ON THE TACTFUL SPECIFICATION OF MEANING:
A REVIEW OF HARRÉ AND LAMB'S
THE ENCYCLOPEDIC DICTIONARY OF PSYCHOLOGY

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The Encyclopedic Dictionary of Psychology (Harré & Lamb, 1983) seems most aptly described as a heroic attempt to codify the heterogeneity of contemporary psychology. In their preface, the editors indicate that they invited 16 distinguished psychologists, philosophers, and physiologists working in the field of psychological knowledge to contribute 15 major topics each in their area for detailed treatment, plus up to 75 related but minor topics whose understanding was essential to a grasp of the field. The result is a volume in excess of 700 pages, containing approximately 1300 entries prepared by over 250 specialists. Entries on major topics (e.g., schools of psychology, distinguished figures, fundamental concepts) are as long as 1500 words, whereas entries on minor topics are 150 words or fewer in length. Entries include cross references, and there is an index at the conclusion of the volume.

Of the 250 contributors, over 150 report affiliations with British or British Commonwealth (less Canadian) institutions, and over 80 with US or Canadian institutions. The editors feel that there are three main bases for contemporary psychology: (a) cognitive psychology, concerning the acquisition and processing of information; (b) psycholinguistics, concerning the study of the most specifically human phenomenon, language; and (c) neuroscience, concerning the study of the brain and nervous system as the physical basis of thought, feeling, and

action. Thus, included are topics from clinical, developmental, educational, historical, occupational, personality, philosophical, physiological, and social psychology, the emotions, ethology, psycholinguistics, and learning and memory. Emphasis is placed throughout on intersections with related disciplines such as sociology, neuroscience, and artificial intelligence. The basic tenets of numerous schools and movements are also presented, along with information on such fringe areas as folk psychology, pop psychology, and even plant psychology. The entries are constructed in such a way as to highlight areas of disagreement, so that readers may decide among alternatives when confronted with controversial topics. Aside from the aforementioned biases about the structure of the field, the editors think there are few biases about the entries themselves. Indeed, it is reassuring to read that one contributor notes, "the mind-body problem has by no means achieved a resolution" (p. 397).

There is little doubt that the volume maps well onto contemporary psychology viewed as a whole. There is something for everyone here, but of course it is hard to imagine a text for general use that does not come to grips with economic reality by catering to the broadest possible audience. Nevertheless, there is much that is commendable in the *Dictionary*. Many entries pertaining to conditioning and learning are written by Mackintosh—for example, those on "conditioning" and "learning: two factor/single factor theories" are exemplary. Entries on "critical theory," "historiography of psychology," and "history of psychology" are well written

I thank Joel Rynders for his assistance in the preparation of the review. Reprints may be obtained from the author at the Department of Psychology, University of Wisconsin-Milwaukee, Milwaukee, Wisconsin 53201.

and informative. A two-page entry on "intelligence and environmental influences" is remarkably comprehensive for such a controversial topic. For those interested in philosophical issues, there are entries on "privacy," "other minds," and "pain: philosophical concern with" that are well conceived and written generally from a Wittgensteinian point of view. For those interested in methodological issues, there are entries on "hypothetical constructs and intervening variables," "empirical methods in social inquiry," and "empirical techniques in experimental psychology." Indeed, the latter entry concludes:

If we understood better how these shifts of emphasis [between research guided by theoretical consideration and by methodological quirks of particular paradigms] have occurred, and could trace better how they have been resolved, and how dissatisfactions with the *status quo* has [sic] produced new theories and new paradigms, we might learn to advance our science better. (p. 202)

References to Skinner and his brand of behaviorism appear in quite a few surprising places. For example, he is cited in the entry on operationalism, on first language acquisition ("Skinner had attempted to formulate a theory of language acquisition in strictly behaviorist, stimulus-response terms," p. 237), and on meaning, which suggests that Skinner's position involves "reducing meaning to stimulus-response regularities" (p. 374). Although, as these passages indicate, what Skinner has actually said may not be accurately reported, it is somewhat reassuring to see that at least someone recognizes that he has said something on the issue in question.

