

## The Operational Analysis of Psychological Terms

"Is operationism more than a renewed and refined emphasis upon the experimental method (as understood already by Galileo, if not even by Archimedes)—that is, a formulation of modern scientific empiricism and pragmatism (especially of the Peirce-Dewey variety), mainly of criteria of factual meaningfulness and empirical validity?" An answer to this question will define the position to be taken in what follows. Operationism is not regarded as a new theory or mode of definition. The literature has emphasized certain critical or hitherto neglected instances, but no new kind of operation has been discovered and none should be singled out. There is no reason to restrict operational analysis to high-order constructs; the principle applies to all definitions. This means that we must explicate an operational definition for every term unless we are willing to adopt the vague usage of the vernacular.

Operationism may be defined as the practice of talking about (1) one's observations, (2) the manipulative and calculational procedures involved in making them, (3) the logical and mathematical steps which intervene between earlier and later statements, and (4) *nothing else*. So far, the major contribution has come from the fourth provision and, like it, is negative. We have learned how to avoid troublesome references by showing that they are artifacts which may be variously traced to history, philosophy, linguistics, and so on. No very important positive advances have been made in connection with the first three provisions because operation-

From *Psychological Review*, 1945, 52, 270-277. *The symposium on operationism conducted in the September, 1945, issue of the Psychological Review was suggested by Edwin G. Boring, who proposed a set of eleven questions to which participants might address themselves. Several references to these questions by number have been omitted in the present version and others have been replaced by brief paraphrases.*

ism has no good definition of a definition, operational or otherwise. It has not developed a satisfactory formulation of the effective verbal behavior of the scientist.

The operationist, like most contemporary writers in the field of linguistic and semantic analysis, is on the fence between logical "correspondence" theories of reference and empirical formulations of language in use. He has not improved upon the mixture of logical and popular terms usually encountered in casual or even supposedly technical discussions of scientific method or the theory of knowledge (e.g., Bertrand Russell's recent *An inquiry into meaning and truth*). *Definition* is a key term but is not rigorously defined. Bridgman's original contention that the "concept is synonymous with the corresponding set of operations" cannot be taken literally, and no similarly explicit but satisfactory statement of the relation is available. Instead, a few roundabout expressions recur with rather tiresome regularity whenever this relation is mentioned. We are told that a concept is to be defined "*in terms of*" certain operations, that propositions are to be "*based upon*" operations, that a term denotes something only when there are "*concrete criteria for its applicability*," that operationism consists in "*referring any concept for its definition to . . . concrete operations . . .*" and so on. We may accept expressions of this sort as outlining a program, but they do not provide a general scheme of definition, much less an explicit statement of the relation between concept and operation.

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."<sup>1</sup> 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.

<sup>1</sup> Stevens, S. S. Psychology and the science of science. *Psychol. Bull.*, 1939, 36, 221-263.

It is not my intention to attempt a more adequate formulation here. The fundamental revision is too sweeping to be made hastily. I should like, however, to try to make a small but positive contribution to this symposium by considering a few points which arise in connection with the operational definition of psychological terms. Much of the material which follows is adapted from a much longer work now in preparation, in which the necessary groundwork is more carefully prepared.

The operational attitude, in spite of its shortcomings, is a good thing in any science but especially in psychology because of the presence there of a vast vocabulary of ancient and non-scientific origin. It is not surprising that the broad empirical movement in the philosophy of science, which Stevens has shown to be the background of operationism, should have had a vigorous and early representation in the field of psychology—namely, behaviorism. In spite of the differences which Stevens claims to find, behaviorism has been (at least to most behaviorists) nothing more than a thoroughgoing operational analysis of traditional mentalistic concepts. We may disagree with some of the answers (such as Watson's disposition of images), but the *questions* asked by behaviorism were strictly operational in spirit. I also cannot agree with Stevens that American behaviorism was "primitive." The early papers on the problem of consciousness by Watson, Weiss, Tolman, Hunter, Lashley, and many others, were not only highly sophisticated examples of operational inquiry, they showed a willingness to deal with a wider range of phenomena than do current streamlined treatments, particularly those offered by logicians (e.g., Carnap) interested in a unified scientific vocabulary. But behaviorism, too, stopped short of a decisive positive contribution—and for the same reason: it never finished an acceptable formulation of the "verbal report." The conception of behavior which it developed could not convincingly embrace the "use of subjective terms."

