The Behavior Analyst 1982, 5, 53-64 No. 1 (Spring)

Defining Applied Behavior Analysis: An Historical Analogy

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This article examines two criteria for a definition of applied behavior analysis. The criteria are derived from a 19th century attempt to establish medicine as a scientific field. The first criterion, experimental determinism, specifies the methodological boundaries of an experimental science. The second criterion, philosophic doubt, clarifies the tentative nature of facts and theories derived from those facts. Practices which will advance the science of behavior are commented upon within each criteria. To conclude, the problems of a 19th century form of empiricism in medicine are related to current practices in applied behavior analysis.

The purposes and practices of applied behavior analysis have recently received considerable attention. Many scholars in the field have discussed the types of activities in which applied behavior analysts ought to engage. Some have stated that the field has become too technological, or that research findings no longer relate to behavior principles, or that the improvement of behavior has replaced the investigation of independent variables as a primary goal of the field (see Birnbrauer, 1979; Deitz, 1978; Hayes, 1978; Michael, 1980; Pierce & Epling, 1980). Others have agreed that many of these trends are occurring but have stated that these activities are the proper domain of the field (see Azrin, 1977; Baer, 1981).

So far, these discussions have been phrased so that they seem to be about which direction is *best* for the field, or even what is "good news" and "bad news" within the field (Michael, 1980; Baer, 1981). It seems to me, however, that these discussions are actually about something quite different. Through these various commentaries, applied behavior analysts are working toward a *definition* for applied behavior analysis. Such a definition must include criteria (see Wittgenstein, 1965, pp. 24-25) through which decisions can be made about what does or does not "count" (see Harzem & Miles, 1978, Ch. 2) as an applied behavior analysis. By advocating one or another point of view, these authors have been commenting on the criteria they think should define this field.

Arguments over criteria for a definition of applied behavior analysis are fairly recent. The definition presented by Baer, Wolf, and Risley (1968) was probably the first, and little or no disagreement was originally stated about their seven criteria. They discussed their criteria as dimensions by which to evaluate "a study which purports to be an applied behavior analysis" (p. 92). The current discussions, however, suggest that their criteria are either no longer relevant (Hayes, Rincover, & Solnick, 1980, showed how research in the field decreasingly meets the requirements of the dimensions) or, in cases where the same criteria are used to support either type of position, that they are no longer sufficiently specific.

Through these previous discussions, several alternative sets of criteria have been suggested. One alternative was to define the field as an all-inclusive category. This approach would rather quickly halt the discussions but would not really clarify anything. As Harzem and Miles (1978) explained, a concept which excludes nothing is only confusing. To make sense out of the field requires that some types of activities *not* be applied behavior analyses. Deciding what to exclude is only the other side of the problem of deciding what to include and that is what the discussions are already about.

Behavior analysts could also return to the "applied versus basic" distinction but they would soon realize that clear criteria for that distinction are not available and the problems would remain. Criteria based primarily on the type of behavior with which a researcher works are somewhat arbitrary and open ti many differences of opinion.

Another form of distinction which I have previously advocated (Deitz, 1978) is

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between activities which are or are not scientific. That question has historically received more thorough attention, and several sets of criteria for a definition of a science are readily available. In this article, I will present the details of one of these sets of criteria which could adequately define applied behavior analysis as an experimental science. These criteria are drawn from early writings in the field of medicine. To hold, an analogy between 19th century medicine and 20th century applied behavior analysis must be supported. There are two bases on which I feel such an analogy can be justified.

First, more than 100 years ago the field of medicine was engaged in discussions similar to those now occurring in applied behavior analysis. At that time some physicians, like some current behavior analysts, argued that their task was to tend to the sick; they were to use any available skill to cure their patients. Other physicians, like other current behavior analysts, argued that producing a cure was not enough; a physician must also strive toward learning why the cure was effective.

Second, medicine in the 19th century was a comparatively young field as applied behavior analysis is a comparatively young field today. In the 1860's medicine was only just emerging from the influence of vitalism as applied behavior analysis is now emerging from the influence of mentalism. It was clearly the case in 19th century medicine that there were few established "facts." There is also some question about the number of established facts available to current applied behavior analysts.

