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WE NEED A NEW MODEL OF TECHNOLOGY

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I have long preferred the phrase *behavioral technology* to refer to the results of the science of experimental and applied behavior analysis. My reason for wanting to use the term *technology* is that this is how the natural sciences refer to their applied capabilities. Saying that our applied skills constitute a technology is a generous assessment, however, because references to technology in the natural sciences differ significantly from our use of the term. In the natural sciences, technology most often refers to consistently effective techniques whose mechanisms have been largely explained by the science.

These demanding criteria are infrequently met in the applied offerings of psychology or even behavior analysis. If we cannot explain our applied procedures with an experimental literature that fully details critical variables and how they work in terms of the basic laws of behavior, and if the effects have not been shown by experimental investigation to be consistently effective, then our procedures might be more appropriately called craft, skilled experience, professional lore, or common sense. It is not even enough that a behavior-change procedure may be effective for reasons unknown or merely suspected. In other words, this conception of technology says that the experimentally derived understanding of the way a procedure works is more important than its origins, the consistency of its effectiveness, or the effectiveness of competing approaches.

The experimental requirements of this approach to developing technology involve both learning about the origins and current sources of control over target behaviors as well as identifying and analyzing separately the elements of a procedure to determine the role of each in the overall effect. This information will clarify the environmental requirements and options contributed by the behaviors under study, as well as those elements critical to a procedure's effects that must not be tampered with and those that can be modified under certain conditions. The next step is to determine the mechanisms by which the elements contribute to the procedure's effects, both separately and collectively. These experimental requirements will generate a literature of thematic studies that explain fully the procedure's functions and mechanisms in terms of the basic laws of the science of behavior.

This conception of technology requires a second empirical process that candidates must survive. The procedure must be evaluated under realistic, applied conditions and shown to be consistently and practically effective. This may sometimes require only descriptive studies, but it usually calls for more than one or two demonstrations. It means that if the effects obtained under typical field conditions are in any way different than those already established in the analytical literature, they must be described and perhaps studied. It also means that the requirements and effects of population, setting, and administrative factors must be identified; this will often lead to more analytical research.

This approach to developing a technology is not a very accurate description of how applied behavior analysis has developed its procedures. Although we can boast a large applied literature, we have not focused on studying target behaviors, and our procedures have generally not received this kind of systematic experimental attention. Analytical efforts have tended to be fairly superficial, emphasizing procedures rather than behavior and falling well short of experimentally explicating procedural mechanisms at the level of basic principles of operant behavior.

We do a somewhat better job at evaluating procedures under field conditions. Successful demon-

Some of the points argued in this paper are presented more fully in Johnston (in press).

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strations are of limited usefulness, however, when our understanding of why they work is based primarily on superficial topographical similarities to basic operant principles. Without this understanding, we cannot be very confident about what it will take to produce effective results under other conditions. Although our description of procedures sometimes uses the terminology of basic conditioning processes, the exercise is more often nominal than experimentally functional (Johnston, 1988).

I believe we need to work toward a model for developing and evaluating behavioral technology consistent with the approach of the other natural sciences. I have proposed such a model (Johnston, in press), although it cannot be properly summarized here. Its focus is on developing an experimental literature that is comprehensive in both its attempt to understand target behaviors and its search for the how and why of applied procedures. With such an experimental data base, practitioners will be in a better position than at present to select procedures based on a set of clear behavioral and procedural requirements for effectiveness, with reasonable confidence that meeting those requirements will assure success.

Such a model has many important implications for our field; I would like to suggest a few that are especially relevant to the theme of this series of papers. First, it suggests that we should make a clear distinction between technological research and technological application. Instead of thinking of the field of behavior analysis in terms of a basic versus applied division of labor, we should further subdivide applied activities into research versus service. (Even better would be a major division of research versus service, with research subdivided into basic versus applied.)

Technological research focuses on developing ways of controlling behavior for practical purposes. Even so, its experimental methods should usually be indistinguishable from those of basic research. Most such studies should be thematic and analytical in style, answering questions about behavior, its controlling variables, procedural components, administrative influences, and so forth. Only a relatively small part of this literature should focus on evaluation per se.

