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Tactics: In Reply . . .

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After *Tactics* was published, I received many invitations to speak to interested groups about the topics that concerned me in the book. Without, I hope, offending anyone, I accepted none of those invitations. I felt that I had said all I had to say, and that I had said it as well as I was capable of saying it, so there was no point in rehashing it all. Although I have written several papers on methodology since then, none were explicitly directed at points raised in the book; all dealt with matters that came to my attention after I had completed *Tactics.* One might reasonably ask, then, why I have chosen now to end a 30-year period of silence. This is a question I have also asked myself.

The answer lies close at hand. When Sam Deitz invited me to take part in this special section of *The Behavior Analyst,* and when he listed some of those who might contribute, I felt immensely complimented. I realized, too, that it would be ungrateful and actually churlish to refuse to take part. After all, to see one's work influencing the work of others, particularly others whom one respects deeply, is a scientist's ultimate reinforcer.

Then, as the papers came in, one by one, shepherded expertly by Jay Moore, I knew I had made the right decision. The comments, both laudatory and critical, were clearly coming from friends. These people, secure about the worth of their own contributions, felt no need to attempt to enhance their own reputations by diminishing mine. In fact, some of them have summarized contents and objectives of *Tactics* much more elegantly than I had done. So, this 30th anniversary remembrance by a group of major contributors to the science of Behavior Analysis is for me a momentous occasion. Let me offer a single grateful "Thank you" for all the direct and indirect compliments the commentators have thrown my way. I could not be anything but pleased that *Tactics* has influenced our field in the ways they describe. Then, after this clearly inadequate acknowledgment of their good will, let me show my respect for their thoughtfulness by giving my best attention to the questions they have raised. I shall take up the contributions in the order in which I received them.

First, Jim Johnston in *What It Means To Be a Scientist*: Jim echoes a comment I have heard expressed many times and in many ways. While appreciating the book's descriptions of experimental tactics and data evaluation, some colleagues have reproached me for not having said more about the experimental questions to which the methods were to be applied. They wanted not just descriptions of how research is done, but discussions of what questions should be researched, and why. Some wanted more theory, some more philosophy, and some, like Jim Johnston, wanted to see more comparisons of questions asked in Behavior Analysis with those asked in various areas of Psychology.

The complaints are, of course, correct. The book says little about theoretical or philosophical bases for specific behavioral research. This was quite deliberate. After all, the specific research that is most needed now is not necessarily the research that will be most needed later. When the title was originally being discussed, one suggestion was "The strategy of scientific research," or even "Tactics and strategy . . . ." I insisted on just "Tactics" because I knew that I was not writing about strategy at all. I was convinced then, as I am now, that the tactics I was describing were general, not restricted to the study of behavior. The methodology described in *Tactics* does differ from standard practice in Psychology, but is consistent with standard practice in other areas of science. That point was not generally appreciated at the time, and, I believe, remains unappreciated by many even today (but see Thompson, 1984). The tactics of Behavior Analysis arise out of a long and continuing tradition. Behavior analysts need not act defensively because of their differences with Psychology; they can stand proud in their connections with more advanced sciences.

The separation of tactics from strategy was therefore deliberate. Most of those who wanted more than methodology have realized, of course, that they were really asking for additional books, in the tradition of Keller and Schoenfeld's *Principles of Psychology* (1950), or Skinner's *Science and Human Behavior* (1953), or Catania's *Learning* (1984), or Lee's *Beyond Behaviorism* (*1988*). I do not believe that I am or ever was the person to write books like those (or the one Jim Johnston would like to have seen on the quantitative treatment of experimental data). Still, if my separate treatment of methodology has caused investigators to regard experimental design as a goal for its own sake, unconnected to substantive and philosophical considerations, then that is a most unfortunate outcome. I have written elsewhere "that an experimental design is empty until it is applied to a problem. . . . It is sometimes difficult to brush away the uncomfortable impression that . . . instead of fitting designs to problems, investigators are devising problems to fit the designs" (Sidman, 1981, p. 127). To the extent that *Tactics* has been responsible for such a development, then I must confess to having been remiss in not clarifying more effectively the relations between method and substance.

