative and integrative capacities which, to a large degree, remain to be established. By comparison, the simpler assumption that timeout and loss are aversive events permits analysis in terms of the onset, offset, or postponement of specifiable stimuli. (c) The operations involved in scheduling timeout and loss, by comparison with those involved in scheduling positive reinforcement, are not easily translatable. Although responding on schedules of timeout or loss may lead to net increases in positive reinforcement, it does not follow that such schedules are functionally equivalent to schedules of positive reinforcement.

For the above reasons, it seems premature to conclude that the positive reinforcement account necessarily is the appropriate one. Further, even if such an account eventually can be developed, significant extensions of knowledge about positive reinforcement still may result from continued study of timeout and loss within aversive schedules.

EXPERIMENTAL ANALYSIS OF INDIVIDUAL DIFFERENCES

A special set of concerns about laboratory research with humans pertains to the range of behavioral differences that subjects bring to the laboratory. Here we are referring to what have traditionally been called "individual differences," that is, those differences associated with age, gender, intellectual ability, personality, and so forth. It is important to come to grips with such differences because of the serious obstacles they pose for the laboratory study of human behavior.

Experimental control over the behavior of individuals is the hallmark of operant analysis. Acceptable levels of control are recognized when functional relationships between experimental conditions and behavior can be replicated within a given subject and from one subject to another. When infrahuman animals are the subjects, a researcher may increase the likelihood of successful replication by specifying the subject's environment in minute detail, both within and without the experimental setting. In many instances an animal's entire life history can be specified, including such details as the size, content, and scheduling of meals, dimensions of living space, and distribution of time spent in light and dark. To ensure replicability across animals, subjects can be selected from the same species, strain, cohort, sex, and even the same litter. The aim of these tactics is to reduce within-subject and between-subject variability, but obviously such close control cannot be accomplished in the study of human behavior, and the consequence is that the range of individual variation is large and difficult to specify.

Although individual differences among human subjects pose obstacles for a systematic analysis, various remedies are available. Classifications of human differences frequently reflect social and cultural concerns more than scientific ones. Consequently, differences that customarily are ignored or deemphasized in animal research may assume exaggerated importance when humans are the subjects. For example, a researcher working with rats may be indifferent as to whether the subjects are 6 months old or 12 months old, and use subjects of both ages interchangeably. But this age difference is equivalent to 15 or 20 human years by one estimate, and potential differences in the performances of 20-year-olds and 40-year-olds would attract attention in human research. For the operant researcher, lack of appropriate data makes it difficult to decide whether age differences of this magnitude can safely be disregarded in operant research with humans (or should be taken into account in research with rats).

In considering which of the various human differences may influence the outcome of a particular experiment, the issues are not all that different from those encountered in the animal laboratory, where the potential influences of subject characteristics are reduced by selecting individuals from circumscribed groups. A similar strategy is available in research with human subjects, where the prudent researcher can seek uniformity by studying individuals of the same age, gender, educational background, and so forth. From this standpoint of experimental control, development of guidelines concerning which personal characteristics should be held constant in this way requires research contrasting performances of individuals with different characteristics. From a more theoretical standpoint, individual differences can be viewed not so much as a problem of control but rather as a set of variables worthy of study in their own right.

In the main, operant researchers have not regarded the study of individual differences as a legitimate subject matter. Perhaps this is because they identify such interests with research in the area of personality, where group-statistical traditions and mentalistic theories are at odds with the radical behavioristic underpinnings of operant analysis. However as several authors have noted, the study of individual differences is not the sole province of personality theory (Brogden, 1972; Cronbach, 1957; Hull, 1945). Other areas of psychology that are perhaps more palatable to behavior analysts, such as comparative and developmental psychology, also are concerned with individual differences, either across species or at different points in the lifespan of individuals within a species.

A more likely explanation for the lack of interest in individual differences may be found in some of the methodological prescriptions that have influenced the development of behavior analysis. In a classic treatment, Sidman (1960) argued against searching for differences in the behavior of various subjects, emphasizing instead the importance of uncovering similarities across subjects in terms of common behavioral processes, controlling variables, and experimental techniques. Developing this theme further, Johnston and Pennypacker (1980) argued that "detailed quantitative differences among subjects should be distinctly secondary in interest to the universality of the form of functional relations" (p. 402).

The advocated strategy concerning individual differences is nicely illustrated by a set of cumulative records published a quarter-century ago by Skinner (1956).