The debate in medicine was resolved, at least at the level of intellectual discourse, when Claude Bernard (1813-1878), the founder of modern experimental physiology, presented two criteria for defining medicine as a science. Bernard presented his argument in a book titled, *An Introduction to the Study of Experimental Medicine* (1865/1957) where he stated that an experimental medicine must follow "the principle of determinism united with philosophic doubt" (p. 172). His thorough explanations of determinism and doubt served as the bases for the establishment of an experimental medicine and continue as the bases of most modern scientific thought. My purpose is to see how useful Bernard's criteria are for a definition of a scientific applied behavior analysis. Accordingly, all quotes are from his 1857 volume, republished in 1957.

To that point, and with the help of some other 19th and 20th century investigators, I plan to review the criteria of experimental determinism and philosophic doubt, show how they clarified the goals and practices of an experimental medicine, and suggest points on which they are relevant to the practices of a scientific applied behavior analysis. I will also show how in the past rather useless practices in medicine arose when these criteria were not followed and will suggest how that omission could also affect applied behavior analysis.

EXPERIMENTAL DETERMINISM

Experimental determinism is the first of the necessary criteria defining the practice of experimental medicine, or any science for that matter. The principle of determinism states that nature is orderly and lawful. With determinism, the experimenter works from the assumption that there are necessary and sufficient causes of phenomena and that these causes can be discovered.

The study of these causes (the conditions under which an event will occur and without which the event will not occur) is the practice of science. If at first some phenomenon seems indeterminate, the investigator is required to continue experimenting until the causes of that phenomenon are discovered. As Bernard stated,

> Indeed there must be error or insufficiency in the observation; for to accept a fact without a cause, that is, indeterminate in its necessary conditions, is neither more nor less than the negation of science. (p. 54)

This strict notion of experimental determinism has come under some recent criticism. The argument has been presented that since the advent of 20th century physics, mostly since the proposal of Heisenberg's uncertainty principle, this explanation of determinism is passé. Without going into a lengthy discussion of this issue, it must be mentioned that Heisenberg defended a notion of causality. As late as 1979, Heisenberg stated,

> We have casuality in that sense—that in order to influence something, there must be an action from one point to the next point; no action can happen if there is not this connection. (Heisenberg, 1979, p. 11)

He also defended the necessity of assuming that there are natural laws which can be uncovered; he stated, "In physics, we can only work with the assumption that we have natural laws. If we have no natural laws, then anything can happen, and we can only describe what we see, and that's all" (Heisenberg, 1979, p. 15). While many ideas in science may have changed in this century, it appears that this description of experimental determinism remains relevant.

But the principle of experimental determinism is far more than an assumption on which to begin a science. It is the principle of determinism which guides the methodological practices of the researcher. Specific methodological practices can be derived from a thorough analysis of this assumption. Bernard completed such an analysis, and his suggestions are remarkably similar to many current ones in the analysis of behavior (see Johnston & Pennypacker, 1980; Sidman, 1960).

Bernard began with the acknowledgement that working with the human organism was extremely complex. Primarily because of that difficulty, an adequate experimental method was important. As Bernard explained, "The more complex the science, the more essential it is, in fact, to establish a good experimental standard" (p. 3). Without this standard, the science cannot progress. Bernard stated, "a bad method . . . of research may cause the gravest errors, and may retard science by leading it astray" (p. 15).

Bernard derived three requirements for an adequate method from the principle of determinism and showed how each of these was essential for an experimental medicine. First, "the principle of experimental determinism does not admit of contradictory facts (p. 173)." When two findings are incompatible, this requirement forces the researcher to continue investigating a problem until a firm conclusion can be drawn regarding the causes of that problem. However, the researcher must guard against looking for conclusions which only seek to confirm his hypotheses, a caution also posed to behaviorists by Sidman (1960); Bernard stated,

> Our experimenter puts questions to nature; but that, as soon as she speaks, he must hold his peace; he must note her answer, hearing her outand in every case accept her decision . . . he must never answer for her nor listen partially to her answers by taking, from the results of an experiment, only those which support or confirm his hypotheses. (pp. 22-23)

Bernard's own work in medicine provides an excellent example of continuing on a problem until an adequate resolution has been reached (Bernard, p. 173). In his laboratory, Bernard once produced diabetes by puncturing the floor of the fourth ventricle. In eight or ten attempts at replication, he failed to produce diabetes. As Bernard said, he now had one positive fact (which also was somewhat unexpected) and eight or ten negative facts. At that point he could (a) deny the positive fact (since it "shouldn't" have happened anyway), (b) claim that the procedure produced the effect 10% of the time, or (c) continue in an attempt to determine the necessary and sufficient causes of a phenomenon which had obviously occurred. His requirement against contradictory facts forced the third option and he was eventually successful.

