The operational analysis of psychological terms

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Abstract: The major contributions of operationism have been negative, largely because operationists failed to distinguish logical theories of reference from empirical accounts of language. Behaviorism never finished an adequate formulation of verbal reports and therefore could not convincingly embrace subjective terms. But verbal responses to private stimuli can arise as social products through the contingencies of reinforcement arranged by verbal communities.

In analyzing traditional psychological terms, we need to know their stimulus conditions ("finding the referent"), and why each response is controlled by that condition. Consistent reinforcement of verbal responses in the presence of stimuli presupposes stimuli acting upon both the speaker and the reinforcing community, but subjective terms, which apparently are responses to private stimuli, lack this characteristic. Private stimuli are physical, but we cannot account for these verbal responses by pointing to controlling stimuli, and we have not shown how verbal communities can establish and maintain the necessary consistency of reinforcement contingencies.

Verbal responses to private stimuli may be maintained through appropriate reinforcement based on public accompaniments, or through reinforcements accorded responses made to public stimuli, with private cases then occurring by generalization. These contingencies help us understand why private terms have never formed a stable and uniform vocabulary: It is impossible to establish rigorous vocabularies of private stimuli for public use, because differential reinforcement cannot be made contingent upon the property of privacy. The language of private events is anchored in the public practices of the verbal community, which make individuals aware only by differentially reinforcing their verbal responses with respect to their own bodies. The treatment of verbal behavior in terms of such functional relations between verbal responses and stimuli provides a radical behaviorist alternative to the operationism of methodological behaviorists.

Keywords: awareness; behavior, verbal; behaviorism, methodological; behaviorism, radical; operationism; philosophy of psychology; private events; reference; semantics; subjectivity–objectivity; verbal community

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

Operationists, like most contemporary writers in the field of linguistic and semantic analysis, are on the fence between logical "correspondence" theories of reference and empirical formulations of language in use. They have 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 *An Inquiry into Meaning and Truth,* 1940). *Definition* is a key term but is not rigorously defined. Bridgman's (1928; see also 1945) 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" (Stevens 1939). Only in a very limited

area (mainly that of autonomic responses) is it possible to regard a 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.

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 nonscientific 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 claimed 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 1934) 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 the aspect or part of nature which it singles out. One may often avoid that mistake by substituting *term* 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 entirely 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 definition 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 nothing 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, responses to that world are like responses 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 a 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 with 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 intermittent 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 nonverbal 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 noninstrumentally, set up.) There are two important possibilities. The surviving covert response may be regarded as an accompaniment of the overt one (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 responses, as in (1). On the other hand, the covert response may be *similar to*, though probably less intense than, the overt one 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 behavior, 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 generalization 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 generalization, in response to private events. The transfer is based not on identical stimuli, as in (3), but on 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 the response when it is made to public stimuli, the private case occurring by generalization 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 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 deprivations associated with reinforcing consequences rather than antecedent stimuli. The community is skeptical of statements of this sort, and any attempt to talk about one's 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 on the basis of collateral behavior associated with hunger, as in (2), or as a description of behavior with respect to food, or of 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. Using a stomach balloon, one might condition the verbal response exclusively to stimulation from stomach contractions. This would be an example of either (1) or (2) above. Or speakers might be trained to make nice observations of the strength of their 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 other kinds of sensing.

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. *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 reinforcement which is contingent upon a certain property of stimuli. Both speaker and community (or psychologist) have access to the stimulus, and the contingency can 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 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 nonbehavioral 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 can 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. One becomes aware of what one is doing only after society has reinforced verbal responses with respect to one's 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, psychologists must deal with their own verbal practices by developing an empirical science of verbal behavior. They cannot, unfortunately, join logicians in defining a definition, for example, as a "rule for the use of a term" (Feigl 1945); they 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 their use of terms; and it is not logic but science.

Philosophers will call this circular. They 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. E entually 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.

Some afterthoughts on methodological and radical behaviorism

In the summer of 1930, two years after the publication of Bridgman's *The Logic of Modern Physics*, I wrote a paper called "The Concept of the Reflex in the Description of Behavior" (Skinner 1931), later offered as the first half of a doctoral thesis. Although the general method, particularly the historical approach, was derived from Mach's *The Science of Mechanics* (1893), 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* (1928), 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 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 hackwork, 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.

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 reexamination 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 terms like "consciousness," "will," or "feeling" than for "phlogiston" or "*vis anima*." 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 18th-century chemist was talking about when he said that the Metallic 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 operationism 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 Unified 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 "nonphysicalistic" self-knowledge.

The position is not genuinely operational because it shows an unwillingness to abandon fictions. It is like saying that although the physicist must admittedly confine himself to Einsteinian time, it is *still true* that Newtonian absolute time flows "equably 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.

It may be worthwhile to consider four of the principle difficulties which arise from the public–private distinction.

1. The relation between the two sets of terms which are required has proved to be confusing. The pair most frequently discussed is "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 agreers. This is not an essential part of operationism; on the contrary, operationism permits us to dispense with this most unsatisfying solution of the problem of truth. Disagreements can often be cleared up by asking for definitions, and operational definitions are especially helpful, but operationism 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 intersubjective 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 wholehearted 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 whereas the radical behaviorist may in some cases consider private events (inferentially, perhaps, but nonetheless meaningfully), the methodological operationist has maneuvered himself into a position where he cannot. "Science does not consider private data," says Boring (1945). I contend, however, 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, whereas 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 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 confusion which seems to have arisen from opera-

tionism – 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 some years ago. (This is, of course, a private event.)

NOTE

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Stimulus-response meaning theory

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Skinner's account of how subjective psychological terminology gets its meaning relies on his views about meaning in general. Though not extensively laid out in "Terms," their general outline emerges clearly enough to show how radically mistaken they are. So there must be a lot wrong also with Skinner's account of the meanings of psychological terms, but I shall not follow out those consequences; my topic is the underlying stimulus–response approach to meaning in general.

To evaluate Skinner's views about meaning we must first cleanse them of their most unrealistic assumption, namely that the basic linguistic performance is the uttering of a single word. When Skinner speaks of "the circumstances under which the response 'length' is emitted" he is not discussable. Apart from certain highly specialized circumstances, such as helping with a crossword puzzle or displaying reading skills, there are no circumstances under which that one word is uttered in isolation. And when he implicitly contrasts "I see red" with "red," calling the former an "expanded expression," he puts the cart before the horse. Although we grasp sentences only through understanding their constituent words, the notion of meaning attaches primarily to whole sentences and only derivatively to smaller units such as words. Our primary concept of meaning is that of something's meaning *that P,* and the notion of word meaning must be understood through the idea of the effect on a sentence's meaning of replacing this word in it by that. Try to imagine a tribe that has a word for trees, a word for sand, a word for fire, and so on, but that does not use these words in sentences to say anything about trees, sand, or fire. The supposition makes no sense: If the noises in question are not used to say anything, to express whole "that-P" messages, there is nothing to make it the case that the noises are *words* at all.

However, when Skinner and other stimulus–response meaning theorists focus on the single word, perhaps they are really thinking not of the word "red," say, but rather of the one-word sentence "Red!," meaning something like "That thing (in front of me) is red." Let us suppose this, and forget that it still makes no sense of "the response 'length.'"

The activity of labeling whatever public or private item one is presented with is a rare event. Even if we allow for it to be done in normal sentences with several words each – for example,

"This is a chair" or "That is a Ming vase" – it does not happen often, and there is no reason to take it as paradigmatic of linguistic behavior, or as central or basic in it.

Let us set that fact aside also, and attend to the tiny fragment of linguistic behavior that does fit this pattern. Still there is trouble for Skinner's theory of meaning. I am confronted by something red; it is a stimulus, to which I respond by saying "(That is) red." In calling these items a "stimulus" and a "response" respectively, Skinner is implying that the former causes the latter: Like most stimulus–response meaning theorists, he is apparently attracted by the idea that the meanings of our utterances are determined by the very same items that cause them. In his own words, the "referents" of what we say "control" our saying it, and he ties control to prediction, speaking of a "science of behavior in which we are to predict response through, among other things, an independent knowledge of the stimulus."

The phrase "among other things" is needed in that sentence. Without it, Skinner would be implying that linguistic behavior is vastly more predictable than it really is, in the manner of the stimulus–response meaning theorist who once wrote: "If you want a person to utter the word chair, one of the best ways is to let him see an unusual chair" (Miller 1951, p. 166). That is plainly false, of course, and no one would write it who was not in thrall to a bad theory. In a large range of situations we can predict something about the world from a fact about what is said – for example, someone's saying "This is a chair" is evidence that he is probably in the presence of a chair – but predictions running the other way are nearly always quite hopeless (this point is made by Ziff 1970, p. 73; see also Ziff 1960, secs. 46 and 54). But Skinner says "among other things." We are to suppose that the causally sufficient conditions for a person's uttering "(That is) red" consist in (i) a red stimulus in conjunction with (ii) a set of circumstances C which always mediates between a stimulus and an utterance whose meaning is somehow given by the stimulus. If the theory is not that there is a single value of C such that someone who undergoes a red stimulus in C circumstances says something like "That is red," someone who sees a chair in C circumstances says "That is a chair," and so on, then there is no theory. The aim is to say something *systematic* about how the meanings of utterances relate to their causes, and that requires a *general rule* enabling us to read off the meaning of an utterance from the facts about the causal chain that produced it. We shan't get that merely by learning that in each case the causal chain includes, together with a lot of other stuff, something constitutive of the meaning of the utterance. We need a systematic way of filtering out the "other stuff" in order to isolate the element that gives the meaning; and so, as I said, we need a single value of C that tells us in each case which part of the causal chain gives the meaning and which part belongs to the all-purpose "other stuff." (For a fuller defense of this, see sec. 6 of Bennett 1975.)

That is the project of Skinner's kind of stimulus–response meaning theory. (There is another kind – no better but different – according to which meaning is determined not by the stimuli to which an utterance is a response but rather by the responses to the utterance considered as stimulus. For more on this, and on relations between the two, see secs. 7 – 9 of Bennett 1975.) As a project, it has no hope of success: There is no reason to think there is anything remotely resembling a general truth of the form "Whenever anyone encounters an F item in C circumstances he utters something meaning that the item is F." Let C be somewhat vague and tattered around the edges; let it also be less than perfectly unitary, consisting perhaps of about 17 disjuncts; lower your sights by looking only for a rule that applies about 20% of the time; help yourself to two or three further indulgences as well. Still the project will have no chance of success. It assumes a world-to-meaning relationship that simply doesn't exist.

This is not to deny that when a person says something

meaning "That is an F," the odds are that he is confronted by an F, that he has been in perceptual contact with it, and that this contact is part of the causal history of his making that utterance. That much is true, and is presumably the launching pad from which Skinner and the other stimulus–response meaning theorists have embarked on their theory. But it is a truth that brings no comfort to stimulus–response meaning theory, as can be seen by seeing *why* it is true. The explanation is as follows.

When a person utters something that means that a certain thing x is F, he is likely to have some one of a certain cluster of intentions (intending to get someone else to think that Fx, or intending to fix in his own memory his belief that Fx, or the like); if he has such an intention, he probably believes that Fx; and if he believes that Fx then the odds are that x is F-like and that the person has been caused to believe it is F by a perceptual transaction with it. And so someone who says "That is red" has probably been acted upon perceptually by something red.

This involves several probabilities each falling short of certainty; multiply them all together and the upshot is a long way below certainty. Still, it provides an inference from "He has just uttered 'That is a chair' " to "He has recently encountered a chair" which has some cogency: If I had to bet on whether someone had recently seen a chair, I would be interested to learn that he had recently said "That is a chair." But for obvious reasons it provides a vastly less secure basis for inferring the utterance from the perception. Granted that when the utterance occurred it was partly as a result of the perception, there is no systematic and manageable way in which it could have been predicted with as much as 1% probability, except in special cases where the perception is accompanied by a threat or a bribe. Furthermore, there is good reason to think that it is not a strictly causal flow from the perception through the belief to the utterance, and that the causal explanation of the utterance will run along physiological channels and not psychological ones. For a lot of argument to this effect, see Fodor (1980). The best argument, in a nutshell, is as follows. It seems reasonable to think that (i) any item of linguistic behavior admits of a correct causal explanation in physiological terms, and that (ii) there is *no* systematic mapping between facts about mental content and associated facts about physiological states, and that (iii) there *is* a systematic mapping between any two correct causal explanations of the same phenomenon. Thus, my suggested route from the perceptual encounter through to the utterance, as well as failing to support a prediction, also fails to be strictly causal. How then can I offer it as a replacement for, or improvement on, what Skinner is trying to get?

Well, useless as this relation between world and meaning is for Skinner's purposes, it is the nearest thing to his theory that is anywhere near true. What is most striking about it is that it depends essentially upon two of the concepts – intention and belief – that belong to that "subjective psychology" that Skinner thinks he can safely disregard as being of merely antiquarian interest, like phlogiston and *vis anima*. Now, quite a lot of philosophers of psychology these days are also inclined to drop the concepts of intention and belief or to look forward to the day when we shall be able to do so (see Churchland 1981), and for all I know they are right. I am not contending that a good scientific account of behavior must involve those concepts, but only that they are required for any semblance of a systematic link between meaning and circumstance of utterance. Like some others, I think that *the very notion of meaning* depends essentially on intention and belief, and cannot stand if they fall (see Armstrong 1971; Bennett 1976; Grice 1957; Schiffer 1972), but I do not insist on that either. All I need is the much securer thesis that any *systematic bridge between meanings and circumstances of utterance* must involve intention and belief.

Incidentally, once that fact has been faced we can liberate ourselves from the restriction to utterances such as "That is a chair" and "This is red" and "I feel a pain." Skinner's attempt to explain the meanings of psychological terms depends essentially

on his taking that kind of utterance as paradigmatic, but it obviously isn't, and now we can break free from it. Instead of the restricted thesis "When someone utters something meaning that some present thing x is F, it is fairly likely that the thing is F-like and the speaker has recently had perceptual contact with it," we have the much more widely applicable thesis "When someone utters something meaning that P, it is fairly likely that there is evidence that P and the speaker has recently had perceptual contact with some of it." In this statement, of course, we must understand "evidence" as "what would count as evidence for the person whose utterance is in question," and so the notion of evidence we are using here further involves the concept of belief: what counts for a person as evidence that P is, roughly, what inclines him to believe that P. But that is not a further trouble for Skinner's program, because even within the tiny area to which the program is confined it doesn't work – doesn't achieve the beginnings of an approximation to the truth – except with help from the concepts of intention and belief.

Waiting for the world to make me talk and tell me what I meant

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Like so much of Professor Skinner's work, "Terms" separates him from the main thrust of operationism and from the main body of behaviorism. Yet history rarely sees subtle differences. For example, Piaget and Inhelder (1969) miss such distinctions when they brand Skinner a "copy theorist" indistinguishable from other behavioral associationists such as Pavlov or Hull. Why have such criticisms, or those of Chomsky (1959), been so lasting when in fact Skinner's use of terms such as operant, discriminative stimulus, and reinforcement could be used to refer to and "explain" many phenomena treated by cognitive psychologists (Catania 1979)? Perhaps the reason that Skinner has been a focal target of criticism from cognitively oriented psychologists is that the differentiation of himself from other behaviorists and other operationists has never culminated in the promised program of research in human behavior that would demonstrate the differences between the old and the new operationism. That is the main point of this commentary, in which we are trying to stay within Skinner's purview, refraining from discussion of that purview itself.

What are the distinctions whereby Skinner differentiates himself from previous operationists? He offers a "definition of definition" rather than mere correspondences between concepts and the operations by which they are defined or between terms and the criteria for their application. The definition of definition is a statement of the social community's contingency of reinforcement for a term. Thus, psychologists must develop

> an empirical science of verbal behavior. They cannot. . . join logicians in defining a definition, for example as a "rule for the use of a term" (Feigl 1945); they must turn instead to 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 their use of terms.

Since it has previously been concluded that there is no basis for differential reinforcement of private events – no "inner" reinforcing – public verbal responses are the only admissible data for operationism. The promise of this 1945 paper, then, is that an analysis of reinforcement contingencies from the verbal community for verbal behavior will lead to truly operational definitions of terms and therefore to a complete behaviorism.

Twenty-four years later, Skinner seems to have rescinded this promise of operationism. In 1969, he insisted that an observer of contingencies, even the simple contingencies in an operant

conditioning chamber, will not be able to describe the contingencies.

> Over a substantial period of time he has seen various stimuli, responses, and reinforcers appear and disappear. The fact remains that *direct observation, no matter how prolonged, tells him very little about what is going on* . . . *If he could not see what was happening in a relatively simple experimental space, how can we expect him to understand the behavior he sees in the world around him?* . . . It is only when we have analyzed behavior under known contingencies of reinforcement that we can begin to see what is happening in daily life. (Skinner 1969a, p. 9–10, italics in original).

Thus, operationism really requires the demonstration of behavioral control. But how and over what? Is the verbal behavior that is to be "operationalized" the specific words spoken, the inflection, the intensity pattern, the temporal pattern, other features of how words are spoken, or the entire class of synonymous ways that the same thing could be said, or all of these, or something still more? And how, for example, would we operationalize the term *had* in the sentence from a grammar lesson "Mary, where Jane had *had*, had *had had; had had* had the teacher's approval." Such examples exert enough control (in Skinner's terms) upon most of us out here in the language community that we express our acceptance of the grammatical nature of the statement and acknowledge the independence of the grammatical rule from the specific stimulus words.

Although Skinner did not seem to follow through on the distinction between his use of operational definition and his behaviorist predecessors' use of that term, others have explored with other vocabularies what Skinner knows as the verbal community's contingencies for verbal and vocal behavior (Bruner 1975; Wells 1981). However, this endeavor has culminated in a framework that includes consideration of the active "intentions" of both the language community and the speaker. Words and word combinations have different meanings in the language community depending upon the conditions under which they are emitted. The stimuli are not the sounds uttered or even such utterances in the environmental context. Aspects of both must be intentionally selected by an active language community attempting to reconstruct messages from the environmental context and the sounds uttered (Brinker 1982). This active process occurs even when infants emit sounds that could not possibly be words: Adults behave as if these sounds meant something (Bruner 1975).

Even when research on the semantic and pragmatic development of language contains the data that could be relevant to "Terms" (see Segal 1975), a successful analysis of this language data from an operational point of view seems unlikely, given Skinner's 1969 rejection of the possibility of making sense of such observations. Moreover, although organisms freely emit behavior (Skinner 1938), the structure of behavioral repertoires and the probabilities as to which of several behaviors would be emitted – surely prolegomena for such an analysis – were never seriously studied within the operant framework. Nor was the impact of a history of learning upon an organism's performance in a new contingency ever seriously examined.

The Skinnerian picture seems to be of a passive individual who brings nothing to contingencies of reinforcement. He waits for referents to talk about. When a referent comes along he uses terms that are or have been positively reinforced. Thus, he learns the appropriate verbal behavior to talk about public and ultimately private things. It is this passive view of human nature in Skinner's later writings that was *not* necessary on the basis of his early theoretical distinctions (1935a; 1938), but *was* necessary to be consistent with Skinner's operationism. Moreover, this passive view reduces the possibility of a serious and complete contingency analysis of verbal behavior.

Skinner's sense of operational definition in "Terms" then, promises a program of research in which natural contingencies of reinforcement by the verbal community for verbal behavior provide the concept of definition. Yet it has fallen to others to

study such contingencies from a standpoint quite removed from either traditional or radical behaviorism (Bates 1976; Bruner 1975; Greenfield & Smith 1976). Skinner (1969a) seems to have moved away from his 1945 position and abandoned the possibility of understanding any behavior, verbal or otherwise, based on an analysis of natural contingencies. The later position is that understanding is equivalent to experimental control. It is this position, rather than the 1945 one, that is very poorly suited to an analysis of verbal behavior. It is the requirement for experimental control of verbal behavior that has produced the anti-Skinnerian position reviewed by Herrnstein (1977). Skinner (1977) himself feels that such criticism does not apply to his own verbal behavior.

What all this shows perhaps is the power of derivative fashions over the best of 20th-century psychology. The fashion here is operationism which, after the disillusionments of World War I and the ensuing fever for pure objectivity, had grown out of the logical positivism of the Vienna Circle into a promise of a "Unity of Science" for all who would accept its simple rules of initiation. And psychology, weakened with the ineptness of its earlier misguided attempts at a science of consciousness (Fechner, Wundt, Titchener, et al.), wearily climbed on the bandwagon and tried to behave like physics.

But operationism was soon cast off by physicists themselves. It contained logical contradictions (e.g. a "thing" measured or observed in two ways is really two things) and regressions (e.g. how do we operationally define the operationally defining measuring instruments?), and was an insensitive bull in the china shop of psychology with nowhere to go (e.g. how do we operationally define dreams?). As Skinner himself points out in "Terms," Bridgman's (1928) formulation "cannot be taken literally." We note also that in his last sentences some of Professor Skinner's earlier enthusiasm for operationism seems to have become attenuated. For the sake of his own important theorizing, we wish he had never had any enthusiasm for it at all.