Jim Johnston also calls attention—rightly I believe—to a much neglected feature of behavior that has attained a steady state. Behavioral stability is important as a criterion to be met before comparing different conditions or—as Jim stated—as a "gatekeeper for phase change decisions." As he points out, however, there is more. Also important is an understanding of the variables that are responsible for maintaining any specific behavior in a steady state, and of the variables that generated the steady state in the first place. This last question, the one most neglected by behavior analysts, is for me the most interesting. Generally, it involves the investigation of transition states, and in particular, of that special transition state which we call "learning." How does behavior arrive at a steady state? How do we move it from one steady state to another? How do we introduce new steady states into a learner's repertoire?

Tactics said little about methods for investigating how to get behavior into a particular steady state, how to produce a particular transition—that is to say, how to teach. At the time, we knew little about such matters, except for response shaping, and that procedure was more used than studied. Teaching was largely a matter of arranging reinforcement contingencies; even rats and pigeons eventually adapted to incredibly complex contingencies. "Learning theory" did not much concern itself with teaching. In the past 20 years, however, the problem of how to teach effectively has occupied most of my own effort both in the laboratory and in applied settings. I have learned much about transitions from existing steady states to new steady states, and so have many other behavior analysts. If I were to rewrite *Tactics,* the topic I would expand the most is the investigation of transition states.

Jim is most troubled about what he feels is the omission in *Tactics* of a basis for handling problems of behavioral measurement that have arisen in the context of the burgeoning operant research with human subjects. He points out that "the convenience of functional response class definitions in the animal laboratory contrasts sharply with definitional dilemmas in applied settings, where response classes are typically assigned by the requirements of applied concerns." This practice in applied settings is, however, changing. Witness the resurgent interest of applied researchers in what they are calling "functional analysis." I think Jim can lay this particular concern to rest. Applied behavior analysts, apparently rediscovering Skinner's (1935) definition of response classes (e.g., Carr & Durand, 1985; Carr, Newsom, & Binkoff, 1980; Iwata, Dorsey, Slifer, Bauman, & Richman, 1982; Parrish, Cataldo, Kolko, Neef, & Egel, 1986), are finding that functional response class definitions lead, after all, to effective treatment.

Alan Baron, in *Experimental Designs,* suggests that new kinds of problems being addressed by behavior analysts—for example, research on aging–may require a greater tolerance of between-group investigative techniques than was evident in *Tactics.* Alan clearly understands and appreciates the distinctions between individual-subject and group-statistical investigations, but I am not certain that he appreciates fully the differential consequences of the two approaches to the study of behavior. Let me lead up gradually to this point.

First, Alan cites the statement in my preface that describes my own discomfort over the prospect that some might interpret my descriptions of experimental practices as rules that experimentalists have to follow. That discomfort still remains. To the extent that my descriptions have been taken as boundaries beyond which experimenters must not venture, the book will have had an effect whose possibility I was clearly aware of and tried to warn against.

On the other hand, new practices, introduced as the science progresses, need not require us to abandon methods or general approaches that have proven themselves. For example, by studying learning in the individual subject we eliminate the need to average out intersubject variability, but we still often average an individual's performance over time or trials. Also, our subject is usually learning more than we are measuring; we often unwittingly perform another kind of averaging by compressing into a single summary measure (e.g., rate of a particular response, its average latency, its distribution of interresponse times, or "percent choice" out of a set of experimenter-specified options) a performance that is actually made up of many units. These problems, however, are not inherent in individual-subject research methodology. Cumulative records, multiple-event records, and trial-by-trial plots of the outcomes of three-term contingencies all provide ways to observe the course of learning without having to average individual behavior over time; instead of hiding an individual's variability, we can expose it and try to account for it. Also, in using programmed-instruction techniques, we break performances down into their component units and teach the subject each unit separately. By so eliminating averages across the component units, we eliminate a large source of variability; nonlearners become learners. These solutions to problems of individual-subject methodology do not require the abandonment of that methodology; rather, they enhance it.

