DEFINITION IN BEHAVIORAL SCIENCE:
A REVIEW OF B. B. WOLMAN'S
DICTIONARY OF BEHAVIORAL SCIENCE¹

WILLIAM M. BAUM

HARVARD UNIVERSITY

A dictionary can be an important book. At its best, it can define a field of study by delineating a coherent terminology. Failing this, it can still accomplish the same for areas within a field. At the least, it should describe the word usage of specialists in the various areas, so that a layman or specialist attempting to broaden his knowledge will have an easy guide to interpreting statements he may read or hear.

EVALUATION

It would probably be unrealistic to expect coherent terminology even for the diverse subjects that go by the name "psychology"; for a field as broad as "behavioral science", even more so. This dictionary clearly aimed at more modest goals, for it reveals no attempt at overall coherence.

What of the second possible objective? The reader of this journal will wish to know if the dictionary provides coherent terminology for a science of behavior. Here again, no attempt at coherence is evident. We find operant conditioning defined as "a form of conditioning", but no entry under "conditioning". "Aversive conditioning" is called "a form of counter conditioning", but no definition of "counter conditioning" is offered. The word "punishment" appears repeatedly in definitions of various terms, but is nowhere defined itself. Under "discriminative stimulus" and "stimulus, discriminative", we find two different definitions. One of three definitions of "generalization" refers to Pavlovian stimulus generalization, but under "stimulus general-

ization" itself, we find only "stimulus generalization; primary", with no explanation of the modifier "primary" and no other type of stimulus generalization defined.

Inconsistency

This lack of cross-referencing and standardization of terms might be less troublesome if the terms, defined and undefined, were used consistently within definitions and from one definition to another. No such luck. The beginning of the definition of operant conditioning, "a form of conditioning in which reinforcement is contingent upon the occurrence of the response", suggests that it is a type of procedure. Two sentences follow, distinguishing operant conditioning from instrumental conditioning, which in its definition is called a procedure. Consequently, the next sentence seems perplexing: "... the primary measure of operant conditioning tends to be the rate of responding". Can one measure a procedure, or is it now a process? The next sentence offers no resolution because it begins, "Operant conditioning experiments usually involve a small number of subjects . . .". If it were a procedure, then why insert "experiments"? Are these experiments *with* operant conditioning (procedure) or *on* operant conditioning (process)? Or, worse yet, there is a third possibility: "operant conditioning" might here refer to an entire set of problems or an area of inquiry. This third possibility is borne out in such statements later in the definition as, "Operant conditioning often leads to a behavior analysis . . ." and "Attempts to account for behavior in terms of physiology or biology play little part in operant conditioning."

Such inconsistencies abound throughout the book. No distinction between "reinforcer" and "reinforcement" appears. Conditioned reinforcement, for example, is defined as, "A rewarding event or state of affairs . . .", while a

¹New York: Van Nostrand-Rheinhold, 1973. Pp. ix + 478. \$19.95. Preparation of this article was supported by Grant GB-28493 from the National Science Foundation. Reprints may be obtained from the author, Department of Psychology and Social Relations, William James Hall, Harvard University, Cambridge, MA 02138.

conditioned reinforcer as, "A stimulus which has reinforcing properties . . .". Whereas free-operant avoidance is attributed to Sidman, "response-shock interval" is attributed to Skinner. And so on. The student of behavior who seeks a coherent account of the terms he uses need not look here.

One reason behind the inconsistency appears on the title page of the book. Besides the editor, a list of 90 collaborators is given. Among these appear at least seven well-known students of behavior: C. B. Ferster, H. F. Harlow, N. E. Miller, O. H. Mowrer, J. M. Notterman, W. N. Schoenfeld, and B. F. Skinner. The diverse views of these men would make integration of their contributions an editor's nightmare. From the definitions themselves, it appears that the editor may not even have tried.

The third possibility remains. Could the dictionary serve as a source book for laymen and psychologists unversed in behavioral terminology to familiarize themselves with its usage? The answer is unequivocally, "no". Someone who desires an easy, accurate introduction to behavioral terminology should steer clear of this book. It is disorganized, incomplete, inaccurate, and poorly written.

Disorganization

We have already seen one aspect of the disorganization in the inconsistency of usage within definitions and from one definition to another. Although some terms are given multiple definitions, no systematic attempt to catalog various usages is evident. The reader bears the full burden of piecing together the pattern of use of such terms as "operant conditioning", "reinforcement", "punishment", and "stimulus". Add to this the absence of definitions for such key words as "conditioning", "reinforcer", and "contingency", and one can imagine the unwary reader wandering helplessly in a jungle of confusion.