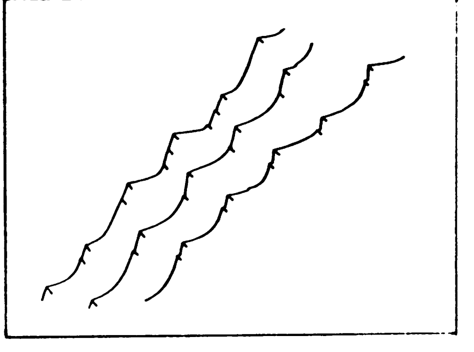

FIGURE 1. Cumulative records of responding under a multiple FI FR schedule. One of the records was made by a pigeon, one by a rat, and one by a monkey. Which is which? (From Skinner, 1956, p. 230). Copyright (1956) by the American Psychological Association. Reprinted by permission of author.

Figure 1 shows the records, which were obtained from three organisms responding on a multiple fixed-interval fixed-ratio schedule. The records are indistinguishable, even though one was obtained from a rat, one from a pigeon, and one from a monkey (Skinner declined to reveal which was which). "And when orgnisms which differ as widely as this nevertheless show similar properties of behavior," Skinner commented, "differences between members of the same species may be viewed more hopefully. Difficult problems of idiosyncrasy or individuality will always arise as products of biological and cultural processes, but it is the very business of the experimental analysis of behavior to devise techniques which reduce their effects . . ." (Skinner, 1956, p. 231).

Often overlooked is that although Skinner, in this quotation, pointed to the value of identifying experimental manipulations that override the influences of subject variables, he did add an important proviso. In its entirety, Skinner's conclusion was that ". . . difficult problems of idiosyncrasy or individuality will always arise as products of biological and cultural processes, but it is the very business of the experimental analysis of behavior to devise techniques which reduce their effects *except when they are explicitly under investigation*" (Skinner, 1956, p. 231). Thus the search for common behavioral properties is not seen as being at odds with attention to individual differences, the study of their origins, or their interactions with experimental variables. One explicit investigation was contributed at an early date by Skinner himself, who collaborated with Heron in a study of extinction in maze-bright and maze-dull rats (Heron & Skinner, 1940). Later, Skinner and Lindsley compared schedule-maintained performances of normal human subjects with those of subjects suffering from varying degrees of psychopathology (Lindsley, 1962). Although the behavior of the psychotics had much in common with that of normals (and with infrahumans) under standard laboratory conditions, the focus was on the differences. The research led to objective operant techniques of psychological diagnosis and evaluation appropriate to institutional settings.

In our own laboratory, we have used research designs which combine the investigation of basic processes with the investigation of individual differences in operant behavior. This has been accomplished by replicating an experiment with individuals characterized by differences in social variables such as the subject's use of illicit drugs or more fundamental variables such as the subject's age. At the least, these procedures have the potential for strengthening conclusions about basic processes by systematically replicating experimental effects across subject types. The procedures also leave open the possibility of observing interactions between subject characteristics and basic processes. Both kinds of findings have been obtained in recent experiments.

In one study (DeWaard, 1980) the generality of the matching law relating response and reinforcement rates was investigated by comparing performances of subjects who reported habitual use of drugs with those who did not use drugs. It has been suggested that individuals who use drugs differ from nonusers in terms of the reinforcement processes that control their behavior (e.g., Cahoon & Crosby, 1972). Since performances on concurrent schedules are thought to provide an index of sensitivity to reinforcement rates (Baum, 1973), systematic differences between the concurrent performances of users and nonusers might clarify theoretical accounts of drug use in terms of reinforcement processes.

The results are presented in Figure 2, which shows the relationship between responding on two concurrently available keys and the rates of monetary reinforcement associated with the two keys. The data are from young male industrial workers; two (703 and 624) were extensive users of illegal drugs while the others (152 and 061) reported no use. Despite this major difference in the lifestyles of the subjects (the men were not under the influence of drugs during the experimental sessions), the ratios of the response rates on the two keys were systematically related to the ratios of the reinforcement rates in all four subjects. Linear equations fitted to the data accounted for 98 to 99 percent of the variance, and the slopes of the lines ranged from .58 to .84, reflecting consistent "undermatching"—preference for the response alternative providing more reinforcement was less extreme than predicted by a perfect matching relationship. These results are in accord with those obtained in animal studies, where undermatching is commonly observed (Myers & Myers, 1977). Thus, DeWaard's