My main point here is that we can maintain our receptivity to new practices without being forced either to abandon proven methodologies or to return to methodologies that we had found unsatisfactory. Yet we do have to make choices, even at the risk of being considered narrow minded. To advocate a "conciliatory position" on the problem of individual-subject vs. group-statistical investigation is to suggest that we have so few sound criteria for making the choice that we might just as well settle the matter by political negotiation. Consider everything—yes; accept everything—no. Giving full weight to all options leads not to progress but to paralysis.

Second, although most behavior analysts at the time I wrote *Tactics* were concentrating on studies of steady-state behavior, I hope I did not suggest that the value or validity of single-subject methodology is restricted to steady-state analysis. Alan Baron's statement, "the need for inferential statistics is obviated because behavior is observed as a steady state," is correct but incomplete. A more critical foundation for the abandonment of inferential statistics is the treatment of variability not as an inherent feature of behavior but as a clue for the investigation of controlling variables. Steady-state analysis, widespread and valuable as it may be in providing a productive context for studying individuals rather than group averages, is certainly not a defining feature of Behavior Analysis in general; I know that my own research during the last 30 years has concentrated on the study of transition rather than steady states, both with nonhuman and human subjects.

That is exactly my point. My own research and that of many others shifted into radically new directions—transitions rather than stable states, human subjects both handicapped and normal, behavior so complex that we were for many years not prepared methodologically or conceptually to look at it, and new experimental questions that raised problems of technique, control, measurement, and procedural specification which *Tactics* had never explicitly cited. Yet we have been able to proceed under the same general rubric—analyzing the behavior of individuals, carrying out experimental analyses of variability, arriving at principles by induction from cumulated observations, and gaining new insight into relationships between the behavior of our subjects and our own behavior as scientists.

New developments have indeed brought changes in the way we conduct experiments, but for many of us those changes have been entirely consistent with the kinds of scientific practices that *Tactics* described. I am a social creature, so the fact that many respected colleagues have found *Tactics* useful does indeed warm me, but even if I were a social isolate I would be able to take satisfaction from the fact that *Tactics* has continued to guide my own progress as a scientist through all of the byways into which my research has led me.

How do I account, then, for Alan Baron's observation that Psychology has failed to jump on the bandwagon of single-subject experimentation, and for his data on the increasing use of inferential statistics in *JEAB*? Nobody should be surprised about the situation in Psychology, which is simply not interested in behavior. Psychologists have ignored not just the research methods of Behavior Analysis, along with its data and principles, but have been reluctant even to acknowledge the existence of Behavior Analysis.

As for the increased appearance of traditional psychological methods in *JEAB*, I suspect that we are dealing here with a social rather than a scientific phenomenon. As long as behavior analysts continue to call themselves psychologists, their behavior will remain strongly controlled by that reinforcing community. It might be interesting to see Alan's simple count of articles of each type in *JEAB* weighted by some kind of evaluation of their effects—perhaps their relative frequency of citation by investigators who embrace various methodological and conceptual orientations. Alan makes two observations that are consistent with this interpretation: (a) The increase of inferential statistics in *JEAB* has been accompanied neither by serious criticisms of single-subject methodology nor by an increase in studies concerned with issues that *require* group designs, and (b) "many of the experiments which used group-statistical procedures could have proceeded along the lines recommended in *Tactics* had the researchers chosen to do so."

Alan, however, offers a different interpretation of these observations. He concludes that the situation reflects a growing conviction that the two approaches are interchangeable. They are not interchangeable, of course, and Alan nicely summarizes the reasons why they are not. He is, nevertheless, concerned to resolve what he considers "discrepancies between the rules set forth in *Tactics* and the ways in which experiments sometimes are conducted." (I wish he had referred not to "rules set forth" but to "practices described.") In attempting such a resolution, he comes down finally to those instances in which the variable under study is not itself subject to experimental manipulation within an individual–for example, species, gender, and age. Although he clearly recognizes the costs of group-statistical designs, he seems to be arguing here that when the variable being studied cannot be manipulated within an individual subject, those costs are somehow lessened.

