

# Applied Behavior Analysis, Service and the Acquisition of Knowledge

Jay S. Birnbrauer  
DeJanette Center for Human Development

From the outset, according to Yates (1970), one of the hallmarks of behavior therapy has been a close relationship between service and research. The original work in the tradition that has become applied behavior analysis was virtually all research oriented. The marriage of service and research was consummated with the publication of JABA, and the definition of behavior analysis provided by Baer, Wolf & Risley (1968). They defined applied behavior analysis as "the process of applying sometimes tentative principles of behavior to the improvement of specific behaviors, and simultaneously evaluating whether or not any changes noted are indeed attributable to the process of application—and, if so, to what parts of that process. In short, analytic behavioral application is a self-examining, self-evaluating, discovery-oriented research procedure for studying behavior." (P. 91). In a footnote to that definition, Baer, Wolf & Risley comment that improvement of behavior is certainly the aim of applied work, but that "it can be just as illuminating to demonstrate how behavior may be worsened, and there will arise occasions when it will be socially important to do so." On the masthead of JABA this definition is summarized as: applied behavior analytic research is "experimental research involving applications of the experimental analysis of behavior to *problems* of social importance."

But 1968 was a long time ago, and applied behavior analysis has chalked up many accomplishments of which it can be justly proud; many people are being served much better now as a result of our efforts. In addition, the relationship between research and service has changed. The definition of applied research has not so much changed, but become rigidified or petrified. Jon Bailey (Ref. Note 1) at last year's MABA convention defined applied research in the following way: Applied research (1) meets the usual criteria for experimental control, reliability of data, etc.; (2) includes a dependent variable which is a socially important behavior of the client or clients; (3) is conducted in a natural habitat of the client or clients; and (4) demonstrates a socially significant improvement in behavior. My purpose is to discuss the implications of restrictive definitions of applied research such as this one and the package approach advocated by Azrin (1977). That definition

precludes, or at least *discourages*, analytic research or parametric research by applied investigators. If principle-oriented research is not applied enough, where will it be published? Will the right people, applied behavior analysts and consumers, read it *and* be able to make use of it? I am concerned about our ability to generate new knowledge and incorporate it into our service activities. Presently, the emphasis is on service and cures. I am not against either. The question is: Can we acquire knowledge, general functional relationships, through cure-oriented demonstrations?

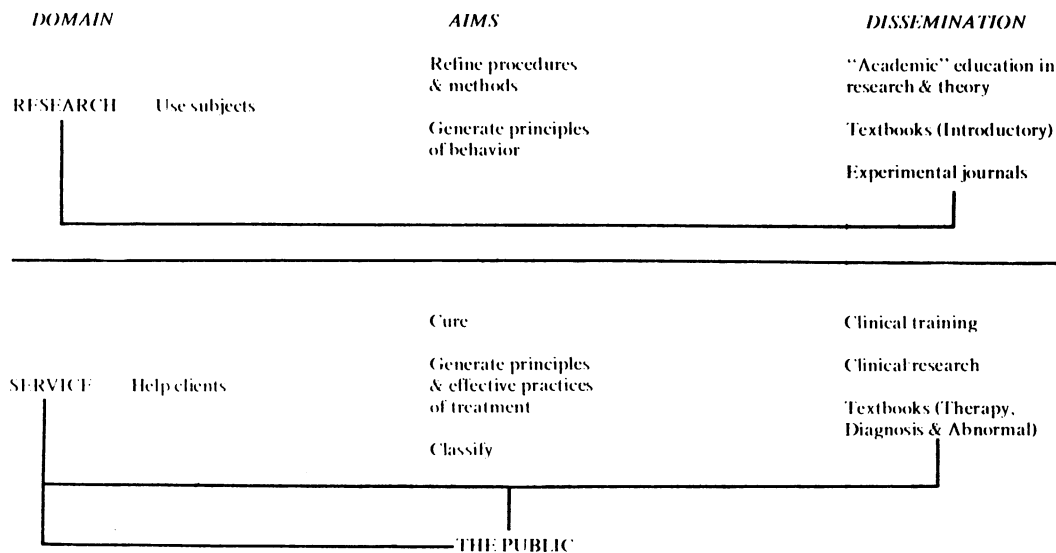
I shall take a historical approach and describe the models of research and service that characterize the short history of behavior analysis. Relationships that exist between research and service may be summarized in the extreme as the parallel play model and the cooperative play model, with the cooperative play model being the obviously more advanced and desirable stage. The parallel model is depicted in Fig. 1. It is also labeled the Pre-1959 Model, in recognition of the first applied behavior analytic paper that I know of, Ayllon & Michael's "The Psychiatric Nurse as a Behavioral Engineer," which appeared in JEAB (Ayllon & Michael, 1959). In the parallel model, there are people who do research and people who serve. Some serve and do research, but these are separate activities in this model. That is, some people sometimes wear their research hats and sometime their service hats. For example, during this era my research was concerned with discrimination learning in retarded children. My service activities, at the same time, consisted of psychodiagnosis using projective tests and psychotherapy with adults.

