COGNITION, BEHAVIOR, AND THE EXPERIMENTAL ANALYSIS OF BEHAVIOR

CHARLES P. SHIMP

UNIVERSITY OF UTAH

A case history illustrates how one research program in the experimental analysis of behavior evolved somewhat differently from the modal research program represented in this journal. A chief issue that seems to be responsible for this difference is the role attributed to theory in behavioral research: Skinner's views on the nature and function of theory and on the nature of observation combine to produce a certain kind of picture of behavior. The classic conception of reinforcement contingencies is tied to this particular picture. But this picture may be incompatible with, and certainly is different from, other possible pictures. Reinforcement contingencies that place greater emphasis on the local temporal patterning of behavior seem tied to some of these alternative pictures of what behavior is. These other pictures encourage a wide range of theoretical approaches, including cognitive ones, various kinds of mathematical analyses, and computer-simulation methods to characterize entire behavior streams. In the future, perhaps the experimental analysis of behavior will accept a somewhat different range of views on the nature and function of theory, a correspondingly different set of experimental methods, and alternative ways of talking about behavior.

Key words: molar theory, molecular theory, cognition, observation, associationism, computer models

---

To predict what a scientific movement such as the experimental analysis of behavior will look like several years in the future must seem extraordinarily presumptuous to one who, like the present writer, feels consternation that he can not predict what a pigeon in a highly controlled environment will do in the next several seconds.

It was my misfortune never to have had the pleasure of meeting Don Hake. It is my understanding that his generosity was such that he might have permitted me to dedicate this paper to him, even though he might have disapproved of parts of it.

When one reviews the history of his own research program, as I do here, one is struck forcibly by the magnitude of his intellectual indebtedness to his teachers, through both explicit instruction and personal example. The truth, appalling though it is to this writer, is that there is no important idea in the present case history that can not be traced to the work of D. S. Blough, R. B. Millward, G. H. Bower, or W. K. Estes, except perhaps for an idea or two about contextualism, and those are freely adapted from the writings of L. Wittgenstein and N. R. Hanson.

Reprints may be obtained from the author, Department of Psychology, University of Utah, Salt Lake City, Utah 84112.

Perhaps the future could be predicted more knowledgeably if we better understood the past. Within the experimental analysis of behavior we have Skinner's (1956) case history as one kind of helpful role model for describing the past. It seems uniquely fitting to begin an analysis of the future of the experimental analysis of behavior in the inductive spirit of a case history focusing on the behavior of a single organism. The specific history I recount is merely my own and, unlike Skinner's, it involves none of the events that importantly shaped the form of the experimental analysis of behavior. Yet, the evolution of this particular research program raises a number of conceptual and methodological issues that may be relevant to the future of the experimental analysis of behavior.

The research program began with a narrowly defined problem: the utility of ideas about optimality for the description of steady-state behavior in discrete-trial probability-learning situations. At present the program involves the use of computer-simulation models, based on underlying

mechanisms of short-term memory and associative learning, to describe and explain entire behavior streams in a variety of situations. In describing the evolution of this program, I will emphasize the critical theoretical choice points at which it diverged from the modal behavioral research strategy described in this journal. Methodological, theoretical, and philosophical issues raised at these critical points will be discussed. A considerable portion of this discussion is focused explicitly on views promoted by B. F. Skinner. Although instances of research programs within the tradition of the experimental analysis of behavior can be found that importantly differ from Skinner's, still, a sociological analysis of the field (e.g., see Shimp, 1976) shows that its dominant characteristics, having to do with what constitute appropriate experimental methods, with the nature of theory, and with ways of talking about behavior, importantly resemble those advocated by Skinner.

HISTORICAL CONTEXT

Many of the issues to be developed here were implicit in the literature on probability learning in the 1950s and 1960s. In mathematical learning theory, clearly articulated theoretical mechanisms were designed to explain the statistical structure of response protocols from initial acquisition to asymptotic levels (Estes, 1959). Among these proposed mechanisms were rules for sampling from theoretical populations of stimulus elements, specific causes for changes in the conditioning status of those elements, and quantitative descriptions of those changes. According to such a position, the overall relative frequency of a particular choice did not directly measure, reveal, define, or reflect a link in a causal chain or any causal mechanism. Such a statistic was simply a number that had some utility in diagnosing problems with a particular theory. Other statistics, especially those describing sequential properties of a response protocol, ultimately proved to be more diagnostic of a theory's strengths and weaknesses (Anderson, 1964; Myers, 1976).

While such molecular theoretical approaches were being developed, other lines of inquiry were attributing to overall choice proportion a more grandiose, more direct theoretical interpretation that made this statistic a direct reflection of basic psychological processes. Thus, Bitterman and his colleagues used this choice proportion in discrete-trial probability-learning situations to develop ordinal rankings of animal intelligence (Bitterman, 1965), and elsewhere overall steady-state choice proportions from certain "free-operant" situations were used to define or directly measure relative response strength and corresponding strengthening properties of reinforcement (Herrnstein, 1961). This use of overall choice proportion was sharply different both from the position of mathematical learning theory (Estes, 1959) and from that of Skinner (1950), who adopted a more molecular approach involving an explanation of overall choice proportion in terms of combinations of various local choice probabilities. The molar interpretation, however, became the dominant one within the experimental analysis of behavior.

A CASE HISTORY: ISSUES IN THE EVOLUTION OF A MOLECULAR THEORY

When I began to conduct empirical research on choice in animals, I found that the overall choice probabilities were adequately described neither by existing versions of mathematical learning theory nor by molar response-strength ideas (Shimp, 1966). However, if more properties of behavior than just the overall choice proportion were taken into consideration, the data seemed more compatible with mathematical learning theory than with molar response-strength views. The idea that underlying psychological processes, such as those quantified by mathematical learning theory, were responsible for the local structure of behavior seemed to be a more flexible one and more likely ultimately to be able to assimilate a broader range of data than the

idea that overall choice proportion directly revealed, defined, or indicated basic strengthening properties of reinforcement. More specifically, I interpreted overall choice proportion to be a statistic that was not descriptive of any controlling relation, not a link in a causal chain, not part of a causal explanation—in short, a statistical by-product (Shimp, 1966). At the same time, I followed Anderson (1964), Estes (1959), and others, and advocated the idea that causal mechanisms were to be uncovered through analysis of the sequential structure of behavior.