This first requirement is also important if applied behavior analysis is to be defined as a scientific field. Applied behavior analysts would need to continue work in an area until a satisfactory resolution of the causes of a social problem is achieved. Much of the current work in the field, however, stops short of this goal. Foxx and Schaeffer (1981), for example, instituted a lottery to reduce levels of driving of a company's employees. While overall driving decreased, some participants actually increased their driving. They did not continue their investigation until they discovered the factors responsible for the differences between subjects. In fact, when discussing the average decrease, they stated, "it is impossible to say exactly what caused the experimentals' driving reduction because several factors were operating during the lottery condition besides the simple lottery contingency" (p. 283).

While Foxx and Schaeffer probably contributed to the solution of an important social problem, their study does not meet this first requirement specified by experimental determinism. They would have had to continue their efforts until all participants decreased their driving and until they had accounted for the factors which produced this result. A scientific applied behavior analysis needs that information. This is not to say that Foxx and Schaeffer produced something of little value. Their findings may be of immeasureable value for society; for a science of behavior, however, little information was acquired.

Other efforts in the field more closely approximate Bernard's first requirement. Barton (1981), for example, was interested in improving sharing by children. His approach was to investigate the conditions necessary to produce sharing. He analyzed the components of a treatment package to see which were or were not effective. His research not only improved sharing, it allowed him to make statements about the factors responsible for sharing.

Many current applied behavior analyses approach this scientific requirement; some, however, stop too soon. If the field is to be defined by scientific standards, researches must be continued until answers are derived. These efforts will require studies with several experiments, each working toward teasing out the necessary and sufficient conditions for some phenomenon. Success is achieved when the results are replicable—when all subjects exposed to the conditions behave in essentially the same ways. If data are not reported or subjects eliminated from a study, these must be done according to defensible rules (see Johnston & Pennypacker, 1980). Efforts which do not meet these specifications may be valuable for improving society but they do not meet this first requirement defining a science of applied behavior analysis.

The second requirement for experimental method derived from the principle of determinism was that this principle "ejects causeless and irrationale facts from science" (Bernard, p. 178). Only facts for which the necessary and sufficient causes have been identified are useful for a science. While this issue may seem too obvious to discuss, "causeless" medical phenomena were prevalent in the 19th century. Rather than search for the causes of these phenomena, medical personnel invoked the doctrine of vitalism, a doctrine stating that human functions occurred because of some special, indeterminate human force, to explain these phenomena. The principle of experimental determinism does not allow for such an explanation, however. Bernard wrote,

> But vitalistic ideas . . . are just a kind of medical superstition—a belief in the supernatural. Now, in medicine, belief in occult causes . . . encourages ignorance and gives birth to a sort of unintentional quackery; that is to say, the belief in an unborn, indefineable science. (p. 67)

While most behavior analysts would agree, at least in principle, with this analysis, some current versions of behavior analysis appear to begin to slip into a 20th century form of vitalism, and this should be avoided if applied behavior analysis is to be defined as a science. Some statements made in the name of cognitive behaviorism (e.g., Mahoney, 1974) seem to reflect the belief in a special human force. To assume that behavior is caused by a nonphysical (or even physical) cognitive event is not only a potential conceptual error (see Harzem & Miles, 1978; Skinner, 1977), it can contribute to methodological shortcomings as well.

Often, these shortcomings derive from being misled into treating the cognitive event as the sole independent variable of interest. Research stops before the causes of the cognitive event are explored. Wilson and O'Leary (1980), for example, state that "we tend to react, not to the *actual* environment, but to the environment as we *perceive* it" (p. 244). Such a statement may be perfectly true but it can mislead researchers to account for behavior in terms of perceptions rather than also to account for the perceptions in terms of discriminative conditions responsible for them (see Skinner, 1974). This practice potentially "encourages ignorance and gives birth to a sort of unintentional quackery" (Bernard, p. 65) as much in the practice of applied behavior analysis as it did in the practice of medicine.