Skinner on the verbal behavior of verbal behaviorists

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Skinner's scenario for fixing the reference of psychological terms has the structure of a Greek tragedy, in which the verbal community acts as chorus, instructing the tragic subject in how to name his agonies. The ancients left unexplained the manner in which choruses came by their knowledge, and it is no less a puzzle how the verbal community in Skinner's semantical story comes by its cognitions, all the more so if the story is true. For the question then is how anyone ascends from such basic verbal reports as "toothache" in the presence of toothache, or "red" in the presence of red, as emitted by the well-conditioned subject, to the rich, dense metalanguage of the story itself. The unwritten program of the paper is to show how so exiguous an input gets processed to yield an output as rich as the paper that presupposes the program, if its author began the way its subject does. It is Skinner's belief that we shall, by procedures scarcely more complex than those through which the meaning of "toothache" gets transmitted to otherwise inchoate agonizers, arrive at an understanding of the verbal behavior of scientists. Self-understanding must after all be an aim of psychology, psychologists being human; and if "knowing oneself" is limited even in the case of simple names of simple pains, how likely is it that reflexive knowledge – knowledge *de se* – can be attained of science at the level of science? The question is whether *Verbal Behavior* would have been possible if verbal behavior at large is analyzable as it is said to be here. This I shall show reason to doubt. But I must first applaud

Skinner's recognition that science itself cannot be left outside science, and that what he fears may look like circularity – characterizing a practice in the language of the practice characterized – is not an obstacle to but a condition for the validity of any analysis that pretends to adequacy. *Quis custodiet ipsos custodes* – Who shall guard the guardians? – a problem for the implementation of Skinner's utopia, has its methodological counterpart here.

The genius of tragedy is inseparable from the genius of comedy, Socrates observed after a legendary night of drinking, and it is comic that the conditions for instructing in the reference of psychological predicates immediately gives rise to the Problem of Other Minds, as well as the lesser possibility of malingering pretense on the subject's part. For the collateral accompanying stimuli – "hand to the jaw, facial expressions, groans, and the like" – can be present in the absense of the pain. *Logical* behaviorism, which seeks to define psychological terms through what for Skinner are merely collateral accompaniments, is a radical effort to abort skepticism by making it linguistically inexpressible. But Skinner's native radicalism is tempered by a certain realism: it is "a simple fact" that private stimuli occur and that a (humanly) important class of psychological terms take them as their primary referenda. Besides, logical behaviorism is dogged by the circumstance that at best disjunctive definientiae leave psychological terms ultimately ambiguous, neat translations being hard to come by. If this is so for toothaches, think how much more true it is as we rise to such civilized feelings as gratitude to M. Swann for his gift to the family of a case of Asti, which the narrator's aunts, in *Du côté de chez Swann*, report with such obliquity that even those who know them best, let alone the intended beneficiary of their thanks, are left unclear as to what is said and what is felt. Feelings like gratitude, pride, jealousy, and love typically occur within networks of other feelings as well as beliefs and other propositional attitudes, and it may often take the omniscient powers of a chorus to know what is really going on in alien breasts, as readers of Proust know. And matters are complicated by the intentional structure of many important feelings, which enables them to occur in the absence of stimuli correspondent with their contents – as when someone is grateful to his god (when there is none) for his many blessings (when there aren't any). Toothache is minimally intentional, but even "toothache" requires the user to know something about teeth and appreciate that pains have location. Yet even here, in this minimal case, collateral reference is sufficiently dilating as to foreclose, on Skinner's view, precision of reference.

Now my problem is less with whether his account of the reference of psychological terms is adequate than with whether his analysis of the emission of "Red!" in the presence of red gives an adequate model of the verbal behavior of scientists, though the two problems are deeply connected. The implicit semiotics is this: A verbal report is reinforced only when it is emitted in the presence of the stimulus that the emitted term denotes when the emission is correct. The burden of the paper concerns those cases in which the stimulus, though real, is inaccessible to the agents of reinforcement, in contrast to the standard case where it is accessible to emitter and reinforcer equally. But the terms I regard as central to science are not of this latter sort, but denote things and events inaccessible to anyone, and only loosely connected via definitional ties to "stimuli" themselves intepretable only against a background of typically complex theory. Now there are very familiar programs of analysis that maintain programmatically that all such theoretical terms may be defined without remainder in the idiom of terms that refer merely to what Skinner will call stimuli. Since Skinner has been realist enough to resist logical behaviorism, it is difficult to see how he can consistently yield, strongly tempted as his paper implies he is, to logical empiricism. But once one admits into the language of science terms as loosely tied to stimuli as theoretical terms are, the

schedules of reinforcement will be no more stable here than they are with psychological terms. But if this means, in the latter case, that "it is . . . impossible to establish a rigorous scientific vocabulary for public use," well, it should be so in the former case as well, which means it is impossible, unless one yields to one form or another of radicalism, to establish a rigorous scientific vocabulary at all!

If, in the case of private stimuli, the lack of precision in referring terms makes it impossible for the speaker to know himself, in the case of null stimuli it must equally follow from the correspondent lack of precision for theoretical terms, that we cannot know the real world the terms refer to either. A theory of the semantics of scientific terms that makes it impossible for science to attain its cognitive aims had better be carefully considered, and this certainly must hold for a theory of verbal behavior that makes it impossible to understand the language of science. The symmetries suggest, however, that if we are to make knowledge possible by relaxing the demand for sharpness of reference in the one case, we have no logical basis for not relaxing it in the other direction as well, enabling self-knowledge to arise together with the possibility of knowledge of the world. But as these demands are symmetrically loosened, the picture of verbal behavior limned in "Terms" seems decreasingly adequate to the language of science. In compensation, I would propose that the relevance of immediate inner experience to self-understanding is probably as circumscribed as the relevance of immediate outer experience to understanding the deep realities of the world. Our representations of either must be considerably more complex than mere constellations of verbal reports. The creative individual, in science as in sensibility, will often have to teach the verbal community a thing or two.

Wishful thinking

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Even bearing in mind that "Terms" is a "theoretical" paper, not a report of experimental work, I am struck by how totally *ideology driven* the claims in it are. There is no glimmer of brute empirical fact cited to motivate or support the claims expressed. In particular, no puzzling or recalcitrant or otherwise inexplicable facts about human behavior are shown to succumb nicely to the theory proposed (always a persuasive theme in selling a way of doing science). Instead what we have here is the extrapolation of a creed: working out the details of what the devout behaviorist *has to say*, figuring out the kosher categories into which all facts must be cast, *no matter how the facts come out*. Skinner's role in "Terms" is thus analogous to the theologian's role in codifying, extending, and proselytizing for a system of dogmas.

Skinner, foe of ideology that he is, may take this observation as a particularly shrill criticism, but that is not how I intend it. Every scientific "school" I know anything about has its theologians, and they perform a singularly useful – perhaps even indispensable – service. They clarify the "position," showing what one is committed to if one does science in that way, and this not only sharpens the edges of the theories so that they can better be put to empirical test for confirmation and disconfirmation, it also generates new questions and problems for the theorists and experimentalists to explore.

There is good scientific theology and bad scientific theology, however; one of the benchmarks of excellence is forthrightness and explicitness of claims – leading with one's chin and giving the skeptics and critics an unmistakable target to challenge. Skinner, however, feints and weaves. We get bold declarations ("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"), but then discover that they don't mean what they seem at first to

mean, since the host of obvious counterinstances one could cite does not count against the claim, for one reason or another.

It is an interesting exercise to go through the sentences of "Terms" one at a time and ask oneself: What would it be, *exactly*, to disagree with this claim? One of Skinner's favorite auxiliaries is "may," which occurs with great frequency in "Terms." Here are just a few examples: "the surviving covert response may be regarded as," "a response may be emitted in the presence of," "we may understand why terms referring to private events have never formed a stable and acceptable vocabulary of reasonably uniform usage." "Statements about private events may be under control of the deprivations associated with reinforcing consequences rather than antecedent stimuli." "'I am hungry' may therefore be variously translated."

A further review of the text shows variations on the theme: "Might" and "could" and "possible" are high-frequency items. What is frustrating about these terms is that they have several quite distinct dictionary meanings, and it is often not clear from context which way the reader is intended to go. Sometimes it seems to be the "may" of doctrinal permission ("The communicant may take the wafer on the tongue or in the hand"), and since it's a free country, who could argue with that? Sometimes it seems to be the "may" of mere logical possibility ("It may rain tomorrow, and then again it may not"), and who in his right mind would quarrel with that? Sometimes there is a hint that much more is being asserted: that what *may* be regarded as such and such may *correctly* be regarded as such and such; that when a response *may* be under the control of x or y or z, it *cannot* have any other explanation, it *must* be under the control of exactly one of x, y, and z, and so on. But these stronger claims are not forthrightly made. So who knows what doctrine is being asserted? There is a way of reading almost every sentence of "Terms" so that the staunchest, most radical "mentalist" could agree with it. But we know that that would be a misreading; we are meant to understand that this is a behaviorist manifesto, but exactly which manifesto it is has been left to the intuition of the reader.

There is a reason, I think, for the high frequency of what Skinner would probably call the "may" response in "Terms." What Skinner was proposing at the time was a certain brand of wishful thinking that *might* have worked – but didn't. Every science must simplify, and even oversimplify, its phenomena in search of tractable ways of manipulating, and conceiving of, the "basic" forces, processes, principles. As investigators in artificial intelligence would say, you have to find a "toy problem" you can master first, and no one can give rules or "criteria" for a "good" simplification. "Terms" is a paper about behaviorism's proposed simplifications, and while in the cold light of retrospect we can see that they were not good choices, they were probably well worth a try. "Maybe," Skinner is saying, "we can get away with this crude version of 'translation,' this tractably simple substitute for 'meaning,' this theoretically easy way with reference and consciousness." It is not that Skinner and other behaviorists were oblivious to the ravishing complexities of human behavior, but that they hoped – not unreasonably – to bootstrap their way to some manageably doable science of human behavior with the aid of a little wishful (or even willful) thinking. There is probably no alternative to that basic strategy; today's cognitive scientists just as willfully propose their own oversimplifications. One of these years those defenders of mysterious complexity who hang around waiting to say "I told you so" will be silenced by success.

Private reference

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This commentary elaborates the theory of reference implicit in B. F. Skinner's canonical paper, "The Operational Analysis of

Psychological Terms" in an effort to further distinguish Skinner's radical behaviorism from the logical or philosophical behaviorism of Ludwig Wittgenstein's *Philosophical Investigations* (1953). Wittgenstein rejects the notion that we refer to private stimulation (i.e. "sensations") or at least rejects the notion that we do so in the very same way we refer to people or parades. I argue that, in a Skinnerian analysis, there is no essential difference between the way we "refer" to public things and events and private stimulation.

Our elaboration of Skinner's analysis of reference may begin with Skinner's *Verbal Behavior* (1957), in which he discusses the reference of *tacts*. Tacts are, roughly, verbal operants evoked by some particular object or event. Generally, their referent is the object or event evoking them; for example, the referent of "That animal is a lion" might be a lion or (in the event of an error) some large dog whose presence prompted the remark. Sometimes, however, a tact may be evoked by an object or event that is not the referent itself, but only causally linked with the referent in some way; for example, "That bear stole our food again" in response to a bear track found near an empty picnic table. Nonetheless, in these cases too, the referring response and the referent may be said to be causally linked; that is, the bear referred to may be said to have caused the prompting stimulus, the bear track. *Generalizing, we may say that referents or objects or events causally linked with referents are responsible for the referring response.*

Although Skinner does not consider reference in contexts other than tacts, it is possible to do so. What Skinner calls *echoics* is an example. Roughly, an echoic is a verbal operant evoked by another verbal operant of the *same form*. Suppose a wife says to her husband over the phone, "A skunk got in the basement" and the husband turns to his secretary and repeats, "A skunk got in the basement." The wife's response is a tact, the husband's an echoic; yet husband and wife refer to the same thing, namely, to the skunk. This same analysis also applies to what Skinner classifies as *intraverbals* or verbal operants evoked by other verbal responses having a *different form*. Thus, suppose instead of merely repeating the wife's report, "A skunk got in the basement," the husband had said, "There is a polecat in my cellar." In that case, his remark would have been an intraverbal. Nonetheless, it still would have referred to the same skunk. Generalizing we may say that the referents of intraverbals and echoics are the same as the referents of the tacts to which they may be traced. Since the referent or some object or event causally linked with the referent is responsible for the tact and the tact in turn is responsible for the echoic or the intraverbal, the referent or some object or event causally linked with it is ultimately responsible for the echoic or intraverbal, too.

When we turn from public objects or events to private stimulation, the same essential causal or functional relations exist between referent and referring response. When I say, "The pain is in my neck," I am emitting a tact evoked by the private stimulation in my neck and this is precisely what I am referring to as well. Another person cannot be said to emit a tact *directly* under the control of that very same stimulation for the simple reason that the stimulation is only in my body and not in the other person's body as well. For this reason, the stimulation evoking the tact and the referent of the tact are distinct. Another may, nonetheless, emit a tact ("Richard is in pain") under the control of an object or event causally linked with the painful stimulation occurring in my body; he may, for example, see me holding my neck and moaning and take that as "evidence" that painful stimulation is occurring in my neck. In both cases (whether I or another describes my pain), the referent (the painful stimulation) is what is ultimately responsible for the verbal response. Had the other person's tact ("Richard is in pain") been emitted at the sight of blood, then we could still say that the evoking stimulus is a condition (damaged tissue) causally linked with the referent (the painful stimulation). Thus,

the essential causal or functional relationships are no different from cases in which the referent is a public object or event. Nor is there any cause to exaggerate the importance of the fact that only I can *directly* tact the painful stimulation. There are parallel situations that can arise even when we are dealing with public objects or events. If, for example, I am the only one who witnesses a certain event, say the eruption of a volcano, then I alone am in a position to emit the relevant tacts (e.g. "The dust of the volcano went miles into the air") in a "direct" way. Indeed, each of us is in a position to emit very few tacts of this "direct" sort with respect to most of the things to which we nonetheless refer.

A great many of our statements referring to the private stimulation of others are emitted as intraverbals. That is, for the most part we rely upon the tacts of the person in whose body the stimulation occurs. (Obviously, this is not the case when we are establishing such tacts in the young. In those cases, we rely upon the measures noted by Skinner in "Terms.") In any case, when intraverbals are emitted, the painful stimulation is responsible for both the tact of the person in pain and ultimately, therefore, for the intraverbal as well. And in these cases, moreover, that very same private stimulation is what both tact and intraverbal refer to. Thus, if someone says, "The discomfort is in Richard's neck" upon hearing my report, "The pain is in my neck," both responses refer to and are the result of the painful stimulation in my body. Similar consideration would apply when a parent echoes his child's pain report, "The pain is in Margaret's tummy." Here, too, the parent's echoic and the child's tact refer to and are the result of the very same painful stimulation occurring in the child's body.

In conclusion, then, it has been argued that there is no essential difference between public and private reference in a Skinnerian analysis. In both cases, the very same sort of functional relations may be seen to obtain between referent and referring response. This is, I believe, a very great advance over Wittgenstein's notion that there is some essential difference between the two cases – a suggestion that only mystifies us, since it is never spelled out in a clear or detailed way.

Sensation and classification

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The aspect of Skinner's canonical target article on psychological terms on which I want to focus attention is that of the role of stimuli and responses in the classification of sensations. On Skinner's view, when subjects report certain private stimuli, sensation classification takes place. Something is called a pain rather than an ache, and a sharp pain rather than a dull one. These classifications involve as prime movers both the previous stimuli for the sensations and the consequent responses; that is, the surrounding community operantly conditions subjects to classify sensations in terms of the stimuli that produce them and the responses that they produce. Stimuli and responses may vary, and there may also be publicly unobservable stimuli and responses. Thus, classifications are pegged by conditioning to a tangled skein of stimuli and responses.

If we consider Skinner's view of sensation classification in the light of the currently regnant philosophy of mind – functionalism or the causal theory of mind (e.g. Churchland & Churchland 1981; Lycan 1981) – we see immediately that Skinner's account bears a striking resemblance to the functionalist or causal account. On the functionalist or causal account, sensations are classified in terms of their causal roles. If

there is a difference between one kind of sensation and another, there is a difference in their causes and effects.

Resemblance between Skinner's and the functionalist account is no accident. Skinner is a kind of functionalist, for he has always found it necessary to interpret behavior as standing in functional relations with environmental and physiological events. But his psychology tends to concentrate on three sorts of behavioral relata or effects: (1) movement of a joint or limb in service of the *creature as a whole* such as kisses and key pecks; (2) locomotor acts such as walking and jumping; and (3) speech acts (Skinner 1957) such as *tacting* (roughly, stating) and *manding* (roughly, commanding and requesting). What "Terms" contains is atypical: a glimpse of Skinner's view of sensory experience below the level of joint or limb movements, locomotor acts, and speech. Here, I think, is where confusion in interpreting Skinner arises. Malcolm (1964), in a widely read discussion, called attention to Skinner's view of sensation classification. But he argued that the view implied that introspection does not occur, that reports of sensations by subjects of the sensations are based on observations of their movements and locomotor acts.

Contrary to Malcolm's interpretation, Skinner argues that classification by subjects is immediate, in the direct report of sensations under the aspect of the stimuli that produce them and the responses they produce. Subjects do not observe movements and then classify. They immediately respond to their sensations – both "feel" and report them – as typed according to their causes and effects. A person knows what it is like to have a sharp pain as a result of having conditioned responses of the sharp pain sort – where sharp pain sort is defined in terms of stimuli and responses associated with sharp pains.

"Terms" and sections of *Verbal Behavior* (pp. 130ff.) explain, on my reading, how such conditioned responses are possible. The key idea is that reinforcement by outside observers fixes or pegs certain overt responses (introspective reports, e.g.) and covert responses (introspections) to sensations by virtue of their associated stimuli and responses. Subjects learn to "feel" or perceive what is distinctively sharp about sharp pains. This is what their typical stimuli and responses consist of. For example, a sharp pain is a pain felt to be the sort one usually gets from knives and tacks. A burning pain is a pain perceived to be like those produced by contact with fire or hot surfaces. An adjective such as "blinding" reported of a sensation suggests that the character of the sensation has something about it that makes a subject close his eyes or shuts off his vision. Each of these ways of characterizing sensations involves reference to the typical stimuli–responses of the sensation. For Skinner, subjects are taught to make such discriminations or classifications by the surrounding verbal community, which makes reinforcement for introspective reports (and by generalization for introspections) contingent upon whether the subject of the sensation classifies sensations by reference to their typical stimuli–responses.

My reading welcomes Skinner as a contributor to current debate on sensations. There are several ways to make this point. It seems promising, for example, to consider how Skinner would respond to the inverted qualia objection to functionalism (e.g. Block 1978). The heart of the objection is that it is possible for sensations to remain the same (in kind) on introspection when their roles change. But Skinner should retort that this is not possible. The operant conditioning of introspections to sensations as-classified-by-stimuli–responses means that if stimuli–responses or roles change, introspections would change also. What sort of sensation a person has – or what it is like to have a certain sensation – cannot be detached from the stimuli and responses associated with the sensation.

Another point worth mentioning is that Skinner's account of sensation classification makes for symmetry between classifications by subjects and outside observers. Both subjects *in intro-*

spection and observers through inference from associated stimuli and responses classify sensations the same way for Skinner: in terms of their associated stimuli and responses. Introspective classifications are pegged to stimuli and responses by the mechanism of operant conditioning. A recurrent problem for functionalism is to explain the introspective classification of sensations without appeal to exclusively introspectible qualities or so-called intrinsic properties. The importation of the mechanism of operant conditioning from Skinnerian psychology might be the solution to this problem.

In summary, reflection on *Terms* should serve to locate Skinner in the center of current debate on the classification of sensations. When he discusses certain private stimuli, he is discussing sensations. And his view, like that of the currently regnant philosophy of mind, is that types of sensations are defined by their causal roles. The distinctive contribution of Skinner to the debate in question is the postulation of operant conditioning as the mechanism whereby subjects classify sensations in terms of their causal roles.

Operationism, smuggled connotations, and the nothing-else clause

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Scientific language contains two types of words: those that are also used in the ordinary language of the scientist, and those that have been specially developed for specific use in the science. The latter, that is the technical terms, are generally more precise than ordinary words in the sense that there is little ambiguity about the phenomena to which they refer. This is simply because an *a priori* agreement exists in the scientific community as to exactly how a given technical term shall be used. Some technical terms are coined for the purpose: for example, *neutron, haemoglobin, trigonometry*, and *bacillus*. In some sciences, notably psychology, however, a different practice is common. Selected words of ordinary language are used *as if* they were technical terms. This has resulted, as we shall see, in considerable confusion. It is important, therefore, to note that the sort of terms discussed in "Terms," that is verbal responses to "private" stimuli, are not, as Skinner's title implies, psychological ones. They are words of ordinary language.

The characteristics of words in ordinary language are quite different from those of technical terms. Ordinary words do not have predetermined meanings because they do not come into use as a result of prior deliberation. Moreover, as any good dictionary will show, there is no word that has only a single meaning. Ordinary language functions perfectly well, however, for two main reasons. First, the context in which a word is used makes clear its meaning on that occasion. The word "reinforcement," for example, has very different (but not unconnected) meanings when it is used in discussions of military strategy, architecture, and psychology. Second, the sort of accuracy generally necessary in science is not demanded in ordinary discourse. The statement "Jane smiled," for example, does not invite questions as to the extent and direction of Jane's facial movements, or about the precise criteria by which the smile was distinguished from a grimace or a laugh.

When a word is considered apart from context there is nothing to indicate what meaning should be given to it. For example, despite the fact that a false belief to the contrary is common, the question, What is "mind"? is not answerable because it does not make clear which of a multitude of usages is in question; for example, "my mind is on other things," "mind that child," "have you lost your mind," "he has a good mind," and "my mind is made up." None of these statements calls for the speaker to subscribe to any "theory" of mind, dualist or

otherwise, and the word "mind" presents no difficulties when, in Wittgenstein's terms, it is used in its "original home," namely "everyday usage" (1953, sec. 116). Confusion occurs only when *the* definition is sought for a word such as "mind."