The evolution of the analysis of choice next passed through two significant steps. First, Nevin (1969) gave an example of a situation in which a statistic descriptive of the local structure of behavior failed to track local reinforcement contingencies. It was as if the overall, molar, choice proportion *did* reflect a basic controlling relation. And it was as if the local structure of behavior was insensitive to local contingencies and was irrelevant to the description of a molar causal analysis. Second, Herrnstein (1970) explicitly elevated the empirical matching relationship to the status of a law. Subsequently, the predominant theoretical analyses of choice have been of the same general type as Herrnstein's: molar in the sense that a behavior stream lasting several hours is described by one or two numbers (e.g., the mean rate or relative rate of key pecking), the analysis ignores local structure (e.g., distributions of interresponse times, changes in local response rate), is usually restricted to "steady-state" performance that is defined in terms of arbitrary stability criteria, and is algebraic in format (e.g., Baum, 1973, 1981; Hanson & Timberlake, 1983; Prelec, 1982; Rachlin & Burkhard, 1978; Rachlin, Kagel, & Battalio, 1980; Staddon & Motheral, 1978). Thus, the dominant theoretical trend in the study of choice in the experimental analysis of behavior has for some time corresponded more closely, in its reification of overall choice proportion, to Bitterman's (1965) analysis of probability learning than to that of more molecular approaches such as mathematical learning theory.

While the experimental analysis of behavior moved in the direction of molar theory, I began to collect empirical relationships of the type that seemed relevant to the development of a molecular theory of operant behavior. In particular, because overall choice proportions seemed to me to be no more likely to be diagnostic of causal mechanisms in free-operant behavior than they had been in discrete-trial probability learning, I began to study how various reinforcement parameters affected the local temporal structure of behavior. The resulting work described behavior mostly in terms of interresponse times, interchangeover times, and distributions of run lengths (Shimp, 1975, 1976, 1982b). As it turned out, when reinforcement was made dependent upon various kinds of local temporal patterns of behavior, the results nearly always revealed powerful control, were consistent with a molecular analysis, and seemed outside the scope of a molar analysis.

The literature that developed subsequently on ways in which reinforcement determines the local temporal organization of behavior seemed to be diagnostic of the dimensions of behavior, and this, in turn, had implications for the nature of a reinforcer and for the determination of the experimental procedures useful in clarifying the basic nature of operant behavior (Shimp, 1975, 1976). However, researchers mostly continued to use the experimental methods developed in the 1930s and to promote molar theories that reified mean response rate. Molar theorists did not clearly articulate why a molecular analysis was inappropriate, irrelevant, or unnecessary. Rather, their theories simply ignored molecular data without explanation or justification, even though, with the exception of a very few empirical results (Catania, 1963; Nevin, 1969), there seemed little that could be interpreted as direct support for a molar position; that is, very few instances could be identified in which molar phenomena remained invariant over changing local patterns of behavior.

In contrast, some of those who were developing molecular accounts tried more

clearly to justify their approach and to put it in the perspective of larger theoretical issues. It seemed incumbent on molecular theorists, that is, to try to uncover the implicit, unstated assumptions of the molar approach that might explain why molecular results, which perhaps were incompatible with these assumptions, were ignored.

Several articles in this journal and elsewhere analyzed the problem (Hale & Shimp, 1975; Peele, Casey, & Silberberg, 1984; Platt, 1979; Shimp, 1976; Silberberg, Hamilton, Ziriaux, & Casey, 1978). A wide range of conceptual and methodological issues were shown to be relevant to the molar-molecular distinction. Among these were the following. Skinner had argued that mean response rate was the fundamental datum and the principal dimension of behavior (Skinner, 1950, 1961, 1966). I have already noted that, while molar analyses tended to reify this variable to the point where it was thought directly to express causal laws (Herrnstein, 1970), molecular analyses relegated it to a much less important status of rather arbitrary and unrevealing statistic (Hale & Shimp, 1975; Shimp, 1969, 1975, 1976). These opposing views on the significance of mean response rate led to corresponding differences in the judged suitability of different reinforcement contingencies. According to the molar view, classical schedules such as variable-interval schedules were, and still are, particularly suitable for the discovery of basic behavioral laws because they are assumed to allow mean response rate to vary meaningfully as a function of independent variables. That is, they allow it to vary without complicating issues of large-scale temporal patterns of the type that are observable in a cumulative record and without requirements imposed on the local structure of behavior. But according to the molecular view, these schedules mix together in a quite complex manner the reinforcement of different local patterns of behavior and do not permit the direct expression of causal laws (Shimp, 1976, 1979, 1982b).

According to the molar analysis, a key

peck is a suitable unit of analysis because it is freely occurring, has a mean rate that can and does vary over a wide range, and seems to express smooth and orderly relationships (Skinner, 1950, 1961, 1966). According to molecular analysis, a key peck is unlikely to be a unit of analysis by virtue of the strong possibility (Hawkes & Shimp, 1975) that reinforcement operates on local temporal patterns of behavior, rather than on the behavior temporally contiguous with a reinforcer (Skinner, 1950, 1961, 1966) or directly on long-term mean rate of pecking (Baum, 1973); as I described in detail previously, it is, therefore, doubtful that there is such a thing as a "free operant" (Shimp, 1975, 1976). The concept of a "free operant" implicitly makes assumptions about the temporal duration of an operant (that it is instantaneous), about the role of behavior-reinforcer contiguity (Catania, 1971; Hawkes & Shimp, 1975), about the nature of the underlying decision-making (Shimp, 1975), and about the role of direct observation in the determination of the unit of analysis (Shimp, 1975)—all of which are at best controversial.

According to the molar analysis, a cumulative record directly reveals the fundamental datum (Skinner, 1950); according to the molecular analysis, it is a macroscope that conceals the local behavioral regularities that diagnose controlling mechanisms (Estes, 1959; Shimp, 1975). The cumulative record also seemed especially useful according to certain ideas about the nature of direct observation: One can see mean response rate as a line on a cumulative record. Skinner (1956) has gone so far as to say one sees behavior itself in a cumulative record. However, one can not ordinarily, in this sense, see the individual interresponse times and other local patterns involved in a molecular analysis.