Research people use people and other animals as subjects to develop more sensitive methods of investigation, to arrive at principles of behavior or theory, and to publish articles which are experimental, basic or academic. They also frequently are professors and they educate persons to do the same things they have been doing. Sometimes the education "succeeds" and their students enter the research domain.

1. Bailey, Jon. We used to all read JEAB, but is it still necessary? Paper read at the Third Annual Midwestern Association of Behavior Analysis convention, Chicago, May, 1977.

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Fig. 1. The Pre-1959 "Parallel" Models of the Relationship between Research and Service



Service people receive much of their early education in the research domain but cross over to the service domain. While service people are influenced by their prior training and often publish in academic journals, their service activities are influenced differently. In this model, clients are treated, helped or educated. There might be some attempt at objective evaluation of the effectiveness of services offered, but usually the principal formative influence is experience—personal and vicarious. Experiences are translated into principles of treatment and published in clinical journals or books. Clinicians also train clinicians in service and in clinical research. The clinical research training may be very different from the basic training, for example that given by psychoanalytic supervisors, or fairly similar.

The consuming public receives virtually all of their information about psychology from the service domain. The influence from the research domain is negligible, and likewise the influence of the public on the research domain is negligible.

This model has its advocates and still describes some branches of psychology and certainly other fields of science. The major advantage is that, free of constraints to solve practical social problems, research may be more systematic and thus is more likely to generate functional relations that have some generality and to elucidate the parameters of

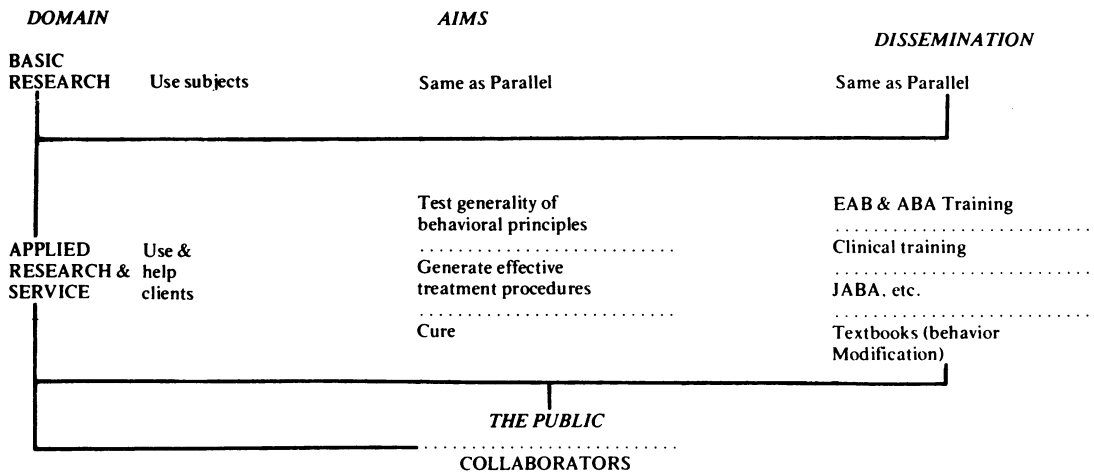
those relationships. The disadvantages are obvious and need not be listed here.