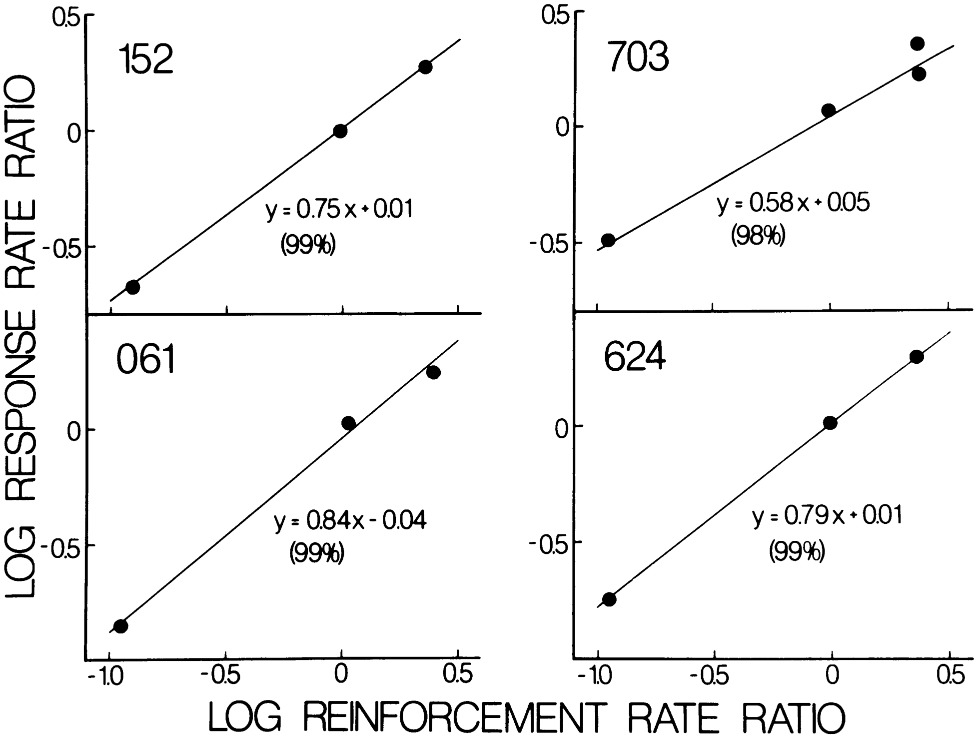

FIGURE 2. Logarithms of ratios of response rates as a function of logarithims of ratios of reinforcement rates. Subjects (young male industrial workers) responded on concurrent VI VI schedules with a 2-sec DRL contingency applied to both responses. Least-square lines fitted to each subject's data are drawn in the panels. The logarithmic equations and percent of variance accounted for by the lines (in parentheses) are given alongside. Subjects 703 and 624 were drug users; the others were not. (from DeWaard, 1980, p. 89.)

experiment not only demonstrates the applicability of animal-based findings to human behavior, but also shows that the functional relations are unaffected by major differences in the extra-experimental histories and repertoires of the subjects.

Another line of research is concerned with the role of reinforcement variables in age-related behavior. As noted by Labouvie-Vief (1977), among others, traditional conceptions of old age have overly emphasized the biological origins of aging and the conclusion that behavioral deficits may be beyond the adaptive capabilities of the older adult. The alternative approach suggested by operant methods and concepts is to seek environmental manipulations that will support more adaptive behavior (Hoyer, 1973; cf. Lindsley, 1964).

The first step in our program of research is to determine what behavioral processes, if any, may be affected by age. To this end, we have compared the behavior of young male college students with that of men enrolled in a course audit program for older adults (Perone & Baron, 1982). Figure 3 shows cumulative records taken during the initial sessions of a recent experiment. The subjects' task was to acquire a chain of 10 responses distributed over 4 keys; completed chains were reinforced with money on a variable-ratio schedule and errors produced a brief timeout. The response pen of the recorder stepped with each correct response and was reset at 5-min. intervals; below, the event pen recorded errors as downward deflections. Although all four subjects acquired the chain by the end of the second session, there are clear differences between the two young men (YJC, age 18, and YDF, age 22) and the two old men (OKH, age 62, and OAW, age 74). For the old men, acquisition was slower and the progression of within-session improvement was less certain by comparison with the young men. In addition, the terminal rates of the oldest subject (OAW) were far below those of the others.