I have argued (Shimp, 1976) that these differences between molar and molecular approaches resemble to a remarkable degree the differences between two different approaches to human verbal learning—that represented by Postman and Underwood (1973), which derived from simple assump-

tions about associative learning, and that of Tulving and Donaldson (1972), Anderson and Bower (1973), and many others. In this earlier paper, I described how the latter, corresponding to a molecular analysis of operant behavior, involves units of analysis having significant organization and temporal patterning, but a "free-operant," by sharp contrast, is viewed as essentially instantaneous (Skinner, 1950, 1961). The molecular analysis replaces the idea of contiguity with one of short-term memory, and adopts a flexible position on the nature of acceptable theoretical concepts, especially insofar as their relation to direct observation is concerned (Shimp, 1976).

The preceding description of differences between molecular and molar analyses is one that makes sense from a molecular perspective. From a molar perspective, both the preceding description of a molar analysis itself and of the differences between it and a molecular analysis may seem importantly inaccurate. For example, a molar theorist might want to argue that he/she has no commitment to temporal contiguity, because behavior is averaged over long time periods, or because sometimes records are kept of time allocated to a position in a chamber or time allocated to responding at a fixed basic "tempo" (Gilbert, 1958). But in all these cases, the local temporal structure of behavior is ignored (Baum, 1975), or is assumed to be random, as in the case of a fixed tempo with random interburst times, such that mean response rate can reveal causal laws (Rachlin, 1978; Skinner, 1956).

There is still another way in which a molar theorist might assert that the preceding list of differences between molar and molecular analyses is either incorrect, inapplicable, or irrelevant. Skinner (1956), in his description of the invention of schedules of reinforcement, makes no mention of a commitment to theoretical assumptions, such as to temporal contiguity. Furthermore, it is plausible that if asked, he would say that he had no such theoretical idea in mind when he invented these contingencies. In this sense, he did not invent them to be consistent

with a theoretical commitment to temporal contiguity. Yet, I have argued both that a variable-interval schedule of reinforcement, with a key peck or lever press as the unit of analysis and mean response rate as the fundamental datum, is just what one would invent if one had such a theoretical commitment, and that it clearly is not a method one would use if one had a theoretical commitment to a molecular analysis (Shimp, 1975, 1976). The use of a variable-interval schedule would seem to involve these molar commitments, whether the user is aware of it or not.

So far my case history has suggested that a research program in the molecular analysis of behavior seems incompatible with some of the theoretical assumptions that underlie molar analyses. These molar theoretical assumptions deal with what behavior is and what a reinforcer does to it. Now let us turn to another related theoretical commitment having to do with the nature of observation and its relation to what behavior is.

OBSERVATION AND BEHAVIOR

The behavior of observing has a special status in the experimental analysis of behavior. Like his position on theory, Skinner's position on the nature of observation is to an important degree only implicit: Nowhere does he explicitly state his position and relate it to alternative possibilities, so we can determine only moderately well what he means. The meaning of observation appears to be intimately related to that of behavior, inasmuch as observation has a role in the determination of an operant and, accordingly, in the determination of the fundamental datum. Recall that a freely occurring, virtually instantaneous response is said to have the virtue that it is "easily observable" and that the resulting fundamental datum can be observed in a cumulative record (Skinner, 1950, 1961). We also frequently find the phrases "directly observable behavior," "observable subject matter," and "observable fact." "Observable fact" to Skinner seems to refer to observable behavior and the fundamental datum (Shimp, in press).

There apparently is something about the behavior of observing that makes observed behavior special, in the sense that observed behavior is viewed as being more scientific, more empirical, less speculative than, say, a cognitive process such as short-term memory. The knowledge acquired through observation is thus a privileged sort. It is factual, it is empirical, and it escapes Skinner's criticism of other knowledge that is dependent on a theory involving nonbehavioral terms. Knowledge about behavior that is acquired through observation is preferable to more indirect forms of knowledge about behavior (such as that provided by a theory involving underlying cognitive processes) because it is claimed to be at the same level of analysis as behavior itself: It is not in some other dimensional system. This privileged status of that which is directly observed is so great that it can motivate a behavioral definition of memory as the direct observation of the past (Wilcox & Katz, 1981).

This view of the importance of observation may have had an important consequence for the case history we are examining. If it is considered that one can see behavior in a cumulative record (Skinner, 1956), that which is easily observed in a cumulative record, response rate, becomes more important, and that which can not be observed (e.g., distributions of interresponse times) becomes less so. This difference between molar and molecular analyses in terms of that which can be observed in a cumulative record also may be part of the historical origins of a myth that there is a difference in levels of analysis between molar and molecular analyses. The phrase "levels of analysis" appropriately involves the idea of reductionism, but the difference between molar and molecular analyses has nothing to do with reductionism. In the context of behavior analysis, these two kinds of analyses simply are different ways of looking at the very same thing, a behavior stream. There is no shift as from behavior to biology, or from biology to chemistry, or chemistry to physics.

In the philosophy of knowledge, philosophy of science, and history of science, a

point of view has emerged according to which facts are not the kinds of things one observes or sees. Facts are certain kinds of linguistic propositions and they do not necessarily refer to observables and are not necessarily objective (Hanson, 1958; Kuhn, 1962; Manicas & Secord, 1983; Rorty, 1979; Wittgenstein, 1953). Observation, according to this point of view, is theory laden: A scientist sees what he or she is intellectually prepared to see. The classic reversible figure of the duck-rabbit (Wittgenstein, 1953) appears as either animal depending on the surrounding visual context. This dependency of what one sees on the surrounding visual context is generalized by the newer philosophy so that there is a dependency of what one sees on nonvisual contexts such as one's acquired knowledge. In the case of a scientist, the context that affects what one sees includes the theoretical knowledge he has learned. This is an important component of Wittgenstein's treatment of the nature of knowledge, of private events, and of behavior, and seems to me to be omitted or underrepresented in some recent accounts interrelating his views with those of behaviorism (Day, 1969; Harzem & Miles, 1978).