A considerable advantage is gained from dealing with terms, concepts, constructs, and so on, quite frankly in the form in which they are observed—namely, as verbal responses. There is then no danger of including in the concept that aspect or part of nature which it singles out. (Several of the present questions seem to mix concept and referent; at least they seem to become trivial when, in order to make the mixture less likely, *term* is substituted for *concept* or *construct*.) 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). If

two quite separate sets of circumstances are revealed, then there are two responses having the form "length," since a verbal response-class is not defined by phonetic form alone but by its functional relations. This is true even though the two sets are found to be intimately connected. The two responses are not controlled by the same stimuli, no matter how clearly it is shown that the different stimuli arise from the same "thing."

What we want to know in the case of many traditional psychological terms is, first, the specific stimulating conditions under which they are emitted (this corresponds to "finding the referents") and, second (and this is a much more important systematic question), why each response is controlled by its corresponding condition. The latter is not necessarily a genetic question. The individual acquires language from society, but the reinforcing action of the verbal community continues to play an important role in maintaining the specific relations between responses and stimuli which are essential to the proper functioning of verbal behavior. How language is acquired is, therefore, only part of a much broader problem.

We may generalize the conditions responsible for the standard "semantic" relation between a verbal response and a particular stimulus without going into reinforcement theory in detail. There are three important terms: a stimulus, a response, and a reinforcement supplied by the verbal community. (All of these need more careful definitions than are implied by current usage, but the following argument may be made without digressing for that purpose.) The significant interrelations between these terms may be expressed by saying that the community reinforces the response only when it is emitted in the presence of the stimulus. The reinforcement of the response "red," for example, is contingent upon the presence of a red object. (The contingency need not be invariable.) A red object then becomes a discriminative stimulus, an "occasion," for the successful emission of the response "red."

This scheme presupposes that the stimulus act upon both the speaker and the reinforcing community; otherwise the proper contingency cannot be maintained by the community. But this provision is lacking in the case of many "subjective" terms, which appear to be responses to *private* stimuli. The problem of subjective terms does not coincide exactly with that of private stimuli, but there is a close connection. We must know the characteristics of verbal responses to private stimuli in order to approach the operational analysis of the subjective term.

The response "My tooth aches" is partly under the control of a state of affairs to which the speaker alone is able to react, since no one else can establish the required connection with the tooth in question. There is noth-

ing mysterious or metaphysical about this; the simple fact is that each speaker possesses a small but important private world of stimuli. So far as we know, his reactions to these are quite like his reactions to external events. Nevertheless the privacy gives rise to two problems. The first difficulty is that we cannot, as in the case of public stimuli, account for the verbal response by pointing to a controlling stimulus. Our practice is to *infer* the private event, but this is opposed to the direction of inquiry in a science of behavior in which we are to predict response through, among other things, an independent knowledge of the stimulus. It is often supposed that a solution is to be found in improved physiological techniques. Whenever it becomes possible to say what conditions within the organism control the response "I am depressed," for example, and to produce these conditions at will, a degree of control and prediction characteristic of responses to external stimuli will be made possible. Meanwhile, we must be content with reasonable evidence for the belief that responses to public and private stimuli are equally lawful and alike in kind.

But the problem of privacy cannot be wholly solved by instrumental invasion. No matter how clearly these internal events may be exposed in the laboratory, the fact remains that in the normal verbal episode they are quite private. We have not solved the second problem of how the community achieves the necessary contingency of reinforcement. How is the response "toothache" appropriately reinforced if the reinforcing agent has no contact with the tooth? There is, of course, no question of whether responses to private stimuli are possible. They occur commonly enough and must be accounted for. But why do they occur, what is their relation to controlling stimuli, and what, if any, are their distinguishing characteristics?

There are at least four ways in which a verbal community which has no access to a private stimulus may generate verbal behavior in response to it:

(1) It is not strictly true that the stimuli which control the response must be available to the community. Any reasonably regular accompaniment will suffice. Consider, for example, a blind man who learns the names of a trayful of objects from a teacher who identifies the objects by sight. The reinforcements are supplied or withheld according to the contingency between the blind man's responses and the teacher's visual stimuli, but the responses are controlled wholly by tactual stimuli. A satisfactory verbal system results from the fact that the visual and tactual stimuli remain closely connected.