Given such a vast undertaking, it is not surprising there are also a few features of the dictionary that stand out in other ways. It is not immediately apparent why there should be separate entries on "concept: animal," "concept: human," and "concept learning," let alone why the entries should be written

by three different authors. There are separate entries for "intentional action" and "intentionality," again each written by a different author. Similarly, there are separate entries for "learning: theories of" and for "learning theory," each composed by a different author. Is a reader to assume some discontinuity among the topics so treated that requires this sort of fractionating? The biographical entry on Skinner is half of one column on a two-column page, whereas those on Freud, Jung, and Piaget are at least two full pages each. Perhaps the authors and editors feel this represents their relative importance in the field, and in fact it may from their perspective. Indeed, the entry on "learning: theories of" is written by Mackintosh, who concludes the entry by noting:

It is equally important to recognize that some aspects of human behavior are probably not illuminated by analogies with animal conditioning experiments. Theories of learning based on such experiments are surely insufficient to explain the way in which young children learn to speak their native language or to solve differential equations. . . . A theory of learning based on conditioning experiments with rats or pigeons . . . does not take us very far in understanding the cognitive processes involved in this sort of behavior. (p. 339)

Apparently, the author's position is that there are in fact things called cognitive processes that do mediate this sort of behavior in humans, and it is an empirical question as to the extent to which comparable processes exist in lower animals. The entry on "learning and motivation: animal studies" is written largely from a Hullian point of view. There is a short entry for "operant conditioning," not written by Mackintosh, but no entry for "respondent conditioning." There is an entry for "reflex," which concludes, "The automatic depression of the brake pedal by a car driver in an emergency is an example of such a conditioned reflex" (p. 522). There is no entry on the method of Ivanov-Smolensky.

Perhaps most troublesome is the entry on reinforcement: "a pigeon punished with a mild electric shock is likely to learn not to peck the disk in future and the pecking response is said to be negatively reinforced" (p. 525), and "Whatever the theoretical interpretation of autoshaping experiments it is clear that Skinner was incorrect in asserting that reinforcement is arbitrary in the sense of establishing connections between any type of stimulus and any element of the animal's behavior repertoire" (p. 526). Note also that the biographical entry on Skinner asserts, "His belief that any reinforcer could be used to modify any aspect of behavior was subsequently shown to be incorrect" (p. 577). The latter two statements may be especially compared with what Skinner actually said: "No reputable student of animal behavior has ever taken the position 'that the animal comes to the laboratory as a virtual *tabula rasa*, that species differences are insignificant, and that all responses are about equally conditionable to all stimuli'" (Skinner, 1969, p. 173). In any case, it is somewhat disappointing to see that even such fundamental points as these do not seem to be reported accurately in a volume that is presumed to be taken as authoritative.

Dictionaries are of course complex things. It is traditional to characterize a dictionary as something that contains definitions, and then to characterize a definition as providing the meaning of a term via a summary of the conditions of use. There is perhaps no harm in this approach, so long as we recognize that words are not objects that attach to other objects called meanings, and then conclude that the way to establish meaning is to investigate how an individual puts the word to us, much as a pick or shovel might be put to use. Just as it might sound forced to say that a rat uses a lever press or a pigeon uses a key peck, so is it forced to say that a person uses a word. The ease with which we do say, in anything other than a colloquial sense, that we use words gives ample testimony to mentalism in our culture.