In the 1960's, applied behavior analysis did develop from the parallel stage to the cooperative stage. The cooperative model is depicted in Fig. 2. The distinctive features of the model are that applied research and service are combined. Clients are used and helped with the aim of learning how to help better. There is intercourse between the basic and applied domains; applied persons receive training in both the experimental analysis of behavior and applied behavior analysis; and the public first influenced both domains in a very direct fashion through their participation as collaborators.

To illustrate from my own experience—having had the extreme good fortune to be re-educated by Bijou, Baer, Wolf, etc., I put away my Rorschach kit, and during this era my colleagues and I engaged in a demonstration project for severely/profoundly retarded persons. Bill Redd and I studied the effects of reinforcement schedules on the agent as a discriminative stimulus in a simulated pre-school setting (Redd & Birnbrauer, 1969). I ran studies of conditioned reinforcement using flashing lights and button-pressing in our human operant laboratory (Birnbrauer, 1971), and used all three settings—ward, simulated class, and

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Fig. 2. The 1960 "Cooperative" (ABA) Model



laboratory—in our study of the generality of punishment (Birnbrauer, 1968).

Another characteristic of the 1960 Cooperative Model is that most of the work in the applied domain was, at least *thought* to be, based on procedures and principles of behavioral psychology. The aim was to establish the generality of, or constraints upon, as Baer, Wolf & Risley put it, “sometimes tentative principles of behavior.” Inclusions of this aim made this model especially rewarding to us, because we could emphasize the theoretical gains to academic audiences and clients’ gains to the public.

The 1960 Cooperative Model has, it appears, been superseded by yet another model. This change has been inadvertent. It was not a result of dissatisfaction with the Cooperative Model like the dissatisfaction which produced the transition from the Parallel Model to the Cooperative Model. Rather, the change has taken place because of dissatisfaction with the types of studies that were being conducted. Most of the studies during this era were experimental analyses of single cases which placed greater emphasis upon generation of principle than cure. Research with single clients, technical language, and less than impressive gains in client behavior are difficult to sell to the public and granting agencies. Thus, there was a deliberate shift toward impressing the public with what we could do. So, the latest 1970 model depicted in Fig. 3 is the result.

In the present model, basic researchers still use subjects to refine methods and evolve principles or

theory. The major change is that training in the basic research domain now includes both applied and basic research technology and principles. In other words, the training that was given earlier in the applied domain is now being given to experimental psychologists. The reason is obviously the decreased demand for basic investigators.