Operationism has been the most influential attempt in psychology to deal with this difficulty. In essence it seeks to institute a single (operational) definition for a term that has numerous uses. These uses must be eliminated if a single definition is to stand. For this reason the nothing-else clause in Skinner's definition of operationism is crucially important. As Skinner notes, however, operationism has failed – though not because the nothing-else clause is negative, but because it has not been observed.

In the first place, it has proved impossible to eliminate the ordinary-language connotations of a word. For example, operationally defining "stress" as immobilizing a rat for 48 hours has not prevented the same psychologist from making assertions about job stress, marital stress, and the like. Indeed, operational definitions have been used to *smuggle* into scientific statements claims that are unwarranted by data. The confusion is made worse by the impossibility of legislating a single operational definition for a term. Different individuals have used different operational definitions for the same term, and the same individual has used different operational definitions from time to time. Thus, the very purpose of operationism in psychology has been thwarted.

Skinner offers an entirely different approach to the problem that operationism failed to resolve. I shall term this the "special theory" of verbal behavior, as it is a specific application to the issue at hand of his "general theory" of verbal behavior. This theory is a monumental contribution to our understanding of language. It is also a curiosity of the intellectual history of this century because, for various reasons – none of them sound – it has been neglected in favor of linguistic theories of no lasting value. Nevertheless the special theory does not effectively deal with the problems of scientific discourse. This is because these problems are conceptual whereas the theory is empirical. In fact Skinner noted this distinction, some years after the first publication of "Terms," as follows: "Behaviorism is not the science of human behavior; it is the philosophy of that science" (1974, p. 3). By the same token, the special theory of verbal behavior is a scientific theory, whereas issues concerning the language of science are problems of philosophy of science. Only the theory of verbal behavior depends upon empirical evidence whereas the philosophy of science entails conceptual analysis (cf. Harzem & Miles 1978).

Consider, for example, a child (or for that matter, an adult) saying "Mama!" when in pain. Merely to assert that here "Mama" is associated with pain stimuli does not render it any the less correct that "Mama" refers to the individual's mother. Moreover, "Mama" may also be uttered under a variety of other stimulus conditions; when one is unhappy, wistful, overjoyed, and so on. And this, of course, again entails the problem of ambiguity that operationism failed to remedy. For a different example, consider the words used by Skinner in his definition of operationism: "observation," "procedure," "step," "intervene," and so on. Knowing the stimulus conditions prevailing at the time he wrote them will help us to comprehend neither the words nor the definition. What is needed is a conceptual analysis. The techniques of conceptual analysis, mostly developed by Wittgenstein (1953), Ryle (1949), Austin (1946), and other "linguistic" philosophers, in the years following the first publication of "Terms," constitute a major support for behaviorism as the philosophy of the science of behavior (see Harzem & Miles 1978 for a detailed discussion). These techniques provide significant new insights concerning, for example, "mentalistic" terms. It is high time that they were recognized and used in contemporary behaviorism. For without them many of the puzzles of the language of a science of behavior will remain unsolved.

What, then, is Skinner's operationism?

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Although much of Professor Skinner's essay, "Terms," is a critique of ways in which operationist notions are commonly applied and understood, he clearly identifies his own work with an operationist position. What sort of operationism, then, is his? "Terms" offers no direct statement of this except through example; it provides only clues as to the role of operationist principles in behavioral analysis considered as a whole theory. Since those clues provide an indistinct and perhaps misleading impression of Skinner's operationism, they bear examination in relation to some of his other work.

A salient clue appears toward the end of the first section of "Terms," when Skinner asserts that contingencies of reinforcement provide the proper operational basis for analyzing psychologists' use of terms. One might infer from this, as critics have inferred from other of his writings, that for Skinner contingencies of reinforcement are the only admissible operations in a scientific accounting of behavior. Indeed, he places these among the most fundamental of interpretive principles. However, Skinner's approach to behavioral science also includes, at the very least, elicitation as the defining relation of reflexive behavior. After all, Skinner was the first to distinguish clearly control of behavior by elicitation from control by consequences (1935b; 1937), and two of his early papers (1931; 1935a) provide some of the most astute analyses of elicitation that are to be found anywhere. But these do not exhaust the range of behavioral processes that Skinner entertains. In "Selection by Consequences" (1981 and this issue, q. v.), he asserts the validity of selective consequences other than the reinforcement principle. And in a recent exchange with Herrnstein (Skinner 1977a), it became clear that Skinner is willing to entertain additional formulations for dealing with "phylogenic behavior," which seems to be maintained neither through elicitation nor through reinforcement.

A key feature of Skinner's operationism, while implicit in his many positive contributions, is explicitly identified mainly by exclusion. Part of the exclusion is specified in "Terms" when he questions the usefulness of operationalizing mentalistic terms. He uses similar arguments to put aside less mentalistic terms that are also derived from vernacular explanations of behavior. In such cases, as illustrated here for "being conscious" and for "matters of reference or definition," Skinner accounts for domains of phenomena in which vernacular or mentalistic terms are commonly invoked, but he does not use such labels to shape his enterprise. Examining his rationale still further, one finds him in a later paper, "Are Theories of Learning Necessary?" (Skinner 1950), putting aside not only mentalistic and vernacular terms as useful foci for operational definition, but also rejecting certain technical terms – those that appeal to "events taking place somewhere else, at some other level of observation, described in different terms, and measured, if at all, in different dimensions" (Skinner 1950, p. 193). Thus his enterprise is not a pursuit of engrams, or of the nature of an association, as could be said of other behaviorists. Nor is it an attempt to give scientific legitimacy to psychological terms from ordinary language, as could be said of much of the current fashion in psychology. Rather, Skinner's behavior analysis is a conceptual fabric in which operations are themselves the very warp and weft. Further, it is a bona fide theory, monistically construed, of "the 'real' or 'physical' world (or at least the 'one' world)." Skinner's specification of operations, then, is an attempted characterization of features of the world as they affect behavior. The theory is an attempt to describe efficiently the effective environment in interactions between behavior and environment.

With hindsight, it seems unfortunate to have asserted this position was "nontheoretical," for this appears often to have led to its being misunderstood. Skinner's assertions that the causes of behavior are in the environment are read by his critics as logical howlers, or even as claims to metaphysical truth. If one clearly identifies such assertions as stating an assumption of a theory – the key axiom in a "bold and exciting behavioristic hypothesis," philosophically trained readers are obliged to entertain the assumption while reading on, whether or not the statement violates commonly held assumptions.

An additional issue is the place of logic within Skinner's system. An explicit message of the essay is that logic is neither the starting point in his approach nor the ultimate source of its validity. In elaborating the rubric of discriminandum, response, and consequence, Skinner provides an interpretive account of the scientist's working – and of what it means to be discriminating and aware, as indeed a scientist may be. "It is not logic, but science," in that these relationships efficiently characterize the phenomena whereby scientific activity is effective, and thus valid. The reader might conclude that for Skinner, "logic is out," for "Terms" gives no hint of the fact that the circumstances in which we ordinarily speak of logic do have a place within his system. The interpretation that handles them resembles the one he presents here, but the discriminanda are not mainly one's own behavior, as in the case of awareness, but rather are special products of behavior – rules and algorithms. Most of this elaboration came later than "Terms" and is worked out in "An Operant Analysis of Problem Solving." (Skinner 1969b and this issue q. v.). Thus, logic is still in, but not in a keystone position. One finds it instead as a category under rule-governed behavior, in an exposition that clarifies a basic fact that is obscured in everyday usage: Only part of the behavior described as logical is functionally attributable to formal logic. So, contrary to a likely inference from "Terms," rules of formal logic do play a role in Skinner's system. Still, deemphasis of that role is appropriate to Skinner's present article, for within behavior analysis the role of what we commonly call logic is not a definitive one that justifies either theory or scientific practice.

Skinner on sensations

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What does the word *toothache* mean? In the view of a mentalist, it means a personal experience, a private sensation; in the view of an operationist, it means the public moaning and grasping of the jaw containing an abscessed tooth. As his 1945 paper, "Terms," indicates, Professor Skinner is an operationist. For him, the word *toothache* means not the private stimulus that elicits its use but the public stimuli that control reinforcement of its use. Furthermore, Skinner resists the moderate suggestion that *toothache* means both private sensation and public accompaniments. He prefers the more provocative thesis that its meaning is exhausted by talking about the latter. No fence sitting for him. Radical behaviorism or none at all. Toothache is to be defined solely in terms of its dental causes and behavioral manifestations.

What, exactly, is Skinner saying here? Definitions properly so called are equations, assertions of identity. They have the form "a = b," and they mean "a is the same thing as b." By saying that *toothache* is to be defined in terms not of private sensations but of public accompaniments, does Skinner mean either to deny that there is such an experience as toothache or to assert that it consists in moaning and grasping of the jaw of an abscessed tooth? Such is the usual interpretation of his views, but I do not think it will fit "Terms." As I read him, Skinner is saying here that *toothache* denotes neither a private sensation nor its public accompaniments but an *unknown bodily condition* normally caused by an abscessed tooth and normally manifesting itself in moaning and grasping of the jaw. To say that we can only define this condition by talking about its public causes and symptoms is

to say not that it is identical with these but that we know how to identify it only by referring to these.

If Skinner is often taken as denying the very view that I have here attributed to him, part of the reason may be that he does deny a superficially similar view. This view, which he attributes to such "methodological behaviorists" as E. G. Boring, is the doctrine that toothache is that *unobservable experience* normally caused by an abscessed tooth and normally manifested in moaning and grasping of the jaw. Skinner certainly rejects this doctrine, which sounds very much like the one I have attributed to him. However, there is a considerable difference between the two. On Boring's view, nobody can know what another's toothache feels like, or tell someone else what his own toothache feels like. To know what a toothache feels like, one must have it; and no one can have anyone else's toothache. By contrast, Skinner says that one can know what someone else's toothache is like in two ways. First, one can know that it is the sort of experience that people have under certain conditions; for it is defined by reference to those conditions. Thus, one can know that a piercing toothache is like the ache one feels when one's skin is pierced by a knife; for that is its definition. Second, one can learn what another person's toothache is like by discovering its physiological properties; thus, we might one day discover that someone's having a toothache consists in his brain being in a certain state.

The distinction just stated will be clearer if I explain it by means of an analogy. In front of a room two people are in clear view. X says, "Behind the screen between those two people is a person whom you do not see but whose voice you hear. We do not know what he looks like, but we could find out if we could get behind the screen." By contrast, Y says, "Behind the screen between those two people is an invisible and intangible person. We do not know what he looks like, and we never shall; but we know he is there because we can hear him." X is Skinner; Y is Boring (at least as Skinner sees him). What X and Y say will sound identical to those who detect no important difference between an unseeable person and one that is merely unseen. Similarly, those who uncritically and incoherently assume (as Boring apparently did) that an unobservable experience could be identical with an observable state of the body will see no important difference between the doctrine I have attributed to Skinner and the doctrine he attributes to Boring and repudiates as untenable. However, they are worlds apart. Boring has postulated an unknowable; Skinner has not.

It is true that, at the present moment, both Boring's and Skinner's toothache are unknown in the sense that we lack knowledge of their intrinsic properties. We know toothache only as that organic condition, whatever it may be, typically caused by an abscessed tooth and typically causing moaning and grasping of the jaw. We do not know whether toothache is a brain state or a muscular condition or both. For this reason, Skinner often says that there is little profit in talking about this undetermined state. Doing so is rather like trying to say what the person behind the screen looks like ("He is tall and has brown hair") before we have seen him. It would be better, thinks Skinner, to wait until we can have a look – especially since the thing making the sounds might be not a person but a record player or two persons talking alternately. Similarly, it would be better, thinks Skinner, to wait until we have independent information about the intrinsic properties of such states as toothache – especially since, so far as we know, there may be not one but many different physiological conditions answering to the one word *toothache*. Skinner's cautions against postulating unobserved states may be unjustified, but they do not amount to denials that such states exist.

In summary, I read Skinner as arguing in "Terms" not that toothache just is its overt accompaniments, but that it is the physiological state or states that these usually accompany. His claim that we can only define *toothache* in terms of abscess and moaning and grasping of the jaw, I take to mean not that

toothache just *is* abscess plus moaning and grasping of the jaw but that, lacking ability to specify the physiological properties of toothache, we are able to identify it only by talking about its usual causes and symptoms. Doing so may not provide us with the best kind of identification, but for the moment it provides us with all the definition we have.

Social traits, self-observations, and other hypothetical constructs

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In returning to read Skinner's original writings, one is struck with the contrast between the much maligned and simplistic "Skinnerian" position and Skinner's own work. Whatever one's theoretical stance, it is hard to read more than a few pages of Skinner and not find a compelling logical argument. Likewise, one is reminded in "Terms" of the characteristic that marks so much of Skinner's work, and that is most responsible for his position within and outside the discipline of psychology: Skinner has never been content to apply his functional approach exclusively to limited problems of the laboratory, but has, throughout his career, grappled with crucial philosophical issues. It is this great breadth that is, more than anything else, the basis of Skinner's important contribution to contemporary thought.

In "Terms," Skinner introduces issues that continue to be of great interest to those studying personality and social psychology. For instance, the abundant research on "self-perception" owes much of its impetus to Bem's (1967) radical behaviorist analysis of "cognitive dissonance" research. In fact, 35 years after Skinner's paper appeared in the *Psychological Review*, one of us published a paper there dealing with the issue of self-observation of one's own "traits" (Kenrick & Stringfield 1980), and the lines of reasoning there can be traced directly through Daryl Bem (Bem 1967; Bem & Allen 1974) to Skinner.

From the vantage point of the recent research on trait measurement, we wish to make two points regarding Skinner's analysis. One is that Skinner may yet be making too much of the distinction between public events and private events as they occur in natural (nonlaboratory) settings. The other is that people can be taught to make the important and useful discrimination between those covert events with public concomitants and those without such accompaniments, and this distinction is a useful one for psychology.

With regard to the first point, public language may not be as closely discriminating as Skinner implies, but may instead approach the imprecision of describing private events. Nevertheless, both may still have a rough utility. In learning to apply terms to publicly available events, one is not usually dealing with phenomena as stable and reliable as a "red" ball. Many of the interesting (and survival relevant) discriminations have to do with applying social labels (e.g. "aggressive," "friendly," "seductive") to overt behavior. Unfortunately, such behavior is often subtle and transient. Consider, for instance, the case of aggressive behavior, which occurs infrequently, briefly, and which is, except in rare instances, modified and attenuated by situational constraints. In addition, a given instance of overt behavior may not look the same to (or even be processed by) observers at different vantage points. Behavior that looks like a friendly pat on the back to one observer may appear to be an aggressive and competititive act to another, and may not even be processed by a third observer. Thus, the "sharpening of reference that is achieved, in the case of public stimuli, by a precise contingency of reinforcement" may not be possible in many important cases of overt social behavior. Even so, recent research has demonstrated that our reports about the

"traits" of those we know well, for all the ambiguity and complexity of their basis, are well corroborated by other familiar observers. These findings have gone contrary to the expectations of many social psychologists who, focusing on all the potential sources of unreliability in trait ascription, came to believe that traits existed mainly in the "eye of the beholder" (see Kenrick & Dantchik 1983). With all their problems, social-trait terms do nevertheless have utility, and the same case may be extended to reports of private events. If we were to disregard descriptions of private events solely because they are often inexact or ambiguous to the outsider observer, we would by the same reasoning have to discard descriptions of overt social behavior. Rather than do this, however, we would argue that the evidence from the social realm should encourage us to give more credibility to actors' reports about their internal states. Not only are such states frequently salient and easily discriminable, but they may be no more subject to bias than reports about overt behavior, and like such reports, they may nevertheless have an important utility.

A related point regards Skinner's contention that "differential reinforcement cannot be made contingent upon the property of privacy." This statement can be interpreted in two ways. If we take it to mean that the community cannot differentially reinforce two covert events, it is true, but rather obvious. If, however, we interpret it in its literal sense, to imply that the community cannot provide feedback that will allow for a discrimination between those internal events that have public concomitants and those that do not, it is false. For instance, if I say "I feel anxious," an observer may respond, "Yes, you're shaking like a leaf" or "That's funny, you look calm." The self-observer's ability to make such a distinction is, in fact, of practical utility to the personality researcher. Subjects in the Kenrick and Stringfield (1980) study were able to provide such information successfully, and this proved useful in enhancing the strength of the correlations between self-reported trait standing and criterion ratings (made by others). Neither parents nor friends could accurately gauge the emotionality experienced by people who describe their emotion as private, while parents and friends could reliably assess the emotionality of those who described their emotionality as public. This finding was recently corroborated in a more intensive investigation by Cheek (1982).

A final point we wish to make is that while there is some utility in dealing with constructs "in the form in which they are observed," this analysis of overt verbal responses can take us just so far. Skinner is to be lauded for showing the limitations of the earlier operationism, but he does not go far enough in making the case for inference-based approaches to science. After all, the elements of the periodic table were placed by Dalton's inference, and Mendel established the existence of "genes" by inference. In the behavioral realm, there is some utility in performing a functional analysis of the verbalizations of schizophrenics, in the interest of modifying their utterances to bring them into an acceptable range for social discourse. [See also Schwartz: "Is there a Schizophrenic Language?" *BBS* 5(4) 1982.] However, no amount of such proximate functional analysis would by itself have led one to suspect a genetic involvement in the disorder, a discovery that could ultimately prove useful in understanding and treating the disorder. Similarly, a functional analysis might be useful in understanding the circumstances surrounding the complaints of a conversion hysteric, but an operant approach to modifying the verbal behavior of such an individual might be misplaced indeed, given the research indicating that the majority of individuals so diagnosed actually had serious physical symptoms (Slater & Glithero 1965; Whitlock 1967).

In summary: (1) Skinner's functional analysis of psychological terms continues to have diverse ramifications throughout the field. (2) He may have overstated the differential accuracy with which words describing public and private events are used in normal language. (3) The usefulness of a functional approach does not negate the use of approaches relying upon hypothetical constructs (provided that these are ultimately verifiable).

The flight from human behavior

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"Terms" is undoubtedly one of the most important papers that Skinner has written. It is also one of the most neglected. Thereon hangs a tale of misrepresentation, misunderstanding, or simply confusion on the part of behaviorists and non-behaviorists alike; a tale, moreover, that reveals a strange reluctance by behaviorists to grapple with the central problems of human psychology.

In his book *The Behavior of Organisms* (1938) Skinner wrote that the importance of his science of behavior, then based upon research with animals, lay in the possibility of its eventual extension to human affairs. He speculated that the only differences existing between the behavior of rat and man – apart from differences of complexity – might be in the area of verbal behavior (p. 442). His paper on operationism pursues the direction he had earlier signalled and is an attempt to extend his account of animal behavior to humans, and in particular to verbal behavior. Implicit throughout "Terms" is the recognition of something special about human behavior – the salient characteristic being that not only can humans like rats "see objects," but also that they can "see that they are seeing them." That is, humans become aware or conscious of their own behavior, and in a way that is true of no other animal species. The great achievement of "Terms" is that it shows that "consciousness," which has long been ignored or denied in both behaviorist and nonbehaviorist sectors of psychology, is, after all, amenable to scientific analysis. Far from being forever locked away in the purely private domain of an individual's "mind," it has its origin (and therefore its decipherment) in the most public of arenas – the "verbal community." We learn from our parents and others how to use words to describe the environment and our own overt behavior, and we also learn to describe stimuli and behavior that are not directly observable by the verbal community, such as our "having a toothache" and our "seeing red." Over time, much of this verbal commentary on our own behavior itself becomes covert and elliptical in form, but it remains behavior nonetheless, and as such is subject to a behavioral analysis.

This analysis, dealing as it does with the role of covert stimuli and covert behavior, contrasts with the approach of methodological behaviorism which maintains that, since there can be no public agreement about unobservable events, they cannot be included in a scientific account. Skinner, never one to balk at a lack of public agreement, cogently argues that this is an outmoded view of science and that there should be no aspect of human activity left out of account on the grounds that it is not publicly observable or that it has to be inferred from other events. It is this concern with the role of "private events" in human behavior that distinguishes his approach and is, indeed, at the heart of his radical behaviorism (Skinner 1974, p. 212).

Thus it is surely a strange irony of contemporary psychology that an approach which, as far back as 1945, established its identity on the basis of its recognition of the "inner life" of humans should so often be charged with the error of ruling it out of court. It is widely asserted, for example, that Skinner's is a "black box" account of human behavior, that it does not deal

with consciousness and cognitive processes, that it eschews the analysis and modification of private events, and that it shuns inferential accounts of behavior because they are unparsimonious (see Chomsky 1975; Harré & Secord 1972; Kendall & Hollon 1979; Koestler 1967; Ledgwidge 1978; Locke 1979; Mahoney 1977; Wilson 1978). Recently, for example, a new movement within clinical psychology, known as *cognitive behavior therapy* has found it necessary to adopt the conceptual apparatus of cognitivism apparently out of a mistaken belief that the behavioral approach cannot deal with the modification of people's covert behavior (see Lowe & Higson 1981; Zettle & Hayes 1982). It may be partly the responsibility of behaviorists themselves that such misconceptions about radical behaviorism are so widespread. For, unhappily, despite the clear theoretical lead given by Skinner in this paper, radical behaviorists have been reluctant to investigate the role of language in human learning. Although Skinner's account of the development of human "consciousness" is similar in many respects to that of Vygotsky (1962) and Luria (1961), it has not had anything like a comparable impact on psychological research. Whereas Vygotsky's ideas inspired valuable research on the way in which self-descriptive verbal behavior develops and interacts with other behavior (cf. Luria 1961; Sokolov 1972), there has been little empirical investigation of the ideas that Skinner outlines in "Terms" and goes on to elaborate in subsequent publications (e.g. Skinner 1953; 1957; 1963; 1974). Instead, radical behaviorist research has been concerned almost exclusively with animal behavior or with human behavior treated as if it did not differ significantly, in terms of controlling variables, from the key peck of the pigeon or the lever press of the rat.