The third requirement is that "the principle of determinism requires comparative determination of facts" (Bernard, p. 181). Through this requirement Bernard is demanding what he called proof and counterproof for the establishment of a fact. The experimenter must determine that the event occurs in the presence of the cause *and* that it does not occur in the absence of the cause. He also required that the fact occur in each of several individuals. Finally, he stated that facts be determined without the use of statistics.

These requirements were stated to insure that medical personnel went to sufficient lengths to determine the causes of disease. Much of the research in medicine during the 1800's was poorly conceived and executed. For example, Bernard criticized one researcher interested in the analysis of urine for collecting specimens from urinals in a train station to "present an analysis of average European urine" (Bernard, p. 135). Such a finding could neither advance knowledge of urine nor of the factors responsible for differences among individual specimens.

This third requirement sounds remarkably like the requirements stated for behavior analysts by Sidman (1960), Johnston and Pennypacker (1980), and others. Behavior analysts have traditionally studied individual behavior in order to determine the causes of behavior. They have followed intrasubject designs (the ABAB reversal design is a pointed example of obtaining proof and counterproof) and avoided the use of statistics to summarize data or test hypotheses.

Current research in the field shows some changes from these patterns of research (see Hayes, Rincover, & Solnick, 1980). While applied behavior analysts are not presenting data on average American talkouts, there are many published studies which do not report data on individuals. These studies may assist teachers in dealing with classes but they cannot give an account of individual action within those classes. Also, behavior analysts are relying more heavily on inferential statistics. It would be difficult to argue that some descriptive statistics are not useful so long as data are summarized under strict requirements (see Johnston & Pennypacker, 1980). Using inferential statistics to test hypotheses presents more of a problem, however. It is difficult if not impossible to detect the determinants of an individual's behavior through such methods.

If applied behavior analysis is to be defined as an experimental science, Benard's requirements must be fully met for they specify the criteria on which such a definition rests. To Bernard, these issues are neither debatable nor avoidable for a scientific field. He stated that they were essential in even the most obvious cases; they are "a kind of *order* which we must blindly follow even in cases which seem the clearest and most rational" (p. 181).

What is most essential for behavior analysts is that Bernard made no distinctions based on types of medical personnel concerning these rules. A physician in private practice or a physician in the laboratory were to behave in the same way toward their subject matter. For Bernard these rules "are absolutely the same as those which should be followed in therapeutics" (p. 190). Everyone in medicine needed to seek to regulate and master the causes of disease. For applied behavior analysts to accept this analogy, the criteria would have to be the same for those in applied settings as for those in the laboratory. All would need to seek to regulate and master the causes of behavior.

Before decisions can be made about the usefulness of these requirements of experimental determinism for defining applied behavior analysis, one additional issue needs to be considered. This can be done by questioning the analogy between the state of medicine in the mid-19th century and the present state of applied behavior analysis. Few reliable facts were available to practicing physicians when Bernard made his suggestions. Diseases could not be regulated for their causes were not understood. The strict requirements of Bernard seemed necessary if medical personnel were ever to reach that understanding.

Behavior analysts need to examine how well they understand the causes of behavior. If it is determined that sufficient information about those causes is already available, it may be justifiably argued that all behavior analysts do not need to follow Bernard's requirements (or those of Sidman, 1960, or Johnston & Pennypacker, 1980). Some behavior analysts could use the known facts to improve conditions without strict experimentation. Like some practicing physicians of today, some behavior analysts could be "nonanalytic, standard, routinized, packaged, empirical—and effective" (Baer, 1981, p. 87). If sufficient information is not available, it is more likely that Bernard's requirements are necessary.

There are two ways to address questions about the sufficiency of what is "known" in applied behavior analysis. First, the body of fact can be carefully examined and categorized. While there has been no systematic attempt at this form of examination, some authors have discussed the completeness of the behavioral data base. Ribes (1977) stated that behavior analysts do not have the necessary information upon which to build a technology. Epling and Pierce (Note 1) commented on the large number of areas on which careful analysis is still necessary. Sidman (1978) and Ruggles and LeBlanc (in press) have questioned the amount of information available on stimulus control. As Sidman (1978) wrote, "Our understanding of the reinforcement contingency, however, is considerably more advanced than our understanding of stimulus control" (p. 265).