Many researchers within the tradition of the experimental analysis of behavior behave in a way that seems to constitute a commitment to the idea that there is a "sense-datum language" that objectively, atheoretically gives the facts obtained directly from observation. Such facts are assumed to include, as I have mentioned previously, the fundamental datum, mean rate of a freely emitted, essentially instantaneous operant. On the other hand, if one adopts a more Wittgensteinian view of the nature of seeing, and of scientific seeing in particular, then this commitment would seem a dubious practice (Hanson, 1958). In terms of the analysis given above, the methodology Skinner advances as objective and factual may be interpreted as involving theoretical commitments. I will suggest below that these particular theoretical commitments add up to commitment to a metatheoretical position, namely, to a form of the philosophy of knowl-

edge advocated a hundred years ago and more by the British Empiricists.

Skinner's view that observation is a kind of behavior that gives privileged knowledge is so intimately connected with his views on the correct behavioral unit of analysis and the fundamental datum, that if his views on these latter two issues are wrong, then so is his view of the relation between observation and behavior. If that relation is jeopardized, so is the very meaning of behavior (Shimp, 1976).

What is an alternative to Skinner's view on the relation between observation and behavior? One alternative amounts to acknowledging that we do not know what behavior is, that what has passed for the fundamental datum is only an arbitrary statistic, and that behavior can only be defined, understood, manipulated, or explained in the context of a theory that has as one of its functions the description of what behavior is. Thus, the message advocated in Skinner's paper "Are Theories of Learning Necessary?" should be stood on its head. The question is not: Shall we or shall we not have a theory of behavior? It is: In terms of which theory shall we know what behavior is? (Shimp, *in press*). This leads us to the next topic, the general issue of the role of theory in the experimental analysis of behavior.

THE ROLE OF THEORY: PAST, PRESENT, AND FUTURE

Some of the most striking features of the experimental analysis of behavior involve the characteristic ways in which it deals with theory. A common attitude toward theory has been one of skepticism and criticism, mixed with an advocacy of the inductive development of theories consisting of empirical generalizations expressed in terms of the principal dimensions of behavior (Skinner, 1950, 1961, 1966). This inductive approach is said to permit the development of a science of behavior without experimenters' having to commit themselves to speculative theoretical assumptions expressed in terms other than those of behavior itself (Skinner, 1950, 1961). Thus, in Skinner's case history

of his own research program (Skinner, 1956), he does not consider the possibility that theory might determine in some important ways what laboratory methods one adopts, what is observed, which statistics one computes, or even facts themselves. These possibilities, so much a part of contemporary history and philosophy of science (Lakatos & Musgrave, 1970; Rorty, 1979), do not form a significant part of the discussions within the experimental analysis of behavior on what a theory is and what it is good for. Perhaps these relativistic and contextualist analyses, now common elsewhere in psychology (Manicas & Secord, 1983), will become more common in the analysis of behavior (Woolfolk & Richardson, 1984). In view of such a possibility, let us briefly consider what a contextualist or relativistic analysis of the experimental analysis of behavior might reveal.

Relativistic analyses of any scientific field emphasize that it is defined in part by metatheoretical assumptions that often are implicit in empirical methodology. This is the idea that experimental methods are "theory laden" (Hanson, 1958; Kuhn, 1962). How can one make explicit the implicit theoretical assumptions to which a field is committed? An answer to this question has two parts. First we can ask: Do the methods make sense in terms of a particular theory? That is, if one subscribed to this theory, would one use these methods? Second: Do the methods fail to make sense in terms of other theories? That is, if one subscribed to some other theory, would one reject these methods? If answers to both questions are yes, then reasonably it could be said that the use of these methods, empirical laboratory methods though they may be, constitutes a commitment to a particular theory, namely, the theory in terms of which the methods make sense.

Two different sets of empirical methods are used to study operant phenomena—molar and molecular. As noted previously, the former are by far the more common. The use of these molar laboratory methods may be seen by the process described in the

previous paragraph to constitute a commitment to the assumptions of early associative theory—to the version of associative learning theory popular in the 1930s when the “basic” reinforcement contingencies, apparatus, and techniques of data analysis were invented. The features of a molar analysis that have been claimed (Shimp, 1975, 1976) to constitute such a commitment involve the nature of the behavioral unit as one that is virtually instantaneous and therefore easily observable (Skinner, 1950, 1961, 1966), and the interpretation of sequential phenomena, including the grammar of natural languages, as behavioral chains (Epstein, Lanza, & Skinner, 1980). Numerous commentators have made similar observations (e.g., Anderson & Bower, 1973; Chomsky, 1959). On the other hand, it is also common to encounter rebuttals to these charges. For example, Malone (1982) has written that “S-R associationism has never characterized Skinner’s views, nor those of the *Journal of the Experimental Analysis of Behavior*. . . . (his) position has nothing in common with rigid S-R associationism” (p. 206). Interestingly, it is possible simultaneously to assert that both Malone and Chomsky are correct. Malone seems to be describing the verbal behavior of those engaged in the experimental analysis of behavior: Virtually no one explicitly espouses S-R associationism or any other kind of associationism. On the other hand, the view Chomsky advocated, and the view I am here advocating, is that the commitment to associationism is built into the classic laboratory methodology of the experimental analysis of behavior. This commitment is no less real for being implicit and therefore in a sense invisible.

An approach to theory adopted by many proponents of behavior analysis is expressed by Nevin (this issue) as clearly, as elegantly, and as forcefully as perhaps it can be. Nevin’s approach is inductive in the sense that behavioral laws that are empirical generalizations of ever-increasing scope, and abstraction, are built up cumulatively. It is algebraic and deals with statistics that are

obtained from “steady-state” conditions. It is molar in the sense that the defining exemplar (the generalized matching law) describes behavior streams in ways that involve averaging over different local temporal patterns.

The preceding discussion suggests that historically the role of theory in the experimental analysis of behavior has been represented substantively by an associative theory implicit in experimental methods and formally by an inductive, algebraic approach. That this historical role continues today is shown in this issue by several articles, perhaps most clearly by Nevin’s.