Similarly, in the case of private stimuli, one may teach a child to say "That hurts" in agreement with the usage of the community by making the reinforcement contingent upon public accompaniments of painful stimuli (a smart blow, tissue damage, and so on). The connection between public

and private stimuli need not be invariable; a response may be conditioned with merely periodic reinforcement and even in spite of an occasional conflicting contingency. The possibility of such behavior is limited by the degree of association of public and private stimuli which will supply a net reinforcement sufficient to establish and maintain a response.

(2) A commoner basis for the verbal reinforcement of a response to a private stimulus is provided by collateral responses to the same stimulus. Although a dentist may occasionally be able to identify the stimulus for a toothache from certain public accompaniments as in (1), the response "toothache" is generally transmitted on the basis of responses which are elicited by the same stimulus but which do not need to be set up by an environmental contingency. The community infers the private stimulus, not from accompanying public stimuli, but from collateral, generally unconditioned and at least non-verbal, responses (hand to jaw, facial expressions, groans, and so on). The inference is not always correct, and the accuracy of the reference is again limited by the degree of association.

(3) Some very important responses to private stimuli are descriptive of the speaker's own behavior. When this is overt, the community bases its instructional reinforcement upon the conspicuous manifestations, but the speaker presumably acquires the response in connection with a wealth of additional proprioceptive stimuli. The latter may assume practically complete control, as in describing one's own behavior in the dark. This is very close to the example of the blind man; the speaker and the community react to different, though closely associated, stimuli.

Suppose, now, that a given response recedes to the level of covert or merely incipient behavior. How shall we explain the vocabulary which deals with this private world? (The instrumental detection of covert behavior is again not an answer, for we are interested in how responses to private stimuli are normally, and non-instrumentally, set up.) There are two important possibilities. The surviving covert response may be regarded as an accompaniment of the overt (perhaps part of it), in which case the response to the private stimulus is imparted on the basis of the public stimulus supplied by the overt response, as in (1). On the other hand, the covert response may be *similar to*, though probably less intense than, the overt and hence supply the *same* stimulus, albeit in a weakened form. We have, then, a third possibility: a response may be emitted in the presence of a private stimulus, which has no public accompaniments, provided it is occasionally reinforced in the presence of the same stimulus occurring with public manifestations.

Terms falling within this class are apparently descriptive only of be-

havior, rather than of other internal states or events, since the possibility that the same stimulus may be both public and private (or, better, may have or lack public accompaniments) seems to arise from the unique fact that behavior may be both covert and overt.

(4) The principle of transfer or stimulus induction supplies a fourth explanation of how a response to private stimuli may be maintained by public reinforcement. A response which is acquired and maintained in connection with public stimuli may be emitted, through induction, in response to private events. The transfer is not due to identical stimuli, as in (3), but to coinciding properties. Thus, we describe internal states as "agitated," "depressed," "ebullient," and so on, in a long list. Responses in this class are all metaphors (including special figures like metonymy). The term *metaphor* is not used pejoratively but merely to indicate that the differential reinforcement cannot be accorded actual responses to the private case. As the etymology suggests, the response is "carried over" from the public instance.

In summary, a verbal response to a private stimulus may be maintained in strength through appropriate reinforcement based upon public accompaniments or consequences, as in (1) and (2), or through appropriate reinforcement accorded by induction when it is made to public stimuli, the private case occurring by induction when the stimuli are only partly similar. If these are the only possibilities (and the list is here offered as exhaustive), then we may understand why terms referring to private events have never formed a stable and acceptable vocabulary of reasonably uniform usage. This historical fact is puzzling to adherents of the "correspondence school" of meaning. Why is it not possible to assign names to the diverse elements of private experience and then to proceed with consistent and effective discourse? The answer lies in the process by which "terms are assigned to private events," a process which we have just analyzed in a rough way in terms of the reinforcement of verbal responses.

None of the conditions which we have examined permits the sharpening of reference which is achieved, in the case of public stimuli, by a precise contingency of reinforcement. In (1) and (2) the association of public and private events may be faulty; the stimuli embraced by (3) are of limited scope; and the metaphorical nature of those in (4) implies a lack of precision. It is, therefore, impossible to establish a rigorous scientific vocabulary for public use, nor can the speaker clearly "know himself" in the sense in which knowing is identified with behaving discriminatively. In the absence of the "crisis" provided by differential reinforcement (much of which is necessarily verbal), private stimuli cannot be analyzed. (This has little or nothing to do with the availability or capacity of receptors.)