From a more behavioristic perspective, dictionaries are verbal products composed

by someone in contact with some kind of discriminative stimulation. The verbal product, in the form of visual stimulation, then exerts discriminative control over the behavior of someone who comes into contact with it subsequently. The verbal products are primarily tacts and autoclitics. The term *meaning* is appropriate to this matter in two ways: meaning for the speaker/composer, and meaning for the listener/reader who comes into contact with the verbal product. Of course, additional discriminative control may arise for the listener through attendant processes of abstraction, metaphorical extension, generalization, and so on, in accordance with the reinforcing practices of the verbal community, thereby providing for what are called alternative usages, deductive explanations, and so on. In short, meaning for the speaker derives from the conditions that control the emission of the term, and for the listener, from the discriminative value of the term as a stimulus. In any case, as Catania (1968) noted, the progressive refinement of terminology is often part of a science itself. Precision of usage allows accurate and unambiguous communication of experimental design, apparatus technology, and results, so that readers may benefit from the work of others. Accordingly, it is healthy to be concerned with definitions, meaning, and usages, because the terms we use accommodate effective action with respect to the world.

From a more traditional perspective, definitions have been considered important for another reason: their relation to explanation. Perhaps the modal kind of explanatory practice consists of formulating a series of statements involving the explanandum, or a statement of the event to be explained, and the explanans, a statement that is the explanation taking the form of an empirical covering law. The principal features of this sort of practice have been described as follows (Robinson, 1981):

1. The explanans must be a general or "covering" law, universally applicable to the class of events of which the explanandum is an instance.

2. The explanans must be true.
3. The explanandum must be logically deducible from the explanans.
4. The content of the explanandum must be empirical.
5. In summary, once the antecedent conditions have been stated, the explanandum (typically, an observed effect) becomes logically required by the covering law (explanans) of which it is an instance. (p. 15)

Definitions are critical in the framework of this approach to explanation because they pertain to the truth of the explanans and to the applicability of the problem at hand to the covering law—for example, by being an instance of it. Accordingly, an unequivocal definition of terms is necessary before all else. Such definitions are established operationally. Given that the covering law may be specified in operationally defined terms, and the observed effect may be similarly specified, the latter is to be deduced as an instance of the former. The covering law and the deduction from it, given adequate definition, are to be taken as explanation.

There is no doubt much to recommend this approach, but unfortunately it has all too often supported a mentalistic epistemology. It has been taken as obviously true that humans are the sorts of creatures who operate in two dimensions, one physical and the other mental. Mental phenomena are said to be somehow representative of the physical phenomena. In particular, humans are taken to reproduce or construct their own private worlds of “immediate experience” in the mental dimension. By means of logical operations they then carve up the products of immediate experience and assign symbols to them. Definitions and meanings become a matter of establishing what the terms stand for, symbolize, or refer to. Traditional views of science as involving constructs, hypotheses, postulates, and so on are nearly all predicated on this sort of approach. Meaning therefore becomes largely a matter of symbolism, reference, or correspondence between mental and physical

or between the objective and the subjective. It was this approach to which Skinner was objecting when he said:

The weakness of current theories of language may be traced to the fact that an objective conception of human behavior is still incomplete. The doctrine that words are used to express or convey meanings merely substitutes ‘meaning’ for ‘idea’ (in the hope that meanings can then somehow be got outside the skin) and is incompatible with modern psychological conceptions of the organism. Attempts to derive a symbolic function from the principle of conditioning (or association) have been characterized by a very superficial analysis. It is simply not true that an organism reacts to a sign ‘as it would to the object which the sign supplants.’ Only in a very limited area (mainly in the case of autonomic responses) is it possible to regard the sign as a simple substitute stimulus in the Pavlovian sense. Modern logic, as a formalization of ‘real’ languages, retains and extends this dualistic theory of meaning and can scarcely be appealed to by the psychologist who recognizes his own responsibility in giving an account of verbal behavior. (Skinner, 1945, pp. 270-271)

The view of human activity Skinner criticizes in the passage above has been enthusiastically embraced and indeed promoted by the academically influential, such that it has now become the mainstream epistemological position. Indeed, it is even an ontological position.