What then could be said of the future role of theory in the experimental analysis of behavior? If we examine existing trends in the development of operant theory, we see not only many examples of molar theory (Hanson & Timberlake, 1983; Prelec, 1982; Rachlin, 1983), but also well articulated models based on psychophysical principles (Gibbon, 1977), or on assumptions about how reinforcement promotes activity (Killeen, 1982); and we see also a greater use of computer techniques to evaluate the consequences of various kinds of assumptions about operant behavior (Hinson & Staddon, 1983; Shimp, 1984a, 1984b; Zirix & Silberberg, 1984). Let us focus on just one of these trends, the greater exploitation of computer-simulation approaches for the development of operant theory. In so doing, we will bring up to date the case history in the molecular analysis of behavior, the beginnings of which were described earlier.

One virtue of a computer-simulation approach is that it permits the exploration of the predictions of a set of quantitative assumptions that otherwise would be inaccessible or unknowable due simply to the intractability of the required mathematical derivations. With computer-simulation tools, anyone who can write programs can explore fairly sophisticated theoretical possibilities. Consider for the present just one exemplar of this approach. The program called AL (for Associative Learner) generates entire response protocols for hypothetical organisms (“stat birds”) responding

on a wide range of schedule contingencies. So far, AL can duplicate a variety of features of molar and molecular performances of real organisms in concurrent ratio schedules (Shimp, 1984a), concurrent interval schedules (Shimp, 1984b), concurrent IRT schedules (Shimp, 1978, 1984b), DRL schedules (Shimp, 1978), and temporal bisection experiments (Shimp, 1978, 1981). The assumptions AL makes about the world involve stimulus encoding, short-term forgetting, acquisition of new associative knowledge, and ways in which acquired knowledge becomes translated into behavior. As I described earlier (see "Observation and Behavior"), these assumptions seem to require a position on the nature of theory and on the relation between theoretical variables on the one hand and observation on the other that is different from the modal position in the experimental analysis of behavior. AL is not built up inductively from empirical generalizations in the manner recommended by Nevin (this issue) and Skinner (1950, 1961). In addition, the theoretical assumptions involve variables and processes that are not even in principle observable. For example, AL encodes a stimulus in terms of various features that we can not identify directly. Similarly, AL establishes associations in an all-or-nothing manner that is not directly observable and that ultimately can be evaluated only in the context of AL's overall adequacy (Hanson, 1958; Kuhn, 1962; Lakatos & Musgrave, 1970).

Computer models of AL's general type put a variety of conceptual issues in a new perspective. For instance, AL generates entire response protocols, not just the kinds of statistics that historically have entered into molar or molecular analyses. According to AL, *neither* of these two classes of statistics define causal relations. According to models of AL's type, no empirical relation directly reveals a causal relation. The causal relations are expressed instead in terms of the basic theoretical assumptions of the model. As a consequence, models of this type suggest that problems such as the discovery of the "fundamental dimensions of behavior"

can not be solved or even defined outside the context of particular theories (Shimp, in press). Models of this type also give a different perspective on statements of the type, "All roads lead to (generalized) matching" (Nevin, this issue). According to AL, generalized matching is not a general property interrelating behavior and reinforcement. AL produces it only in certain specific schedule contexts. In other contexts, including some interresponse time and timing phenomena, generalized matching has nothing to do with AL. AL's general properties do not involve generalized matching; they involve the causal relations expressed in its basic assumptions about encoding, learning, forgetting, and the translation of knowledge into action. The general lesson here is that generality is at the level of theory, not at the level of an empirical relationship such as generalized matching.

PRESENTLY IDENTIFIABLE TRENDS

Two present trends in the experimental analysis of behavior may be important to the overall evolution of the field. First, cognitive and ordinary-language accounts of behavioral phenomena are appearing more frequently. Second, a host of new problems are being investigated, with a growing fraction of these being related to cognitive issues and to various kinds of molecular analyses. Each of these developments is worthy of brief comment.

On the Evolution of the Language of Behavior

The language suitable for behavior analysis is a controversial and exceedingly difficult but important conceptual issue (Branch, 1977; Day, 1969; Harzem & Miles, 1978; Himeline, 1983; Jans & Catania, 1980; Malone, 1982; Ryle, 1949; Shimp, 1976; Skinner, 1969; Zuriff, 1979). Consider an example of a form of translation advocated by Jans and Catania (1980) and Himeline (1983). They first advocate replacement of words like "memory" with words like "remembering" and then state that remembering is behavior. Thus, a cog-

natively flavored or even mentalistic word from ordinary language, like "memory," gets translated into a linguistic form suitable for behavior analysis: A memory is not observable but behavior is. Although virtues of such a translation are widely recognized (Hanson, 1958; Wittgenstein, 1953), there is also a troublesome problem. In actual laboratory practice, is memory any different from behavior?

Let us take a specific example: Is short-term memory in delayed matching-to-sample experiments conceptually different from the statistic, mean response rate, so important to the modal research program in the experimental analysis of behavior? Recall that mean response rate may be viewed as a theory-laden concept. According to a molecular analysis it is not a dimension of behavior, and it is not "on the same level" of analysis as behavior (Shimp, 1975, 1976). Neither is it directly observed. It simply is a statistic that researchers compute from publicly observable data because a literature that is both empirical and conceptual, namely the literature on molar analyses of operant behavior, suggests it is worthwhile to do so. This literature includes ordinary language metaphors, involving "strengthening," "shaping," and so on. Short-term memory is the same kind of concept. It refers to a statistic (percentage of correct choices) that is used within an empirical/conceptual matrix that is our only guarantee of its significance. In this sense there is no important philosophical, ontological, epistemological, or scientific difference between short-term memory and mean response rate. Each has similar virtues and problems. Thus, one wonders what a behavioral translation such as that proposed by Hineline, and Jans and Catania, can accomplish.