Finally, then, Alan Baron's suggestion that Behavior Analysis be more tolerant of statistical design and evaluation brings up my most serious disagreement with his presentation. *Tactics* did not "condemn" inferential statistics, but rather, put them in their place. Group statistics can have great utility, but that utility does not reside within a science of individual behavior. Johnston & Pennypacker (1980) pointed out that statistical generalization can be appropriate and useful when the behavior of individual members of the class being investigated is of no concern. (Cook, 1990, has recently provided several excellent examples of such useful statistical data.) Nevertheless, if a variable cannot be manipulated within an individual, and if inter-subject variability cannot be reduced to the point where small groups show differences that are significant in magnitude or importance, then the use of statistical control will yield data that differ qualitatively from data produced by experimental control. This is true whether the group-statistical design was required or not. What I called "basic" and "engineering" research should not be confused; they yield different kinds of knowledge.

This difference holds even when we, as basic scientists, are concerned about variables like species, age, and gender. Important as these factors are, until we can devise experimental preparations that reduce variability, our investigations will yield data relevant to engineering rather than to basic concerns. Statistical studies can tell us how many subjects—but not which ones—are likely to show a particular effect of, say, age variation. Statistical analysis cannot yield a functional relation between the age of any individual and the effect in which we are interested.

Our own behavior in relation to our data differs vastly when we derive knowledge by inductive rather than statistical inference. Chiesa (1990) nicely summarized the difference:

> Unlike the body of knowledge derived from the statistical inference model which may be altered or modified by a simple change in procedure, a change in the "usual" and "convenient" level of confidence, principles derived from accumulated observations are held to be reliable until *new* evidence contradicts them. . . . the result of a test of significance counts as evidence for or against scientific assertions, and . . . the same result (evidence) may either be in favour of or against a scientific assertion in relation to a level of confidence, a matter of procedure. Inductively derived general principles are so derived by virtue of evidence from many sources and many instances of observation and are only contradicted by *independent* evidence. They hold true by observation, rather than by procedure. (pp. 99–100)

I do not believe the cause is hopeless, that variables like age lie beyond the scope of a basic science of individual behavior. First, however, uncritical acceptance of variables like time, developmental status, phylogenetic status, gender, etc. will have to give way to a consideration of the behavioral processes that the use of such terms keeps us from looking at. We know about many behavioral processes—for example, those involved in the establishment and maintenance of two-to n-term contingencies. How do *these* change over time, with development, from species to species, male to female? I have elsewhere suggested that quantitative assessment of the parameters of known behavioral processes

> can provide a description of an individual's developmental progress that is based not on normative standards but on an experimentally verifiable account of behavioral development. . . . effects (of variables) will be specifiable as changes in the absolute status of one or more behavior processes, with no need for interindividual comparisons. (Sidman, 1986b, p. 51)

To accomplish this will require considerable work and ingenuity, but in principle, the task is feasible. Research questions that now seem to require deviations from the functional analysis of individual behavior will then be subject instead to individual analysis.

In *Tactics of Scientific Research and Organizational Behavior Analysis,* Bill Redmon comes up with data to support his contention that *Tactics* has been useful in his field. Given Bill's reputation as a competent behavior analyst, I can evaluate his data in full confidence that the publications he cites came from more than one author. Still, I must confess to some surprise, especially when Bill points out, "literally thousands of conditions change" from one organization or department to another, and "precise replication can rarely be achieved." He also points out, however, that even under these inherently "noisy" conditions, organizational behavior analysts need to "replicate research as best we can to build a base for subsequent extensions." Given the obvious utility of organizational behavior analysis as an "engineering" science, Bill is telling us that if it is to function also as a "basic" science, discovering and evaluating new causal relations, it must go beyond Psychology's standard research plan. I am pleased that he sees *Tactics* not as creating rule-bound scientists but rather, as encouraging flexibility in the search for new ways to ask and answer the questions which arise in his field of interest.