As if the lack of a systematic pattern of usage were not enough, inconsistent listing makes it difficult to locate terms. In explanatory remarks at the beginning, the editor instructs the reader that he will find a term listed by its key noun. So, for example, one looks for "operant conditioning" under "conditioning, operant" and "schedule of reinforcement" under "reinforcement, schedule of". One might argue against this scheme, but it could help the reader find terms quickly. More

the pity, then, that the editor failed to follow his own advice! Many terms have no listing by key noun, but are listed and defined where simple alphabetization would put them. For example, we find "response-reinforcement contingency", but not "contingency, response-reinforcement", "contiguous conditioning", but not "conditioning, contiguous", "response-shock interval", but not "interval, response-shock", "sensory basis of reinforcement", but not "reinforcement, sensory", and many more such. The reader who persists in looking for information about one term or several related terms will find his task much prolonged by unpredictable listing.

Incompleteness

One might suppose that all this confusion reflects merely the confusion of actual usage. If so, the presentation would be confusing, but at least it would be complete. We have already noted, however, that several key terms, such as reinforcer, punishment, and contingency, go entirely undefined. Many of the definitions, moreover, omit important alternative views. In the three definitions of "stimulus", for example, we are told that it "elicits action", "causes the organism to act", and "elicits response". With elicitation the only allowed relationship between stimulus and response, what possibility remains for an accurate definition of stimulus control? Indeed, we find it defined as: "The extent to which the value of an *antecedent* stimulus determines the *probability* of the occurrence of a conditioned response" (*italics added*).

Inaccuracy

Incompleteness shades into inaccuracy; the two can scarcely be distinguished. Some of the definitions already quoted may have seemed peculiar, if not erroneous. Few would disagree as to the inaccuracy of: "*stimulus, discriminative*. In operant conditioning the stimulus which elicits the correct response". With Skinner listed as one of the consultants, it is difficult to see how the definition could miss the mark so widely. So, too, with the definition of behaviorism, which is said to take "overt behavior . . . as the subject-matter of psychology", and which is further defined as "An atomistic theory for which reflexes and the conditioned reflex were the basic units".

A subtler sort of inaccuracy consists in failing to distinguish opinion and theory from accepted fact. For example, in the definition of avoidance conditioning, the explanation in terms of two-factor theory is mingled together with a description of the procedures. The issues are scarcely clarified by the closing paragraph:

Free operant avoidance is maintained without a conditioned extroceptive (*sic*) stimulus. The stimuli controlling this behavior is (*sic*) still in dispute; thus, the nature of avoidance behavior itself is still unsettled.

In the definition of operant conditioning, we find:

. . . response rate as a measure gives at least a simplicity, if not clarity, to the analysis and interpretation of data. Operant conditioning often leads to a behavior analysis in which explanation of behavior is reduced to description, and causation of behavior reduced to explication of functional relations.

The implied slurs on the experimental analysis of behavior—that response rate somehow fails as a dependent variable, and that description in terms of functional relations is somehow inadequate—suggest that the writer has allowed his own hostilities to be incorporated into the definition.

Some inaccuracies appear to arise from simple carelessness. In the definition of operant conditioning, for example, the experimental chamber is described as a “sound-proof, light-resistant box”, the operandum is called a “bar depressor”, and then: “when . . . the operant has been performed . . . the animal is eligible for reinforcement”. Mere proofreading might have saved the definition of pseudo-conditioning: “The elicitation of a response to a previously neutral stimulus when that stimulus is presented following a series of conditioned (*sic*) stimuli.”

Poor Writing

Indeed, better proofreading might have substantially improved the intelligibility of many of the definitions, because numerous errors of spelling and grammar occur throughout. The earlier quote from the definition of avoidance conditioning illustrated some typi-

cal errors. Time and again one encounters cryptic nonsentences, such as one definition of response: “the class or the organisms executing processes” (*sic*). The definition of negative reinforcement, inaccurate even if it were intelligible, stands out: “The use of coercive stimuli for reduction or prevention of probability of reinforcement”.

Some of the quotes so far may have seemed poorly written. The prose is consistently vague and wordy. Some of it would be unworthy of an undergraduate theme. Under “sensory basis of reinforcement”, for example, we find: “Head turning to one side or the other may be rapidly conditioned in the neonate by presentation of a sweet nipple.” And then: “Adversive (*sic*) reinforcement is also observed in which organisms avoid and learn instrumental responses to turn off or reduce noxious stimulation.”