One can only speculate about the factors responsible for behaviorists' neglect of the complexities of human behavior. From the start what was attractive for many about the Skinnerian system was the new methodology and techniques that it introduced for the prediction and control of animal behavior, together with the basic conceptual apparatus within which the effects of the environment on behavior could be expressed. On the other hand, "Terms," together with Skinner's other writings on the philosophy of science and on the development of human, as opposed to animal, consciousness, was perhaps not known, and certainly was not widely appreciated. Instead, earlier notions, dating from Watson, of what behaviorism was about and the prevailing zeitgeist of positivism overshadowed behaviorism's principal theoretical innovation. Thus, for many aspiring behavior analysts, it became almost a matter of ideological purity to deny the existence or efficacy of any event that could not be publicly and directly observed and measured. Watson's (1913) ban on introspection, although no longer justified by Skinnerian theory, continued to hold sway and had particularly bad effects. If, as Skinner argues, what is unique about humans is their capacity to reflect upon their own behavior, then not allowing subjects to report such behavior served only to distance it from behavioral analysis.

So it is that almost 40 years have elapsed since "Terms" was written and yet its challenge to contemporary psychology remains. For example, Skinner's hypotheses that "being conscious, as a form of reacting to one's own behavior, is a social product" and that "one becomes aware of what one is doing only after society has reinforced verbal responses with respect to one's behavior as the source of discriminative stimuli" have not yet been systematically investigated. Moreover, little is known about the ways in which the rest of human behavior is affected when this form of consciousness develops. Could it be the case, as recent evidence suggests, (i) that the effects of reinforcement are altered qualitatively when subjects acquire the skill of generating verbal descriptions (of whatever accuracy) of their own behavior and its consequences, and (ii) that human performance that is free of this "interfering" consciousness is indistinguishable from that of animals? (see Lowe 1979; 1983; Lowe, Beasty & Bentall 1983). How much of our behavior is conscious

in the sense that Skinner posits, and, finally, is it possible within the context of an overall behavioral analysis to alter "consciousness," thereby enabling humans to control more effectively their own behavior and their conditions of existence? Radical behaviorism offers a coherent conceptual system and methodology which, as this paper of Skinner's demonstrates, can be applied to human as well as to animal behavior. It would seem, then, a particularly suitable approach to adopt in the investigation of such questions, and it is issues such as these that should surely be central to any human psychology.

Radical behaviorism and mental events: Four methodological queries

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This somewhat neglected paper, "Terms," is one of the most important theoretical articles that Skinner ever wrote, and his arguments are as worthy of attention today as they were in 1945. The paper is Skinner at his consistent best (or worst, for nonbehaviorists) and this friendly critic puts four questions to the author:

1. In his initial definition, legitimate (cognitive) operations are "the logical and mathematical steps that intervene between earlier and later statements, *and . . . nothing else.*" Are these confined to deductive (algorithmic) steps? And even if the mathematics is like that, is its embedding interpretive text reductive, all such "intervening" (theoretical) terms being explicitly defined by means of stimulus, response, and S–R dispositions? If a looser, conjectural relation – as in normal scientific theorizing about postulated entities – is allowed, just what does this kind of behaviorism forbid?

2. Skinner's brilliant analysis of why verbal operants reporting inner events are imprecise shows why the introspectionist program degenerated. If the discriminations and shapings had been precise, so that a high degree of reproducibility existed in the domain of self-report about inner events, what then would have been the thrust of the behaviorist thesis? If most verbal accounts by naive subjects concerning inner events had the high predictability and order of, say, a naive sophomore's lab report on his negative afterimage, would behaviorism have been a significant methodological proposal? Now of course it was the way it was; but the contemporary cognitive psychologist, whether experimental or clinical, will argue that certain subdivisions of that subject matter do have the scientific reproducibility of the negative afterimage, and that, given Skinner's analysis, there is no good methodological reason to exclude them. That puts us on a slippery slope, because reproducibility, consistency, clarity, and the like are matters of degree. More complicated properties of the visual field less replicable than, say, shadow caster experiments, or "fuzzy" clinical events, like the Isakower phenomenon (Hinsie & Campbell 1970, p. 334) in psychoanalytic therapy (uncanny sensations of equilibrium and space, unclear objects rotating or rhythmically approaching and receding, crescendo-decrescendo sensations localized in mouth, skin, and hands), might have enough consistency, as rough but complex patterns, to be admissible. It is not clear what Skinner can say as a matter of *principle* rather than a matter of *varying degrees of reliability* against such "subjective" reports. But does he want to? Intimately associated with that problem is the question of how much inner structure is to be attributed to such an entity as a visual image when it appears to play the same role that an external stimulus does with respect to the verbal operant describing it. Consider the eidetiker who cannot tell us how many teeth the crocodile had in the picture we showed him earlier but who can, on request, "call up the

image" and then proceed to count the teeth off his crocodile image and get it right. [See also Haber: "The Impending Demise of the Icon" *BBS* 6(1) 1983.] I can imagine Skinner saying here, "Well, but we do not *have* to say that there is an image which . . . ." a locution recurring frequently in his writings. That brings me back to my first question about operations, because the fact that we do not *have* to speak a certain way about an inner event, that the behavioral data do not *coerce* us to say that, is of course not equivalent to saying that it is unreasonable to say it, or that it wouldn't be good scientific strategy to allow ourselves to say such things. Inductive (ampliative) inference about the empirical world is just not the same as strict deduction, and it is not a fatal objection to a theoretical concept's introduction to point out that no observational datum compels you to infer it.

3. Can state variables like emotions and drives (postulation of which was beautifully justified in Skinner's 1938 book despite his subsequent distaste for them) play the role of private stimuli? As I understand his position they cannot, but the model as presented in "Terms" is that of discriminative stimulus, and the examples used (like toothache) make it easy to think of them as stimuli. Does that mean that we do not believe that people, having acquired language, should be able to report on inner states if these lack the usual "stimulus" properties, such as a structure, reference to a sensory modality, or being "events" rather than "states"?

4. Why does Skinner want to reduce the logical and epistemological concepts of truth and validity to behaviorese? It is not necessary for the coherency of his position, and it gets him into trouble with the logicians. We do not reduce the *concepts* of geometry, analysis, or number theory to the behavior of mathematicians, and in fact we could not operate in these disciplines if we did because our knowledge of mathematical behavior is too primitive, as I'm sure Skinner would agree. Why, then, is it necessary to behaviorize logic? Deducibility as norm – distinguished from inference as (psychological) fact, as an empirical transition in discourse – is part of mathematics, and of logic. Suppose no mathematician succeeds in proving Goldbach's Conjecture (every even number is a sum of two primes) before the sun burns out. Nobody will have been reinforced for emitting such a valid chain of mathematical operants. Does Skinner want to say that in that case the Goldbach Conjecture would be neither true nor false? Logic and mathematics being more advanced and rigorous than the science of behavior, isn't it an undoable (and needless) task to reduce the former to the latter? Similarly, if a rat that is suddenly shifted from continuous reinforcement to a fixed ratio schedule requiring 192 responses per food pellet starves, the truth of the matter is that the pellets are objectively available, whatever the rat knows or does. The objective truth of the proposition "food available" does not depend on the rat's behaving and being reinforced. Why should it depend on the psychologist's asserting it? As Skinner's radical behaviorism differs from "methodological behaviorism" partly in its consistently physicalist ontology, his insistence on psychologizing all concepts of logic and epistemology is puzzling and, I suggest, not defensible.

On Skinner's radical operationism

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Professor Skinner's contribution to the 1945 Symposium on Operationism is a landmark paper in the development of behavioristic epistemology and philosophy of science. During the decade immediately preceding the Second World War, logical positivism and operationism as interpreted by Stevens (1939), Boring (1936), and Bergmann and Spence (1941) had established in psychology an intellectual position that Skinner terms "methodological behaviorism." According to methodological behaviorists, science should be restricted to publicly observable, intersubjectively verifiable phenomena. As Skinner acknowledges, this restriction was not without some virtue, but the problem was that methodological behaviorists nearly always conceded the existence and importance of mental events as distinct from physical or behavioral events at the same time that they ruled mental events out of scientific consideration. This practice was perhaps most conspicuous in the "science of science," when scientists analyzed their own scientific behavior. Scientists simply took it for granted that mental events taking place in "immediate experience" constituted the essential basis for science; the issue was how to deal respectably with the events from the mental dimension. In brief, operationism came to imply the symbolic representation of the scientist's mental events by means of a set of measurements, so that agreement could be reached about the concepts involved. Accordingly, operationism became the cornerstone of the new scientific epistemology.

As certain passages in "Terms" indicate, Skinner had clearly had enough of this interpretation and the mentalism upon which it was predicated. The symposium offered a formal opportunity to challenge the conventional practices, and challenge them he did. The article itself mixes Skinner's critical assessment of conventional practices with his revolutionary, constructive proposals derived from the behavioristic perspective. Running throughout his critique is the attack upon the mentalistic, if not dualistic, bifurcation of nature into physical and nonphysical (i.e. "mental") ontological realms. Thus, perhaps the most central of his criticisms is that the conventional interpretation of operationism implicitly assumes that the scientists' language is a logical activity, taking place in some other dimension, which is related in some causal way to a nonphysical copy – imperfect, transformed, or otherwise – of reality called immediate experience. Why was it supposed that there were two dimensions? As Skinner asked later, Who sees the copy in the other dimension? Moreover, if meaning in language is essentially a referential or symbolic activity that links entities, concepts, or categories from immediate experience with reality, what is the origin of the entities, concepts, and categories in the first place? Where do they come from? Do they come from the pineal gland, Broca's area, or an Apperceptive Mass? Are they learned? If so, what processes are involved in their acquisition? What terms apply to the analysis of this activity, those from the presumed mental dimension or those from the physical dimension?

A second criticism, following from the first, concerns the general conception of human beings with regard to matters of epistemology. Given that behavioral matters are physical matters, and physical matters are observable, does it follow that something unobserved is something unobservable, that unobservable implies nonphysical, and nonphysical further implies mental, which in turn means that the whole business has to be dealt with in a different way by science, if science can deal with it at all? Skinner's argument, in "Terms" and subsequently, is that although private events aren't "observed" by more than one person, they need not be construed as nonphysical, that is, as mental, such that they need be dealt with in a special way. Thus, they are indeed amenable to scientific analysis. Moreover, private events have no special causal status; in particular, they do not produce knowledge. Rather, they are behavioral matters. From this perspective, truth follows from a consideration of pragmatic utility in behavior, rather than from a consideration of public status vis-à-vis private status.

A third criticism is that by failing to speak plausibly of private events and embracing instead every variety of explanatory fiction, one is in fact operating counterproductively. One is insulating private events from analysis by assuming that they are actually ineffable and therefore not amenable to scientific analysis. Thus, most methodological operationists assume an ironic

posture: They implicitly acknowledge private events as causal, if only for themselves, but then they state their laws only in terms of publicly observable variables. In effect, methodological operationists regard introspective reports of their own immediate experience as incorrigible, but at the same time mistrust introspective reports of their subjects' immediate experience, a curious inconsistency at best.

The major portion of "Terms" is in fact constructive and concerns how private events can be approached from the fresh perspective provided by a behavioral viewpoint. Of course such private phenomena as descriptions of toothaches, images, and thinking must be accommodated in any adequate science of behavior, but that assertion doesn't mean that some measurement must be taken to symbolize what the scientist is talking about. Rather, private events have to do with the discriminative control by private stimuli over subsequent operant behavior, generally verbal behavior. As is stated in another section of the original symposium, Skinner was indeed filled with his unwritten book – Skinner's contribution was extracted from the work that was to become *Verbal Behavior* (1957). Private events may therefore be approached from that direction. How do private stimuli gain control given the problem of privacy? Skinner notes that they are present when the verbal community differentially reinforces responses on the basis of public stimuli (ways 1 and 2, and, through generalization, way 4), or that they supply a weaker form of the same stimulation as does the public response (way 3). Thus, Skinner was perfectly willing to talk about the relation between covert phenomena and verbal behavior, but he was unwilling to grant the mentalistic premises (a) that anyone's language, including the scientist's, was essentially descriptive of private, mental entities or logical relations among them, or (b) that the causal analysis of behavior essentially involved specifying the nature of any affective or effective, prebehavioral neurophysiological activity that occurred when organisms came into contact with their environment. The subjective verbal report and the process by which covert behavior exercises discriminative control over subsequent operant behavior must be dealt with, but these two processes are the ones that need to be assessed in connection with the relation between private phenomena and language. In particular, the whole business of language as logical symbols describing the contents of immediate experience was simply the wrong way to go. Boring should have been frightened; Skinner was rejecting his entire world view.

Now, both Skinner and a methodological behaviorist might agree that one can't scientifically analyze a "mental event," but the bases for their positions are entirely different. Skinner would say that "mental events" are explanatory fictions – neural, psychic, or conceptual creations empowered with precisely the characteristics necessary to explain what needs to be explained. Skinner calls instead for some assessment of what the person is talking about when talking about images and the like, not so that some measurement can be taken, but so that the controlling contingencies can be examined, if only by the single person involved. In contrast, the methodological behaviorist declines to comment on mental events, but for another reason: They aren't intersubjectively verifiable. One can have a science only about things that can be agreed upon, for example, by being measured. One must specify what measured behavior serves as the index for and gives evidence of the operation of the underlying mental event. It follows that all sorts of nonsense can be pursued under such a program, and Skinner felt obliged to repudiate the position. Thus, to call Skinner "a practising operationist," as does Boring, requires considerable clarification as to what kind of operationism Skinner was practising. Skinner's repeated emphasis on the observability of behavioral processes should certainly not be taken to mean that he endorsed the practice of reifying the "mental" in terms of the "physical" through taking measurements, which is the all-too-frequent but erroneous interpretation of Skinner's opera-

tionism. In fact, it is to just that interpretation that Skinner has spent much of his professional life objecting.

"Terms" is now over 35 years old, and its message is as timely today as then. In a way, its continued timeliness is tragic, because it means that despite the availability of this remarkable article for those 35 years, we have failed to act upon its message as we should and move forward. Perhaps the most appropriate step to take at this point is finally to implement the operational program as Skinner envisioned it, on the basis of a functional analysis of verbal behavior. To do so requires in part the recognition that the explanatory verbal behavior of scientists be dealt with at a single level of observation, rather than as an indicant of things going on somewhere else, in some other dimension, to be described, if at all, in different terms. Whether scientists will see the mentalism inherent in their ways, given that they have not done so for the preceding 35 years, is questionable.

Logic, reference, and mentalism

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While there is much in this paper that seems to me entirely right, I shall confine my discussion to three points where in my view Skinner has got it wrong.

Logic. Skinner draws a distinction between "logical theories of reference" on the one hand and an account of reference based on a "scientific analysis of verbal behavior" on the other, and envisages that the latter will ultimately supersede the former.

Although it is difficult to be certain what Skinner is actually saying in these passages, he seems to think that the only arguments recognised as valid by logicians are those that conform to the explicitly stated rules of an existing logical calculus. In fact logicians are well aware that human beings who have never heard of logic or still less of a logical calculus have been giving valid agruments in support of their conclusions and detecting fallacies in the arguments of others long before the first treatise on logic was ever written.

Reasoning in accordance with the principles of logic, like all verbal skills, is, as Skinner himself (1969a, chap. 6) puts it, "contingency shaped" rather than "rule governed" behaviour. The principles of logic formulated by the logician are an abstraction from the intuitive contingency-shaped inferential practice of thinkers, not a set of verbally formulated rules which the thinker is obliged to follow if he is to reason correctly. [See also Cohen: "Can Human Irrationality Be Experimentally Demonstrated" *BBS* 4(3) 1981.]

The logician's concern is to give formal expression to the principles whereby we relate the truth value of one statement to the truth value of another. It is therefore a reasonable criticism of the accounts of language and its meaning given by logicians that they concentrate on those aspects of an indicative sentence and its utterance that determine its truth value and ignore imperatives and interrogatives (Skinner's "mands") where the concept of "truth value" has no obvious application. However, to talk, as Skinner does, as if questions of truth value are irrelevant from the standpoint of an empirical science of verbal behaviour is equally unbalanced.

As I have suggested elsewhere (Place 1981b) Skinner's cavalier attitude towards truth in his account of verbal behaviour (Skinner 1957) stems from his preoccupation with verbal behaviour from the standpoint of the speaker whose interest, *qua* speaker, lies in the effectiveness of verbal behaviour as a device for manipulating the behaviour of the listener. He ignores the standpoint of the listener from whom the truth value and hence the reliability of what is communicated by others is of vital concern.

Reference. The effect of Skinner's preoccupation with verbal behaviour viewed from the standpoint of the speaker to the exclusion of that of the listener is also apparent in the account of reference which he offers as an empirical scientific alternative to "logical theories of reference." This leads him to concentrate on the case in which the speaker names an object when confronted by an instance of objects of that kind as his paradigm case of the referring function of verbal behaviour, whereas the problem of reference, when viewed from the standpoint of the listener, is the problem of how verbal behaviour emitted by the speaker can prepare a listener to encounter a situation that is not only *not* impinging on his sense organs at the time, but never has done in that precise form in the past. Reference is not, as Skinner supposes, a matter of the stimulus control exercised by nonverbal stimuli over the verbal behaviour of the speaker. It is a matter of the stimulus control exercised by verbal behaviour emitted by the speaker over the verbal and nonverbal behaviour of the listener.

Mentalism. As Skinner conceives it, the problem about our ordinary psychological vocabulary is that the controlling stimuli to which, on his account, these words refer are accessible only to the individual to whom the words in question apply. For him "being in pain" is the paradigm case of a psychological expression. What he fails to appreciate is that "being in pain" is one of a very small number of expressions in our very extensive psychological vocabulary whose primary use is indeed in the context of first-person sentences that report the occurrence of a private event of which the listener would not otherwise be aware. As Ryle (1949) points out, the majority of the psychological terms we use in everyday life occur primarily in the context of the third-person sentences that we use to describe, explain, and predict the public behaviour of other people, especially verbs like "knowing," "believing," "thinking," "wanting," and "intending," which comprise what behaviourists like Skinner dismiss as "mentalistic" explanations. To say of someone that he knows, believes, or thinks that so and so is the case, that he wants or intends to do something is not to assert the occurrence of a private event or indeed the existence of a private mental state, it is simply to say something about what the individual in question could or would publicly say and do if certain broadly specifiable contingencies were to arise. More recent work (Place 1981a) on the intensionality of the grammatical objects of these psychological verbs suggests that what we are dealing with here is a device whereby the individual's behavioural dispositions are specified in terms of how he would describe the situation and his objectives with respect to it. This in turn suggests that the use of mentalistic terms in the explanation of behaviour involves the assumption that the behaviour in question is governed by a verbal formula or "rule" that "specifies" the contingencies involved (Skinner 1969a, pp. 146–52) and hence that the use of such explanations for scientific purposes is not, as Skinner believes, objectionable in every case, but only insofar as this assumption of a consistent rational and causal connection between what is said and what is otherwise done fails to hold.

Mental, yes. Private, no.

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Skinner's most valuable contribution to psychology (so far) is the concept of the *operant.* This concept, pursued consistently, provides a psychology of the whole organism independent of physiology, neurology, endocrinology, and the like. There is no room in such a psychology for consideration of private, internal events.

An operant is a class of behavior defined by its consequences rather than by its antecedents. Thus, a rat's bar press as an operant may be defined in terms of the closure of a microswitch but not in terms of the neural events inside the rat that precede and, in a physiological sense, cause and control the bar press. Such internal physiological events undoubtedly occur, but they are irrelevant to operant conditioning; the history of reinforcement of the bar press is both necessary and sufficient to explain (i.e. predict and control) bar presses.

The other behavioral class in Skinner's science of behavior is the class of *respondents.* A respondent is indeed defined according to its antecedents. But these antecedents must be external. Otherwise, one could consider a rat's bar press, controlled as it must be by internal physiological events, to be a respondent. If no external stimuli are found that reliably elicit a response such as a rat's bar press, Skinner does not ask you to look for stimuli inside the rat. It is always possible to discover or invent such stimuli. That is the path that Watson and Hull took (and on which they lost their way). To look inside the rat for the cause of a bar press is to assume that the bar press is a respondent (and to abandon the search for the cause of the bar press in the contingencies of its reinforcement). Skinner, instead, considers a response with no apparent eliciting stimulus to be an operant which may be more or less manipulable by contingencies of reinforcement.

It is inconsistent with this notion of the operant to say, as Skinner does in "Terms," that a toothache is a private event. In a (truly) Skinnerian science of psychology, a toothache must be a respondent or an operant (or some combination of the two). If the stimulus is considered to be the diseased tooth and the diseased tooth is supposed to be part of the person who has the toothache, then the toothache is an operant and consists of the class of *overt* behavior to which the label "toothache" is given. Alternatively, for the sake of analysis, one may want to consider the diseased tooth apart from the person with the toothache. In that case the toothache may be a respondent consisting of whatever behavior is elicited by that tooth (as determined by laws of the reflex). The operant toothache may well consist of a different, even nonoverlapping, class of behavior from the respondent toothache. In either case, however, the toothache is overt, public behavior.

