

SKINNER (1938) AND SKINNER (1945)

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Abstract: Skinner's first book, *Behavior of Organisms* (1938), views psychology as the study of the behavior of the "organism as a whole." Such a conception excludes internal events such as neural states or covert muscular movements (parts of the organism) from behavior analysis. Skinner's article, *The Operational Analysis of Psychological Terms* (1945) retracts this exclusion in that it hypothesizes covert behavior (behavior of part of the organism) in behavioral analysis of psychological (i.e., mental) terms. The present article argues that this retraction was a mistake. If one takes a wider, molar view of behavior, there is no need to hypothesize internal events in the behavioral analysis of mental terms. Psychological (that is, mental) terms may be understood as patterns of overt behavior of whole organisms—and are no less real, no less subject to behavior analysis, for that.

Key Words: Skinner, mind, behavior, covert behavior, mental terms, radical behaviorism, behavioral patterns

The purpose of this article is to compare two seminal works of B. F. Skinner—his first book, *The Behavior of Organisms: An Experimental Analysis* (henceforth *B of O*), published in 1938, and an article, "The Operational Analysis of Psychological Terms" (henceforth *OAPT*), published in *Psychological Review* in 1945. *B of O* makes a sort of sandwich; in introductory and concluding chapters Skinner argues with his predecessors (Pavlov mostly) and contemporaries (Hull and Tolman mostly), presents "A System of Behavior" (the title of the first chapter), defends his own radically different approach to the study of behavior, and speculates on the direction it might take. The meat of the book, the middle chapters, consists of a host of experimental results, some previously published (between 1930 and 1937) and some not previously published.

In *OAPT* Skinner lays the philosophical groundwork for "radical behaviorism," a version of behaviorism in which private events (events occurring within the skin), observable by the person who has them and by no one else, are the physical basis for most of our mentalistic vocabulary (that is, for "psychological terms"). Many, if not most contemporary Skinnerians identify themselves as radical behaviorists. I believe that adoption of radical behaviorism was and is a serious mistake; it is inconsistent with the basic arguments of *B of O* and is detrimental to both theory and practice of the kind of behaviorism set forth in *B of O*. I will first present my own understanding of those arguments and then discuss *OAPT* and how it represents a retreat from them.

The Behavior of Organisms: In his preface to the seventh printing of *B of O*, written in 1966 (28 years after the first printing and 52 years ago) Skinner proudly claims that the cumulative records and other data reported in 1938 will seem “quite crude,” to the then modern reader, as compared to then current (1966) work with more refined techniques. He implies that the behavior system proposed in 1938 had progressed and developed, as a science should, based on the studies described in *B of O*. He was correct then, in 1966, and the same holds for work done in 1966 relative to that published in *Journal of The Experimental Analysis of Behavior* today. However crude they were, Skinner’s (1938) experiments with a lever box (he never called it a Skinner box) and his focus on cumulative records of individual rats with individual reinforcement histories must have seemed a breath of fresh air in contrast to the hypothesis-driven, statistically analyzed group studies of rats running in mazes that constituted the bulk of experimental work on learning in 1938.

To give you an idea of the originality of these experiments let me describe one, with eight rats as subjects (pp. 220-223). The discriminative stimuli were light-on and light-off. The first lever press after the light came on was reinforced; after that, the light stayed on for five minutes with no reinforcement and then went off; then, the first response after the light went off was reinforced, the light stayed off with no reinforcement for five minutes and then came on—and so on, repeating the cycle. The positive discriminative stimulus (S^D it was already called) was a change—from dark to light or light to dark; the negative discriminative stimulus (not yet called S^A) was the continued presence of light or dark. The rats quickly learned to press the lever after a light came on or went off and to stop pressing until the next stimulus change. The effective S^D here, Skinner says, “has become the *change* from one stimulus to another” (italics in original) rather than the light or dark themselves. Moreover, the discrimination was learned just as fast as it was when the positive and negative discriminative stimuli were presence versus absence of light. Here is a case where rats were equally (or more) sensitive to a change in stimuli as they were to the presence or absence of a stimulus. There was no need to extinguish responses to the continued absence of the light after the light went off and then recondition them when the light came back on. Skinner remarks on the rats’ initial sensitivity to relative rather than absolute stimulation (but does not extend his research from relative stimulation to relative response rate or relative reinforcement rate.) This area—relative reinforcement and the study of choice—so much a part of modern operant research, was explicitly excluded by Skinner from *B of O*. Why? To answer this question, we turn to the bread of the sandwich, the introductory and concluding chapters where the system that guides the experimental work is presented and defended.