The contingencies we have reviewed also fail to provide an adequate check against fictional distortion of the relation of reference (e.g., as in rationalizing). Statements about private events may be under control of the drives associated with their consequences rather than antecedent stimuli. The community is skeptical of statements of this sort, and any attempt by the speaker to talk to himself about his private world (as in psychological system making) is fraught with self-deception.

Much of the ambiguity of psychological terms arises from the possibility of alternative or multiple modes of reinforcement. Consider, for example, the response "I am hungry." The community may reinforce this on the basis of the history of ingestion, as in (1), or collateral behavior associated with hunger, as in (2), or as a description of behavior with respect to food, or stimuli previously correlated with food, as in (3). In addition the speaker has (in some instances) the powerful stimulation of hunger pangs, which is private since the community has no suitable connection with the speaker's stomach. "I am hungry" may therefore be variously translated as "I have not eaten for a long time" (1), or "That food makes my mouth water" (2), or "I am ravenous" (3) (compare the expression "I was hungrier than I thought" which describes the ingestion of an unexpectedly large amount of food), or "I have hunger pangs." While all of these may be regarded as synonymous with "I am hungry," they are not synonymous with each other. It is easy for conflicting psychological systematists to cite supporting instances or to train speakers to emit the response "I am hungry" in conformity with a system. With the balloon technique one might condition the verbal response exclusively to stimulation from stomach contractions. This would be an example of either (1) or (2) above. Or a speaker might be trained to make nice observations of the strength of his ingestive behavior, which might recede to the covert level as in (3). The response "I am hungry" would then describe a tendency to eat, with little or no reference to stomach contractions. Everyday usage reflects a mixed reinforcement. A similar analysis could be made of all terms descriptive of motivation, emotion, and action in general, including (of special interest here) the acts of seeing, hearing, and so on.

When public manifestations survive, the extent to which the private stimulus takes over is never certain. In the case of a toothache, the private event is no doubt dominant, but this is due to its relative intensity, not to any condition of differential reinforcement. In a description of one's own behavior, the private component may be much less important. A very strict external contingency may emphasize the public component, especially if the association with private events is faulty. In a rigorous scientific vocabulary private effects are practically eliminated. The converse does not hold. There

is apparently no way of basing a response entirely upon the private part of a complex of stimuli. *A differential reinforcement cannot be made contingent upon the property of privacy.* This fact is of extraordinary importance in evaluating traditional psychological terms.

The response "red" is imparted and maintained (either casually or professionally) by reinforcements which are contingent upon a certain property of stimuli. Both speaker and community (or psychologist) have access to the stimulus, and the contingency may be made quite precise. There is nothing about the resulting response which should puzzle anyone. The greater part of psychophysics rests upon this solid footing. The older psychological view, however, was that the speaker was reporting, not a property of the stimulus, but a certain kind of private event, the sensation of red. This was regarded as a later stage in a series beginning with the red stimulus. The experimenter was supposed to manipulate the private event by manipulating the stimulus. This seems like a gratuitous distinction, but in the case of some subjects a similar later stage could apparently be generated in other ways (by arousing an "image"), and hence the autonomy of a private event capable of evoking the response "red" in the absence of a controllable red stimulus seemed to be proved. An adequate proof, of course, requires the elimination of other possibilities (e.g., that the response is generated by the procedures which are intended to generate the image).

Verbal behavior which is "descriptive of images" must be accounted for in any adequate science of behavior. The difficulties are the same for both behaviorist and subjectivist. If the private events are free, a scientific description is impossible in either case. If laws can be discovered, then a lawful description of the verbal behavior can be achieved, with or without references to images. So much for "finding the referents"; the remaining problem of how such responses are maintained in relation to their referents is also soluble. The description of an image appears to be an example of a response to a private stimulus of class (1) above. That is to say, relevant terms are established when the private event accompanies a controllable external stimulus, but responses occur at other times, perhaps in relation to the same private event. The deficiencies of such a vocabulary have been pointed out.

We can account for the response "red" (at least as well as for the "experience" of red) by appeal to past conditions of reinforcement. But what about expanded expressions like "I see red" or "I am conscious of red"? Here "red" may be a response to either a public or a private stimulus without prejudice to the rest of the expression, but "see" and "conscious" seem to refer to events which are by nature or by definition private. This violates

the principle that a reinforcement cannot be made contingent upon the privacy of a stimulus. A reference cannot be narrowed down to a specifically private event by any known method of differential reinforcement.