In the case of thoughts, feelings, and other mental events, there is usually no apparent objective cause like a tooth that may be alternately considered inside or outside the organism. There is (usually) no apparent external antecedent stimulus that can be said (by the laws of the reflex) to elicit these mental events. Such events are thus operants – overt actions controlled by their consequences. Nothing in "Terms," nothing Skinner has written, and nothing in nature contradicts this idea. The main difference between a rat's hope and a rat's bar press is not that one is private and internal (even partially) and the other is public and external. Both are wholly external and (at least potentially) public, but one takes longer to occur than the other.

In "Terms" Skinner suggests that mental terms are used in ordinary speech to refer to private events and that, because it is so difficult for the verbal community to control such events, any analysis of mental terms as operants and respondents would be strained at best and ultimately futile. But Skinner gives unnecessary ground to his critics by this suggestion. As he indicates, in teaching people to use the mentalistic vocabulary, it must be overt behavior that society observes and then rewards or punishes. It would seem to follow that a person who uses that vocabulary to refer to private events must be using it incorrectly. Thus, a boy who says he is hungry just after he has eaten a big meal is either ignored or punished. Hunger pangs are not relevant here. In general, the use of mental or emotional terms without (eventual) support by overt behavior ("I love you," being perhaps the most egregious example of such use) is frowned upon. When we use those terms we are in much the

same position as the boy who cried wolf. People will respond only so many times without confirmation. And it is not to private, but to public events that they look for confirmation.

It would seem to be an important task for psychology to determine what the (overt behavioral) criteria are for the use of mental terms, how they change with circumstances, how they interact with one another. Before doing this job, it may be necessary to widen the conception of the operant, as originally advanced by Skinner, from a single discrete event (such as a lever press) to a complex pattern of events that may occur over days or weeks and (consequently) to alter the notion of reinforcement from contiguity between a pair of discrete events (response and reward) to more complex correlations that have meaning only over an extended period (see Commons, Herrnstein & Rachlin 1982). When the important variables of such molar behaviorism are discovered, the mentalistic vocabulary will, I believe, come nicely to hand.

To the extent that mental terms refer to the overt behavioral context of immediate behavior it is possible to use them in a behavioral science. To the extent that mental terms refer to the covert or internal context of immediate behavior they have no place in behavioral science, because such use of mental terms converts observable operants into hypothetical respondents.

B. F. Skinner's operationism

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"Terms" represents a brilliant and powerful innovation in the development of behaviorism. The paper presents Skinner's conception of operationism and outlines a framework and set of problems for a radical behaviorist analysis of verbal behavior. Skinner (1957) develops the program further.

Skinner's operationism is quite different from the operationism of the logical positivists (Hempel 1965b; 1965c). Skinner rejects the aim of providing complete, explicit (behavioristic) definitions of (psychological) terms from ordinary language. He also rejects any form of operationism that requires a statement of logically necessary or sufficient conditions for the correct application of technical scientific terms. Like the positivists, Skinner does acknowledge the influence of Mach (1919) and Bridgman (1928), and he clearly draws the term operational definition from the latter. Unlike the positivists, Skinner limits himself to endorsing Mach's historical method and the procedure Bridgman ascribes to himself, namely, observing what people (e.g. scientists) do with the terms they use. As construed by Skinner, Bridgman's procedure makes the task of the logician and philosopher of science a task for psychology. The type of "psychological" investigation Skinner proposes is an experimental analysis of the contingencies of reinforcement under which those verbal *responses* ordinarily classified as verbal *reports* are acquired and maintained. Skinner's operationism is, thus, one part of the radical behaviorist program for the experimental analysis of verbal behavior.

Skinner explicitly requires that his operationism solve the problem of explaining how verbal responses are brought under the control of private stimuli (i.e. stimuli that only the responder can discriminate and respond to). This requirement marks a distinction between radical behaviorism and methodological behaviorism, since methodological behaviorism presupposes that private stimulation lies outside the realm of scientific investigation.

The program Skinner proposes escapes the standard objections to methodological behaviorism and the operationism of the logical positivists (contra Boden 1972; Koch 1964; Scriven 1956).

In addition, the program provides principled reasons for behaviorists' long-standing suspicion of scientific use of commonsense psychological terms and for the behaviorist conclusion that introspection is an inappropriate method of investigation in science.

Serious attempts to evaluate Skinner's program must begin with a clear appreciation of how radical a program it is. Like his contemporary Quine (1960), Skinner rejects the use of the "intentional idiom" in scientific descriptions and explanations of verbal behavior. For example, verbal behavior ordinarily classified as first-person reports of concurrent psychological states (e.g. "My tooth aches," "I am depressed.") are not to be treated as reports or statements at all, much less as reports or statements that are accurate, reliable, true, or correct. (For discussion of the difference this makes see Ringen 1977; 1981.) Explanations of these verbal responses are to be given in terms of the contingencies of reinforcement by which they are shaped and maintained. Explanatory reference to meanings, intentions, or psychological states of the speaker is prohibited.

Recent work in the history and philosophy of science (e.g. Kuhn 1962) has emphasized that the more radically the commitments embodied in a given research program diverge from those of whoever attempts to assess it, the greater the difficulties objective assessment presents. For all of us whose customary ways of speaking and thinking embody western cultural traditions, considerable difficulty attends objective assessment of Skinner's program. The intentional idiom, which Quine and Skinner proscribe, constitutes an absolutely fundamental feature of our customary ways of describing and explaining all human action, and especially action that involves language. It is hard to imagine anything more radical or revolutionary than the attempt to describe and explain human verbal behavior without the concepts the intentional idiom embodies. Indeed, without this idiom it is difficult to find anything coherent to say about verbal behavior. [See also Dennett: "Intentional Systems in Cognitive Ethology" *BBS* 6(3) 1983.]

When faced with such difficulties, it is only prudent to ask whether there is any reason to pursue Skinner's program or even to make the considerable effort required to understand what the program involves. It is instructive to reflect on the reasons Skinner suggests. Quite clearly his reasons do not include a commitment to the operationism logical positivists recommend. Rather, Skinner's own statements (e.g. 1931; 1959) suggest that his rejection of the intentional idiom derives from two sources: an interpretation of the history of science according to which scientific progress occurs only after anthropomorphic conceptions have been rejected, and suspicion that reference to psychological states will be problematic in putative explanations of behavior because these states are not identified independently of the behavior or functional relations they are to explain. Evidence of successful development of the program aside, Skinner's commitment to operationism is linked to its promise in eliminating anthropomorphism and explanatory vacuity from a scientific study of behavior.

Chomsky (1959) and others provide considerable reason for Skinner to be concerned about explanatory vacuity in existing radical behaviorist accounts of verbal behavior. (Major criticisms are directed at explanatory references to unobserved covert behavior – "Terms" – as providing stimuli for verbal responses – see point 3 in "Terms" – and to unspecified dimensions of generalization in accounts of responses occurring under public stimulus conditions which differ from those under which the response has previously been conditioned – see point 4.) Hence, there is reason to conclude that Skinner's operationism has not, in fact, served one of the functions it was designed to serve. In addition, strong arguments have been given (e.g. Hempel 1965a; Taylor 1964; Woodfield 1976; Wright 1976) that explanatory use of concepts embodied in the intentional idiom need not be vacuous in any sense that concerns

Skinner. Thus, we are free to wonder whether anthropomorphism really is misplaced in a scientific study of human (verbal) behavior. Whether it is misplaced or not can be determined only by comparing the results of serious attempts to provide a scientific analysis of behavior without the use of concepts embodied in the intentional idiom with the results of attempts in which those concepts occur essentially. The radical behaviorism of Skinner and the contemporary cognitivism inspired by Chomsky provide an opportunity for such a comparison. Both programs have been defended and elaborated in work subsequent to Chomsky's (1959) well-known critique of Skinner (1957). Quine (1970), MacCorquodale (1970), Fodor, Bever, and Garrett (1974), and Winokur (1976) provide a place to begin comparing the results of pursuing the programs. Lacey (1974) provides some useful guidance.

There is more than one way to access an image

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For Professor Skinner, science depends on operationalism. He argues that private stimuli cannot be operationally defined; only the verbal response to a private stimulus can be so defined. One could dismiss this argument as outdated, since mainstream psychology abandoned its obsession with operationalism in the 1940s and has since migrated toward the philosophy of critical realism. However, to disregard "Terms" on this basis would be to miss some of the compelling differences between modern behaviorism and cognitive psychology that are relevant today.

The most important issue that Skinner addresses is the question of how and why people respond to private stimuli. This is indeed one of the current concerns of experimental psychology. There is a search for the nature of internal representations (private stimuli) and cognitive processes (private events). Skinner predates, and is in agreement with, some contemporary arguments that it is impossible to know the nature of an internal representation (Anderson 1978; Palmer 1978). However, current controversy is based on mathematical analyses and pertains only to internal representations in isolation and not to the processes (one could call them behaviors) that operate upon them.

Skinner believes that "internal representations" and "mental processes" are fictions, yet private stimuli and private events are not. He agrees that there are "images" but disagrees that they can be studied. His basic premise is that we can only study a verbal response like "red" in the context of a history of verbal responses to some public red. We cannot study the private stimulus to which it may refer. In other words, we cannot find the reference to "red" in the internal event (except physiologically, which is not relevant to the issue), so we must find it in the contingencies of reinforcement that correlate with, or, as Skinner would say, control the verbal response.

This line of thought can be extended to any response that is symbolic of the private stimulus red. If subjects were asked to press a key whenever they imagined the word red, the evaluation of the response would not lie in inferences of processing strategies and comparative analyses of internal representations. It would, rather, be possible to examine only the contingencies of reinforcement that lead to the key response. In this case the key response and the verbal report "red" presumably refer to the same stimulus – the color red. If we compared the verbal report "red" to the manual key response in the same experiment, I suspect we would find that the conditions under which the key response and the verbal response were emitted would

be the same. For one moment, let's accept Skinner's operationalism and analyze the history of reinforcement contingencies for saying "red" and of reinforcement contingencies for pressing keys. It is probable that the two histories would be very different (except by the greatest coincidence), yet it is clear that the responses have the same referent. It is not the functional analysis of key pressing and verbally saying "red" that will reveal how the same referent can result in two diverse responses (responses that have different reinforcement histories). Rather, the question is how reorganization (an internal process) occurs to form a new relationship between a referent and a response. Knowing how verbal reports to private stimuli are shaped does not answer this question.

A second, somewhat related, problem is that parametric variations in the response seem to be of little importance in "Terms." The verbal response "red" may be said with greater intensity and more rapidly when a traffic signal turns red than when one is asked the color of a dress. Contingencies of reinforcement could presumably explain a part of the differences in intensity, since the effect of ignoring a red light may be much greater than that of ignoring the color of clothing. Reinforcement contingencies, however, are not sufficient to explain all the factors contributing to parametric variations in response patterns.

When Shepard and Metzler (1971) presented two figures in different orientations and found that reaction time increased linearly with the degree of difference, the contingencies of reinforcement that contribute to faster and slower responses are not obviously relevant. Shepard interpreted these data in terms of images and internal referents, and his subjects verbally reported the experience of "seeing" a rotating image. It is true that Shepard may be wrong about the nature of the private event, just as a behaviorist could be wrong about the contingencies of reinforcement that contribute to the response. Yet, as I understand Skinner's view, we would have to regard the differences in reaction time in Shepard and Metzler's study as responses that must be analyzed in themselves. It appears that Skinner would deemphasize the reaction-time data and analyze the contingencies of reinforcement that "control" reporting the experience of having an image, including the reference to images by Shepard and Metzler and the rest of the scientific community.

This approach leads Skinner to argue that the verbal reports of scientists should be analyzed in the same way as their subjects' verbal responses. It is an interesting question how words function in the thinking and behavior of scientists. Skinner's orientation, however, leads to an infinite regress.

Suppose we decide to define operationally the concept "red" according to Skinner's recipe of operationalism. We seek the contingencies of reinforcement that have shaped the verbal report "red," and we look at the contingencies in the present use of the word "red." As noted above, a person may yell "red" when the driver of a car is about to run a traffic signal, and say "red" more softly when commenting on the color of a dress. Privately, the two verbal reports of "red" refer to two very different meanings. So we must operationalize two instances of "red," the intense verbal report of red and the less intense verbal report of red. Now we have a new task – to define intensity operationally. According to Skinner, "intensity" consists of the conditions under which the word "intensity" is used. Thus, the use of the phrase "intense red" now is the verbal report of the person who defines intensity. We have louder "red" and softer "red" referring to the contingencies of reinforcement surrounding the response "intense" combined with the contingencies of reinforcement surrounding the response "red." We now need a verbal report of the person who is reporting the difference between these two responses. This verbal report, in turn, needs analysis in the form of another verbal report. Something is surely amiss.

B. F. Skinner's theorizing

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In 1938 (the year of *The Behavior of Organisms*) B. F. Skinner
began developing a technology of behavior. He has worked at it
over the years. His achievement has been awesome, inspiring:
It has yet to be rivaled.

But even the best of technologists, and the best of engineers,
can succumb to a lust for philosophic theorizing, and Skinner
has been no exception.

By 1945 ("Terms") Skinner had other things in mind beside
technology. Though he would talk (albeit in passing) about this
technology and our need for it, Skinner had become more and
more concerned with theorizing. He proceeded from describing
operant behavior and how to shape it all the way to theorizing
about every feature of human life willy nilly, behavioral or not.
By 1974 (*About Behaviorism*) Skinner was openly pursuing an
elusive weltanschauung: Philosophy had replaced technology.
With fast talk from a strategic armchair, Skinner extended his
theory of behavior by definition and redefinition, rather than by
experiment.

Consider, for example, what has happened to Skinner's con-
ception of behavior over the years. In 1938 it was clear and in
line with his practical aim: immediate, overt, and observable
behavior was the relevant datum to describe and control. There
was no need to explain or deny the existence of other forms of
behavior, let alone mental states, events, or processes. But by
1974 that conception had been expanded beyond all belief: Any
sort of matter became behavioral in all sorts of ways; so knowing
that something is so became a form of behavior, and so did
thinking a thought. How could these count as immediate, overt,
and observable? A new label was created: "covert behavior."
Covert behavior is minuscule and after the fact; in truth, is it
behavior at all? And scurrying along with covert behavior, in
About Behaviorism, came current behavior, probable behavior,
perceptual behavior, past behavior, future behavior, and, cer-
tainly, whatever behavior was needed to fill the bill of a tech-
nological bird fishing for philosophic frissons in Plato's wordy
meander.

When reading Skinner, one must ask oneself, Is this the
technologist or a philosopher speaking? Early on he is almost
exclusively the first; by 1974 he is the second. The first is more
intriguing than the second, and so are his position and its value.
It is a technology, and its value is that of a technology – a way of
changing the ways in which humans (and nonhumans) behave;
to have these means available, Skinner needed only modest
means – his unvarnished definition of behavior and his notions
of operant and respondent conditioning. If these means were to
need supplementation, the reasons would be technological: The
results, being unsatisfactory, could only be aberrant. In
"Terms" Skinner, perhaps in passing, says the only criterion for
the utility of a notion is whether it helps one get anywhere in
controlling things. That is the great technological Skinner
speaking, and espousing the criterion of a technology. What
replaces it, or supplants it, when the philosopher king speaks?
Large gestures about science and what is prescientific; there are
motions made to scientific revolutions in physics, break-
throughs here and there, and how all the dross – the phlogiston
and ether and élan vital – has gone by the boards. Somewhere in
all this there is supposed to be a lesson for psychology, but the
lesson is lost at the level of slogans we can all agree to: Do we all
agree to accept no explanatory fictions? How do we now tell a
fiction from a fact, a decoy from a duck? When Skinner was a
behavioral engineer, he knew what his criterion was: utility. In
his philosophic period, which seems to have afflicted him even
as early as 1945, Skinner lacked a criterion for discriminating

between psychological phlogiston and the daydreams of cog-
nitive psychologists. He gestures and promises and displays
high ideals, which serves merely to turn behaviorism into a
posture – defiant and quixotic.

Some will wonder at Skinner's "operational" definitions. We
wonder at the attempt. Why did he feel the need?

Even a genius can be seduced by philosophy.

A behavioral theory of mind?

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How timely it is to reread "The Operational Analysis of Psycho-
logical Terms," a remarkable gem of Skinner's prodigious out-
put of seminal publications. Especially during this age of
cognitive psychology, many readers may be surprised to dis-
cover Skinner's idiosyncratic but carefully reasoned analysis of
"private events." They may be equally surprised by the unusual
metaphysical and epistemological positions that Skinner as-
sumed in his first detailed treatment of mentalism.

The uninviting and misleading title of this important article
has undoubtedly contributed to its neglect. Instead of revealing
Skinner's distaste for operationism, it suggests yet another arid
exercise in deriving operational definitions of psychological
phenomena. It also seems likely that the more alluring titles of
some of Skinner's other well known articles, such as "Are
Theories of Learning Necessary?" and "Why I Am Not a Cog-
nitive Psychologist," have led many psychologists to conclude
that Skinner is antitheoretical and that he denies the existence of
mental events.

The truth of the matter is that Skinner has a theory of
behavior, that he acknowledges the existence of an inner mental
life, but that he also argues forcefully against the Cartesian
dualism implied by traditional (operational) definitions of cog-
nitive phenomena (see Terrace 1970). In short, Skinner's 1945
classic is an appeal to psychologists to regard thoughts, beliefs,
perceptions, memories, feelings, and so on, as bona fide subject
matter for psychology, a subject matter that, from Skinner's
point of view, obeys the same laws as those that govern overt
behavior.

It is important to recognize that Skinner's penetrating analy-
sis of private events occurred well in advance of the rise of
modern cognitive psychology. It is widely recognized that the
metaphor of the computer revolutionized the study of cognition
by showing how complex processes can be conceptualized as
material phenomena that obey mechanical laws and how cog-
nitive phenomena can be studied meaningfully without reduc-
ing them to the electrical activity of the computer's hardware.
Solely on the basis of his careful analysis of behavior, Skinner
provided his own monistic alternative to the dualistic mentalism
inherent in traditional definitions of cognitive events. He also
argued convincingly that psychologists need not concern them-
selves about the locus of private events in the nervous system
(Skinner 1950). Thus, long before the paradigms of modern
cognitive psychology began to take root, Skinner insisted on a
materialistic and nonreductionistic approach to its subject
matter.

Skinner parts company with most other psychologists con-
cerned with private events by his unwillingness to regard them
as introspective givens. Statements such as "I feel or think X"
prompt Skinner to ask what variables are responsible for the
occurrence of a particular feeling or thought. That question is
seldom asked because, by their very nature, private events
seem to be insulated from external influences. Skinner nev-
ertheless maintains that the experience of a private event
presupposes public intervention, at some earlier time, by other

members of the "verbal community." According to Skinner, we "know ourselves" only because others direct our attention to what we think, feel, or do. Children, for example, learn when it is appropriate to say "I think," or "My stomach aches," or "I had a bad dream" only after listening to innumerable comments or queries such as, "You look deep in thought. Are you thinking about X?" or "Are you upset because you have a stomach ache?" or "What were you dreaming about when you woke up crying?"

Skinner's view of the ontogeny of private events is consistent with a wide range of psychological theorizing. Skinner himself reminds us of Freud's belief that it is our natural condition to be unconscious of our actions, thoughts, feelings, and so on, and that mental activity does not presuppose consciousness (see Skinner 1969a, p. 225). Piaget commented extensively about the kinds of training that his daughter needed to understand when she was thinking and that her head was the locus of her thoughts (e.g. Piaget 1929, p. 44). At least one social psychologist (Bem 1967) has noted the similarity between the logic of Skinner's analysis of how we come to know about private events and the logic of attribution theory, a theory that claims that particular kinds of social interactions determine how we describe our thoughts and feelings. It is also of interest to note that Jaynes's review of early history led him to conclude that consciousness is a relatively recent development, a development that Jaynes claims occurred after the invention of writing (Jaynes 1976). Jaynes hypothesized that, prior to the appearance of man's sense of consciousness, his language made reference only to objects and events of the external environment and that man had no vocabulary with which to refer to his mental life – or, for that matter, to himself. When, on occasion, he heard "inner voices," they were interpreted as the voices of gods or as hallucinations. Only as a result of violent upheavals did early societies develop the cultural practice of teaching their members to identify their inner thoughts and feelings and to attribute those thoughts and feelings to themselves.

Skinner's counterintuitive hypothesis about private events (that they owe their existence to the public efforts of others who teach us how to respond verbally to internal stimuli) was an effective reply to Boring, Stevens, and other like-minded operationists who argued that the study of private (and, therefore, scientifically inaccessible) events should be limited to their public manifestations. Skinner not only revealed the dualistic flaw of such operational definitions but also defined a radically new view of private events.

For a variety of reasons that view has not received the attention it deserves. One problem stems from some unexplored ramifications of Skinner's analysis of private events. Another is Skinner's reluctance to consider private events other than those he so insightfully defined. Ironically, Skinner does not appear to have recognized that the struggle against mentalism or, more specifically, dualism, has been won. Thanks, in large part, to his own efforts, modern studies of human and animal cognition need not concern themselves with the ghost in the machine.

Before reviewing the import of recent developments in cognition, let us consider the following implications of Skinner's hypothesis about private events: (1) Private events are conscious, (2) consciousness presupposes language, and (3) only human beings experience consciousness.

Since so much of Skinner's view of consciousness hinges on the verbal labels we have been taught to apply to internal stimuli, it is important to ask whether a verbal label is a necessary or sufficient condition for consciousness. That we are conscious of unlabeled images suggests that verbal labeling is neither necessary nor sufficient (see Skinner's examples of "operant seeing," 1953, pp. 270ff.). Even if one wanted to argue that verbal labels were a necessary or a sufficient condition for consciousness, we would still need to know why we label certain internal stimuli and not others. Skinner's suggestion (1969a, pp. 157ff.) that consciousness functions to help us cope with difficult

situations, (i.e. situations in which the cause of the problem is behavior of which we are unaware) is a promising start. I doubt, however, that Skinner would argue that such situations are the only cause of consciousness.