The implications of the mentalistic position have been engaged repeatedly, not only by Skinner but also by Wittgenstein. In brief, what is critical in the position is the matter of privacy. The traditional position entails the notion that a human is somehow a privileged spectator with privileged access to a special dimension called immediate experience, which may then be described in terms of private language. Both Skinner and

Wittgenstein recognized the need to accommodate language that is essentially descriptive of private phenomena, but the matter as they formulated it is radically different from the traditional position. For them, operationism is a radically different matter, not of establishing what the term symbolizes, corresponds, or refers to in immediate experience (which presupposes that there is a dimension of immediate experience that contains things that can be symbolized), but rather a matter of analyzing the independent variables that operate during a verbal episode. These variables may lead to verbal behavior that might be described as descriptive or as simply a part of ordinary discourse, among a variety of other possibilities, none of which is meaningfully designated as symbolic.

The point is that definitions, meanings, and usages are matters of psychology, and not of another dimensional system in which some transcendental entity called logic dictates what is permissible and what is not (Skinner, 1945, p. 277). Indeed, an understanding of logic involves an understanding of a variety of behavioral processes (Skinner, 1974, chap. 8). Day has commented on the implications of the behavioristic position:

In responding to professional language, the radical behaviorist has his own new course to follow: he must attempt to discover the variables controlling what has been said. Even the most mentalistic language is understandable and valuable in this sense. The meaningfulness of psychological and mental terms provides no insuperable problem, provided the verbal practices of both speaker and hearer have been shaped by overlapping verbal communities. The meaning of such terms can be clarified by an attempt to assess the observable (not necessarily publicly observable) events that act as discriminative stimuli in control of emission of the term. This kind of analysis is what Skinner has in mind when he speaks of

"operational definition." (Day, 1969a, p. 320)

It is distinctly Skinnerian to urge the empirical study of whatever intellectual activities happen to characterize successful scientific behavior. It is true that many scientists, and notably psychologists, *think* they know what science is or *think* they know what is involved in legitimate scientific explanation . . . However, their *thoughts* in this respect have still to be accounted for just as much as Skinner's even if the value in trusting such conventional views were not to be questioned. It is hardly necessary to elaborate upon how sharply conventional attitudes toward scientific explanations have been questioned, even recently and by the most competent of scientists . . . To fail to view the problem of explanation in this fashion, as *inescapably* an empirical and behavioral problem, is perhaps to miss the force of what well may be Skinner's major contribution to psychological thought. (Day, 1969b, p. 505)

Accordingly, it is perhaps incorrect to indicate that a definition specifies how a term *should* be used, in the sense that the usage of the term is a matter of logic. A definition is something that no one, or no logical system, can legislate in this sense. To talk of how terms should be used is to do nothing more than to talk of whether a particular instance of the emission of the term is consistent with the conventional reinforcing practices of the verbal community, or is under some other kind of stimulus control. Assessing the adequacy of definitions in a dictionary is at least partly a matter of assessing how much the stimulus control over the composition of the definition resembles the stimulus control that exists in the verbal community with respect to the usage of the term defined.

The *Dictionary's* definition of the term *reinforcement*, cited above, is perhaps a case in point. In its most abstract reading, the term *reinforcement*, as defined, implies a strengthening of a response via the presenta-

tion of a stimulus of primary biological importance. Such a presentation, however, is presumably contingent upon a prior stimulus, in which case we are concerned with classical conditioning and the strengthening of respondents, or upon a response, in which case we are concerned with operant conditioning and the strengthening of operants. The *Dictionary's* definition of reinforcement does not seem to respect a distinction between these two sorts of contingencies, and instead seems to hinge simply upon the operation of presenting a stimulus that affects the response in question. Because shock has an intuitively negative connotation, the operation of presenting the shock comes therefore to be mistakenly identified as "negative reinforcement." Anyone familiar with teaching beginning psychology has no doubt encountered similar problems when trying to establish systematic usages of technical terms.