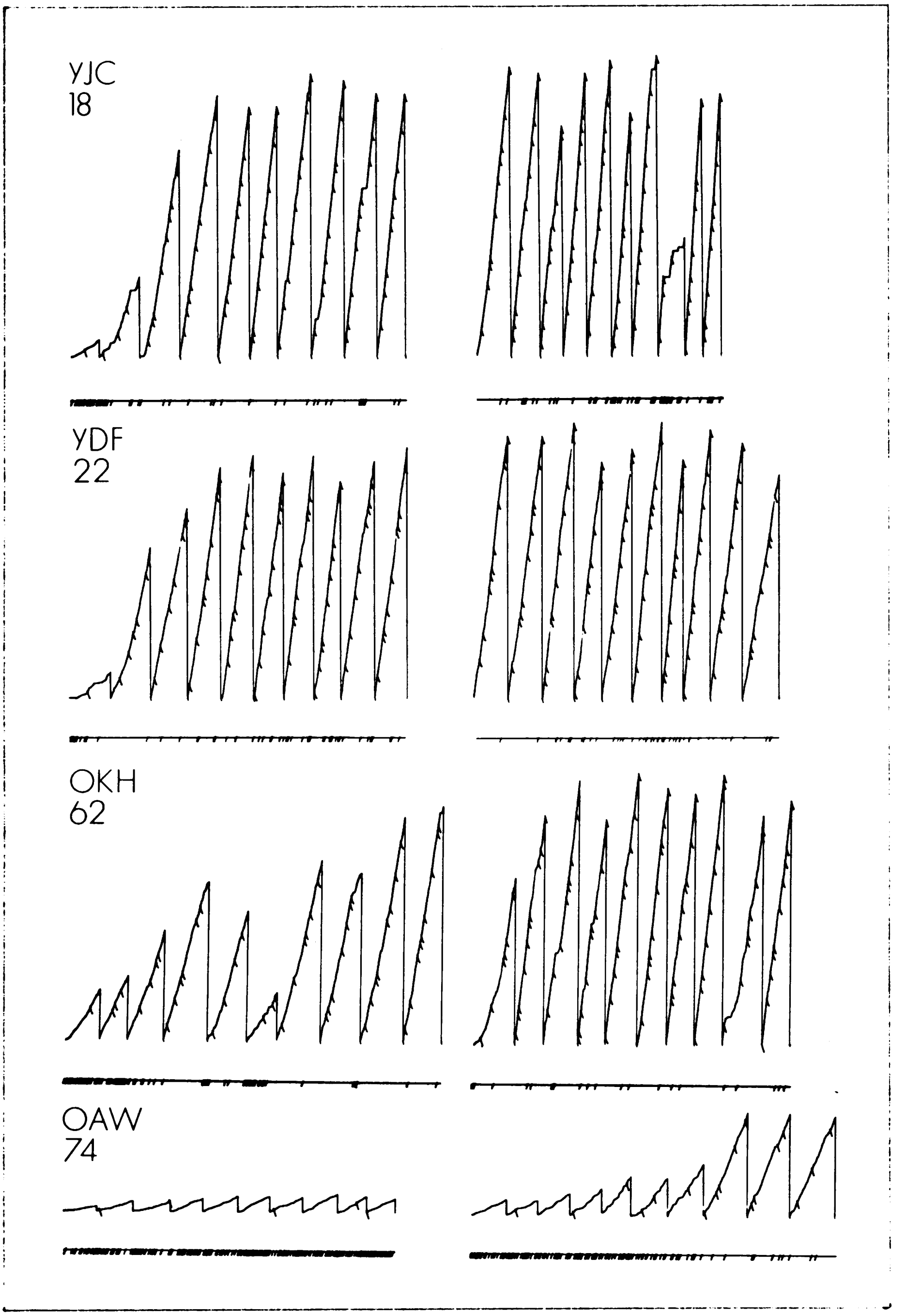

FIGURE 3. Acquisition of a chain of 10 responses by two young men (YJC and YDF, ages 18 and 22) and two old men (OKH and OAW, ages 62 and 74). The upper pen advanced with correct responses, deflected downward with reinforcers, and reset at 5-min. intervals. The lower pen deflected downward with errors. Data are from two consecutive sessions (see text for details).