A moment's thought should reveal why the basic objection to Skinner's explanation of consciousness is one of those he raised against mentalistic explanations in general. Skinner notes that to say that John did X because he thought Z is to beg the question, Why did John think Z? To answer that question by asserting that John thought Z because he applies verbal response Z to internal stimulus z' is to beg the question, Why the occurrence of verbal response Z?

Skinner's insistence that all mental activity be characterized as private (conscious) events, under the control of particular internal stimuli, would seem to deny the existence of unconscious private events. So extreme a position is understandable in a zeitgeist in which reference to mental processes of any sort implied a dualistic view of psychology's subject matter (in "Terms" Skinner writes that "the distinction between public and private is by no means the same as that between physical and mental)." However, Skinner's more recent publications (1974; 1977b) suggest that he has yet to acknowledge that the study of cognitive phenomena does not presuppose dualism.

Skinner also doesn't appear to recognize that much of human and animal behavior can no longer be explained by reference to the three-term contingency (a discriminative stimulus, a response, and a reinforcer) that he applied so imaginatively to a large variety of examples of human and animal behavior. A basic problem arises when organisms respond appropriately in the *absence* of any relevant environmental stimulus (see Hunter 1913; Terrace 1983a). This state of affairs has motivated the study of representations of environmental stimuli, in both human and animal subjects (e.g. Bousfield & Bousfield 1966; Bower 1972; Mandler 1967; Olton & Samuelson 1976; Roitblat 1980; Shepard 1975; Shimp 1976; Terrace 1983b). The study of representations in animals is of especial interest because of their nonverbal nature (Terrace 1982). [See also Roitblat: "The Meaning of Representation in Animal Memory" *BBS* 5(3) 1982.]

What separates Skinner from the modern study of cognitive processes is his reluctance to acknowledge that the study of representations does not imply a regression to mentalism. Indeed, the study of representations can be regarded profitably as an extension of the study of stimulus control (Terrace 1983a). Asking about the nature of a representation is simply to pose the questions, What features of an environmental stimulus are coded by the organism and how does the organism represent those features to itself when it must respond in the absence of the environmental stimulus?

An instructive example of the need to include representations of environmental stimuli in the experimental analysis of behavior can be seen in a pigeon's performance on a matching-to-sample task (Skinner 1950). In the original version of this paradigm, the pigeon was shown a sample stimulus (either red or green). A few seconds later, two choice stimuli (red and green) were added, one on each side of the sample. The subject was rewarded if and only if it selected a choice stimulus that matched the color of the sample stimulus.

Subsequent research showed that Skinner's description of the pigeon's behavior as "matching" was a misnomer. When confronted with novel samples (in conjunction with appropriate novel choices), performance fell to chance (Cumming & Berryman 1965). What the pigeons seemed to have learned was to respond to the left choice when confronted with stimulus configuration 1 and to the right choice when confronted with stimulus configuration 2, and so on.

A variety of recent studies has shown that it is possible to obtain generalization of matching-to-sample (Premack 1976; Zentall 1983). Accordingly, it is necessary to ask how one might characterize the stimulus that results in matching. It cannot be the physical identity that exists between the sample and the

choice stimuli. The experimental literature indicates that physical similarity *per se* fails as often as it succeeds in producing generalization of matching. The only alternative is to postulate some internal response, generated by the organism, which yields an internal "same" stimulus. That stimulus, in turn, leads to the correct choice. In short, successful generalization of matching must mean that the subject makes a judgment of "sameness" before responding to the correct choice. Specifically, the subject must transform the environmental stimuli provided by the experimenter into an intermediate cue that indicates which choice it should select. [See also Premack: "The Codes of Man and Beasts" *BBS* 6(1) 1983.]

The importance of taking into account the subject's contribution to the stimulus complex that results in accurate matching-to-sample performance is especially apparent when a delay is interposed between the presentation of the sample and the presentation of the choices (Grant 1983; Roberts & Kraemer 1982; Roitblat 1980). Accurate responding under those circumstances suggests that the subject has access to a representation of the sample when the choice stimuli are made available.

Skinner should be heartened by these and other demonstrations of the feasibility of studying complex processes in humans and animals from a monistic and a materialistic point of view. Rather than regard such developments as contrary to the tenets of radical behaviorism, Skinner should welcome them as significant extensions of the approach to cognitive events that he introduced in "Terms."

On the operational definition of a toothache

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Psychology was in its most formative stage in the 1930s, when the philosophy of science was in its heyday. Many of its elements are to be found in Professor Skinner's paper "Terms": the fictionalism of Mach, the physicalism and the problems of the public and the private of the Vienna Circle, and the operationism of Einstein and Bridgman. The psychologists wanted to know how a science of man was possible, and they turned to the philosophers as the only authorities they knew for guidance; for the acknowledged scientists, qua scientists, of course did not understand the principles of their own subject, no matter how skillfully they might use them. But the philosophers of science did not understand them either, and they led the psychologists up the garden path.

In "Terms" Skinner tells us that experience is "a derived construct to be understood only through an analysis of verbal . . . processes." But one always supposed that verbal processes reported experience, whether "inner" or "outer," or at least reported the content of experience, what was experienced; and if so, experience can hardly be a construct out of verbal behavior. Words themselves, we are told, are not signs or symbols used to express or convey meanings. Words are responses to stimuli resulting from reinforcement by the verbal community. In other words, *all words* are meaningless physical effects caused by specific kinds of physical stimuli – including this paper by Skinner, what I am writing now, and the various verbal *effects*, *caused* in you, the reader. If this is so, there is no meaning, no understanding, no judgment – and no science. Or shall we suppose that we are in some God-given privileged position in our investigations, possessing in ourselves faculties that we deny in those we study, like the spiritually enlightened in Plato's allegory of the cave? Well, apparently we are, right up to the end: Then, however, "we shall be able to include, and

perhaps to understand, our own verbal behavior as scientists." To do this, of course, we would have to treat our own verbal behavior as meaningful in order to prove that it wasn't. In fact, there would be nothing to explain. There *is* nothing to explain insofar as people think rationally, and since science is supposed to be, par excellence, a rational activity, there shouldn't be much to explain in it; and if it wasn't very largely rational there wouldn't be much point in listening to its explanations as to why it wasn't! Psychology must, on pain of otherwise cutting off its own head, presuppose that human discourse is very largely rational – that *it isn't caused by stimuli*. [See also Cohen: "Can Human Irrationality Be Experimentally Demonstrated" *BBS* 4(3) 1981 and Kyburg: "Rational Belief" *BBS* 6(2) 1983.]

One had supposed that the methodological and radical behaviorists agreed that science was, by definition, concerned with the publicly observable and publicly testable world, and that the real difference between them was that the former accepted and the latter denied that there was a private mental world – a difference that would appear to be of no material consequence. Skinner, however, denies this. It meant for a start that the methodological behaviorists were soft on those old explanatory fictions, consciousness, feeling, and the will, and looked for behavioral manifestations that could be given operational definitions. Of course, our intuitive concepts will not do for scientific – or indeed for philosophical – purposes. Our intuitive concepts of truth and knowledge, as Carnap (1962) pointed out, need explication. But that does not mean that they should be abandoned. Some scientific concepts have not proved very fruitful: The medieval concept of impetus (which, until it was finally dissipated like the heat in a poker, was supposed to keep a projectile in motion) was abandoned in favour of the concept of momentum; and the concept of phlogiston, one of Skinner's examples, was abandoned in favour of that of oxygen. But the concepts of electricity, heat, velocity, and so on, have simply been modified. And it was empirical science that was the judge in each case.

But Skinner is still right in rejecting the program of methodological behaviorism. Suppose for the moment that consciousness, feelings, and the will are real. Surely the manifestations in behavior of these intentional states can only be intelligibly described in terms of the intentional states themselves. If so, the program is self-defeating. The solution might be to abandon the notion that psychology cannot be a science unless it restricts its subject matter to what is publicly observable, and so to abandon behaviorism with it. Skinner, of course, does not abandon behaviorism; indeed he reaffirms his credo. But, incredibly, he drops the requirement of publicity. Or does he? He, too, he says, has a toothache, and a toothache is a private event. But it is private only in the sense that the only system that is directly "wired up" to the tooth in question is the physical system called "Skinner": the toothache is a purely physical event, just like the radioactive event that is manifested in the click in the Geiger counter. Skinner, it seems, does not suffer from toothache like ordinary mortals; he just displays the kinds of behavior one usually associates with a toothache – play acting, some would call it. [See also Searle: "Minds, Brains and Programs" *BBS* 3(3) 1980.]

What is wrong with operationism is not that no explicit statement of the relation between concept and operation has been provided. It is that the very character of the relation has been misconceived. One's actions are not defined by one's bodily movements but the reverse – in order to know what sort of operation a person is performing one must know what he is trying to achieve – one must know what velocity *is* before one can set out to measure it. Newton was well aware that there was no operation by which he could measure velocity as he understood it, that is, motion relative to God's Sensorium (I ignore his bucket experiment), and he used the "fixed" stars as a surrogate framework instead. The concept determines the operation, the operation does not define the concept. There may, of course, be

something wrong with the concept, something that a consideration of the operation determined by it may reveal. Philosophical analysis is required to reveal such deficiencies and decide where the fault lies. Unfortunately the scientist rarely has the philosophical training for the job – or the philosopher the necessary conceptual background.

Radical behaviorism and theoretical entities

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After nearly four decades, "Terms" retains its significance and its brilliance. But along with its liberating impact on behaviorist thought, it is also the source of certain ambiguities and confusions persisting to the present. I address two of these.

1. Ironically, commentators on the history of behaviorism commonly ignore the historical context of this article. "Terms" was presented as part of a symposium on operationism in psychology. Skinner was concerned with distinguishing his approach from the operationism of his Harvard colleagues S. S. Stevens and E. G. Boring. The latter was not a behaviorist, and the former only marginally so. Certainly neither was part of the behaviorist mainstream devoted to the study of conditioning and learning and to the development of a science of behavior. Yet over the next 40 years, the distinction between the position of Skinner on the one hand and the operationism of Boring and Stevens on the other hand came to be regarded as a major distinction between Skinner's "radical" behaviorism and all other forms of behaviorism. It is commonly thought that only radical behaviorism admits private events into the science of behavior, while all forms of methodological behaviorism are restricted to publicly observable entities and events. While this distinction may differentiate Skinner from Boring and Stevens, it does not distinguish him from nearly any other major behaviorist. Watson, Weiss, Tolman, Guthrie, Hull, and Spence all included private events, such as "implicit," "covert," and "incipient" responses, in their behavioral systems. Furthermore, they suggested that these unobserved events can serve as stimuli for verbal responses, including reports of emotions, pains, and images. What distinguishes Skinner from these other behaviorists is not his legitimization of private events but the fact that he provides the most coherent account of how these events come to function as stimuli for verbal behavior. Thus, contrary to common opinion, the admission of private events into a behavioral science does not distinguish radical behaviorism from other forms. In sophisticated methodological behaviorism, scientific data are derived by observation, and private events are postulated as hypothetical constructs. This hypothetical nature of private events leads to my second point.

2. In "Terms" Skinner states: "Our practice is to *infer* the private event." Similarly, he speaks of considering private events "inferentially." This implies that private stimuli, as inferred events, are theoretical entities as opposed to observables. On the other hand, Skinner (1969a, p. 242; 1974, p. 17) at times writes as if private events are observed rather than inferred because they are observed by the person in whose body they occur and whose verbal behavior they control. Contemporary researchers in behavior therapy have extrapolated this position to an extreme in some cases. Ignoring Skinner's cautionary attitude toward the reliability of reports of private events, they treat the patient's first-person reports about covert events as genuine data reports observed by a "public of one."

I believe that private events must be considered inferred entities (i.e. theoretical) for two reasons. First, if psychology is to be the "psychology of the other one" in Meyer's (1921) felicitous phrase, then even if a subject may be said to be observing a private event, the experimenter, representing the

science, must be said to infer the private event. Second, Skinner's statement that private events are discriminative stimuli for certain verbal responses is, at present, no more than a plausible hypothesis. No evidence is currently available to show that verbal responses enter into causal relationships with private events as required by the hypothesis, or that these private events are stimuli in the sense of conforming to the same laws as their overt counterparts. Therefore, the existence of private stimuli controlling verbal behavior is an inference. Even the subject's verbal reports provide no observational evidence for the hypothesis since they are in the form "I have a toothache" rather than "Private stimulus X is controlling my verbal behavior." It must be concluded that the scientific status of private stimuli is that of a hypothetical construct.

Author's Response

Coming to terms with private events

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When I was asked to participate in the symposium for which "terms" was written, I was at work on a manuscript that would be published 12 years later under the title *Verbal Behavior* (Skinner 1957). It was an interpretation of the field of language which avoided "ideas," "meanings," "information," and all the other things said to be expressed by a speaker or communicated to a listener. Although I had lost interest in the operationism of the thirties, I still called myself an operationist and thought that certain parts of the manuscript were suitable for the symposium. They concerned the place of private events in the analysis of verbal behavior, in particular the privacy of "sensations" and "feelings," which were still important to psychologists of the time, particularly E. G. Boring, who had organized the symposium.

In traditional terms the question I addressed was this: How is it possible to learn to refer to or describe (and I would say hence know) things or events within our own bodies to which our teachers do not have access? How can they tell us that we are right or wrong when we describe them?

I used as an example a special type of verbal response called (in my manuscript) the *tact*. It will be important in what follows to define this term here as clearly as possible. It refers to the probability of occurrence of a verbal response (say, *chair*) as it is affected by a stimulus (say, a chair or chairlike object). At any given moment a native speaker of English possesses the response *chair* in some strength (where "in strength" means "with a given probability of emission"). During a quiet walk in the woods it is weak. In a furniture store, it is strong, even though not being actually emitted.

The response *chair* in its relation to a chair as a controlling stimulus is a tact (and the chair is then said to be tacted); it is *not* a "reference to a chair," or a "statement about chairs," nor does it "express the idea of a chair," or "denote a chair," or "name a chair." It is simply

a probability of emission of *chair* as a function of a particular kind of stimulus. Tacts sometimes occur alone, but they are usually parts of larger samples of verbal behavior. They can be, but need not be, explicitly taught, as when a child is taught to name objects.

(The response *chair* is not always a tact. If it occurs because it has often been followed by the appearance of a chair as a reinforcing consequence, it is a *mand*, a "request for a chair." If, because it often occurs in expressions like *table and chair* or *sitting in a chair*, it is strengthened when *table and* or *sitting in a* is read or heard, it is an *intraverbal response*. If it is strong because someone else has just said *chair*, it is an echoic response. If the speaker is simply reading the word, it is a textual response. These kinds of verbal responses are not important for the present Response.)

Speakers acquire and emit tacts under many different states of deprivation and aversive stimulation and when many different kinds of reinforcing consequences follow. Such reinforcements are mediated by other people. There are no important nonsocial consequences of saying *chair*, at least until the speaker himself becomes a listener. The question I raised in "Terms" was this: How can we tact private stimuli inaccessible to the verbal community which arranges the necessary contingencies of reinforcement? For *chair*, substitute *pain*, and one reaches the problem of "the operational definition of a psychological term."

"Terms" argues that there are only four ways in which we can learn to tact private stimuli: (1) The verbal community can base its reinforcements on associated public stimuli. (2) It can use public responses made to the same stimuli. (3) Some private stimuli are generated by covert behavior to which responses can be learned when the same behavior is overt. (4) The tact can be metaphorical and acquired when made to similar public stimuli. Now, nearly 40 years later, I do not see any other possibilities.

Before taking up specific commentaries I list some common misunderstandings:

1. I was not trying to bring *sensations* back into behaviorism. By toothache, I mean only the stimulation arising from a damaged tooth. We must wait for physiology to supply further details.

2. Although private stimuli are often salient, the public accompaniments used by the verbal community often continue to contribute to the strength of a tact. I may say, "I am hungry" mainly because I see myself eating voraciously, a public stimulus.

3. A tact, once established or in the process of being established, usually figures in larger samples of verbal behavior to which terms like reference, denotation, and description are often applied, but the term is not itself correctly thus used.

4. A tact may have the form of a sentence if it is acquired as such. The whole expression "I'm hungry as a bear" may be a single response and useful as such upon a given occasion. On a different occasion it may be composed as a sentence of which the tact *hungry* is only a part.

5. Verbal contingencies bring responses under the control of single properties of stimuli. Only by looking at a number of instances can we identify the property that is functioning in a tact. *Chair* is, in this sense, an abstract response, but the issue is clearer when the defining property is more often found with other properties, as in

the tact *red*, to the probability of which the size and shape of red objects contribute very little.

I do not think that **Bennett** is right in saying that by calling one thing a stimulus and another a response I am "implying that the former causes the latter" or that "like most stimulus–response meaning theorists, [I am] apparently attracted by the idea that the meanings of our utterances are determined by the very same items that cause them." That is precisely what I am not saying. I am saying that the presence of an object (call it a stimulus) increases the probability that a response will be emitted. This can be fairly easily demonstrated and can indeed be used in predicting a speaker's behavior. Of course, "other things" enter into the actual speaking of a word, and I have dealt with them in detail in *Verbal Behavior*. I cannot agree with Bennett that the statement "If you want a person to utter the word 'chair,' one of the best ways is to let him see an unusual chair" (Miller 1951, p. 166) is "plainly false." Let someone scaling Mt. Everest arrive at the summit and find a chair, and the word "chair" will be pronounced with alacrity. (Incidentally, the reader should not infer that George Miller, from whose book the sentence is taken, is in thrall to a stimulus–response theory of meaning; he is one of its sharpest critics.)

It would be unfair of me to refer to my book, published 12 years after "Terms," if Bennett were devoting his commentary to my paper. But a critic of my theory of meaning must look at my book, where I appeal to much more than a stimulus in accounting for a verbal response. I do not suggest "that the causally sufficient conditions for a person's uttering '(That is) red' consist in (i) a red stimulus in conjunction with (ii) a set of circumstances C which always mediates between a stimulus and an utterance whose meaning is somehow given by the stimulus." What must be taken into account "among other things" is (1) a setting which includes a listener and (2) a long history in which speaking in similar settings has been followed by the reactions of similar listeners. The listeners have supplied the reinforcers which built the functional control exercised by the stimulus.

Typical of modern philosophers, Bennett replaces a history of reinforcing consequences with a currently felt or at least active "intention." His expression "intending to get someone else to think that Fx, or intending to fix in his own memory his belief that Fx, or the like" is an effort to find a current entity to replace the speaker's relation to the listener and the kinds of effects he has had on listeners, especially the effects described in detail in *Verbal Behavior*.

(Far from disregarding intention and belief as "of merely antiquarian interest" I am at the moment involved with a colleague, Dr. Pere Julià, a linguist, in reviewing the use of those words in current philosophy. Bennett's best effort to supply an alternative theory depends, he says, essentially upon intention and belief. From my point of view, it depends upon the personal histories which lead to verbal behavior, histories for which intention and belief stand as current surrogates.

In "Terms" I compare those who teach the meanings of words referring to private events which they themselves cannot see to a blind man teaching someone the names of colors. Obviously the blind man must have collateral

information before he can do so successfully. A solution by Bennett of the problem of the blind man with the concepts of belief and intention would be a useful contribution to this discussion.

An experiment might be helpful. Let us undertake to explain to a bright 10-year-old boy what intentions and beliefs are. When we have finished, the boy must be able to tell us when he has an intention and when he holds a belief. What things shall we point to as we tell him what those words mean? What things must he know about himself to report correctly that he has an intention or holds a belief? I think we shall find that we have taught him to mention actions and to mention or imply their consequences. These are parts of the contingencies of reinforcement of which his behavior is a function. As states of mind, intentions and beliefs are current surrogates of the contingencies. As a behaviorist, I dispense with the surrogates but take the contingencies quite seriously.

Brinker & Jaynes seem to misunderstand my saying that a casual observer can tell very little about what is going on in an operant experiment in spite of its supposed oversimplification. The experimenter sees what is going on in the experimental space much more clearly than the casual observer because he has additional information about the history of the organism – its deprivational state, its history of reinforcement, possibly something about its genetics, and so on. To understand behavior, one must know the history of the organism as well as the present "structure" of the behavior. I do not see how admitting that necessity "rescind[s] [the] promise of operationism." It simply recognizes the need for a closer study of controlling variables.

I think the same thing can be said about casual encounters between people. If the listener "makes sense of what the speaker is saying," both must be members of the same language community (i.e. have had much the same verbal history) and sense will be made more effectively if this particular speaker and listener have shared other verbal experiences. (It often takes a certain amount of time to be clear about what a stranger is saying.)

I agree in general with Brinker & Jaynes's dismissal of operationism as that term is most often used, but behaviorism, when applied to the definition of psychological terms, the subject of the symposium to which "Terms" was contributed, is very close to the spirit of operationism, and I submitted the paper on that understanding. I, too, regret that more work has not been done in line with my analysis in *Verbal Behavior*, particularly in the behavior of young children. The field is only slowly recovering from the developmentalism of Piaget and others, in which the appearance of verbal behavior is followed with little or no attention paid to the contingencies of reinforcement responsible for it.

I agree with **Danto** that an *analysis of my toothache* will not get us very far toward explaining a Greek tragedy or the works of Marcel Proust. Physics is a much more advanced science, but it has not got very far toward explaining the present condition of the universe. Biology and biochemistry are advanced sciences, but they have not got very far toward explaining that rite of spring in which molecules work their way up through the branches

of trees and take their appointed places in leaves and flowers and fruit. Nor has philosophy or religion offered alternative accounts of any of this that satisfy the critical thinker. Let us be content with beginnings.