In contrast, how does a behaviorist use the term *reinforcement*? I confess a certain fondness for Catania's (1984) treatment, so I will use it as the basis for what follows. According to Catania, the term *reinforcement* is appropriate when (a) a response produces some consequence, (b) the response occurs more frequently when it does than when it does not produce the consequence, and (c) the increased responding occurs because the response has that consequence, and not simply as a by-product of the appearance of the consequence within the experimental setting (Catania, 1984, p. 63). Now, evolving linguistic practices concerned with reinforcement may eventually limit the usefulness of a distinction between positive and negative reinforcement (cf. Michael, 1975), but for the present there seem to be distinct usages. The essential features of current usage involve (a) an experimental operation, (b) an effect, and (c) a statement that a functional relation exists between the operation and the effect. The term *positive reinforcement* relates to feature (a), insofar as it is concerned with the nature of the consequence of the response. More specifically, the consequence of the response involves an increase in the level of stimulus energy or energy potential, mea-

sured in the same way the physicist measures energy. The term *negative reinforcement* would seem also to relate to feature (a), in that the change in energy that is the consequence of the response is in the reverse direction, that is, a decrease. The term *punishment* pertains to feature (b), the nature of the behavioral effect. In punishment, the change in behavior is presumably in the opposite direction from that noted in connection with reinforcement. Such distinctions are important in that to claim some response is functionally influenced by positive reinforcement, negative reinforcement, or punishment is to make a prescriptive claim about what persons can do if they want to modify the response in question.

The larger problem remains: How can it be, after so many years and given the existence of so much that has been written, that anyone in a position to compose an entry for a scholarly text still does not correctly distinguish between negative reinforcement and punishment? It would be understandable if the problem were simply one of the intellectual idiosyncrasies of a single author, but the problem seems more general. Mostly to indulge my own curiosity, I randomly sampled terms from three glossaries and then looked in the index of the present volume to see if they were cited in a meaningful way. The three glossaries were from Kimble's (1961) influential text on conditioning and learning, from Catania's (1968) edited book of readings, and from Hilgard, Atkinson, and Atkinson's (1979) introductory text. 64% of the terms from Kimble, 73% of the terms from Hilgard, and 30% of the terms from Catania were represented in the current volume.

Moreover, certain misconceptions of radical behaviorism have been prevalent in psychology textbooks, as documented by Todd and Morris (1983). In brief, these misconceptions involve the following: (a) that it is concerned primarily with the behavior of nonhuman animals, (b) that it is totally environmentalistic, (c) that it espouses an "empty organism" or "black box" approach, (d) that it espouses a simple and

naive theory of language and language acquisition, and (d) that it is limited in its usefulness (Todd & Morris, p. 155). I further note my copy of the Hilgard introductory text states that:

Stimulus-response psychology (or S-R psychology for short) studies the stimuli that elicit behavioral responses, the rewards and punishments that maintain these responses, and the modifications in behavior obtained by changing the patterns of rewards and punishments. Stimulus-response psychology is not concerned with what goes on inside the organism; for this reason it has been called the "black-box" approach.

. . . A strict S-R approach does not consider the individual's conscious experiences. Conscious experiences are simply those events the experiencing person is fully aware of. (pp. 5-6)

Although there may be a way of formulating these issues to bring them into good order, the fact remains that the passage is deceptive regarding the actual behavioristic position, and this passage is adjacent to a picture of Skinner holding a pigeon. How can it be that such a state of affairs exists? Surely, one is forced to conclude that the behaviorist has not had much impact on the field, if so few of the behaviorist's terms are deemed worthy of attention, and those few that are considered worthy are not even given informed analysis. How can it be that such a state of affairs exists?

There are no doubt many factors responsible for the problem. Krantz (1971) in a provocative article suggested that the problem is not entirely one-sided. He noted characteristics of arrogance, intolerance, conceptual imperialism, rudeness, philosophical naivete, and interpersonal insensitivity on the part of behaviorists that precipitated difficulties and that no doubt contributed to a divisiveness between behavioristic and nonbehavioristic orientations to psychology. Such divisiveness is often evidenced by a breakdown of communica-

tion, a lack of utilization of findings, and a failure to cite. Surely, the *Dictionary* is additional evidence that the divisiveness is still there, whatever the reasons.