In addition, it will be recalled that the role assigned to observation in the determination or definition of behavior has led to the construction of behavioral units, like key pecks and lever presses, that are essentially instantaneous. Such a view leads naturally and perhaps inescapably to mediating-chain descriptions of short-term memory phe-

nomena. If a chaining account of memory is not what is intended, then it is unclear what Catania means by "behavior." What behavior? Thus, the motivation for the behavioral translation is, first, unclear because it leads to the same kind of fuzzy concept one started out with and, second, seems to promise more than it can deliver. Because of these problems, and others, one should not be surprised to find a continuation of the trend to permit judicious use of naturalistic language in the description and explanation of behavior. Such accounts have problems, such as that of "Laymanship" described by Skinner (1961). But behavioral accounts also have their full share of problems.

On the Emergence of Cognitive Research Programs

No description of current trends in the experimental analysis of behavior could fail to note the emergence of the field of "animal cognition" (e.g., see Roitblat, Bever, & Terrace, 1984). Topics subsumed by this label are too many even to list here, but some of them promise to shed important light on traditional problems in the experimental analysis of behavior and so deserve special mention.

Increasing attention to the relation between timing processes and familiar schedule effects promises to give us new ways of describing and explaining the effects of reinforcement on temporal properties of behavior (Gibbon, 1977; Hanson & Killeen, 1981; Shimp, 1984a, 1984b). Cognitive approaches to the nature of a concept are changing the way we view the relation between animal discrimination learning and short-term memory (Medin, Reynolds, & Parkinson, 1980; Reynolds & Medin, 1981). Application of mathematical models to human operant performance (Bradshaw, Ruddle, & Szabadi, 1981; McDowell & Kessel, 1979), by providing means by which cross-species comparisons may be made within a meaningful theoretical context, may ultimately give new perspectives on issues in comparative psychology and sociobiology. The development of techniques by means of which animals can be asked to give self-

reports of their own skills may provide ways to begin to study in animals various dissociations primarily associated with humans (Shimp, 1982a, 1983). Such dissociations include those between verbal and nonverbal types of behavior and between rule-governed and contingency-governed behavior (Skinner, 1969).

SUMMARY AND CONCLUSIONS

Trends are emerging in the experimental analysis of behavior that may importantly change what we think behavior is, and what we think its relation to observation is. The critical issue seems to be the dependence of the meaning of behavior on theory. The experimental analysis of behavior was organized at a time when a cumulative-growth picture of science was dominant, and it was assumed by many behaviorists that they possessed a technology and a conceptual approach that granted the products of their research a privileged status. They assumed, that is, that they were participating in the accumulation of observable facts and empirical generalizations that would stand the test of time as speculative theory came and went. They continue to advocate (see Nevin, this issue) that theory can be profitably developed in terms of ever more abstract and higher level empirical generalizations. However, it seems increasingly likely that no clear line can be drawn between fact and theory. Thus, what is an empirical generalization within one conceptual framework may be an unimportant by-product within another. A specific example would be the generalized matching law, which is an important generalization to some but a by-product to others (Miller, 1984).

A more general example of the "theory laden" nature of method is that a particularly simple and restrictive form of associative-learning theory may be seen in many commonly used behavioral methods, even though few today advocate such a theory when it is made explicit.

Perhaps in the boldest terms the lesson to be drawn from the case history I have

recounted is that operant conditioning, operant behavior, and especially, free-operant behavior, are ideas that make sense only in terms of a conceptual framework that is still being developed, so that in one sense, their meanings are still unclear. Perhaps the best, most detailed articulation of their meanings is provided by the molar framework on the nature of behavior (Herrnstein, 1970; Rachlin, 1978; Rachlin et al., 1980). However, the case history in the molecular analysis of behavior I have recounted provides an alternative perspective on behavior, one from which there simply is no such thing as nonelicited, free-operant behavior that may be understood in terms of smooth curves involving mean response rate.

Perhaps the future will find the experimental analysis of behavior acknowledging that a broader range of perspectives on the nature of observation may be useful, that a theory is useful in telling us what behavior is, and that a fact may be a fact, or a controlling relation may be a controlling relation, only in the context of a theory. Such acknowledgments might encourage the development of a wider range of theoretical styles and greater efforts to develop more comprehensive theories.

I have tried to emphasize the preceding point throughout the case history I have summarized. I have suggested that a molar analysis seems linked to assumptions about the nature of a free operant as a behavioral unit, about mean response rate as the fundamental datum, about the nature of theory as a system of empirical generalizations, and about the role of observation in the determination of acceptable variables in a science of behavior. A molecular analysis seems to me to require the rejection of all these assumptions. (The two analyses seem to me to be fundamentally incompatible.) To think of rejecting these assumptions requires that they be seen as things that can be rejected—that is, as components of a tentatively held system rather than as components of a purely empirical foundation of a science of behavior.

If these assumptions characteristic of a molar analysis were rejected, what could be

put in their place? I suspect a molecular analysis is preferable in several ways, including: (1) Reinforcement contingencies that involve the local temporal structure of behavior improve the control of behavior; (2) theoretical approaches such as AL permit a more comprehensive and fundamental description of a behavior stream; and (3) a reduction in intellectual isolation might be brought about by increasing ties with cognitive psychology through appeals to cognitive mechanisms (such as by AL) in the explanation of behavior.

A characteristic of the experimental analysis of behavior that one might hope will not change is its dedication to single-subject research. Although this approach may not be a comprehensive solution to all important problems, it is sufficiently general to ensure a long life for the version of behavioral science based on it. Single-subject research seems mysteriously to combine in a uniquely powerful way the virtues of science and technology. A method that produces desired effects in a single organism seems automatically to define a technologically successful method, and, at the same time, produces the kind of quantitative result so useful to the development of a science.