Danto summarizes the point of my paper quite accurately, and I agree that the "terms. . .central to science are not of this . . . sort." I would also be interested in a further analysis of those terms which are "as loosely tied to stimuli as theoretical terms are." I have had something more to say about that in "Problem-Solving." I do not believe that we must in any sense relax a demand for sharpness of reference.

If I have neglected brute facts, as **Dennett** claims, it is only because I have no reason to rehearse them. The role of the discriminative stimulus in controlling the probability of emission of a response was already well established when I wrote "Terms" and has since been abundantly confirmed. The point of my paper concerned a procedure through which a private stimulus could play the usual role in spite of its inaccessibility to the verbal community which maintains the necessary contingencies. There were no "puzzling or recalcitrant or otherwise inexplicable facts" to be accounted for. The facts were well known to everyone.

My paper was not theoretical. It was an interpretation. Through what fairly obvious ways could the verbal community circumvent privacy? I cannot see any theory in my exposition of four ways in which it could be done or the conclusion that none of these ways leads to a very precise control by private stimuli.

Dennett, along with other commentators, accuses me of dogmatism. I am "extrapolat[ing] a creed: working out the details of what the devout behaviorist *has to say*, figuring out the kosher categories into which all facts must be cast, *no matter how the facts come out.*" And yet he complains at length of my use of "may" and "might," terms which, in all the "dictionary meanings" he cites, suggest far from a dogmatic stance. In order to have it both ways, Dennett says that I am feinting and weaving and that when I say "may" I really mean "must." (The only "bold declaration" that he offers as a sample of my dogmatism occurs in a paragraph in which I say that "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." The paragraph is little more than a definition of "contingency of reinforcement" – a key term borrowed from the experimental analysis of behavior. Dennett gives no example of the "host of obvious counterinstances" that he could cite, but if he means instances in which the community reinforces a response under other circumstances, they are instances I was excluding from the present discussion.)

"Terms" makes a fairly simple point about a kind of verbal behavior – behavior that Dennett would perhaps say "refers to" events inaccessible to those who teach us to speak. I believe the technical terms it uses are consistent with each other and with other terms in the work in progress to which I repeatedly referred. The point was relevant to the symposium because it shows how difficult it is to validate a system of mentalistic psychology which calls for introspection by trained observers. The theological violence of Dennett's commentary suggests that

it must raise particularly troublesome difficulties for his own discipline.

Garrett's paper is a useful interpretation of the relation called reference, particularly with respect to Wittgenstein's insistence that we do not refer to private events. As Garrett points out, such references are no more "direct" than references to other kinds of events. Private events are exclusive, but so are other events with which we alone are in contact. Privacy raises a problem only for those who teach us how to refer. Garrett's analysis of the reference function of intraverbal and echoic behavior is also useful. I have only one criticism to make of his analysis of the tact. Saying "bear" in response to a bear track found near an empty picnic table is a metonymical tact. Saying "A bear has been here" is much more. In a normal occurrence "That animal is a lion" is also more than a tact. The expression contains two tacts: *animal* and *lion*. It also contains additional material serving a function that I call in my book "autoclitic." It includes what linguists call syntax or grammar. If we are to stick closely to demonstrated behavioral processes, only the increased probability of saying *lion* in the presence of a lion is the relation called a tact. The sentence as a whole is controlled by other features of the situation, especially the presence of a listener who is likely to reinforce behavior that proves useful to him in the setting to which the speaker is responding. Short sentences are sometimes learned as units under the control of stimuli in connection with which they can be called tacts, but sentences are usually to some extent composed. Primordial verbal material (tacts, intraverbals, echoics, etc.) are put together with the help of autoclitic devices so that the listener reacts in a more effective way.

I am not familiar enough with "functionalism or the causal theory of mind" to do justice to Graham's commentary, but if for "sensation classification" we may read "stimulus classification" then so far as I can see the comparison is correct. I am not sure, however, that Graham would accept that substitution of terms. One may speak of the cause of a stimulus by distinguishing between the object (for example, a red light) and its stimulating effect (the arousal of nerve impulses in the retina), but I think it is the latter that Graham would want to call the cause of a sensation.

There are different kinds of "painful" stimuli. We classify them with terms like sharp and dull which we take from the objects which cause the pain. As a behaviorist I can say that a sharp object causes the kind of stimulation that evokes the response *sharp pain,* but Graham, I suppose, would want to say that it is the sensation which, in turn, is reported as a sharp pain.

I allowed for that possibility in a passage in "Terms" that I am surprised has gone unnoticed by those who are critical of behaviorism. The passage reads as follows:

"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 nonbehavioral private seeing may be slipped in.

The point is relevant to Malcolm's (1964) contention

that I must deny that introspection is possible. I agree with Graham in saying that Malcolm is wrong, and so are all those who take the operation to be identical with the thing it is said to define. So far as I am concerned, whatever happens when we *inspect* a public stimulus is in every respect similar to what happens when we *introspect* a private one. "Terms" is concerned only with the problems which arise in learning to do so. What people eventually " 'feel' [as] distinctively sharp about sharp pains" may contain no vestige of the stimuli which were needed when they were taught to call them sharp.

There is nothing in **Harzem's** commentary to which I can seriously object. It summarizes a philosophy of human behavior which, as Harzem points out, was shared by Wittgenstein, Ryle, and Austin. (It is often forgotten that Wittgenstein called for animal research to answer some of the questions he raised.) I wish, however, that Harzem had spent more time on the problem of privacy, which is not quite identical with that of mentalism. It is worth emphasizing that an analysis of verbal behavior and of "how words become attached to their meanings" raises what seems to me an insuperable obstacle in the path of any kind of rigorous science of mental life.

Hineline's commentary is a better reply than my own to some of the points made in the other commentaries. His references to my analysis and use of logic are particularly helpful. I am always surprised, however, when it is said that I have *only very recently* acknowledged the role of natural selection in the shaping and maintaining of behavior, although the fact that I am willing to yield some of the place of operant conditioning to its rival is worth repeating (see "Consequences" and "Phylogeny"). I am also glad that Hineline clarifies my objection to theory. I do not object to mentalistic theories of behavior so much because of the mentalism as because of the irrelevance, an irrelevance which also applies at the present time to neurological theories. In the paper on theory to which Hineline refers (Skinner 1950; here, part of "Methods"), I questioned the use of theories that appeal to "events taking place somewhere else, at some other level of observation, described in different terms, and measured, if at all, in different dimensions," but I called for a theory of behavior of a different kind.

Hocutt raises the question of meaning. The colloquial statement that a person "uses a word to express a meaning" appears to be an explanation of the occurrence of the word, but what and where is the meaning? To the mentalist, as Hocutt says, toothache means a personal experience. To a methodological behaviorist it means the setting which is said to give rise to such an experience. To the crude operationist it means the operation from which the experience is inferred. I do not accept any of those views. As a radical behaviorist I would say that if the term "meaning" has any meaning at all, it is the setting which gives rise to the response of the speaker or the subsequent action of the listener with respect to that setting.

I am glad to accept Hocutt's paraphrase that *"toothache* denotes neither a private sensation nor its public accompaniments but an *unknown bodily condition* normally caused by an abscessed tooth and normally manifesting

itself in moaning and grasping of the jaw." But that is not what he says when he writes "for [Skinner], the word *toothache* means not the private stimulus that elicits its use but the public stimuli that control reinforcement of its use." The trouble arises from the words "denote" and "mean." When a person says, "My tooth aches," stimulation from the tooth is in control, but it does not "elicit" the response as in a reflex. It makes a contribution to its strength. "Public accompaniments," such as a cry of pain or a hand to the jaw, play no part at the time. They were important to the verbal community in setting up the response at some earlier date, but this instance of the response is now under the control of private stimulation. With the rest of Hocutt's commentary I generally agree.

I am glad that **Kenrick & Keefe** bring up the relevance of "Terms" to self-perception and Bem's (1967) analysis of cognitive dissonance, and I agree that there are problems of reference with respect to public stimuli as well as private. That, indeed, was the principal contention of physical operationism. What are time, length, force, and so on? I should want to see the same kinds of answers given with respect to psychological traits. Should we try to discover exactly what a trait *is*, or should we look at the facts from which the trait is, as Kenrick & Keefe put it, inferred. The operational answer to Newton's time and space was not to solve the problem by improving the process of inference but to question whether the things Newton thought he was talking about existed. Is there any point in trying to "sharpen the reference" of the word "aggression"? It seems to me much more useful to examine the many instances to which the term has been applied and see whether any single term will prove useful with respect to all of them. It is true that terms from the vernacular can often be redefined scientifically, but they are usually found to acquire different definitions under different circumstances.

It seems to me that Kenrick & Keefe have misunderstood my contention that "differential reinforcement cannot be made contingent upon the property of privacy." I did not mean that a person cannot distinguish between the public and private attributes which underlie the use of a term. I can understand why self-description of the wholly private aspects of an emotion is probably less useful than self-descriptions of their public accompaniments. I was referring to the problem of psychological entities which were by definition exclusively private. The essence of consciousness was once said to be its privacy. But I do not think that is a useful definiens if it means there are no public accompaniments.

Lowe has, predictably, summarized my position correctly, and I am happy to join him in calling for the next step: research on self-knowledge and self-management and their possible effects on human behavior in general. I would formulate his questions in a rather different way, however. I doubt whether "the effects of reinforcement are altered qualitatively when subjects acquire the skill of generating verbal descriptions" of their own behavior and its consequences. When they do so, they generate other controlling variables which play a part in controlling subsequent behavior. That is why it is so hard to do research on operant behavior in human subjects who have learned to analyze the contingencies to which they

are exposed. Their analyses (whether or not they are correct) enter into the control of their behavior as self-generated rules (see "Problem Solving"). Research on human behavior which compares favorably with animal research is most successful in small children and retarded persons or when the contingencies are concealed. My answer to Lowe's second question (Is "human performance that is free of this 'interfering' consciousness . . . indistinguishable from that of animals") is yes, although the data Lowe (1983) cites may prove me wrong.

Meehl poses four hard questions. My tentative replies:
1. The first question concerns my opening definition of operationism, which was not very relevant to the rest of the paper. How we formulate rules as descriptions of the contingencies of reinforcement encountered in nature and society, and how logicians manipulate those rules and derive from them other rules descriptive of contingencies not yet experienced by anyone (and, possibly, never to be experienced by anyone) form too big a field to be characterized accurately with terms as general as deduction, induction, reduction, and so on. I pass.
2. If an accurate introspective vocabulary were available, I should be an ardent introspectionist (as I am, personally, with a far from accurate one). But I regard introspection, like all other forms of "spection," as behavior.
3. I think clinicians sometimes get useful information, from which they can infer something of their clients' histories, from answers to the question "How do you feel about that?" But I am not sure what private stimuli are involved or how many of the stimuli are public. In general, I have said that we cannot introspect cognitive *processes* because we do not have nerves going to the right places. Such nerves would be useful, but verbal behavior and hence introspection arose too late in the history of the species to have made the evolution of such nerves possible.
4. I should not want logicians to use behaviorese, but if I am to analyze the behavior of logicians, I must use my terms, not theirs. Theirs appear among the subject matter. I am willing to use "true" and "false" in logic and mathematics, where they can be defined reasonably well. If the sun burns out before Goldbach's Conjecture has been proved, no one will have been able to say that the conjecture is true or false. In what sense could its truth or falsity exist prior to a proof? If Goldbach had conjectured that where there is smoke there is fire, a very different account of the "truth or falsity" of the behavior would be needed, and those terms would have a very different meaning.

Moore's commentary is useful because it summarizes the argument of my paper in fresh terms and brings it into line with some of the other things that were being said about the operational definition of psychological terms at the time. It also calls attention to an important related problem. Privacy has caused trouble to psychologists and philosophers struggling to exchange views about their mental life. It has also caused trouble, unnecessarily it would seem, to the physical scientist who insists that science is personal knowledge. Polanyi (1960) argued that, and I spent many hours, to no avail, discussing the

point with P. W. Bridgman, whose operationism failed him when it came to his own behavior. The scientist first interacts with the world, like everyone else, in contingency-shaped behavior. He becomes a scientist when he begins to describe the contingencies and to design experiments which make them clearer. The ultimate product, the "laws" of science, governs scientific behavior as a corpus of rules to be followed. The behavior of the scientist in following them is reinforced by the same consequences as the original contingency-shaped behavior, but the controlling stimuli are different (see "Problem Solving"). I take it that Moore is saying that they are free of private stimuli and that those science philosophers who insist that science is personal knowledge only create problems for themselves by returning to contingency-shaped behavior.

I have not said, as **Place** claims, that reasoning in accordance with the rules of logic is "'contingency shaped' *rather than* 'rule governed' *behaviour*" (italics added). All behavior is, I believe, contingency shaped. We take advice and follow rules because of reinforcing consequences which have followed when we have done so in the past. But the behavior referred to by the advice or the rules has other consequences. Thus, if a friend advises me to take one route rather than another on a journey I do so because of what has happened in the past when I have taken advice from him or others like him. In addition, I enjoy a shorter, smoother, or pleasanter journey – the consequences specified in the advice. I obey the laws of government not because I have disobeyed them and been punished but because I was taught to obey them. In addition, I avoid the contingent punishments specified in the laws. One behaves logically by following rules which describe contingencies; at other times one might behave in the same way after having been exposed to the contingencies. The business of the logician is deriving new roles from old and arriving at descriptions of contingencies to which no one has necessarily yet been exposed.

I don't believe my attitude toward "truth" is cavalier. I accept the tautological truth of logic, but I do not think that science, including behavioral science, can be true or false in the same sense – or in any useful sense. Some verbal responses are controlled by sharply defined stimuli which have acquired their power from the part they play in very consistent contingencies. They are as close as one can come to being true. Beyond that I do not think we can go.

Place's concern for the listener seems to me irrelevant. My book *Verbal Behavior* was different from most linguistic material of the time in emphasizing the behavior of the speaker. I did not think that the behavior of the listener called for any special treatment beyond the role played in reinforcing the behavior of speakers. The behavioral processes involved when a person responds to "It is raining" do not differ significantly from those involved in responding to a few drops of rain on the skin or a particular noise on the roof. All three "mean" rain. The "meaning" of a verbal response for the speaker is not the same as its "meaning" for the listener. That is what is wrong with "communication" as making something common to both parties.

Place speaks of "being in pain" when I speak of the stimuli generated by a carious tooth. I chose some such response as "My tooth aches" as a simple example, not as a "paradigm case of a psychological expression." I do not agree that "it is one of a very small number of expressions in our very extensive psychological vocabulary." I agree with Ryle that we are usually talking about behavior when we speak of knowing, believing, thinking, wanting, and intending (I would not be much of a behaviorist if I did not!), but that is not what the psychologists of 1945 were saying. The editor of the symposium (E. G. Boring), a student of Titchener and, through Titchener, Wundt, believed in a world of mental life in which mental events obeyed mental laws observed by "trained observers." These were the things of which I was offering an operational definition.

I found **Rachlin's** paper puzzling. He evidently uses the term "toothache" for all the behavior elicited or evoked by a carious tooth, where I was using it to mean only the stimulation arising from such a tooth. He also speaks of thoughts, feelings, and other mental events and argues that they must be operants because they have "no apparent external antecedent stimulus." But one point of "Terms" was that a substantial amount of behavior that would be called operant was indeed under the control of private stimuli; that was the problem I was discussing. I can't imagine what Rachlin means by a rat's hope or how he knows that it takes longer than a bar press.

I do not see why it follows from the fact that "in teaching people to use the mentalistic vocabulary, it must be overt behavior that society observes and then rewards or punishes" that "a person who uses that vocabulary to refer to private events must be using it incorrectly." To the extent that the private event correlates with the public evidence, terms will be used correctly. Rachlin later makes that point by saying that "to the extent that mental terms refer to the overt behavioral context of immediate behavior it is possible to use them in a behavioral science." But since we do not know the extent to which they do so, any such use is questionable.

It would be ungrateful of me to complain of **Ringen's** excellent summary of my position, and the only remark I have to make is not a complaint. Ringen extends the argument of my paper to cover the behavioristic contention that anthropomorphism, in particular "the cognitivism inspired by Chomsky," is "misplaced in the scientific study of human (verbal) behavior." I would have been willing to make the extension at the time I wrote "Terms" (and indeed was making it in the manuscript from which the paper was essentially taken), but I would put it rather differently today. The explanatory terms which have been used for more than 2,000 years to explain human behavior are troublesome not because they raise questions about dimensions but because they assign the initiation of behavior to the person rather than to that person's genetic and personal history. The problem is centrism rather than anthropomorphism. The terms I hoped to dispense with in my analysis of verbal behavior (terms like meaning, idea, information, and knowledge) represent supposed possessions of the speaker. So far as I am concerned they are inconvenient surrogates of the speaker's history. Their dimensions

(physical, mental, conceptual?) are not really at issue. What causes trouble is the usurpation of the initiating role of the environment.

Robertson raises the question of sensations and images as representations of stimuli. Do we see red as a property of an object, as a retinal response to a given frequency of radiation, as nerve impulses in the optic tract, or as activity in the occipital cortex? As a behaviorist, I must reply that what is happening in retina, optic tract, and occipital cortex are part of seeing red. As a behaviorist, I leave that to the physiologist, who has more appropriate instruments and methods. As a behaviorist, I am concerned only with the way in which a discriminative response (whether it be key press, saying "red," or stepping on the brake of a car) is brought under the control of red objects.

Also as a behaviorist, I am concerned with how a person learns to say "I see red" in both the presence and absence of red objects. It is the word "see" that causes trouble. We teach a child to answer questions like, "Do you see that animal?" or, "Can you see the clock?," but we do so successfully only if we have evidence that the child's responses are correct. The evidence we use usually consists of subsequent behavior, as in answering the question, "What is it?" or, "What time is it?" Certain private events are part of that behavior, and the private events take over control when the child is eventually told to "think of an elephant" or "imagine a clock." We have no evidence that copies of elephants or clocks exist inside the child at any time. Whatever is happening when we see an elephant or a clock does not require a representation.

Stalker & Ziff have assumed that beyond science and technology there lies only philosophy. I have found something else: interpretation. I would define it as the use of scientific terms and principles in talking about facts about which too little is known to make prediction and control possible. The theory of evolution is an example. It is not philosophy; it is an interpretation of a vast number of facts about species using terms and principles taken from a science of biology based upon much more accessible material and upon experimental analyses and their technological applications. The basic principle, reproduction with variation, can be studied under controlled conditions, but its role in the evolution of existing species is a mere interpretation.

Plate tectonics is another example. It is not philosophy but an interpretation of the present state of the crust of the earth, using physical principles governing the behavior of material under high temperatures and pressures established under the conditions of the laboratory, where prediction and control are possible.

Laboratory analyses of the behavior of organisms have yielded a good deal of successful prediction and control, and to extend the terms and principles found effective under such circumstances to the interpretation of behavior where laboratory conditions are impossible is feasible and useful. I do not think that is properly called philosophy. The human behavior we observe from day to day is unfortunately too complex, occurs too sporadically, and is a function of variables too far out of reach to permit a rigorous analysis. It is nevertheless useful to talk about it in the light of instances in which prediction and control

have proved to be possible. It is true that I was in contact with philosophers in the thirties and forties and I believe to my benefit. In particular, I discussed the point of "Terms" with Herbert Feigl, a distinguished member of the Vienna Circle. But I was not "pursuing an elusive weltanschauung." I have not "succumb[ed] to a lust for philosophic theorizing."

My book *Verbal Behavior* was an interpretation of the field. Early on I had removed a few sections that could be said to present facts (about word associations, alliteration, guessing, and so on) just in order to make the nature of the book clearer. The book differed from what might have been called the philosophy of language that was then current in linguistics, semantics, and books like *The Meaning of Meaning* (Ogden & Richards 1938). In turning to the history of the speaker rather than to the presumed current endowments of speech, I could avoid saying that a speaker uses words to refer to things, to express ideas, or to communicate meanings. I questioned the existence of these things in their traditional sense. I could, however, have defined them behaviorally, although the resulting expressions would not have been convenient.

Stalker & Ziff had some difficulty in finding the new kinds of behavior I am said to have used to "fill the bill of a technological bird fishing for philosophic frissons in Plato's wordy meander." The essential dependent variable in the behavioral analysis is the probability of behavior, rather than the behavior itself, and why should I not refer to past, current, and future behavior? I agree that perceptual behavior is difficult, but philosophers have found it so, too. The term is not to be dismissed as a slogan. The expression "covert behavior" was current long before my time, and its referent is familiar to anyone who has talked silently to himself.

Although while I do not, as **Terrace** points out, deny "the existence of mental events," I do not believe they exist. There is an inner behavioral life including private stimuli and private responding. Traditional expressions referring to mental events I regard as surrogates of histories of reinforcement. Thus, for me, the bona fide subject matters are

> not thoughts, but what is happening as one thinks and the history of reinforcement responsible for it;
> not beliefs, but behavior with respect to controlling stimuli and the histories responsible for that control;
> not perceptions, but the current control exercised by stimuli as the result of earlier contingencies of reinforcement;
> and so on.

It is true that "modern studies of human and animal cognition need not concern themselves with the ghost in the machine," but it is equally important that they dispense with the internal origination of behavior.

Terrace begins a review of "recent developments in cognition" with three supposed implications of my hypothesis about private events. I should want to state them in a very different form:

1. "Private events are conscious." The percentage of which we are conscious must be very small. We seldom say we are conscious of interoceptive or proprioceptive stimulation or of much of the exteroceptive stimulation

which can be shown to have an effect on our behavior. "Terms" dealt with responses which are brought under the control of private events by a verbal community.