Even if information on reinforcement is sufficient, recent analyses of stimulus control have shown the importance of this component to building a technology of human behavior. Many important academic and social behaviors have been related to what are sometimes called "cognitive" factors. The study of these factors requires analysis of stimulus control for as Sidman (1978) wrote, stimulus control is "essentially the field of cognition" (p. 265). Ruggles and LeBlanc (in press) have discussed problems in academic instruction of more than simple skills, for example, as developing from inadequate information about stimulus control. If knowledge of stimulus control is insufficient, it would be difficult to build an adequate technology for many of the human behaviors which are most interesting.

The second way to address the question of a sufficient knowledge base is more complicated. This requires behavior analysts to examine the types of researches which have been completed against questions about the causes of behavior. It seems to me that applied behavior analysts know less about the causes of behavior than they know about conditions to impose on existing contingencies in order to change behavior. In other words, research in applied behavior analysis usually is aimed at bringing new contingencies into an existing situation. While in place, these new contingencies are shown to be powerful enough to overcome the effects of the existing contingencies. While laboratory behavior analysts look at the variables which *produce* a behavior, applied behavior analysts typically look at variables which *change* behavior. To the extent that these are different (and they may not be different at all), applied behavior analysts do not have facts about the causes of behavior.

But some behavior analysts would argue that there are certain facts about which we are so sure that we can use them to solve some problems without rigidly following Bernard's requirements from the principle of experimental determinism. That may be so, and on that difference my analogy between the status of 19th century medicine and current applied behavior analysis may break down. It also brings us to examine Bernard's second criterion for defining an experimental science. This second criterion—philosophic doubt—raises questions about the amount of faith scientists should put in their facts, even those of which they are currently "positive."

PHILOSOPHIC DOUBT

While experimental determinism is important for guiding the experimental practices of the researcher, Bernard insisted that it be accompanied by a healthy dose of philosophic doubt. In fact, Bernard thought the scientist should doubt everything but the principle of determinism. It is this doubt which forces the researcher to continue to apply the methodology required by the principle of determinism. So, the second criterion for defining applied behavior analysis as a science is philosophic doubt.

Bernard's discussion of doubt is sometimes confusing. It helps to remember that, first, he is talking about scientists. They are the ones who need to doubt but only in the practice of their science. Whether or not any other person in any other endeavor, or whether or not a scientist when talking of issues beyond his or her field doubts anything or everything is irrelevant. Second, Bernard is saying that scientists should doubt the *facts* in their speicality area. Bernard is not talking about doubting facts in other areas; it does not matter, for example, if a psychologist has total faith in a medical fact. The scientist in medicine, however, should doubt that medical fact. In fact, that doubt is one criterion defining actions of the scientist in that area.

The following example might serve to help clarify the confusion about who should doubt and what they should doubt. Due to the increasing publicity of the problems presented by high cholesterol levels, most of us have attempted to decrease our levels of cholesterol by dietary restrictions and switching to polyunsaturates. We (who are not medical scientists) do not doubt this medical fact. However, scientists in the area do doubt it and continue to examine issues in this field. This is fortunate for recent evidence shows that a switch to polyunsaturated fats may result in "prematurely aging our cells and perhaps even creating additional cancer risks" (Rosenfeld, 1981, p. 24). As Rosenfeld (1981) continued, "If this new irony teaches us anything, it's that all scientific knowledge . . . is tentative" (p. 25). Facts must be doubted by the scientists; new methods and new discoveries may always modify even those facts of which we are most sure.

Bernard's advocacy of doubt is confirmed by the writings of some modern philosophers in their discussions of the principle of induction. While it is generally agreed that statements about lawful relations are inductively derived (i.e., that they are derived from a number of past instances), there is an interesting irony in the principle of induction. No matter how many cases provide evidence for the law, it is never certain. On the other hand, however, it is not uncertain, either. As Russell (1961b) stated,

> Thus our inductive principle is at any rate not capable of being *disproved* by an appeal to experience. The inductive principle, however, is equally incapable of being *proved* by an appeal to experience. (p. 153)

The logic of induction, then, shows that facts are always slightly questionable. If facts are never certain, it becomes more difficult to argue with Bernard's advocacy of scientific doubt.