Technological application should not have to focus on asking experimental questions at all, although these will sometimes arise when procedures fail to produce the desired effects. Efforts here should be on behalf of delivering a service and should primarily involve only assessment, selection and adjustment of procedures, and continuing field evaluation. The more successful a procedure's research history, the less likely it is that practitioners will have to turn a service intervention into an experiment in order to meet applied goals.

The importance of this distinction is in the requirements for conducting sound research versus offering effective service. The overriding goal of technological research is to discover the variables that influence certain forms of behavior, including those embedded in proposed intervention procedures; this challenge requires control. Control, however, often requires some degree of artificiality. On the other hand, technological application requires accommodating the circumstances of field settings, but this usually sacrifices control.

In other words, the goals and requisites of these activities often conflict, making one a poor opportunity for accomplishing the other. When we indiscriminately combine them, as in contemporary applied behavioral research, it may only constrain the effectiveness of both efforts. The methodological necessities for important and revealing technological research and the experimental questions they serve should not be routinely compromised by the practical needs of service delivery. Similarly, applications of established procedures do not necessarily need to be handicapped by all of the niceties of research method when no experiment is conducted.

This distinction suggests a second implication of this model. We should represent the different needs of applied research versus practice in how we accept students into graduate programs, how we train them, and how they are employed. Although the interests and skills needed for effectiveness in these two career directions have much in common, there are important differences. Technological researchers must be both inclined and trained to be good scientists above all else. They need to be expert in the applied and basic literatures, as well as being familiar with the populations and applied settings of

interest. However, the applied components of their training must not compromise their training as researchers. On the other hand, practitioners-to-be must know not just the applied literature but must also acquire clinical expertise and other skills required for effectiveness in applied settings. It may be equally important that these students fully intend to be practitioners rather than researchers.

It might even be argued that practitioners should receive training that is more service oriented than research oriented. The scientist-practitioner philosophy we seem to have uncritically borrowed from clinical psychology (see Barlow, Hayes, & Nelson, 1984) may be counterproductive for this new model. There have been many debates about the scientist-practitioner approach to training practitioners, but it is easy to see that few careers fit its assumptions very well. Not only are most holders of the doctorate in psychology apparently uninterested in being both researchers and practitioners, it is difficult to do both well. The contingencies of service goals and employment often lead to compromises in the quality of both the conception and execution of applied research, which is reflected in a weak clinical literature. Although there will always be a few meritorious exceptions, as a general approach to training practitioners the scientist-practitioner model is easy to argue against. (We might remember that medicine and engineering give their practitioners narrowly professional training.)

The model I have suggested (Johnston, in press) should be seen as enhancing rather than diminishing the role of practitioners. Although practitioners would be selected and trained to be more narrowly service oriented than at present, we would no longer need to define their value by such academic credentials as research publications. In addition to their role in service delivery, they would communicate the demand for particular technologies, describe accidental discoveries from their field experience, and report shortcomings with new technologies. Of course, the state of our present skills falls short of this ideal, and practitioners will for years to come unavoidably find themselves conducting service in a research style in order to obtain desired results. This does not usually result in very rigorous research, however. Furthermore, it usually fails to answer the questions that must be addressed in order to understand the factors that influence target behaviors and how intervention procedures work, both of which are required to make procedures consistently effective.

These are provocative topics, and the brevity of my comments may make these points more vexing than intriguing. Many behavior analysts may even feel that we may never be able to create a technology in the style of the natural sciences because (a) behavior is different from other natural phenomena, (b) our ability to control important variables in applied environments will always be limited, or (c) we are doing pretty well already (i.e., better than anyone else). Such rebuttals seem highly debatable, but they do not seem like sound reasons for not trying. It is difficult to imagine convincing arguments for why we should not strive for technological capabilities as impressive as those offered by the physical and biological sciences.

I am worried that we do not seem to have a model or standard that presently guides our technological research questions. There is evidence for this concern throughout our literature. In the area of retardation, for example, we seem to have focused for years on changing behavior with powerful consequences until the recent "discovery" of functional analysis (i.e., what we used to call behavior analysis) suggested we ought to learn more about behavior and its causes. What worries me most, however, is that we may have lost our sense of what behavior analysis can accomplish, of what is possible, of where we are going. Maybe we have not yet decided.

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