2. "Consciousness presupposes language." Self-knowledge requires verbal contingencies.

3. "Only human beings experience consciousness." The verbal communities which generate such responses have until very recently generated them for human beings only.

With these translations, I do not see the import of the paragraphs which follow in Terrace's commentary. A few remarks: I would certainly not say that "all [the behavior contributing to] mental activity [should] be characterized as private (conscious) events, under the control of particular internal stimuli." We "think" about public stimuli and talk about private ones.

I agree that "the study of cognitive phenomena does not presuppose dualism," but I insist it presupposes inner determination, which is the heart of the matter when one says that one acts because one feels like acting or takes a particular course because one thinks it will succeed.

This is not the place to argue with **Terrace** about "representations" (but see "Behaviorism-50"). It is the essence of behaviorism to argue that one does not take in the world or make copies of it in any form and that behavior which appears to require an internal representation must be explained in other ways. A complete account of an alternative explanation in neurological terms is, so far as I know, still out of reach, but that is also out of my reach as a behaviorist.

Wright goes far beyond the scope of "Terms" to a criticism of what is essentially the argument of the book (*Verbal Behavior*) from which it was in a sense taken. It is true that I was attempting to account for verbal behavior without formulating it as a "report of experience," as "the expression or communication of meaning," or as necessarily involving "understanding" or "judgment," as those terms were traditionally defined. The account worked in a very different way, and if successful it should have included the behavior of scientists if not some essence of "science" as knowledge. I could answer Wright only by reviewing the whole book, and that would be irrelevant here. I may point out, however, that he is wrong in characterizing my position as that "*all words* are meaningless physical effects caused by specific kinds of physical stimuli." The selective action of operant conditioning establishes a controlling relation among *three* things – stimuli (the setting), behavior (in this case, verbal), and the reinforcing consequences (in this case, arranged by a verbal community).

The argument that "psychology must, on pain of otherwise cutting off its own head, presuppose that human discourse is very largely rational – that *it isn't caused by stimuli*" – raises a different point. Apart from the last phrase, with which of course I agree, I make a very different point about rationality. Prior to the advent of verbal behavior (which required the evolution of physiological changes bringing the vocal musculature under operant control), all behavior must have been shaped and maintained by natural selection or operant conditioning. It is true that some linguists and cognitive psychologists have asserted that contingencies of reinforcement *con-*

tain rules, where I would say rather that rules are descriptions of such contingencies, but something else happened when descriptions became possible and rules could be formulated. A different kind of behavior then emerged which needed to be distinguished (see "Problem Solving"). Once people could talk about their behavior and the circumstances under which it occurred, they could begin to give each other reasons for acting in given ways. An early form must have been the command, describing an action and at least implying a consequence of failure to act. Advice and warnings presumably followed in turn. They described behavior and at least implied consequences. The laws of religion and government more explicitly specified behavior and consequences. Behavior that is called taking advice, heeding a warning, or obeying a law, or behavior that follows rules composed upon occasion by an immediate analysis of contingencies can be called rational. The behaver can be said to have "knowledge of the consequences." Nevertheless I doubt that it is true that human behavior is "very largely rational" in that sense. Would that it were!

The point of my paper could have been made in traditional terms. How do we learn the meanings of words? And how do we do so when the things the words mean are not accessible to those who teach us? Why did I not make the point that way? Because I was composing a different account of verbal behavior in which meanings in some Platonic sense did not exist in words but were to be sought among the variables of which verbal behavior was a function (colloquially, the situations in which words are used). For the purpose of "Terms," I chose a very simple functional relation, the discriminative control exercised by a private stimulus.

Zuriff's first point is very important. Methodological behaviorists also talked about private events that serve as stimuli and also about private (covert) behavior. The part of methodological behaviorism I rejected was the argument that science must confine itself to events accessible to at least two observers (the position of logical positivism) and that behaviorism was therefore destined to ignore private events. (Hence the still current popular view that behaviorists confine themselves only to the behavior they can see.) It was Stevens and Boring, not Watson, Weiss, Tolman, Guthrie, or Hull who then continued to believe in the existence of mental life.

But Zuriff misreads my view of the role of the private stimulus. It is true that the practice of the verbal community is to infer the private event in arranging instructional contingencies, but the person who thereby learns to describe the event is responding to it directly, not by inference. It is no doubt wrong of behavior therapists to assume that self-descriptive statements are correct (as it is wrong of Freudian or other kinds of therapists to do the same thing), but within the limits of accuracy of such reports, something can be learned about a person's history by asking how he feels.

The listener who responds to "I am depressed," by acting henceforth as he usually reacts to a depressed person is using inference only to the extent that a person who hears someone say "It is raining" then takes an umbrella. If doing either of these things is using a hypothetical construct, so be it.

References

Anderson, J. R. (1978) Arguments concerning representations for mental imagery. *Psychological Review* 85:249–77. [LCR] .

Armstrong, D. M. (1971) Meaning and communication *Philosophical Review* 80:427–47. [JB]

Austin, J. L. (1961) *Philosophical papers*. Clarendon Press. [PH]

Bates, E. (1976) *Language and context: The acquisition of pragmatics*. Academic Press. [RPB]

Bem, D. J. (1967) Self-perception: An alternative interpretation of cognitive dissonance phenomena. *Psychological Review* 74:183–200. [DTK, rBFS, HST]

Bem, D. J. & Allen, A. (1974) On predicting some of the people some of the time: The search for cross-situational consistencies in behavior. *Psychological Review* 81:506–20. [DTK]

Bennett, J. (1975) Stimulus, response, meaning. *American Philosophical Quarterly* 9:55–88. [JB]

(1976) *Linguistic behaviour*. Cambridge University Press. [JB]

Bergmann, G. & Spence, K. W. (1941) Operationism and theory in psychology. *Psychological Review* 48:1–14. [JM]

Block, N. (1978) Troubles with functionalism. In: *Perception and cognition: Issues in the foundations of psychology*. Minnesota studies in the philosophy of science, vol. 9, ed. C. W. Savage. University of Minnesota Press. [GG]

Boden, M. (1972) *Purposive explanation in psychology*. Harvard University Press. [JDR]

Boring, E. G. (1936) Temporal perception and operationism. *American Journal of Psychology* 48:519–22. [JM]

(1945) The use of operational definitions in science. *Psychological Review* 52:243–45. [taBFS]

Bousfield, A. K. & Bousfield, W. A. (1966) Measurement of clustering and of sequential constancies in repeated free recall. *Psychological Reports* 19:935–42. [HST]

Bower, G. H. (1972) A selective review of organizational factors in memory. In: *Organization of memory*, ed. E. Tulving & W. Donaldson. Academic Press. [HST]

Bridgman, P. W. (1928) *The logic of modern physics*. Macmillan. [RPB, JDR, taBFS]

(1945) Some general principles of operational analysis. *Psychological Review* 52:246–49. [taBFS]

Brinker, R. P. (1982) Contextual contours in the development of language. In: *Children thinking through language*, ed. M. Beveridge. Edward Arnold. [RPB]

Bruner, J. S. (1975) The ontogenesis of speech acts. *Journal of Child Language* 2:1–19. [RPB]

Carnap, R. (1934) *The unity of science*. K. Paul, Trench, Trubner & Co. [taBFS]

Catania, A. C. (1979) *Learning*. Prentice-Hall. [RBP]

Cheek, J. M. (1982) Aggregation, moderator variables, and the validity of personality tests: A peer-rating study. *Journal of Personality and Social Psychology* 43:1254–69. [DTK]

Chomsky, N. (1959) A review of B. F. Skinner's *Verbal Behavior*. *Language* 35:26–58. [RPB, JDR]

(1975) *Reflections on language*. William Collins. [CFL]

Churchland, P. (1981) Eliminative materialism and propositional attitudes. *Journal of Philosophy* 78:67–90. [JB]

Churchland, P. M. & Churchland, P. S. (1981) Functionalism, qualia, and intentionality. *Philosophical Topics* 12:121–45. [GG]

Commons, M. L., Herrnstein, R. J. & Rachlin, H. eds. (1982) *Quantitative analyses of behavior, vol. 2, Matching and maximizing accounts*. Ballinger. [HR]

Cumming, W. W. & Berryman, R. R. (1965) Stimulus generalization. In: *Stimulus generalization*, ed. D. I. Mostofsky. Stanford University Press. [HST]

Feigl, H. (1945) Operationism and scientific method. *Psychological Review* 52:250–59. [RPB, taBFS]

Fodor, J. A. (1980) Methodological solipsism considered as a research strategy in cognitive psychology. *Behavioral and Brain Sciences* 3:63–72. [JB]

Fodor, J. A. Bever, T. G. & Garrett, M. F. (1974) *The psychology of language*. McGraw-Hill. [JDR]

Grant, D. S. (1983) Rehearsal in pigion short-term memory. In: *Animal cognition*, ed. H. L. Roitblat, T. G. Bever & H. S. Terrace. Lawrence Erlbaum Associates. [HST]

Greenfield, P. M. & Smith, J. H. (1976) *The structure of communication in early language development*. Academic Press. [RPB]

Grice, H. P. (1957) Meaning. *Philosophical Review* 66:377–88. [JB]

Harré, R. & Secord, P. F. (1972) *The explanation of social behaviour*. Blackwell. [CFL]

Harzem, P. & Miles, T. R. (1978) *Conceptual issues in operant psychology*. Wiley. [PH]

Hempel, C. G. (1965a) The concept of rationality and the logic of explanations by reasons. In: *Aspects of scientific explanations*. Free Press. [JDR]

(1965b) Empiricist criteria of cognitive significance: Problems and changes. In: *Aspects of scientific explanation*. Free Press. [JDR]

(1965c) A logical appraisal of operationism. In: *Aspects of scientific explanation*. Free Press. [JDR]

Herrnstein, R. J. (1977) The evolution of behaviorism. *American Psychologist* 32:593–603. [RPB]

Hinsie, L. E. & Campbell, R. J., eds. (1970) *Psychiatric Dictionary*. 4th ed. Oxford University Press. [PEM]

Hunter, W. S. (1913) The delayed reaction in animals. *Behavior Monographs* 2:6. [HST]

Jaynes, J. (1976) *The origin of consciousness in the breakdown of the bicameral mind*. Houghton Mifflin. [HST]

Kendall, P. C. & Hollon, S. D. (1979) *Cognitive-behavioral interventions: Theory, research and procedures*. Academic Press. [CFL]

Kenrick, D. T. & Dantchik, A. (1983) Interactions, idiographics, and the social psychological invasion of personality. *Journal of Personality* 51:286–307. [DTK]

Kenrick, D. T. & Stringfield, D. O. (1980) Personality traits and the eye of the beholder: Crossing some traditional philosophical boundaries in the search for consistency in all of the people. *Psychological Review* 87:88–104. [DTK]

Koch, S. (1964) Psychology and emerging conceptions of knowledge as unitary. In: *Behaviorism and phenomenology*, ed. T. W. Wann. University of Chicago Press. [JDR]

Koestler, A. (1967) *The ghost in the machine*. Hutchinson. [CFL]

Kuhn, T. (1962) *The structure of scientific revolutions*. University of Chicago Press. [JDR]

Lacey, H. (1974) The scientific study of linguistic behavior: A perspective on the Skinner-Chomsky controversy. *Journal for the Theory of Social Behaviour* 4:17–51. [JDR]

Ledgwidge, B. (1978) Cognitive-behavior modification: A step in the wrong direction? *Psychological Bulletin* 85:353–75. [CFL]

Locke, E. A. (1979) Behavior modification is not cognitive and other myths: A reply to Ledgwidge. *Cognitive Therapy and Research* 3:119–25. [CFL]

Lowe, C. F. (1979) Determinants of human operant behaviour. In: *Advances in analysis of behaviour*. vol. 1, *Reinforcement and the organisation of behaviour*, ed. M. D. Zeiler & P. Harzem. Wiley. [CFL]

(1983) Radical behaviorism and human psychology. In: *Animal models of human behavior*, ed. G. C. L. Davey. Wiley. [CFL, rBFS]

Lowe, C. F., Beasty, A. & Bentall, R. P. (1983) The role of verbal behavior in human learning: Infant performance on fixed-interval schedules. *Journal of the Experimental Analysis of Behavior* 39:157–64. [CFL]

Lowe, C. F. & Higson, P. J. (1981) Self-instructional training and cognitive behaviour modification: A behavioural analysis. In: *Applications of conditioning theory*, ed. G. C. L. Davey. Methuen. [CFL]

Luria, A. (1961) *The role of speech in the regulation of normal and abnormal behavior*. Liveright. [CFL]

Lycan, W. (1981) Form, function, and feel. *Journal of Philosophy* 78:24–49. [GG]

MacCorquodale, K. (1970) On Chomsky's review of Skinner's *Verbal Behavior*. *Journal of the Experimental Analysis of Behavior* 13:83–99. [JDR]

Mach, E. (1893) *The science of mechanics*. Open Court Publishing Company. [taBFS]

(1919) *The science of mechanics*. Translated by T. J. McCormack. 4th ed. Open Court Publishing Company. [JDR]

Mahoney, M. J. (1977) Reflections on the cognitive-learning trend in psychotherapy. *American Psychologist* 32:5–13. [CFL]

Malcolm, N. (1964) Behaviorism as a philosophy of psychology. In: *Behaviorism and phenomenology: Contrasting bases for modern psychology*, ed. T. W. Wann. University of Chicago Press. [GG, rBFS]

Mandler, G. (1967) Organization and memory. In: *The psychology of learning and motivation*, ed. K. W. Spence & J. T. Spence. Academic Press. [HST]

Meyer, M. (1921) *The psychology of the other-one*. Missouri Book Co. [GEZ]

Miller, G. A. (1951) *Language and communication*. McGraw-Hill. [JB, rBFS]

Ogden, C. K. & Richards, I. A. (1938) *The meaning of meaning*. Harcourt, Brace. [rBFS]

Olton, D. S. & Samuelson, R. J. (1976) Remembrance of places past: Spatial

memory in rats. *Journal of Experimental Psychology: Animal Behavior Processes* 2:97–116. [HST]

Palmer, S. E. (1978) Fundamental aspects of cognitive representation. In: *Cognition and categorization*, ed. E. Rosch & B. Lloyd. Lawrence Erlbaum Associates. [LCR]

Piaget, J. (1929) *The child's conception of the world*. Harcourt, Brace. [HST]

Piaget, J. & Inhelder, B. (1969) The gaps in empiricism. In: *Beyond reductionism*, ed. A. Koestler & J. R. Smythies. Hutchinson. [RPB]

Place, U. T. (1981a) Skinner's *Verbal behavior* I – Why we need it. *Behaviorism* 9:1–24. [UTP]

(1981b) Skinner's *Verbal Behavior* II – What is wrong with it. *Behaviorism* 9:131–52. [UTP]

Polanyi, M. (1960) *Personal knowledge*. University of Chicago Press. [rBFS]

Premack, D. (1976) *Intelligence in ape and man*. Lawrence Erlbaum Associates. [HST]

Quine, W. (1960) *Word and object*. MIT Press. [JDR]

(1970) Methodological reflections on current linguistic theory. *Synthese* 27:325–29. [JDR]

Ringen, J. (1977) On evaluating data concerning linguistic intuition. In: *Current themes in linguistics*, ed. F. Eckman. Wiley. [JDR]

(1980) Quine on introspection in linguistics. In: *A festschrift for a native speaker*, ed. F. Coulmas. Mouton. [JDR]

Roberts, W. A. & Kraemer, P. J. (1982) Some observations of the effects of intertrial interval and delay on delayed matching to sample in pigeons. *Journal of Experimental Psychology: Animal Behavior Processes* 8:342–53. [HST]

Roitblat, H. L. (1980) Codes and coding processes in pigeon short-term memory. *Animal Learning and Behavior* 8:341–51. [HST]

Russell, B. (1940) *An inquiry into meaning and truth*. G. Allen & Unwin. [taBFS]

Ryle, G. (1949) *The concept of mind*. Hutchinson. [PH, UTP]

Schiffer, S. R. (1972) *Meaning*. Oxford University Press. [JB]

Scriven, M. (1956) A study of radical behaviorism. In: *The foundations of science and the concepts of psychology and psychoanalysis*, ed. H. Feigl & M. Scriven. Minnesota Studies in the Philosophy of Science, vol. 1. University of Minnesota Press. [JDR]

Segal, E. F. (1975) Psycholinguistics discovers the operant: A review of Roger Brown's *A first language: The early states*. *Journal of the Experimental Analysis of Behavior* 23:149–58. [RPB]

Shepard, R. N. (1975) Form, formation and transformation of internal representations. In: *Information processing and cognition*, ed. R. L. Solso. Lawrence Erlbaum Associates. [HST]

Shepard, R. N. & Metzler, J. (1971) Mental rotation of three-dimensional objects. *Science* 171:701–3. [LCR]

Shimp, C. P. (1976) Short-term memory in the pigeon: Relative recency. *Journal of the Experimental Analysis of Behavior* 25:55–61. [HST]

Skinner, B. F. (1931) The concept of the reflex in the description of behavior. *Journal of General Psychology* 5:427–58. [PNH, JDR, taBFS]

(1935a) The generic nature of the concepts of stimulus and response. *Journal of General Psychology* 12:40–65. [RPB, PNH]

(1935b) Two types of conditioned reflex and a pseudo-type. *Journal of General Psychology* 12:66–77. [PNH]

(1937) Two types of conditioned reflex: A reply to Konorski and Miller. *Journal of General Psychology* 16:272–79. [PNH]

(1938) *The behavior of organisms*. Appleton-Century-Crofts. [RPB, CFL, PEM, DS]

(1950) Are theories of learning necessary? *Psychological Review* 57:193–216. [PNH, rBFS, HST]

(1953) *Science and human behavior*. Macmillan. [CFL, HST]

(1957) *Verbal behavior*. Appleton-Century-Crofts. [KRG, GG, CFL, JM, UTP, JDR]

(1959) John Broadus Watson, behaviorist. *Science* 129:197–98. [JDR]

(1963) Behaviorism at fifty. *Science* 134:566–602. [CFL]

(1969a) *Contingencies of reinforcement: A theoretical analysis*. Appleton-Century-Crofts. [RPB, UTP, HST, GEZ]

(1969b) An operant analysis of problem solving. In: *Contingencies of reinforcement: A theoretical analysis*. Appleton-Century-Crofts. [PNH]

(1974) *About behaviorism*. Alfred A. Knopf. [PH, CFL, DS, HST, GEZ]

(1977a) Herrnstein and the evolution of behaviorism. *American Psychologist* 32:1006–12. [RPB, PNH]

(1977b) Why I am not a cognitive psychologist. *Behaviorism* 5:1–10 [HST]

(1981) Selection by consequences. *Science* 213:501–4. [PNH]

Slater, E. & Glithero, E. (1965) A follow up of patients diagnosed as suffering from hysteria. *Journal of Psychosomatic Research* 9:9–13. [DTK]

Sokolov, A. N. (1972) *Inner speech and thought*. Plenum. [CFL]

Stevens, S. S. (1939) Psychology and the science of science. *Psychological Bulletin* 36:221–63. [JM, taBFS]

Taylor, C. (1964) *The explanation of behaviour*. Humanities Press. [JDR]

Terrace, H. S. (1970) Towards a doctrine of radical behaviorism. *Contemporary Psychology* 15:531–35. [HST]

(1982) Can animals think? *New Society* 4:339–42. [HST]

(1983a) Animal cognition. In *Animal Cognition*, ed. H. L. Roitblat, T. G. Bever & H. S. Terrace. Lawrence Erlbaum Associates. [HST]

(1983b) Simultaneous chaining: The problem it poses for traditional chaining theory. In: *Quantitative studies in operant behavior: Acquisition*, ed. M. L. Commons, A. R. Wagner & R. J. Herrnstein. Ballinger. [HST]

Vygotsky, L. (1962) *Thought and language*. Wiley. [CFL]

Watson, J. B. (1913) Psychology as the behaviorist views it. *Psychological Review* 20:158–77. [CFL]

Wells, G. (1981) *Learning through interaction: Study of language development*. Cambridge University Press. [RPB]

Whitlock, F. A. (1967) The aetiology of hysteria. *Acta Psychiatrica Scandinavica* 43:144–62. [DTK]

Wilson, T. (1978) Cognitive behavior therapy: Paradigm shift or passing phase? In: *Cognitive behavior therapy: Research and application*, ed. J. Foreyt & D. Rathjen. Plenum. [CFL]

Winokur, S. (1976) *A primer of verbal behavior: An operant view*. Prentice-Hall. [JDR]

Wittgenstein, L. (1953) *Philosophical investigations*. Macmillan. [KRG, PH]

Woodfield, A. (1976) *Teleology*. Cambridge University Press. [JDR]

Wright, L. (1976) *Teleological explanations*. University of California Press. [JDR]

Zentall, T. (1983) Cognitive factors in conditional learning by pigeons. In: *Animal cognition*, ed. H. L. Roitblat, T. G. Bever & H. S. Terrace. Lawrence Erlbaum Associates. [HST]

Zettle, R. D. & Hayes, S. C. (1982) Rule-governed behavior: A potential theoretical framework for cognitive behavior therapy. In: *Advances in cognitive-behavioral research and therapy*, vol. 1, ed. P. C. Kendall. Academic Press. [CFL]

Ziff, P. (1960) *Semantic analysis*. Cornell University Press. [JB]

(1970) A response to stimulus meaning. *Philosophical Review* 79:63–74. [JB]

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