These arguments suggest that discussions mentioned in the last section over how much is known in applied behavior analysis must always end by acknowledging that all that is known is, at best, only highly probable. There is no certainty available through science. Applied behavior analysts can decrease this uncertainty, however, but only through the path dictated by experimental determinism.

This necessarily cautious approach to facts in an area leads to an equally cautious approach to the construction of theories derived from these facts. Theory construction is not always a problem but

Bernard warned that,

> we must believe in a complete and necessary relation between things . . .; but at the same time we must be thoroughly convinced that we know this relation only in a more or less approximate way, and that the theories we hold are far from embodying changeless truths. (p. 35)

Several hindrances to the advance of a science can follow from an overly strong belief in a theory. For Bernard, two of the most problematic occurrences were that theories can create poor researchers and that theories can produce scientists who do no research at all. On the former, Bernard stated, "Men who have excessive faith in their theories or ideas are not only ill prepared for making discoveries, they also make very poor observations" (p. 38). As for the latter, Bernard warned of those he called " 'systematizers'—these men start from a fact which they regard as absolute truth—then they reason logically without experimenting and build a logical system . . . which has no sort of scientific reality" (p. 37).

This caution of theories is not limited to Bernard and his version of the medical model. The physicist David Joseph Bohm in discussing this issue began by commenting on the several phases through which science has progressed. First, science relied on authority; for example, if Aristotle said it, it was true. Second, the influence of Roger Bacon shifted the test of truth from authority to experience. Third, science moved into a phase where special, simple experiences were arranged as a test for truth; experiment replaced experience, for ordinary experience often did not allow the scientist to see what was being tested. Fourth, in a phase especially common to physics, very complex experiments with elaborate, expensive equipment were developed (Bohm, 1979, pp. 126-127).

Bohm (1979) was critical of both the first and last phases for somewhat the same reasons. His criticisms of the first should be somewhat obvious. The reliance on authority, or more particularly, the theories of an authority, could not provide very accurate information for the growth of a science. Even Aristotle, an excellent example of Bernard's "systematizer," while extremely intelligent, posited some rather foolish theories. Aristotle claimed, for example, that men had more teeth than women but never bothered to experimentally test this supposition (see Russell, 1961a).

Bohm's criticism of the last phase shows a more subtle, but equally unproductive, influence of theory on the practices of the scientist. In many ways, phase four is very close to a return to phase one. In physics today elaborate equipment is necessary to conduct research, but often the decision to build a particular apparatus rests upon the decision to test a certain theory—the statements of a particular "authority." This can occur because much of the special equipment in physics is only useful for testing that theory (see Bohm, 1979). This forces the physicist to invest too much of his or her intellectual resources in only testing the correctness of that theory. As Sidman (1960) warned psychologists, and Bernard warned physicians, problems can develop when one conducts research primarily to test certain hypotheses.

This reliance on theory, the avoidance of doubt, has been criticized by famous men of practical affairs, as well. More than theoretical or laboratory scientists find that the formulation of a theory can interfere with their progress. Sherlock Holmes, in the practice of his craft, bemoaned, "The temptation to form premature theories upon insufficient data is the bane of our profession" (Doyle 1930, p. 779). It seems that he could have been describing psychology or physics or medicine rather than detective work.

In general, applied behavior analysts have been quite cautious of theory construction. They have accepted the tentative nature of their facts and continued their investigations. If applied behavior analysis is to be defined as a science, this tradition must be continued and perhaps reemphasized. Even the most clearly agreed on facts must be doubted. There are advantages of such actions for practitioners as well as scientists. The recent analyses by Konarski, Johnson, Crowell, and Whitman (1980; 1981), for example, show how the response deprivation hypothesis may be more effective than the traditionally accepted empirical law of effect for both selecting reinforcers in work toward behavior management and understanding the conceptual bases of reinforcement.