In analyzing age differences such as these, a central issue is the extent to which behavioral deficits observed in older subjects represent transitory reactions to the assessment procedures rather than more general deficits. Most laboratory experiments on aging are based on limited assessments of behavior often lasting no more than an hour, and it is possible that the older individual is at a special disadvantage in such experiments. By comparison with younger subjects, older adults are more likely to react emotionally and negatively to laboratory settings (Hulicka, 1978). Additional time may be needed to allow such reactions to habituate before response capability can be assessed fairly. A further concern is that assessments based on a few observations of naive subjects may differ substantially from assessments based on later observations of experienced subjects. Studies of reaction time, for example, have shown that even this elementary aspect of behavior is improved by simple practice (Murrell, 1970). These considerations raise the question of whether the impaired performances of the older men in Figure 3 could be due to competing reactions elicited by the laboratory procedures, and whether different conclusions would be reached about age differences in learning if such reactions were allowed to habituate before testing. Operant procedures, because of their long-term nature and emphasis on steady-state behavior, would appear to be ideal for addressing these questions. Since operant research extends over a series of sessions there is considerable opportunity for subjects to adjust to the laboratory environment, and critical data are collected only when inappropriate levels of motivation and arousal have been reduced. The central goal of the operant method is to observe behavior as a function of experimental procedures and contingencies, rather than as an expression of transitory expectations and emotional reactions. Thus, the method seems unusually compatible with the study of aging.

At an earlier stage in the development of behavior analysis, it might have been defensible to ignore the age differences apparent in Figure 3 and concentrate instead on the fact that the contingencies promoted learning in all subjects regardless of age. At present, however, these individual differences in behavior are worthy of recognition and study, and we are intrigued by the prospect of locating the sources of the differences and the possibility that they may eventually be brought under direct experimental control.

CONCLUSIONS

Although the laboratory study of human behavior seems an obvious vehicle for strengthening the scientific base of behavior analysis, the place of the human subject within the operant laboratory remains problematic. The prevailing research strategy has been to link principles developed in the animal laboratory to the broad domain of human affairs, either through interpretation of naturally occurring human behavior or through application of the principles to the solution of human problems. The paucity of laboratory research on human operant behavior derives, at least in part, from several misconceptions. These include the possibility that experimental demand characteristics and pre-experimental behavioral dispositions of human subjects contaminate the results; that ethical considerations place undue constraint on the phenomena to be investigated and the research designs to be used; and that uncontrollable variation in subjects' histories and other relevant personal characteristics prevents observation of reliable functional relations between laboratory manipulations and behavior. We have argued that these problems do not pose insurmountable obstacles to the experimental analysis of human operant behavior; that adequate methods of control and analysis are available; and that operant techniques may in fact be better suited to the study of human behavior than techniques in other areas of psychology where human research is more common.

Our comments in this paper are not intended to promote the experimental analysis of human operant behavior as a specialized area of inquiry. Rather, the analysis should be seen as logically continuous with animal research and, indeed, as critical to the elaboration of a truly general theory of operant behavior. To illustrate the place we envision for the human subject in the operant laboratory, consider the current status of human research in two areas that have been studied intensively with animals: (a) the matching law and (b) response patterning under control of intermittent schedules. Recent research on the matching law has tended to confirm findings obtained with animals (e.g., Bradshaw, Szabadi, & Bevan, 1976; DeWaard, 1980), whereas a long line of studies on schedule control appear to contradict animal data in many instances (see Lowe, 1979, for a review). These cases of similarity and discrepancy are worthy of attention, for they have both practical and theoretical significance. From the standpoint of practice, it is clear that the applied behavior analyst may err if predictions about human behavior are based solely on naive extrapolations of animal findings. From the standpoint of theory, human research may support the generality of principles based on animal findings or it may call them into question. Nevertheless, whether the findings are consistent or inconsistent with animal data, research on human operant behavior is not given due attention in relevant theoretical discussions; for example, we know of no general treatment of matching or of schedule control that cites human research.

An issue we have carefully skirted in our discussion of the place of the human subject in the operant laboratory pertains to the role of cognition in human behavior. The traditional objection to the unification of human and animal behavior within a single conceptual framework has been that human behavior is controlled far more by central processes than by the actual environments to which the individual is exposed. The human subject's responses to the variables imposed in an experiment are said to be mediated by such events as thoughts, beliefs, and hypotheses, that is, by inner events not amenable to direct observation or close control. Thus, animals are the preferred subject when the goal of research is to understand behavior as a direct function of environmental influences.