The editor himself might have avoided some confusion by distinguishing between adjectives and nouns. Throughout, key words like “behavior” appear interchangeably as nouns and adjectives. In his explanatory remarks, the editor states that the term “mental disorder” was used interchangeably with “behavior disorder”, even though the title of the book contains the adjective “behavioral”, and we nowhere find the term “mind disorder”.

WHAT SHOULD A DICTIONARY BE?

If we reject this dictionary because it fails to come up to our expectations, we have a responsibility at least to clarify the standard by which we judge. At the outset, three possible goals of a dictionary were suggested: to define a field by showing the interrelationships in usage of its terminology, to do the same for smaller areas within the field, and to provide an accurate guide to usage for laymen and specialists. None of the three precludes the other two. Likely, in fact, to accomplish the first is to accomplish the second and third, and to accomplish the second is to accomplish the third. This ordering, and a professional interest in behavior, prompt us to focus on the sort of undertaking that the first two goals imply.

WHAT IS A DEFINITION?

We should begin perhaps by trying to understand what a definition is. One common view

supposes that definitions are constructed from a set of irreducible undefined terms. We can call this the *deductive* approach, because it likens scientific definitions to those of analytic geometry, which begins with such undefinables as "point" and "line", expresses the simplest definitions with these terms, and complicated definitions always with terms defined more simply. In such a view, a definition constitutes no more than a rule for rewriting. By the use of such rules, even the most complicated definition in analytic geometry can be rewritten entirely in terms of the undefinables. Such a view underlies the "hypothetico-deductive" method of C. L. Hull. Lest anyone suppose that it has passed out of favor today, or that the one who holds such a view is only a man of straw, here is a quote from a colleague's letter, dated 22 January 1974:

... in the case of definitions, the appropriate type are those that can be reduced to a few undefined terms, terms referring to elementary sensory experiences that cannot be defined. However, all else can be defined by these primitives.

Perhaps scientists *should* proceed this way (note the use of "appropriate"), but the history of science suggests that they do not. Consider, for example, the development of the term "stimulus".

Its origin as a scientific term goes back to the birth of the concept of the reflex. Eighteenth-century physiologists applied it, with its nontechnical meaning of "goad" or "spur", to nervous action. It soon became a technical term, meaning "that which provokes nervous action". To Prochaska (1749-1820)², a stimulus excited sensorial nerves, whose action was reflected onto the motor nerves in the *sensorium commune*. Sechenov (1829-1905)² recognized that a sensory event of small magnitude could control a motor action of great magnitude, in the way that a switch can control a powerful electromagnet. By this time (1863)², "stimulus" had lost most of its nontechnical sense of "goad". For Sechenov, it was that which causes reflex action, no longer simply an irritator of nerves. By Pavlov's time (1849-1936)², it had broadened to include any observ-

able event that evoked a measurable reaction of the organism. Skinner (1935; 1938) suggested that a stimulus should be considered not a single event, but rather a class of events having a common effect on behavior. Then, finding the explanatory power of the notion of elicitation too confining, he developed the concept of stimulus control, which related a stimulus not to individual responses, but to patterns and frequencies of responding—that is, to aggregates of behavior.

At each stage in the story, the word was redefined to suit some new conception of the relationship between environment and behavior. Never was the definition changed by deriving it from undefinables. Rather, the word simply came to be used differently.

Inductive Definition

Such a view accords with another approach to definition, based more on actual behavior of scientists than on any formal conception of science. Day (1969) pointed to a line of thought, common to Skinner and Wittgenstein, that equates a word's meaning with its use. Instead of a definition being a statement of equivalence or a formal construction, this view takes it to be a summary of the conditions that are correlated with the word's usage. According to Skinner:

Meanings, contents, and references are to be found among the determiners, not among the properties, of response. The question "What is length?" would appear to be satisfactorily answered by listing the circumstances under which the response "length" is emitted (or, better, by giving some general description of such circumstances). (Skinner, 1945, p. 271)

He is here contrasting his view with the operationism of E. G. Boring and S. S. Stevens, who relied heavily on the writings of the physicist P. W. Bridgman (1927). Bridgman considered the question "What is length?" to be answered by specifying the set of operations used to measure length. His notion of definition belongs to the deductive type, because it specifies an equivalence with terms (operations) that either are themselves undefined or can be reduced to undefinables.