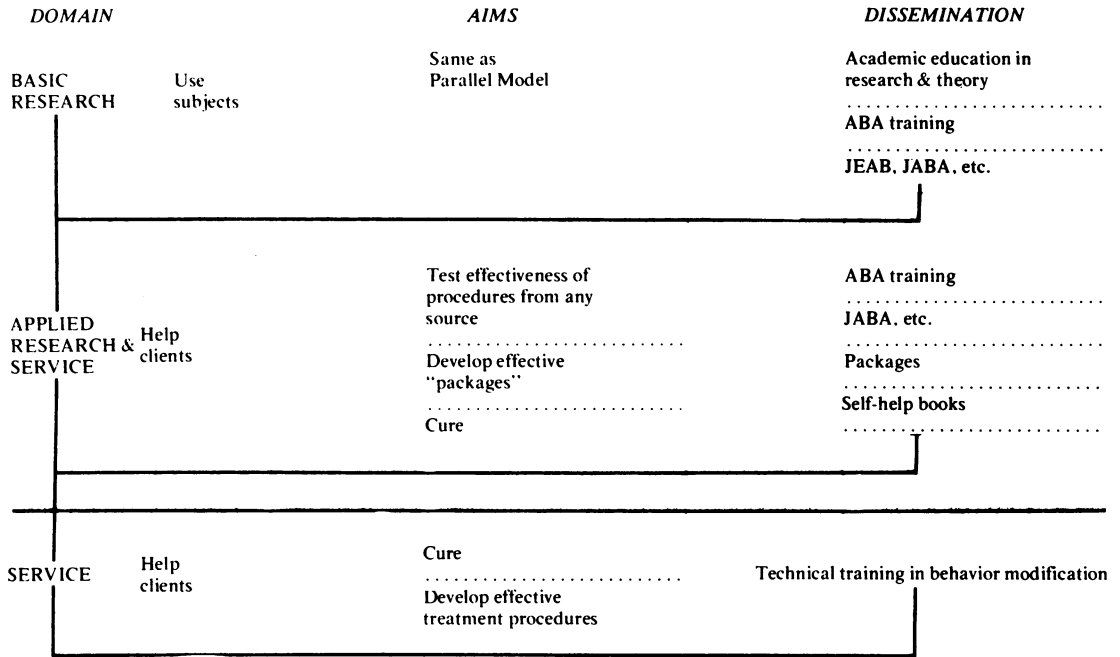
The applied research domain, however, has changed significantly. [Repp, (Ref. Note 2) presents his breakdown of the types of investigation currently going on, which should be of interest.] Secondly, we have witnessed rapid growth of a third domain which I have labeled simply service in Fig. 3.

The changes in the applied domain are, first, that clients must be helped even though the undertaking is research-oriented; clients may no longer be used as subjects. Some JABA reviewers now will reject articles because the result does not appear to be of social significance to the clients. The Baer, Wolf & Risley footnote quoted earlier regarding the potential value of demonstrations of undesired effects is inoperative. Presumably, in addition, studies which suggest important parameters are no longer of interest. This policy, held by some, may be more a result of the fact that the parametric studies I have seen have had other deficiencies; that is, it could well be that reviewers would accept more analytic studies if the results

2. Repp, A. C. & Rose, T. L. Applied behavior analysis vs. behavior modification. Paper read at the Fourth Annual Midwestern Applied Behavior Analysis convention, Chicago, May, 1978.

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Fig. 3. The 1970 "Cooperative" (ABA) Model—With Offspring



were clear, even though the criterion of significant help to the client or clients was not met. Whether or not I am correct, it is clear that some reviewers must be hit over the head with the practical significance of a study before they will regard it as sufficiently applied. This characteristic is reason for serious concern. It means that leaders in the field cannot, or will not, engage in the deductive and inductive thinking that service will always require. Even a demonstration that deals with socially significant behavior in a natural habitat will almost invariably need some modifications to fit the peculiar circumstances the reader is concerned with.

Secondly, the aim of establishing generality of principles, behavioral or otherwise, has been de-emphasized to such a degree that I have eliminated it from the applied domain. In its place is the evaluation of procedures, techniques or ideas from any source. [An increase in technological articles and decrease in conceptually based articles in JABA has been documented by Hayes (Ref. Note 3).] This change is positive in that we were being unnecessarily restrictive in our emphasis upon *behavioral* principles. It is not so positive if we take

note of the fact that *Principles* have been replaced by *Procedures, Methods* and *Ideas*. The latter describes naked empiricism, which leaves the consumer at a loss for deciding how and when to use the procedure. There is great value in relating practices to concepts, principles, and experimental precedents. A good illustration of the point I'm trying to make can be found in the study of imitation as a consequence reported by Kauffman, LaFleur, Hallahan, and Chanes (1975). I can use this study as an illustration because Kauffman and I are pursuing the parameters of imitation as a consequence presently. In the report, the authors describe two ABAB single subject case studies. The first client was a trainable mentally retarded 7-year-old girl who had very sloppy table manners. After baseline recording, they assessed the effects of reprimanding her whenever she made a mess while eating. This reduced the inappropriate behavior somewhat. After a second baseline, the teacher imitated the sloppy eating every time it

3. Hayes, S. A. A technological drift in applied behavior analysis. Paper presented at the Fourth Annual Midwestern Association of Behavior Analysis convention, May, 1978.