Bill Redmon also notes as particularly appropriate in the analysis of organizational behavior the emphasis in *Tactics* on the relationship between the scientist, as a person, and the subject matter. The experimenter does not just stand by dispassionately observing and evaluating results, but interacts with the subjects to produce those results, bringing his or her own reinforcement history to bear on their evaluation. I am still convinced that the acknowledgment of this relationship, a defining feature of Radical Behaviorism, is also one of the hallmarks of the practice of Behavior Analysis.

Yet, it has become evident to me through the years that the emphasis in *Tactics* on science "as an intensely personal affair" has caused some to believe that there are few or no standards that good science has to meet. I have several times been disconcerted to hear sloppy experimentation being justified by appeal to the experimenter's self-knowledge and judgment, or to contingencies in the research, social, or political environment, and to find myself being thanked for my "permission" to do science this way. Bill Redmon is not, of course, advocating this; his comments simply provide me with an occasion for pointing out a real problem that misunderstanding of the relationship between scientist and data has, at times, caused. It would be comforting, I suppose, to be able to show that sloppy experimenters are also sloppy readers, but this would only be a search for the easy way out. I have long believed in Fred Keller's dictum that the student is always right. Consistent with this belief, I can only regret that *Tactics* did not delve more thoroughly into the reciprocal relationship between scientists and their subject matter.

At its best, as in Skinner's *The Behavior of Organisms: An Experimental Analysis* (1938), that relationship yields powerful offspring. Like children, data have to be valued for their own sake, not to satisfy our own needs as "parents." Because we do not always know how to nurture our data effectively, we must be prepared for surprises, but that does not mean everything is to be surprising. We know something about how to interact effectively with our subject matter, and even where we do not, we often have successful models to emulate. The dictum "anything goes" is not useful, and I am sorry that anyone has ever interpreted *Tactics* as supporting it.

In the title of their contribution, *The "It" That Is Steady In Steady States,* Steve and Linda Hayes raise an issue I wish they had addressed more substantively. When I saw the title, I looked forward to suggestions about how to approach problems that arise from the necessity of analysis in any scientific investigation; problems that arise, in this instance, from the necessity to measure a limited sample of behavior—and a limited aspect of that sample—even though we are aware that unmeasured changes may also be taking place. *Tactics* touched on this problem only indirectly, more in connection with transition states and control techniques than with steady states. An important but little studied example is what has often been called "overlearning," seen when an aspect of behavior that we are measuring seems to have reached a stable end state, but retention or transfer tests yield results that depend on how long the stable state has persisted.

Here, and in similar instances, we may rightly ask, "What is—and what is not—steady in the steady state"?

Tactics suggests that further analysis can illuminate these questions. Steve and Linda, however, seem to feel that the very concept of the steady state blinds us to their existence. That can, of course, happen. "Sidman's Law" (often articulated but never before in print) says, "Anything an experimenter can think of, some subject will do" (given, of course, that the act is physically possible). The generalized form of the law applies also outside the laboratory: "Anything anyone can think of, someone will do." I see no reason, however, to assume that the measurement of a steady state will necessarily prevent us from recognizing that other aspects of behavior may be changing, and from investigating those changes. Unlike the hiding of variability by statistical manipulation, the measurement of steady states need not impose a state of ignorance on the experimenter. Yet, even though steady-state measurement does not necessarily produce such ignorance, Steve and Linda's reminder of that possibility may well prove useful.