Recently, efforts have been made to integrate this traditional view of human behavior with some of the contemporary issues addressed by behavioral analysis, for example, to reinterpret what is known about human operant conditioning in cognitive terms (see Bandura, 1969; Brewer, 1974; Thoresen & Mahoney, 1974). Human reactions to schedules of reinforcement are depicted as being under the control of the individual's beliefs and hypotheses about how the schedule works rather than by the actual contingencies. According to this view, a proper account of human performances must focus on these cognitive influences—on "the cognitive representations of contingencies," or on "images and thoughts about existing contingencies"—rather than on the contingencies themselves.

This conception of human behavior raises obvious questions about the suitability of humans as the subjects of an experimental analysis. But it is well to emphasize that these concerns are of quite a different sort from the methodological issues discussed elsewhere in this paper. Whereas we have suggested ways to more effectively study human behavior in the laboratory, cognitive accounts pose the more fundamental question of whether human behavior can be directly controlled at all through environmental manipulation. What we will have to say about these efforts to reinterpret human operant behavior in cognitive terms is not new. The matter has been treated in some detail by other writers who have noted and deplored this growing trend (Branch & Malagodi, 1980; Rachlin, 1977).

It seems evident that accounts of human behavior in terms of unobserved mediational processes are at odds with the basic tenets of behavior analysis. If the primary determinants of behavior are identified with unrecorded inner events, then it is unclear how such events can be studied using agreed-upon methods. But if such events are conceptualized as being ultimately determined by observable features of the environment, then the postulation of intervening processes is superfluous. Holz and Azrin (1966) expressed this dilemma in a critique of the theory that "awareness" of contingencies is a necessary condition for learning to occur:

> Such inner events are plausible . . . but the question posed by the behaviorist remains: Can external events be identified which control the behavior? An appeal to hypothetical constructs which intervene between the controlling environment and the behavior is no resolution. It simply changes the form of the question posed. If awareness is postulated, we must then ask what are the conditions which produce awareness and what conditions cause the response to occur or not once awareness exists? If these questions can be answered, the law of parsimony may again be exercised to eliminate the intervening constructs. (p. 807)

A more behavioral account of human performance in operant experiments, but one which nonetheless relies on mediating processes, emphasizes the role of covert verbal responses. According to this account, as recently discussed by Harzem, Lowe, and Bagshaw (1978) and Lowe (1979), human subjects may verbally describe the contingencies to themselves and such formulations may then serve as stimuli controlling the rate of the reinforced response. One source of these verbalizations are instructions provided by the experimenter, but even when information about the contingencies is not provided, the human subject develops his or her own description of response-reinforcer relationships. Thus, as with explanations couched in more cognitive terms, the reinforced response is seen as under the control of a mediating system ("what the subject says to himself"), in addition to whatever influences are exerted directly by the contingencies.

Shimoff, Catania, and Matthews (1981) recently discussed the view that human operant performance is controlled by covert verbal responses. They noted that information about hypothesized verbal mediators usually is based on verbal reports during post-experimental interviews, and went on to comment:

> In an experimental analysis, our task is not to treat verbal reports as causes, but rather to see how verbal reports, like the nonverbal behavior they accompany, are affected by experimental variables. Both the status of such reports and their correlation with other behavior are problematical. Uncertainties will persist in the absence of adequate accounts of verbal reports as responses. The development of procedures to make possible such an experimental analysis remains an important challenge. (pp. 218-219)

The point at issue, then, does not concern the plausibility of the assumption that covert verbal responses may accompany human performances in the laboratory. What is at issue is the place such unrecorded events should play in the analysis. At the moment there are no convincing procedures which can reveal directly whatever covert verbal behaviors occur during the course of operant conditioning experiments. Also obscure are the procedures which might be followed in an experimental analysis of control by such behaviors over the responses actually recorded by the experimenter. When human subjects behave in inexplicable ways in the laboratory, it is tempting to attribute the failure of the analysis to the human organisms' complex verbal and cognitive abilities. More constructive, perhaps, is to regard such failures in the same way as one would similar outcomes in research with animal subjects—simply as instances in which the environmental contingencies controlling the behavior of interest remain to be identified.

We are convinced that the laboratory analysis of human behavior can contribute to operant psychology. But before it can contribute, it must gain acceptance as a valid approach to the basic study of the behavior of organisms. The aim of our discussion has been to dispell some misconceptions about human operant research, and, in so doing, encourage its acceptance and further its development.

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