By way of distinction, we can call the approach to definition suggested by Skinner the *inductive* approach, because it relies on gen-

²Examples of these writers' uses of "stimulus" can be found in *A source book in the history of psychology* by Herrnstein and Boring (1966).

eralization from observation, rather than derivation from primitives. It has two great advantages: it facilitates identification of differences in word use from one person to another, and it helps clarify the difference between practical and theoretical definitions. By examining actual occurrences of a term, we can see clearly when two people have different meanings for the same term and when someone is suggesting how a term *should* be used, as opposed to how it actually is used. Deductive definitions most often attempt to specify how a term should be used, rather than how it is used. Inductive definitions can do either, depending on whether they are general or specific to a particular researcher or group of researchers.

DEFINITION, DESCRIPTION, AND EXPLANATION

Scientific terms probably occur most often in description. Even if other uses are equally common, descriptive use gives the most information about the circumstances governing a term's occurrence. Statements like, "The chamber contained two operands" and "The data conform to the matching law" tell us the most about the circumstances leading to use of the terms "operandum" and "the matching law". When we describe A in terms of B, we specify some conditions that govern the use of B. For the inductive approach, then, descriptive use constitutes a principal means of definition.

If explanation can be distinguished from description, then it must constitute a second major use of scientific terms. It might include all answers to questions beginning "Why?" Theories and hypotheses would belong to this category, because, even if conjectural, they are offered as explanations. When we make such statements as "The response occurs at rate P *because* response rate obeys law X" or "Such an effect occurs *because* relation Y applies", we are specifying circumstances governing our use of "law X" and "relation Y". When we say that A occurs because of B (an explanation), just as when we say A exemplifies B (a description), we are partly defining B.

Consistency demands that the definitions of "description" and "explanation" themselves be inductive. Day pointed this out for "explanation":

For Skinner, the preference is to view explanations and predictions as aspects of human behavior. Their nature can be understood only after examining the variety of factors controlling usage of the words "explanation" and "prediction". To raise questions concerning how a particular explanation happens to have been given is to inquire about the behavior of the person offering the explanation . . . To raise questions concerning the *adequacy* of an explanation is to inquire about the effects of the explanation upon the behavior of persons who entertain the explanation. (Day, 1969, p. 504)

Skinner has written a great deal about what he considers adequate explanation to be. We may understand these writings as his attempt to specify the conditions governing his own use of the word "explanation".

Explanation as Description

With Skinner and many other psychologists, it is difficult, if not impossible, to distinguish these conditions from the ones governing use of the word "description". This apparent equivalence suggests that, in practice, explanation is description.

Many writers have argued against such a view. Their positions all depend on the notion that answers to the question "Why?" differ from answers to the question "How?". In reply, Pratt (1939) pointed out that science can provide no answer to the "Why?" that cannot be answered as if it were a "How?":

The power of science lies not in its ability to remove mysteries, but in its ability to predict, with varying degrees of accuracy, the course of events in the future. The *how* of things past gives a hint of how things will be in the future. The control thus exercised over nature does, to be sure, rob nature of some of its mystery, but it is not the kind of mystery meant by those who want to know *why* things are as they are. The laws of planetary motion make it possible to predict where a planet will be, but they do not answer the question as to why the planet could not be somewhere else rather than where it is. To say that the planet *obeys* certain laws is only another way of saying that it does what it does. It does not say that the planet

does what it does *because* of something or other. The inability to assign any meaning to this kind of *because* leaves the mystery of the universe just where it always has been and always will be, namely, beyond the range of human solution. (Pratt, 1939, pp. 154-155)

Part of the reluctance to accept the equivalence of scientific *why* and *how* stems from the recognition that it makes scientific explanation tautologous.

The aversion to circular arguments is the last stronghold of that faith which believes that science can furnish for any given array of well-established facts a *why* which is more than a mere statement of how the facts go together. In certain quarters the faith still lingers on that the laws of nature are something more than the events in nature themselves. The events *obey* the laws, which can not be the same as saying that the events obey themselves. If the statement that nature is subject to law really means that nature is subject to nature, then the so-called laws of science explain nothing. They are pompous tautologies, vicious circles.

The majority of present-day scientists . . . have indeed come to realize that a scientific *why* can be no more than an exact *how*, but the corollary is frequently overlooked, namely, that all arguments in support of any given hypothesis must of necessity be circular. (Pratt, 1939, pp. 149-150)

How many psychologists today have grasped this basic point? Recently, Rachlin (1971) felt the necessity to demonstrate that the matching law is tautologous. The paper was widely considered (mistakenly) to discredit the law. Killean (1972) soon after pointed out that to say a law is tautologous is not to say that it is non-predictive.

Pratt sought to show by logical argument that explanation is equivalent to description. For the inductive approach to defining these terms, the question whether they are *used* equivalently remains. However illogically, many psychologists use the terms under different circumstances. If logic will prevail, the two will either ultimately come to be used

equivalently or will take on new usages consistent with the equivalence of the old usages.