The original behavioristic hypothesis was, of course, that terms of this sort were descriptions of one's own (generally covert) behavior. The hypothesis explains the establishment and maintenance of the terms by supplying natural public counterparts in similar overt behavior. The terms are in general of class (3). One consequence of the hypothesis is that each term may be given a behavioral definition. We must, however, modify the argument slightly. To say "I see red" is to react, not to red (this is a trivial meaning of "see"), but to one's reaction to red. "See" is a term acquired with respect to one's own behavior in the case of overt responses available to the community. But according to the present analysis it may be evoked at other times by any private accompaniment of overt seeing. Here is a point at which a non-behavioral private seeing may be slipped in. Although the commonest private accompaniment would appear to be the stimulation which survives in a similar covert act, as in (3), it might be some sort of state or condition which gains control of the response as in (1) or (2).

The superiority of the behavioral hypothesis is not merely methodological. That aspect of seeing which can be defined behaviorally is basic to the term as established by the verbal community and hence most effective in public discourse. A comparison of cases (1) and (3) will also show that terms which recede to the private level as overt behavior becomes covert have an optimal accuracy of reference, as responses to private stimuli go.

The additional hypothesis follows quite naturally that being conscious, as a form of reacting to one's own behavior, is a social product. Verbal behavior may be distinguished, and conveniently defined, by the fact that the contingencies of reinforcement are provided by other organisms rather than by a mechanical action upon the environment. The hypothesis is equivalent to saying that it is only because the behavior of the individual is important to society that society in turn makes it important to the individual. The individual becomes aware of what he is doing only after society has reinforced verbal responses with respect to his behavior as the source of discriminative stimuli. The behavior to be described (the behavior of which one is to be aware) may later recede to the covert level, and (to add a crowning difficulty) so may the verbal response. It is an ironic twist, considering the history of the behavioristic revolution, that as we develop a more effective vocabulary for the analysis of behavior we also enlarge the possibilities of awareness, so defined. The psychology of the other one is, after all, a direct approach to "knowing thyself."

The main purpose of this discussion has been to define a definition by considering an example. To be consistent the psychologist must deal with his own verbal practices by developing an empirical science of verbal behavior. He cannot, unfortunately, join the logician in defining a definition, for example, as a "rule for the use of a term" (Feigl); he must turn instead to the contingencies of reinforcement which account for the functional relation between a term, as a verbal response, and a given stimulus. This is the "operational basis" for his use of terms; and it is not logic but science.

The philosopher will call this circular. He will argue that we must adopt the rules of logic in order to make and interpret the experiments required in an empirical science of verbal behavior. But talking about talking is no more circular than thinking about thinking or knowing about knowing. Whether or not we are lifting ourselves by our own bootstraps, the simple fact is that we can make progress in a scientific analysis of verbal behavior. Eventually we shall be able to include, and perhaps to understand, our own verbal behavior as scientists. If it turns out that our final view of verbal behavior invalidates our scientific structure from the point of view of logic and truth-value, then so much the worse for logic, which will also have been embraced by our analysis.

*The participants in the symposium were asked to comment upon all the papers submitted. Their comments were included in the same issue of the Psychological Review. My contribution follows.*

In the summer of 1930, two years after the publication of Bridgman's *Logic of Modern Physics*, I wrote a paper called "The concept of the reflex in the description of behavior" [see page 429]. It was later offered as the first half of a doctoral thesis and was published in 1931. Although the general method, particularly the historical approach, was derived from Mach's *Science of Mechanics*, my debt to Bridgman was acknowledged in the second paragraph. This was, I think, the first psychological publication to contain a reference to the *Logic of Modern Physics*,<sup>2</sup> and it was the first explicitly operational analysis of a psychological concept.

Shortly after the paper was finished, I found myself contemplating a doctoral examination before a committee of whose sympathies I was none too sure. Not wishing to wait until an unconditional surrender might be

<sup>2</sup> Lyle H. Lanier has called my attention to the fact that Harry M. Johnson summarized Bridgman's argument and applied the operational criterion to the concept of intensity of sensation almost a year before my article appeared (*Psychol. Rev.*, 1930, 37, 113-123).

necessary, I put out a peace feeler. Unmindful or ignorant of the ethics of the academy, I suggested to a member of the Harvard department that if I could be excused from anything but the most perfunctory examination, the time which I would otherwise spend in preparation would be devoted to an operational analysis of half-a-dozen key terms from subjective psychology. The suggestion was received with such breathless amazement that my peace feeler went no further.