There may be other factors. In place of the plaintive cry, "Where have all the behaviorists gone?" perhaps the more accurate questions are, what made the behaviorist think there were so many other behaviorists in the first place, that behaviorists were influential, and that the field was developing as behaviorists thought? The late C. B. Ferster's (1978) review of Honig and Staddon (1977) is a case in point. After having been absent from the experimental scene for some six years, Ferster (1978) comments:

The *Handbook* revives the conflict, that was the dilemma of the "fifties," between descriptive empirical discovery and verbally interesting theoretical comment. . . . I speculated that the extensive theoretical arguments, reminiscent of Hull's and Spence's style of hypothesis testing and deduction, rather than the traditions of JEAB, was in many cases the impetus for the persuasive and communicable style. . . . For one experimenter, the validation of the experiment is the change in the behavior of the individual subject, guided by a principle or instruction. For the other, the observations are merely a first step to secure the personal and social validation that occurs when the theory or conceptual scheme is argued. . . . Is the reinforcer the experience of altering behavior, using the prescription provided by the investigator, or is it the argument and discussion that the results generate? The chapters of the *Handbook* span the full range of this continuum, but this reviewer found a preponderance of the latter, and I must confess a nostalgia for the former. (pp. 347-348)

Accordingly, it seems that not only is there inattention to critical behavioristic practices outside of the field, but among those ostensibly within the field as well.

One interpretation of how all this came about is that during the 1960s there was a rapid expansion of the academic community. Those who came to the field of behavior analysis during this era brought with them a set of alternative practices that simply overwhelmed those of the behavior-analytic movement, which at that time was quite modest. The shift in usage was then the inevitable outcome. Day (1969a) was one of the first to comment on this state of affairs:

The lack of careful study of Skinner's work has led to professional absurdities too numerous to review in detail. Strange blends of Skinner and conventional behaviorism abound. . . . Mentalism among Skinnerians is rampant, and they are quickly trapped up by the operationism of Boring and Stevens. Unfortunately, only very few people have an accurate idea of what Skinner means by operational definition. I have taken the liberty of speaking directly to some of those who preach most loudly a supposedly Skinnerian line. One hardly knows where to begin to analyze the grossly uninformed verbal material that is generated concerning Skinner's work by the typical psychologist. (pp. 326-327)

If the current analysis has any merit, and judging from the appearance of the *Dictionary*, 15 years has not changed the situation much.

Is there a resolution of this problem? I tend to favor Day's recurrent theme that psychologists be a little more concerned with the variables that control their professional, scientific verbal behavior (e.g., Day, 1969a, pp. 319, 328). This concern does not mean that the essence of psychology may be engaged via extensive chains of intraverbals derived from *Science and Human Behavior* (Skinner, 1953), but it does mean knowing the extent to which one's verbal behavior is influenced by Skinner relative to Hull, Spence, and Stevens. It does not mean that *Science and Human Behavior* may be taken as right about all things because Skinner wrote

it — that is the distasteful argument by authority and the problem identified by Krantz (1971). On the other hand, it does mean knowing what *Science and Human Behavior* really does say about the topic in question, as well as what others have written, so that we might better recognize ourselves as a locus, where many sources come together in unique confluence resulting in an equally unique achievement.

In summary, definitions are of course desirable to attain. Refined usages reflect refined stimulus control and more effective action with respect to nature. After all, one of the expressed aims of the scientific method is presumably to sharpen stimulus control by environmental events over verbal behavior with respect to them (Skinner, 1957, chap. 18). I do not think the present volume will stand as an exhaustive, authoritative source on how behaviorists use terms; at least I hope not. The editors undertook a big job, and unfortunately not all turned out well. Should behaviorists buy the book? The book might be useful as a source on the heterogeneity of contemporary psychology, but not on systematic practices in the analysis of behavior.

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