Definition as Description

If we accept the equivalence of description and explanation, then we see a convergence of these two with definition, as well. This follows from the proposition that description and explanation are the chief uses that allow definition. Insofar as definition can be identified with usage itself, rather than description of usage, explanation and description *are* definition.

To illustrate, consider P. W. Bridgman's definition of explanation:

I believe that examination will show that the essence of an explanation consists in reducing a situation to elements with which we are so familiar that we accept them as a matter of course, so that our curiosity rests. "Reducing a situation to elements" means, from the operational point of view, discovering familiar correlations between the phenomena of which the situation is composed. (Bridgman, 1927, p. 37)

We may quarrel with the criterion of familiarity—often the phenomenon explained is more familiar than the terms used to explain it—and we may disagree with the necessity of reduction—the phenomena entering into an explanation may be no less complex than the phenomenon explained—but it is difficult to argue with the basic contention that explanation consists in identification of the observation to be explained with terms that are, if not familiar, at least comfortable. This means that every explanation can be stated in the form: "A occurs because it is an instance of B." The observation A may be an event ("Why does the response occur?") or it may be a relation ("Why does Y increase when X increases?"), and the explanatory term B may be a relation ("When conditions L, M, and N are met, then the response occurs") or a law ("The relation $Y = f(X)$ is a special case of $W = F[Z]$ ").

If explanation consists essentially in identification, then it must be logically identical with description. The statement "A exemplifies B" can be called either an explanation of A by B or a description of A in terms of B.

As an example of usage, this statement might constitute part of the definition of term B. If

a definition summarizes usage, then the definition of term B should predict its occurrence in any given statement of this sort. Depending on whether we take actual usage or the description of actual usage to define a term, explanation and description constitute either the data for definition or definition itself.

There is yet another correspondence. If a definition consists of a summary, then it constitutes an empirical generalization like any other. It enters into statements of the form, "A exemplifies B" as term B, where A represents an instance of usage. For example, the statement "Avoidance exemplifies the negative law of effect" can constitute an instance A of usage of the term "negative law of effect". It exemplifies actual usage, and therefore must exemplify the relation of the definition, "When the term occurs, such-and-such conditions exist". One could say, "This instance of usage conforms to the definition of 'negative law of effect'"—this might be called a description. Or one could say, "This instance of usage occurs because usage of 'negative law of effect' obeys the definition of the term"—this might be called an explanation. Adhering to the notion that a definition consists of a summary of the conditions of use, we still conclude that definition is identical to description and explanation (albeit specific to word usage).

If all three are identical, then all three are incomplete. Like any empirical generalization, a definition will have to be modified to accommodate new instances of usage (new data). Just as there can be no end to description and explanation, so there can be no end to definition. If we can say that science ultimately aims at explanation or description of the natural universe, so too we could say that

science ultimately aims at the definition of its terms.

Returning to our question, "What should a dictionary be?" we conclude that it can never be complete or final, but must be a description of the field at the time. It should reveal the net of interconnected usage, in which, for example, one term can appear in the definition of a second that enters into the definition of the first. Such a dictionary would serve laymen, outside specialists, and researchers in the field. If anyone feels one should be written for behavioral science or only for the experimental analysis of behavior, it remains to be done.

REFERENCES

- Bridgman, P. W. *The logic of modern physics*. New York: Macmillan, 1927.
- Day, W. F. On certain similarities between the *Philosophical Investigations* of Ludwig Wittgenstein and the operationism of B. F. Skinner. *Journal of the Experimental Analysis of Behavior*, 1969, **12**, 489-506.
- Herrnstein, R. J. and Boring, E. G. *A source book in the history of psychology*. Cambridge, Mass.: Harvard University Press, 1966.
- Killeen, P. The matching law. *Journal of the Experimental Analysis of Behavior*, 1972, **17**, 489-495.
- Pratt, C. C. *The logic of modern psychology*. New York: Macmillan, 1939.
- Rachlin, H. On the tautology of the matching law. *Journal of the Experimental Analysis of Behavior*, 1971, **15**, 249-251.
- Skinner, B. F. The generic nature of the concepts of stimulus and response. *Journal of General Psychology*, 1935, **12**, 40-65.
- Skinner, B. F. *The behavior of organisms*. New York: Appleton-Century-Crofts, 1938.
- Skinner, B. F. The operational analysis of psychological terms. *Psychological Review*, 1945, **52**, 270-277, 291-294.
- Final Acceptance 29 April 1974.*