REFERENCES

- Anderson, J. R., & Bower, G. H. (1973). *Human associative memory*. New York: Wiley.
- Anderson, N. H. (1964). An evaluation of stimulus sampling theory: Comments on Professor Estes' paper. In A. W. Melton (Ed.), *Categories of human learning* (pp. 129-144). New York: Academic Press.
- Baum, W. M. (1973). The correlation-based law of effect. *Journal of the Experimental Analysis of Behavior*, **20**, 137-153.
- Baum, W. M. (1975). Time allocation in human vigilance. *Journal of the Experimental Analysis of Behavior*, **23**, 45-53.
- Baum, W. M. (1981). Optimization and the matching law as accounts of instrumental behavior. *Journal of the Experimental Analysis of Behavior*, **36**, 387-403.
- Bitterman, M. E. (1965). Phyletic differences in learning. *American Psychologist*, **20**, 396-410.
- Bradshaw, C. M., Ruddle, H. V., & Szabadi, E. (1981). Studies of concurrent performances in humans. In C. M. Bradshaw, E. Szabadi, & C. F. Lowe (Eds.), *Quantification of steady-state operant behaviour* (pp. 79-90). Amsterdam: Elsevier/North-Holland.
- Branch, M. N. (1977). On the role of "memory" in the analysis of behavior. *Journal of the Experimental Analysis of Behavior*, **28**, 171-179.
- Catania, A. C. (1963). Concurrent performances: Reinforcement interaction and response independence. *Journal of the Experimental Analysis of Behavior*, **6**, 253-263.
- Catania, A. C. (1971). Reinforcement schedules: The role of responses preceding the one that produces the reinforcer. *Journal of the Experimental Analysis of Behavior*, **15**, 271-287.
- Chomsky, N. (1959). Review of Skinner's *Verbal Behavior*. *Language*, **35**, 26-58.
- Day, W. F. (1969). On certain similarities between the *Philosophical Investigations* of Ludwig Wittgenstein and the operationism of B. F. Skinner. *Journal of the Experimental Analysis of Behavior*, **12**, 489-506.
- Epstein, R., Lanza, R. P., & Skinner, B. F. (1980). Symbolic communication between two pigeons (*Columba livia domestica*). *Science*, **207**, 543-545.
- Estes, W. K. (1959). The statistical approach to learning theory. In S. Koch (Ed.), *Psychology: A study of a science: Vol. 2. General systematic formulations, learning, and special processes* (pp. 380-491). New York: McGraw-Hill.
- Gibbon, J. (1977). Scalar expectancy theory and Weber's law in animal timing. *Psychological Review*, **84**, 279-325.
- Gilbert, T. F. (1958). Fundamental dimensional properties of the operant. *Psychological Review*, **65**, 272-282.
- Hale, J. M., & Shimp, C. P. (1975). Molecular contingencies: Reinforcement probability. *Journal of the Experimental Analysis of Behavior*, **24**, 315-321.
- Hanson, N. R. (1958). *Patterns of discovery*. Cambridge: University of Cambridge Press.
- Hanson, S. J., & Killeen, P. R. (1981). Measurement and modeling of behavior under fixed-interval schedules of reinforcement. *Journal of Experimental Psychology: Animal Behavior Processes*, **7**, 129-139.
- Hanson, S. J., & Timberlake, W. (1983). Regulation during challenge: A general model of learned performance under schedule constraint. *Psychological Review*, **90**, 261-282.
- Harzem, P., & Miles, T. R. (1978). *Conceptual issues in operant psychology*. Chichester, England: Wiley.
- Hawkes, L., & Shimp, C. P. (1975). Reinforcement of behavioral patterns: Shaping a scallop. *Journal of the Experimental Analysis of Behavior*, **23**, 3-16.
- Herrnstein, R. J. (1961). Relative and absolute strength of response as a function of frequency of reinforcement. *Journal of the Experimental Analysis of Behavior*, **4**, 267-272.
- Herrnstein, R. J. (1970). On the law of effect. *Journal of the Experimental Analysis of Behavior*, **13**, 243-266.
- Hineline, P. N. (1983). When we speak of knowing. *Behavior Analyst*, **6**, 183-186.
- Hinson, J. M., & Staddon, J. E. R. (1983). Hill-climbing by pigeons. *Journal of the Experimental Analysis of Behavior*, **39**, 25-47.
- Jans, J. E., & Catania, A. C. (1980). Short-term remembering of discriminative stimuli in pigeons. *Journal of the Experimental Analysis of Behavior*, **34**, 177-183.
- Killeen, P. R. (1982). Incentive theory: II. Models