Skinner’s “System of Behavior” is based on the concept of the reflex. The operant is, for Skinner, just another kind of reflex, albeit one without an eliciting stimulus. In his thesis (an entirely theoretical one) Skinner had argued that the concepts of stimulus and response in research on standard reflexes (“respondents”) were actually not physiological “arcs” (chains of S-R connections) within the

organism but generic categories. That is, certain classes of environmental events were capable of eliciting certain classes of overt behavior. The defining properties of these classes were the functional relationships (the “dynamics,” Skinner says) between the two. The job of the researcher was to refine the class boundaries of stimulus (to the organism) and response (of the whole organism) that generated “smooth curves” and preserved these functional relationships. The properties of the functional relationships, not their underlying physiological mechanisms, were “The Laws of The Reflex” that formed the basis of Skinner’s system.

Notwithstanding the generic nature of the stimulus and response, Skinner was concerned to narrow these categories as far as possible consistent with preserving fundamental functional relationships:

One important practice has been observed in the traditional study of reflexes which is of paramount importance in the kind of system here set up and which supports the extended use of the term [extended to include operants]. The practice is that of referring to specific movements of parts of the organism. In spite of the generic nature of the term, the topographical reference has always been relatively narrow and precise. (pp. 439-440)

A person’s knee-jerk reflex is thus a specific movement of his leg not just any reaction he might have to being hit on the knee with a rubber hammer.

Similarly, in defining a rat’s lever press, the operant is not (at least in *B of O*) anything the rat does that causes the lever to move down (such as sitting on it or poking it with its nose) but a specific sequence of movements ending with the rat’s front paw (or paws) on the lever, pushing it down. The experimenter’s job (and one of Skinner’s brilliant abilities) was to design the apparatus so that only this specific movement would be recorded and reinforced.

Given a conception of reflexes (the operant being a type of reflex) “as specific movements of parts of the organism” you would have to begin to study reflexes (as Pavlov did) with the study of specific movements; only after complete understanding of single movements was reached would you proceed to the much more complex case of their interaction. There may well be an interaction between the knee-jerk on the left knee and that on the right knee, but you would have to understand each reflex in isolation before going on to study their interaction. Thus, there was never more than one lever in the Skinner box in the experiments reported in *B of O*.

Another reason why absolute rather than relative response rates are the primary datum in *B of O* is that Hull and Tolman, the two most prominent learning researchers in those days (Skinner was a young upstart), used the maze to study choice. Skinner felt, correctly, that mazes were poor, inefficient instruments to study behavior; he wanted to disassociate himself from this mode of research:

The maze is not a suitable instrument for the investigation of the dynamic properties of behavior. Even when we consider a single ‘choice-point,’ there remain two possible responses – turning right and turning left. No measure of

the strength of either is provided by maze behavior, since a 'choice' reveals only the relatively greater strength of one. Instead of measuring behavior directly, Tolman is reduced to determining a 'behavior ratio,' which is of little use in following the various processes which are the principal subjects of investigation. (p. 437)

It was left to Herrnstein, 23 years later, to publish a study of two operants at once and to discover that relative reinforcement rate and relative response rate (i.e., a behavior ratio) could be fundamental independent and dependent variables just as relative stimulus value could be a fundamental discriminative stimulus. And, it was left to Premack, at about the same time, to show that reinforcement itself was a relative concept – the value of the reinforcer relative to that of the response.¹

Despite Skinner's efforts (largely successful) to abandon old issues and concerns in the study of learning and to start anew, *B of O* contains several lingering concepts that were eliminated in later work. The concept *reflex reserve*, which Skinner was later to drop (along with the concept *drive*), is a central organizing principle. Operant conditioning supposedly creates a reserve of responses that are then expended in extinction. The reflex reserve is surprisingly (to me) pervasive throughout *B of O*. Fixed-interval schedules, for example, are conceived as periods of extinction (depletion of the reserve) alternating with reconditioning by reinforcement of a single response (replenishing the reserve).