I am perplexed by Steve and Linda's statements that it was once common for behavior analysts to use animals with unknown histories, that they only ceased that practice when efforts to replicate results failed, and that *Tactics* or any other behavior analytic treatise promoted the idea that behavior can be defined independently of the organism's history or its current environment. These provocative statements are historically incorrect, and not on a par with the thoughtful comments that comprise the rest of Steve and Linda's contribution. They make it difficult for me to keep my presentation calm, my tone soothing, and I hope will not prevent others from attending to the real contributions Steve and Linda have shown themselves capable of making.

Steve and Linda Hayes recognize that steady states are useful empirically, but they seem not to value such utility. They must know as well as any scientist that whenever we abstract some aspect of nature for the purpose of analysis, "we are acting on a fiction for pragmatic purposes." Surely they do not believe that we can measure evolving interactions between organism and environment *without* creating "a convenient fiction for specific analytic purposes." Simplification is always necessary, even for a contextualist. The very simplification that Steve and Linda deplore is what has given rise to our appreciation of the context in which behavior takes place. I wish they would apply their considerable talents to the development of empirically useful techniques for studying such interactions.

Although it is undoubtedly true that one could always find some aspect of behavior in transition, no philosophical conception, contextualism or any other, can do away with the fact that other aspects of behavior may have attained stability. It is better to accept the empirical reality and build on—or around—it than attempt to hide it in terminological controversy about the definition of behavior. I believe that Steve and Linda's concern about the relation between the definition of a steady state and the definition of fundamental units of behavioral analysis is misplaced. The concept of the steady state implies nothing about fundamental analytic units. Stability is a measurable characteristic of all analytic units, but does not enter into the definition of any particular kind of unit.

I have more recently (Sidman, 1986a) given a fuller exposition of my own conception of the fundamental analytic units of behavior, a conception that I consider entirely consistent with everything in *Tactics*. Indeed, the conception is hardly my own; it has long existed in the repertoire of many behavior analysts. The units I described build directly on the two- and three-term contingencies that have formed the cornerstones of behavior analysis since B. F. Skinner's early work; they do not require the foundations of Behavior Analysis to be torn down and replaced.

More important than their origin is the fact that the analytic units do just what Steve and Linda Hayes attribute to contextualism; the units show behavior and environment to be interactive. I am puzzled at seeing Steve and Linda setting up contextualism as an opponent of what I suppose can now be called "classical" behavior analysis. When I first read their references to contextualism (e.g., Hayes, Hayes, & Reese, 1988), I said to myself, "Wonderful, they are giving good reasons for behavior analysts to pay more attention to problems of stimulus control." Since then, I have seen no need to change my view that contextualism is another name for the study of the stimulus control of behavior.

We have much to learn about stimulus control, but we also have learned much already. This is the second area I would expand in any revision of *Tactics*. Note, however, that I say "expand," not "replace." My own conception of the scientific process is that it is not destructive but constructive, that even in "paradigmatic shifts," we retain and build upon what has been discovered before. Psychology has never been able to do that. Instead, it repeatedly tears down its conceptual and empirical structures and then, using a different metaphor and vocabulary, rebuilds the same ones all over again.

Behavior Analysis need not follow that course. Its structure, based on the concept of contingent relations between conduct and environment, a concept that is securely tied down empirically, is strong enough to support new data. I have yet to be convinced that contextualism requires the structure to be abandoned. I have not seen that contextualism adds anything not already handled by the concept of contingency, starting with two- and three-term contingencies and moving upward. Perhaps more to the point, I have yet to see a single technique of experimental analysis, a piece of experimental data, or a method of data evaluation whose origination required a new kind of contextual orientation. Nor do I find the "new contextualism" generating practical solutions to any of the many real problems that are inherent in the approaches *Tactics* describes.

Still, when Wayne Fuqua, in *Tactics of Scientific Research at 30: Some Personal Reflections,* notes that the book has sometimes been referred to as "The Bible," I have to add a reminder that, like everyone else, the writers of bibles have clay feet. As products of unplanned environmental interactions, they are sometimes led astray. Critics like Steve and Linda Hayes are needed to ensure that the writers of bibles do not lead us down the garden path.