There are some areas in whcih applied behavior analysts need to tread cautiously. Currently, there is a growing practice of building an account of behavior in terms of the principles of what has been called radical behaviorism. Rather than investigating the causes of behavior, some behavior analysts will describe them with reference only to the theory of behaviorism. The accounts derived from such practices may be plausible but there is no way to determine their accuracy without experimentation.

To rely too strongly on these logical systems is to disregard Bernard's criterion of philosophic doubt. For behavior analysts, as Zeiler (Note 2) explained, the principles of radical behaviorism are somewhat irrelevant to the experimental analysis of behavior. Those principles may all turn out to be incomplete or even inaccurate, but the facts derived from the experimentation will remain important to an explanation of behavior. Facts derived from experimentation will modify a theory but the theory cannot obviate the facts. Strong commitment to the theory can, however, mislead or confuse the scientist.

The behavior analyst, then, must maintain doubt, even of the theory most closely related to currently accepted behavioral facts. Doubt will keep behavior analysts on the path dictated by determinism: the search for the variables of which socially important behaviors are a function. As Bernard stated for this criterion of a science:

> In scientific education, it is very important to differentiate . . . between determinism which is the absolute principle of science, and theories which are only relative principles to which we should assign but temporary value in the search for truth . . . By exaggerated belief in theories, we should give a false idea of science; we should overload and enslave the mind, by taking its freedom, smothering its originality and infecting it with the taste for systems (p. 39).

THE PLAGUE OF EMPIRICISTS

The lessons derived from the principle of experimental determinism bolstered by philosophic doubt are important to everyone in the field of behavior analysis. These lessons aid the laboratory investigator and the practicing therapist equally. In fact, the practice of scientific behavior analysis can keep members in the field from becoming 20th century versions of what in the medical profession in the 19th century were called "empiricists." Guiding the avoidance of this transition may be the ultimate contribution provided by using Bernard's criteria to define applied behavior analysis as a science.

Because the word empiricism has somewhat changed in its usage since the middle of the last century, some definitions are probably in order. Today, we think of empiricism as "the practice of relying on observation and experiment especially in the natural sciences" (Webster's, 1977, p. 373)—exactly the functions required by determinism and doubt. In the 18th and 19th centuries, however, empiricism was a "school of medical practice founded on experience without the aid of science" (Webster's, 1977, p. 373). Medical empiricists, often a separate group from physicians, relied on experience, but not on experiment.

This is not to say, however, that those labeled physicians relied on experiment, either. In fact, medical problems often persisted because neither group were guided by both the principle of determinism *and* the practice of doubt. The physicians most often observed neither; the empiricists doubted but avoided the dictates of determinism.

One of the clearest cases of these two approaches to medicine was provided in the 1793 yellow fever plague in Philadelphia (Powell, 1949). During this plague, the physicians relied on a very democratic approach to determining a cure for the disease. They met in a convention early in the summer and voted on a cure based on the theories of the more prominent among them. The majority won and all physicians were expected to use that cure on their patients. Their cure consisted of purges (enemas) and large doses of mercury. The empiricists, much criticized by the physicians, originally used the physicians' cure but after many deaths, dropped it and tried other cures. Throughout the summer, the empiricists tried one cure after another somewhat haphazardly, hoping that one would work. The physicians faithfully used their cure throughout the summer (except in some well documented cases where physicians fell ill; see Powell, 1949).

The illogic of the physicians is clearly a case of the absence of both determinism and doubt. Since we know more today about the effects of mercury poisoning, we can laugh easily at their foolishness. The flaws in the empiricists are more obscure. In terms of social objectives they were admirable; they saved many lives by discontinuing mercury poisoning. Their combinations of whatever drugs and practices they knew of sometimes ended in a cured patient. But they never learned which part of a treatment aided a cure, which part hindered, or which part did nothing at all. The empiricists doubted but failed to follow the dictates of determinism. Adherence to the experimental methodology dictated by determinism could have aided the early discovery of an effective cure.

