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In Response

THE FLIGHT TO THE LABORATORY

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I was very pleased to read the Pierce and Epling (1980) paper "What happened to analysis in applied behavior analysis?". They have identified some important problems and proposed some interesting solutions. Their writing of that article is obviously the product of many determinants, some of which they have tried to identify. However, similar events have determined some different behavior in me. This short note is not intended therefore to be a rebuttal, but is a presentation of slightly different views of the problem of analysis in applied behavior analysis.

Grossly oversimplified, it seems to me that what has happened is that the promise of the early sixties does not appear to have been met. While applied behavior analysis has undoubtedly had some impact, many of the applications have been trivial or transitory in effect. There has been a drift away from the kind of "science" which was once the hallmark of behavior modification. A decade ago, Hopkins (1970) was able to excuse a lack of dramatic progress with the maxim that "the first twenty years are the hardest." I suspect that one of the determinants of the Pierce and Epling paper is the observation that the next 10 years may not have yielded anything significantly different from the first 20.

Before proceeding further, it may be useful to question the major premise. The world often seems to be getting nearer to self-destruction, and further away from any scientific understanding of the design of cultures, and there are good reasons to believe that very few influential people understand or accept the proposition that it is *behavior* which we need to change, rather than the fictional entities of attitudes, confidence etc. Certainly the message of *Beyond Freedom and Dignity* is having less impact than we would like. There are, however, some areas where a behavioral approach is having an effect. Its terms and concepts are entering everyday language, and it is the most common "bias" on training courses for applied psychologists, at least in the United Kingdom. As Hoon and Lindsley (1974) have pointed out, the annual number of behavior therapy articles published now exceeds that of psychoanalytic articles—although I agree that much of the behaviorism around is of the type criticized by Pierce and Epling. The situation may not be so desperate as it sometimes appears, but I do agree with Pierce and Epling that things could be very much improved. So what is the solution? According to Pierce and Epling, it lies in "basic research," as exemplified by those studies published in *Journal of the Experimental Analysis of Behavior (JEAB)*. Branch and Malagodi (1980) also recommend a training in the animal laboratory as a partial remedy to similar problems. I see the reasoning behind this, but there are some hidden complexities which give me cause for concern.

For some years now, there have been fewer and fewer papers in each new issue of *JEAB* which have had interest or relevance to me as a clinical psychologist working within a radical behaviorist framework. This is not because "animal work" has little relevance to clinical psychology—some of it has a direct relevance in terms of useful procedures. The literature of errorless discrimination is a good example here (cf. Cullen, 1976).

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Also, some experiments carried out in an animal laboratory have important conceptual relevance—consider, for example, experiments in "memory" in pigeons as described in Jans and Catania (1980). However, the number of studies having practical or conceptual importance is diminishing. Authors in *JEAB* are changing their practice, and there has been a noticeable and alarming shift in the type of studies published. As Ferster (1978) puts it ". . . . operant conditioning (is) getting bored with behavior."

There appear to be two distinct ways in which this shift is taking place.

1. The first is the trend towards data transformation. What the organism actually does is more and more being used as a tool for the construction of complex mathematical models. One of the "basic principles" which Pierce and Epling commend is the matching law, and by coincidence it seems that this "discovery" was at the forefront of the move towards data transformation. What started as an important observation of the relationship between reinforcement rate and response rate has become, as the influences of more independent variables are examined, a field in which one needs either to be totally immersed, or to approach with a high qualification in mathematics. Otherwise it is difficult to understand what is happening. Catania (1980) pointed out at a recent International Symposium on Steady State Operant Behaviour that the research behavior of *JEAB* authors seems to have come under the strong control of contingencies established by grant-giving bodies, rather than those most useful to fostering a practical science of behavior. Although taken somewhat out of context, what is a practical clinician to do with the multivariate rate equation proposed by McDowell (1980)? The interested reader is urged to consult this source to see just how far from behavior *JEAB* can go.

2. The second reason for questioning relevance of the usual *JEAB* type studies for applied behavior analysis has been highlighted by Branch and Malagodi (1980)—the increase in the use of mentalistic terms. Not only has this trend been recognized by the editors of *JEAB,* but it has been accepted as a reasonable state of affairs, and apparently nothing to worry about.

> "Recently a number of researchers have interpreted their work in relation to theoretical terms derived from cognitive psychology. . . . So long as the methods and findings of empirical work are described objectively and unambiguously, investigators should be free to use whatever theoretical language is most effective in making the significance of the work clear."—Nevin, 1980, p. 134

On these criteria, there is no reason for *JEAB* to reject most of the studies published in, for example, *Journal of Verbal Learning and Verbal Behavior.* Most reputable journals insist on certain basic standards, among which are a clear and unambiguous Method and Results section. One of the major aspects of importance of radical behaviorism is how behavior is interpreted. Nevin (1980) is suggesting a marked and significant move away from this view, but one which has been apparent for some time in issues of *JEAB.* One example of this trend is the paper on organization in memory and behavior by Shimp (1976). Sure enough an alternative view was later presented (Branch, 1977) but the message in Nevin (1980) is clear enough.

For these reasons, I believe that Pierce and Epling (1980) are mistaken in thinking that a flight to *JEAB* will be our salvation. But I think they have identified some real problems, so where do we go from here? Donald Baer may have the answer. On analyzing the relationship between "basic" and "applied" research, he has concluded that there is "no need to discuss relation until you locate two parties" (Baer, 1978). For radical behaviorists there is no distinction between basic and applied research. What there is is a way of trying to understand the world: a science applicable to pigeons in experimental chambers, children in classrooms, and statesmen in high office.

Certainly, laboratory studies of simple operants have been immensely useful in illuminating important basic principles, but there may be reason to believe that we know enough general principles to move away from the laboratory to test the usefulness and generality of these principles. There is the danger that those who stay in the animal laboratory will come under the control of contingencies which provide for novelty seeking. There seems to be a paucity of studies now identifying major and significant functional relationships, but there are many which identify weak and practically useless relationships. Such are the contingencies set up by journal publication and research grant bodies.

There are many competent researchers working with rats and pigeons who have no commitment to the philosophy of radical behaviorism. There are many radical behaviorists who have little interest in rats and pigeons. It is *social behavior,* about which further study of rats and pigeons is unfortunately likely to add little, which is of importance now. Although *Verbal Behavior* is an extrapolation of principles first studied in the animal laboratory, its success at interpretation lies in the analysis of human social behavior. There is precious little in that book which is *directly* derived from the animal laboratory, and in the exceptionally rare instances where the theory has been tested, it has proved to be remarkably robust (cf. McLeish & Martin, 1975).

I am unconvinced that the cure-help approach and behavior analysis are somehow incompatible. A solution to our current difficulties lies not in a flight to the laboratory, but in the application of radical behaviorism—as a philosophy of the science of behavior (Skinner, 1963)—to problems of social importance. When we engage in that exercise, we will then have a chance of effecting important social contingencies. To close with the words of Baer (1978)

"Quite a lot of bar presses, key pecks, switch closings, alley runs and barrier leaps, have already been analyzed and found amenable to reinforcement variables. A good deal of red and green, squares and triangles and other odd shapes, and myriad tones and buzzes have been discriminated under the press of reinforcement contingency. Hardly anything else seems worth trying reinforcement on but language and social problems" (p. 14)

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