Wayne reassures me greatly when he says that he has found the book useful in devising creative solutions to unexpected problems. He, at least, has not taken it as a set of rules to be followed. I could not be anything but pleased in reading his generous and well-expressed appreciation of the book's overall intent and orientation. I have, therefore, nothing to add except in reply to his thoughtful questions.

First: Although all sciences have much in common, it is also clear that they differ considerably from each other. Why else would each exist? Behavior Analysis, too, has much in common with, and many differences from other sciences. The commonalities have more frequently been written about—as in *Tactics*. Critics sometimes interpret this emphasis on the similarities as a claim that the study of behavior requires nothing more, conceptually and methodologically, than the tools which other sciences provide, and the critics object to that notion. Behavior Analysis makes no such claim, however, and behavior analysts should feel under no obligation to defend it.

In the natural course of its development, the science of Behavior Analysis has made many accommodations. It deals with a unique subject matter, and has adapted its methods of inquiry to the requirements of that subject matter. The subject matter does not necessarily differ in being ephemeral—many chemical reactions run their courses rapidly, and the measurable life of elementary physical particles is even more transitory. Other natural sciences have found it possible to measure ephemeral events by looking at their consequences, a methodological solution not unlike the practice in Behavior Analysis. The real difference lies in the fact that in the physical sciences, the consequences of an event are important solely for purposes of measurement or because of their position in a sequence of events. In contrast, the consequences of a behavioral event turn out, themselves, to be powerful determiners of the probability that the event (or members of the event class) will occur again. To explain an event by appeal to its environmental consequences—without invoking teleology—is not a standard strategy in any other science (although Evolution provides a close analogy [Skinner, 1981]), and probably for that reason it has been a difficult notion for some to comprehend.

Another set of accommodations that sets Behavior Analysis apart is perhaps most easily seen by contrast with Cognitive Psychology, which tries to explain conduct not by reference to controlling contingencies in the current and past environment but by appeal to control by postulated mental structures. This is indeed an attempt to mimic other sciences: Genetics explains the continuity of physical characteristics and processes by appeal to specially constructed molecules that are passed along by inheritance; Immunology has found many varieties of cells whose unique construction helps explain how we fight off disease; Neurology and Physiology have found structural characteristics of the nervous system that help explain features of behavior like the speed of reactions, receptivity to energy changes in the environment, and even our ability to speak and understand speech.

Behavior Analysis deals with structures of a different kind. What is a structure except a group of elements that are somehow related to each other? Behavior Analysis appeals to structures that exist in the environment, made up of elements that need be related to each other in no way except as precursors and as consequences of conduct. These structures do not determine behavior in the manner of stimulus and response (Cognitive Psychology, not Behavior Analysis, is the true inheritor of S-R psychology). And they occupy no hypothetical storage space within the organism. Yet, such structures, long after some or all of their environmental elements have disappeared, will continue to interact with other behavioral determinants; they will have become *historical* controlling variables. As Wayne Fuqua indicates, this *is* a unique characteristic of behavior as a scientific subject matter.

Again, the necessary accommodations have already been made. Behavior Analysis has found it useful to accept action (of a variable) at a temporal distance, without attempting (like Cognitive Psychology) to invent mediators, or (like Physiology) to discover them. Real mediating events will undoubtedly be found some day in the nervous system or elsewhere, but their discovery will change neither the practical significance nor the explanatory role of the environment as a source of behavioral control.

Second: I believe that Wayne Fuqua's query about how to decide which experimental questions are important has been addressed most competently in Don Baer's contribution. I could not have done it as well. It is certainly true, though, that in sciences characterized by highly developed theoretical structures, the important questions seem obvious; researchers race with each other to solve them. Even in the most highly developed sciences, however, the fundamental advances are not those which answer the obvious questions—important as those answers may be—but rather, those which turn the attention of researchers and theoreticians in new and unexpected directions. Thus, the basic problem—what are the really fundamental issues to attack?—is no different for behavior analysts than for other scientists. If there is a general solution to that problem, however, a more advanced science of Behavior Analysis is likely to provide it—for all sciences.