Bernard criticized empiricists for this limitation:

> Empiricists, with their faith in the efficacy of drugs as a means of changing the direction of diseases and curing them, content themselves with empirically noting medicinal effects, without trying to understand their mechanism scientifically. They are never perplexed: when one remedy fails, they try another; they always have receipts or formulae at hand for any and every case, because they draw on an immense therapeutic arsenal. Empirical medicine is certainly the most popular. People believe that through a kind of compensation nature provides a remedy for every ill, and that medicine consists in a collection of recipes for all ills, handed down to us, age by age, since the beginning of the healing art. (p. 209)

Bernard felt that physicians could not stop at empiricism; they must "experiment scientifically to understand the physiological mechanism producing disease and the medicinal mechanism effecting a cure" (p. 210).

In the practice of medicine, physicians could not stop at relying only on practices which had been shown to work at some time or another for some ailment or another. When trying the methods provided by empiricism, they must go farther and approach experimentation. Bernard thought, "we must suffer empiricism; but trying to set it up as a system is an unscientific tendency" (p. 211). The best practice of medicine, according to Bernard would come from a fusion of empiricism and experimentation. "Empiricism is nothing but the first step of the experimental method . . . empiricism cannot be the final stage" (p. 210).

Several problems derive from continuing with only strict empiricism. Primarily, these problems are those derived from spurious or adventitious results, a common occurrence when strict experimentation is not employed. As Bernard concluded for medicine,

> But medicine is still in the shades of empiricism and suffers the consequences oif its backward condition. We see it still more or less mingled with religion and with the supernatural. Superstition and the marvelous play a great part in it. Sorcerers, somnambulists, healers by virtue of some gift from Heaven, are held as the equivalent of physicians. (p. 43)

It is on this issue of empiricism unaccompanied by experimentation where a scientific definition of applied behavior analysis becomes most important. Behavior analysts can become 20th century versions of the 19th century medical empiricists if this definition is ignored. To advise behavior analysis to use "whatever works" to solve a social problem or advocating a more "liberal" experimental method when no clear methodology exists to evaluate a problem moves behavior analysis toward the paradigm of the 19th century medical empiricist. Behavior analysts may achieve intermittently admirable results, but questions would remain concerning the meaning of those results.

Probably the clearest case of the parallel between 19th century empiricists and some 20th century behavior analysts lies in the acceptance of results produced by what have been labeled "treatment packages" (Azrin, 1977) as applied behavior analyses. The medical empiricists used many techniques to cure their patients. Since they rarely knew which one would work for a certain disease or why it would work, they often combined many of them and were satisfied as to their effectiveness if their patient survived. In using treatment packages today's behavior analysts follow the same pattern. Several complex, multifaceted procedures are combined, and success is claimed if some general category of socially important behavior is increased or decreased. Effective parental behavior might be increased by a short lesson plus observation in the home plus feedback, for example. Or a child's misbehavior might be decreased by verbal instructions plus omission training plus timeout. The end results of such programs may satisfy their clients and the public, but through these methods, as in the 19th century empircism, little is learned about which part of the treatment aided a cure, which part hindered it, or which part did nothing at all.

A more careful analysis of these treatment packages is required by the fusion of empiricism and experimental determinism, especially if unexpected, detrimental effects (sometimes mistakenly called side-effects but the only difference between side-effects and main effects is in the expectations of the researcher) are possible. Also, like physicians, behavior analysts need to determine the "optimal dose" of the components of a treatment package. So, a scientific applied behavior analysis is not satisfied that a package produced a cure. It suggests not only an analysis of the effects of each component of a package, but also an analysis of the effects of different parameters of those components.

CONCLUSIONS

The continuing advancement of applied behavior analysis will be insured by the acceptance of the criteria of experimental determinism and philosophic doubt as bases of a definition for the field. The methodological practices derived from determinism will structure research so that it clearly remains within the boundaries of experimental science. Maintaining doubt will insure that applied behavior analysts continue their search for the variables which determine socially important behavior. Doubt will also keep them from believing too strongly in behavioral facts or theories which are already available.

Just as Baer, Wolf, and Risley (1968) restricted what efforts should be called applied behavior analyses, these criteria are also restrictive. This is not to say that efforts which do not meet these criteria are less valuable; they only fall into a different category. These other efforts are a first, necessary step, but as Bernard said about empiricism, they "cannot be the final stage" (p. 210). A scientific applied behavior analysis like a scientific medicine can answer its experimental questions *and* lead to solutions for critically important social problems, as well. It seems that no other definition could lead to a more useful result.

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