The point I want to make is that at that time—1930—I could regard an operational analysis of subjective terms as a *mere exercise in scientific method*. It was just a bit of hack work, badly needed by traditional psychology, which I was willing to engage in as a public service or in return for the remission of sins. It never occurred to me that the analysis could take any but a single course or have any relation to my own prejudices. The result seemed as predetermined as that of a mathematical calculation.

In spite of the present symposium, I am of this opinion still. I believe that the data of a science of psychology can be defined or denoted unequivocally, and that some one set of concepts can be shown to be the most expedient according to the usual standards in scientific practice. Nevertheless, these things have not been done in the field which was dominated by subjective psychology, and the question is: Why not?

Psychology, alone among the biological and social sciences, passed through a revolution comparable in many respects with that which was taking place at the same time in physics. This was, of course, behaviorism. The first step, like that in physics, was a re-examination of the observational bases of certain important concepts. But by the time Bridgman's book was published, most of the early behaviorists, as well as those of us just coming along who claimed some systematic continuity, had begun to see that psychology actually did not require the redefinition of subjective concepts. The reinterpretation of an established set of explanatory fictions was not the way to secure the tools then needed for a scientific description of behavior. Historical prestige was beside the point. There was no more reason to make a permanent place for "consciousness," "will," "feeling," and so on, than for "phlogiston" or "*vis animæ*." On the contrary, redefined concepts proved to be awkward and inappropriate, and Watsonianism was, in fact, practically wrecked in the attempt to make them work.

Thus it came about that while the behaviorists might have applied Bridgman's principle to representative terms from a mentalistic psychology (and were most competent to do so), they had lost all interest in the matter. They might as well have spent their time in showing what an eighteenth-century chemist was talking about when he said that the Metal-

lic Substances consisted of a vitrifiable earth united with phlogiston. There was no doubt that such a statement could be analyzed operationally or translated into modern terms, or that subjective terms could be operationally defined. But such matters were of historical interest only. What was wanted was a fresh set of concepts derived from a direct analysis of the newly emphasized data, and this was enough to absorb all the available energies of the behaviorists. Besides, the motivation of the *enfant terrible* had worn itself out.

I think the Harvard department would have been happier if my offer had been taken up. What happened instead was the operationalism of Boring and Stevens. This has been described as an attempt to climb onto the behavioristic bandwagon unobserved. I cannot agree. It is an attempt to acknowledge some of the more powerful claims of behaviorism (which could no longer be denied) but at the same time to preserve the old explanatory fictions. It is agreed that the data of psychology must be behavioral rather than mental if psychology is to be a member of the United Sciences, but the position taken is merely that of "methodological" behaviorism. According to this doctrine the world is divided into public and private events; and psychology, in order to meet the requirements of a science, must confine itself to the former. This was never good behaviorism, but it was an easy position to expound and defend and was often resorted to by the behaviorists themselves. It is least objectionable to the subjectivist because it permits him to retain "experience" for purposes of "non-physicalistic" self-knowledge.

The position is not genuinely operational because it shows an unwillingness to abandon fictions. It is like saying that while the physicist must admittedly confine himself to Einsteinian time, it is *still true* that Newtonian absolute time flows "equally without relation to anything external." It is a sort of *E pur si muove* in reverse. What is lacking is the bold and exciting behavioristic hypothesis that what one observes and talks about is always the "real" or "physical" world (or at least the "one" world) and that "experience" is a derived construct to be understood only through an analysis of verbal (not, of course, merely vocal) processes.

The difficulties which arise from the public-private distinction have a prominent place in the present symposium, and it may be worthwhile to consider four of them.

(1) The relation between the two sets of terms which are required has proved to be confusing. The pair most frequently discussed are "discrimination" (public) and "sensation" (private). Is one the same as the other, or reducible to the other, and so on? A satisfactory resolution would seem

to be that the terms belong to conceptual systems which are not necessarily related in a point-to-point correspondence. There is no question of equating them or their referents, or reducing one to the other, but only a question of translation—and a single term in one set may require a paragraph in the other.