Third: In the process of discussing what makes research important, Don Baer also gets into Wayne's next question, which brings up problems associated with social validation and social control of behavioral research. These are important matters. If I did not think so before, I would have to now, when I see both Wayne Fuqua and Don Baer converging independently on the same concerns. I hope that

Wayne will not feel slighted if I refer his third query to Don's contribution, and if I reserve my few comments for that context.

In *Exploring the Controlling Conditions of Importance,* Don Baer does his usual kind of thing: an articulate, logical, and thorough discussion of an issue, leaving little more to be said. He provides more than a "creative restatement" of the criteria in *Tactics* for evaluating research; he has, through the years, not only clarified and amplified those criteria, but has added to them. He saw immediately that single-subject methodology was made to order for applied research and practice, and put that methodology to work in helping to fashion the new applied science of Behavior Analysis—in which he and his coworkers and students had already become a driving force. His comments here, which are products of his long involvement in that development, bring up considerations that *Tactics* did not address.

Don suggests that the first problem an applied behavior analyst must face is not how to change the client's behavior, but what behavior to change in order to reduce the originating complaints. Knowing him as well as I do, I understand that he is not claiming that we know all there is to know about how to change behavior. He is, rather, pointing out that in any particular case, one must first decide which changes in the client's behavior will eliminate the originating complaints. He seems to be saying that the basic problem is how to change the behavior of the complainers.

It is certainly true that without a complaint by someone—family, guardian, a governmental or social agency, or even the client—the behavior analyst's skill in changing behavior would have no outlet. Also, if behavior analysts fail to reduce the complaints, the complainers will turn elsewhere. Don would like to see some principles, systematic or empirical, that would help predict which changes in the client's behavior would alter the complainer's behavior. He feels that research devoted to this end, although it is of primary importance, has been neglected.

The same concern exists in other professions. The general rule that others follow is simply to do what they are asked to do: A physician will give whatever medicine is needed to relieve a client's pain; a social worker will recommend an appropriate therapist for a husband when his wife complains of abuse; a lawyer will either prosecute or defend, depending on the source of the complaint. I would be surprised if we learned anything new by gathering data on the extent to which these or other actions reduced the originating complaints. These professions are successful—they survive. Our basic behavioral principles tell us that they would not have survived without satisfying the complainers.

Applied Behavior Analysis, too, is a survivor. The most serious threats to its existence arise not from those who request its services but from academic and economic competitors. What our basic behavioral principles do not tell us about the survival of Behavior Analysis is whether, in the process of changing the behavior of complainers in ways that satisfied the complainers, behavior analysts have also changed the clients' behavior in ways that satisfied the needs of the clients. A similar question is being asked more and more frequently these days in reference to Medicine, Law, Law Enforcement, and Education.

Don is clearly aware of this difficulty, referring it to the domains of ethics and public policy. I think a stronger statement is needed. Behavior analysts who concentrate on reducing complaints may find themselves playing the role of "hired guns." They will do whatever satisfies government officials, administrators, and keepers, despite the consequences to the public, to the retarded, psychotic, or elderly patients in institutions, to the inmates of prisons, and to the pupils in schools. It will not be enough to do research on how to reduce complaints. As Don goes on to point out, there is always a cost-benefit ratio, and success in reducing a complaint must be balanced against the short- and long-term costs to the client, to society, and even to the complainer. These constituencies are not always aware of the costs.

I would therefore broaden Don's new

criterion for the importance of research beyond its success in reducing complaints. I would include its effects on competing individual and group interests within society, and its relevance to the survival of the society as a whole. I believe that a science of Behavior Analysis can help evaluate the nature and likelihood of such outcomes, both local and far-reaching, but their acceptability will be judged by other criteria. Like it or not, the behavior analyst will have to be more than just a technician, more, even, than just a scientist.

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