- for choice. *Journal of the Experimental Analysis of Behavior*, **38**, 217-232.
- Kuhn, T. S. (1962). *The structure of scientific revolutions*. Chicago: University of Chicago Press.
- Lakatos, I., & Musgrave, A. (1970). *Criticism and the growth of knowledge*. London: Cambridge University Press.
- Malone, J. C., Jr. (1982). The second offspring of general process learning theory: Overt behavior as the ambassador of the mind. *Journal of the Experimental Analysis of Behavior*, **38**, 205-209.
- Manicas, P. T., & Secord, P. F. (1983). Implications for psychology of the new philosophy of science. *American Psychologist*, **38**, 399-413.
- McDowell, J. J., & Kessel, R. (1979). A multivariate rate equation for variable-interval performance. *Journal of the Experimental Analysis of Behavior*, **31**, 267-283.
- Medin, D. L., Reynolds, T. J., & Parkinson, J. K. (1980). Stimulus similarity and retroactive interference and facilitation in monkey short-term memory. *Journal of Experimental Psychology: Animal Behavior Processes*, **6**, 112-125.
- Miller, H. L., Jr. (1984). On the further maturing of behavior analysis: A review of Commons, Herrnstein, and Rachlin's *Matching and Maximizing Accounts*. *Journal of the Experimental Analysis of Behavior*, **42**, 159-164.
- Myers, J. L. (1976). Probability learning and sequence learning. In W. K. Estes (Ed.), *Handbook of learning and cognitive processes: Vol. 3. Approaches to human learning and motivation* (pp. 171-205). Hillsdale, NJ: Erlbaum.
- Nevin, J. A. (1969). Interval reinforcement of choice behavior in discrete trials. *Journal of the Experimental Analysis of Behavior*, **12**, 875-885.
- Peele, D. B., Casey, J., & Silberberg, A. (1984). Primacy of interresponse-time reinforcement in accounting for rate differences under variable-ratio and variable-interval schedules. *Journal of Experimental Psychology: Animal Behavior Processes*, **10**, 149-167.
- Platt, J. R. (1979). Interresponse-time shaping by variable-interval-like interresponse-time reinforcement contingencies. *Journal of the Experimental Analysis of Behavior*, **31**, 3-14.
- Postman, L., & Underwood, B. J. (1973). Critical issues in interference theory. *Memory and Cognition*, **1**, 19-40.
- Prelec, D. (1982). Matching, maximizing, and the hyperbolic reinforcement feedback function. *Psychological Review*, **89**, 189-230.
- Rachlin, H. (1978). A molar theory of reinforcement schedules. *Journal of the Experimental Analysis of Behavior*, **30**, 345-360.
- Rachlin, H. (1983). How to decide between matching and maximizing: A reply to Prelec. *Psychological Review*, **90**, 376-384.
- Rachlin, H., & Burkhard, B. (1978). The temporal triangle: Response substitution in instrumental conditioning. *Psychological Review*, **85**, 22-47.
- Rachlin, H., Kagel, J. H., & Battalio, R. C. (1980). Substitutability in time allocation. *Psychological Review*, **87**, 355-374.
- Reynolds, T. J., & Medin, D. L. (1981). Stimulus interaction and between-trials proactive interference in monkeys. *Journal of Experimental Psychology: Animal Behavior Processes*, **7**, 334-347.
- Roitblat, H. L., Bever, T. G., & Terrace, H. S. (Eds.). (1984). *Animal cognition*. Hillsdale, NJ: Erlbaum.
- Rorty, R. (1979). *Philosophy and the mirror of nature*. Princeton, NJ: Princeton University Press.
- Ryle, G. (1949). *The concept of mind*. London: Hutchinson.
- Shimp, C. P. (1966). Probabilistically reinforced choice behavior in pigeons. *Journal of the Experimental Analysis of Behavior*, **9**, 443-455.
- Shimp, C. P. (1969). Optimal behavior in free-operant experiments. *Psychological Review*, **76**, 97-112.
- Shimp, C. P. (1975). Perspectives on the behavioral unit: Choice behavior in animals. In W. K. Estes (Ed.), *Handbook of learning and cognitive processes: Vol. 2. Conditioning and behavior theory* (pp. 225-268). Hillsdale, NJ: Erlbaum.
- Shimp, C. P. (1976). Organization in memory and behavior. *Journal of the Experimental Analysis of Behavior*, **26**, 113-130.
- Shimp, C. P. (1978). Memory, temporal discrimination, and learned structure in behavior. In G. H. Bower (Ed.), *The psychology of learning and motivation* (Vol. 12, pp. 39-76). New York: Academic Press.
- Shimp, C. P. (1979). The local organization of behavior: Method and theory. In M. D. Zeiler & P. Harzem (Eds.), *Advances in analysis of behaviour: Vol. 1. Reinforcement and the organization of behaviour* (pp. 261-298). Chichester, England: Wiley.
- Shimp, C. P. (1981). Local structure of steady-state operant behavior. In C. M. Bradshaw, E. Szabadi, & C. F. Lowe (Eds.), *Quantification of steady-state operant behaviour* (pp. 189-203). Amsterdam: Elsevier/North-Holland.
- Shimp, C. P. (1982a). On metaknowledge in the pigeon: An organism's knowledge about its own behavior. *Animal Learning & Behavior*, **10**, 358-364.
- Shimp, C. P. (1982b). Reinforcement and the local organization of behavior. In M. L. Commons, R. J. Herrnstein, & H. Rachlin (Eds.), *Quantitative analyses of behavior: Vol. 2. Matching and maximizing accounts* (pp. 111-130). Cambridge, MA: Ballinger.
- Shimp, C. P. (1983). The local organization of behavior: Dissociations between a pigeon's behavior and self-reports of that behavior. *Journal of the Experimental Analysis of Behavior*, **39**, 61-68.
- Shimp, C. P. (1984a). Relations between memory and operant behaviour, according to an associative learner AL. *Canadian Journal of Psychology*, **38**, 269-284.
- Shimp, C. P. (1984b). Timing, learning and forgetting. In J. Gibbon & L. Allan (Eds.), *Timing and time perception* (pp. 346-360). New York: New York Academy of Sciences.
- Shimp, C. P. (in press). The question: Not shall it be, but which shall it be? Commentary on "Are theories of learning necessary?" by B. F. Skinner. *Behavioral and Brain Sciences*.
- Silberberg, A., Hamilton, B., Zirriax, J. M., & Casey, J. (1978). The structure of choice. *Journal of Experimental Psychology: Animal Behavior Processes*, **4**, 368-398.
- Skinner, B. F. (1950). Are theories of learning necessary? *Psychological Review*, **57**, 193-216.

- Skinner, B. F. (1956). A case history in scientific method. *American Psychologist*, **11**, 221-233.
- Skinner, B. F. (1961). The flight from the laboratory. In W. Dennis (Ed.), *Current trends in psychological theory* (pp. 50-69). Pittsburgh, PA: University of Pittsburgh Press.
- Skinner, B. F. (1966). Operant behavior. In W. K. Honig (Ed.), *Operant behavior: Areas of research and application* (pp. 12-32). New York: Appleton-Century-Crofts.
- Skinner, B. F. (1969). *Contingencies of reinforcement: A theoretical analysis*. New York: Appleton-Century-Crofts.
- Staddon, J. E. R., & Motheral, S. (1978). On matching and maximizing in operant choice experiments. *Psychological Review*, **85**, 436-444.
- Tulving, E., & Donaldson, W. (Eds.). (1972). *Organization of memory*. New York: Academic Press.
- Wilcox, S., & Katz, S. (1981). A direct realistic alternative to the traditional conception of memory. *Behaviorism*, **9**, 227-239.
- Wittgenstein, L. (1953). *Philosophical investigations* (3rd ed., G. E. M. Anscombe, Trans.). New York: Macmillan.
- Woolfolk, R. L., & Richardson, F. C. (1984). Behavior therapy and the ideology of modernity. *American Psychologist*, **39**, 777-786.
- Zirias, J. M., & Silberberg, A. (1984). Concurrent variable-interval variable-ratio schedules can provide only weak evidence for matching. *Journal of the Experimental Analysis of Behavior*, **41**, 83-100.
- Zuriff, G. E. (1979). The demise of behaviorism—exaggerated rumor?: A review of MacKenzie's *Behaviourism and the Limits of Scientific Method*. *Journal of the Experimental Analysis of Behaviour*, **32**, 129-136.