(2) The public-private distinction emphasizes the arid philosophy of "truth by agreement." The public, in fact, turns out to be simply that which can be agreed upon because it is common to two or more agreeers. This is not an essential part of operationalism; on the contrary operationalism permits us to dispense with this most unsatisfying solution of the problems of truth. Disagreements can often be cleared up by asking for definitions, and operational definitions are especially helpful, but operationalism is not primarily concerned with communication or disputation. It is one of the most hopeful of principles precisely because it is not. The solitary inhabitant of a desert isle could arrive at operational definitions (provided he had previously been equipped with an adequate verbal repertoire). The ultimate criterion for the goodness of a concept is not whether two people are brought into agreement but whether the scientist who uses the concept can operate successfully upon his material—all by himself if need be. What matters to Robinson Crusoe is not whether he is agreeing with himself but whether he is getting anywhere with his control over nature.

One can see why the subjective psychologist makes so much of agreement. It was once a favorite sport to quiz him about inter-subjective correspondences. "How do you know that *O*'s sensation of green is the same as *E*'s?" And so on. But agreement alone means very little. Various epochs in the history of philosophy and psychology have seen whole-hearted agreement on the definition of psychological terms. This makes for contentment but not for progress. The agreement is likely to be shattered when someone discovers that a set of terms will not really work, perhaps in some hitherto neglected field, but this does not make agreement the key to workability. On the contrary, it is the other way round.

(3) The distinction between public and private is by no means the same as that between physical and mental. That is why methodological behaviorism (which adopts the first) is very different from radical behaviorism (which lops off the latter term in the second). The result is that while the radical behaviorist may in some cases consider private events (inferentially, perhaps, but none the less meaningfully), the methodological operationalist has maneuvered himself into a position where he cannot. "Science does not consider private data," says Boring. (Just where this leaves my contribution to the present symposium, I do not like to reflect.) But I contend

that my toothache is just as physical as my typewriter, though not public, and I see no reason why an objective and operational science cannot consider the processes through which a vocabulary descriptive of a toothache is acquired and maintained. The irony of it is that, while Boring must confine himself to an account of my external behavior, I am still interested in what might be called Boring-from-within.

(4) The public-private distinction apparently leads to a logical, as distinct from a psychological, analysis of the verbal behavior of the scientist, although I see no reason why it should. Perhaps it is because the subjectivist is still not interested in terms but in what the terms used to stand for. The only problem which a science of behavior must solve in connection with subjectivism is in the verbal field. How can we account for the behavior of talking about mental events? The solution must be psychological, rather than logical, and I have tried to suggest one approach in my present paper. The complete lack of interest in this problem among current psychological operationists is nicely demonstrated by the fact that the only other members of the present panel who seem to be interested in a *causal* analysis of verbal behavior are the two non-psychologists (one of them a logician!).

My reaction to this symposium, then, is twofold. The confusion which seems to have arisen from a principle which is supposed to eliminate confusion is discouraging. But upon second thought it appears that the possibility of a genuine operationism in psychology has not yet been fully explored. With a little effort I can recapture my enthusiasm of fifteen years ago. (This is, of course, a private event.)

## 26

## The Alliteration in Shakespeare's Sonnets: A Study in Literary Behavior

Alliteration is one of the most familiar forms of sound-patterning in poetry and prose. It is said to exist when two or more syllables beginning with the same consonant occur near each other in a given passage. Examples of alliteration are frequently cited as contributing to the effect of a literary work, and it is usually implied that they represent deliberate acts of arrangement on the part of the writer. If this is true, alliteration should throw some light on the dynamics of verbal behavior and especially upon a process which may be called "formal perseveration" or, better, "formal strengthening." Studies of word-association, latent speech, and so on, have indicated that the appearance of a sound in speech raises the probability of occurrence of that sound for some time thereafter. Stated in a different way: the emission of a verbal response temporarily raises the strength of all responses of similar form. The principal characteristics of poetry (alliteration, assonance, rhyme, and rhythm) seem to be exaggerated cases of the tendency toward formal strengthening, and they should supply useful information with regard to it.

In order to determine the existence or the importance of any process responsible for a characteristic pattern in a sample of speech, it is necessary to allow for the amount of patterning to be expected from chance. We cannot assert, for example, that any one instance of alliteration is due to a special process in the behavior of the writer rather than to an accidental proximity of words beginning with the same sound. Proof that there is a process responsible for alliterative patterning can be obtained only through a statistical analysis of all the arrangements of initial consonants in a re-

From *Psychological Record*, 1939, 3, 186-192.