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occurred. Sloppy eating declined significantly. After a third baseline they repeated the imitation procedure and obtained the same positive result, i.e., imitation was an aversive consequence for this child. In the second study the subject was a 9-year-old trainable retarded girl who, during speech lessons, emitted sounds which the authors call "yelps." After baseline the instructor yelped every time the girl yelped, and the rate of yelping increased considerably. They returned to baseline, obtained baseline yelping rates and then repeated imitation. The outcome was the same. In other words, here are two cases using procedures which are appropriately called the same, dealing with two different behaviors in two different children in two different settings. In one case the outcome was that which was desired, and in the second case it was clearly not the outcome that was desired. Now, how do we make use of this study? We have no information for predicting the outcome with imitation as a consequence. Consumers who do not have a behavioral background might conclude that vocal responses should not be imitated if the aim is to decrease; whereas, self-help skills should be imitated. A behavior analyst, however, would not make this conclusion. Another possibility is that older children should not be imitated if the aim is a decrease in behavior; whereas, imitation is aversive to younger children. A behavior analyst would not make this conclusion either. Obviously, some prior conditions must be met for this procedure to function as desired. If imitation could be classed with other, more familiar procedures or related to principles, then we would be able to make use of the procedure more wisely than we can at the moment.<sup>2</sup>

Moving on to dissemination, education at the applied level consists largely of literature and methods of applied behavior analysis and generally very little basic experimental analysis and behavior principles. (It is not uncommon now for instructors to have done no behavior analysis, applied or experimental.) Finally, packages and self-help books join the more traditional methods of dissemination. These outcomes are the result of our having raised the expectations of the public. We have also raised a generation of students who have very little resistance to delayed reinforcement and have demanded, and been given, relevance and packages.

The package conception has been described most clearly by Azrin (1977). The approach is characterized by focus upon outcome or cure and the development of methods which maximize

speed, percentage of clients who benefit, degree of benefit, durability over time, acceptance by consumers; and which minimize costs. No one can argue with the desirability of these goals. In his argument for his reinforcement-based, outcome-oriented strategy, Azrin points out that the 1960 model was not being successful in accomplishing these ends. Azrin is correct in general on this point. Single-subject research, which characterized the 60's, is not suited for demonstrating the relative effectiveness of various approaches on the dimensions listed. It is, at best, slower and it is difficult to sell to consumers. Secondly, Azrin emphasizes the value to him of ideas from sources other than behavioral principles. Few would quarrel with that point either.

The problem with the package approach is that its speed is illusory. This has always been the case in the past, and I fail to see how it can work any differently in the present either. Azrin states that he started out thinking that "applied research was a simple-minded application of basic research principles" (1977, P. 141). The package approach repeats that error, if consumers behave, and often they do, as if cures will follow from "simple-minded use of packages." To the contrary, successful application, after a package has been developed and its effectiveness demonstrated in one setting, depends upon one or both of the following conditions being met:

Packages are accompanied by (1) Criteria for the selection of clients and situations suitable for use of the package; (2) Very carefully described steps in the package; (3) Alternatives to the steps when they cannot be carried out or they prove ineffective with a given client; and (4) Instruction in the form of workshops and hot-lines. Presently only (2) and (4) are being met. To give credit where it is due, Azrin and his colleagues and Achievement Place have done an extremely good job in describing the steps in their packages.

The second alternative is to accompany packages by education of consumers to (1) Apply relevant principles of behavior in making modifications in the package as needed; (2) Objectively evaluate progress toward desired outcomes; (3) Implement and evaluate revisions; and (4) Ideally to publicize their empirically tested innovations. In other words, the second alternative is complete training in applied behavior analysis and its experimental and conceptual precedents, or some systematically organized body of knowledge and methodology.

The first alternative implies completion of a

considerable amount of parametric and analytic research which present views seem to be discouraging. It is the necessity for this additional research which leads me to claim that the package approach saves nothing really, and its speed is illusory. Moreover, it can backfire, especially given the combination of events-packages and limited preparation of behavior analysts and consumers. It is this combination that I wish to call to our attention. I think advocates of the package approach and the de-emphasis of basic instruction take their earlier scientific training and their conceptual system too much for granted. They forget that consumers do not share their conceptual system and scientific training.

I am referring to the third growing group which is described below the dashed line in Fig. 3. These persons are trained in behavior modification techniques and a smattering of behavior analysis. Their objective is to help clients. They have been led to believe that they will succeed by employing the packages that have been prepared for them. (That is an incredibly naive belief, and in itself may support my opinion regarding the adequacy of training.) Without a basic background, it is quite likely that when the package fails and/or is too exhausting or expensive, consumers will rely primarily on experiential feedback. What other choices do they have? Without either the additional information asked for in Alternative 1 or the training in Alternative 2, consumers will purchase other packages, abandon behavior modification or create their own variation thereof, or continue in the trial and error fashion that still prevails in most educational and treatment settings. Thus, this group will separate from the parent fields as indicated by the dashed line in Fig. 3. If my reading of the trends is accurate, then the 1980 model will look like Fig. 4. In short, we will have three domains—basic research, applied research, and service, a model which looks very much like the Pre-1959 Parallel Model.

Fig. 4. The 1960 "Parallel" Model



My crystal ball, of course, may be fuzzy and

unreliable. Advocates of the package approach and the petrified definition of applied research may be fewer than I think. I may also be underestimating the number of people who are engaged in research for the purpose of refining the principles of behavior that have evolved—for the purpose of answering questions of how to make decisions with respect to procedures and what variables to manipulate in order to make a procedure more effective.

We all know that virtually any procedure that anyone tries will be effective sometimes. I am reminded of the time that I told a group of students that I would be willing to bet that if we suspended a schizophrenic person from a meat hook in the corner of a dimly lit room and revolved the person twenty times counter-clockwise, while mumbling something, and then revolved them twenty times in the opposite direction, and did this every day at 3:00, some of the clients would improve. Within days of giving that fictitious example, I was astounded to observe on the television an illustration of the Doman-Dellacato procedures. I kid you not, an aspect of their training program is to hang the child upside down on a meat hook and swing the child back and forth while presenting sight vocabulary cards. If we know that a procedure will work sometimes, then there can be no new information gained from demonstrations of the effectiveness of a procedure in a multiple baseline or other design unless the baseline or comparison conditions are carefully described and in themselves sensible. That change alone, i.e., using well described and meaningful baselines, would change a dull demonstration into a study that enhances our knowledge.

In conclusion, application can never be simple-minded, and we should not encourage the public to think that it can be. I suggest that in addition to cure-oriented packages, we encourage refinement of principles and publication of these studies in journals that will be read by applied behavior analysts and consumers. Furthermore, I suggest that we continue to educate consumers and behavior analysts in basic and applied behavior analysis—its principles and its methods—as well as its methods and procedures, whether they like it or not.

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### FOOTNOTES

<sup>1</sup>I am indebted to Jane Cline for her careful work on the figures and Margaret Reardon who typed several drafts and corrected all the misspelled words and poor grammar. Author's address from 1 July 1978: School of Social Inquiry, Murdoch University, Murdoch 6153 Western Australia.

<sup>2</sup>I very much appreciate Carl Binder's calling my attention to the fact that in this paper I was tossing around terms such as "principles," "conceptual system," and "theory" without clarifying how a functional analytic background assists in using the results of studies which appear in the literature. I hope this illustration fills some of that gap.