Consistent with the importance of the reflex reserve, many of the experiments focus on extinction and number of responses in extinction. For example, a normal cumulative record in extinction fits within a smooth negatively accelerated envelope with occasional slowdowns and compensating speedups, still fitting within the envelope. Sometimes a brief disinhibitory stimulus (say, a loud noise or a bright light) will temporarily speed up responding and sometimes will slow it down (and sometimes have no effect). Skinner presents cumulative records indicating that each speedup or slowdown is followed by a compensatory slowdown or speedup so that the total number of responses emitted over the course of extinction remains about constant. Similarly, if the lever is removed from the Skinner box for a brief period during extinction and then reinserted, responding accelerates to make up, as it were, for the lost responses. But even in these early experiments the concept of reflex reserve is not wholly adequate to explain the data. Sometimes a slowdown is never made up. Later experiments, especially those with pigeons, would force Skinner to abandon the reflex reserve altogether.

At the time when he was writing *B of O* it would only have been experimental findings, not theory that could have caused Skinner to give up the concept of reflex reserve. As long as such a concept was behaviorally defined, Skinner saw nothing in principle wrong with it:

¹ The preface to the seventh printing of *B of O*, written in 1966, discusses applications and extensions of the experiments reported in 1938 but mentions neither Herrnstein's nor Premack's work.

The concepts of 'drive,' 'emotion,' 'conditioning,' 'reflex strength,' 'reserve,' and so on have the same status as 'will' and cognition' but they differ in the rigor of the analysis with which they are derived and in the immediacy of their reference to actual observations. In spite of the conceptual nature of many of our terms we are still dealing with an existent subject matter, which is the behavior of the organism as a whole. Here, as elsewhere in the experimental sciences, a concept is only a concept. Whether or not it is fictitious or objectionable cannot be determined from its conceptual nature. (p. 441)

The reason for retaining, in his scientific system of behavior, the concept *conditioning* and rejecting the concept *cognition* is that Skinner felt the latter carried too much contextual baggage; it "pretends to be dealing with a mental world" (p. 441). Skinner's rejection of *cognition* along with other concepts such as *decision*, *value*, *belief*, and *self-control*, was, I believe, a mistake. Skinner was later to offer 'interpretations' of such terms without attempting to define them operationally or use them in scientific discourse. If he had gone on to define such mentalistic concepts in behavioral terms he would have been less open to challenges from philosophers and others claiming that his chosen area of research was excessively narrow and simple, and that it avoided important psychological questions. Thus, currently, the overt behavior of whole organisms that (in my opinion) constitutes their cognitions, memories, imaginations, decisions, and so on, remains, with few exceptions, largely outside the research interests of behavioral psychologists.

Skinner's great insight in *B of O* was the realization that you could investigate reflexes without having to find a stimulus for every response. Previous behaviorists (Pavlov, Hull, and even Tolman), like contemporary cognitive and physiological psychologists, were essentially connectionists, tracing chains of efficient causes from the environment, through the organism, and back out into behavior. Such internal connections, in the form of internal cognitive or neural mechanisms, were the foci of their interest. In his 1966 preface to *B of O* Skinner writes: "Although [their books] are ostensibly concerned with explaining observed behavior in terms of observable conditions and events, both Tolman and Hull quickly became preoccupied with internal processes" (p. xi). Skinner's chapter in *B of O* on the dangers of premature physiologizing (Chapter 12: "Behavior And The Nervous System") could have been written yesterday and is still relevant. Here is a piece of it – on the sources of the temptation to physiologize:

I have already mentioned (in Chapter One) the primitive and yet not altogether outworn view that the phenomena of behavior are essentially chaotic but they may be reduced to a kind of order through a demonstration that they depend upon an internal fundamentally determined system. This is the view which most naturally presents itself as a materialistic alternative to a psychic or mentalistic conception of behavior. The sort of neural homunculus that is postulated as a controlling force bears an unmistakable resemblance to the mental or spiritual homunculi of older systems, and it functions in the same way to introduce a kind of hypothetical order into a disordered world. (p. 418)

The order that Skinner found in the chaos of behavior was not inside the organism at all, but in the relation of the whole organism and the world. Skinner extended behavioral analysis from relations between stimuli and responses to relations between responses and reinforcement. It may be said that Skinner freed behavioral analysis from the necessity to find (or invent) a stimulus for every response; he thus made it possible to avoid explanations of behavior based on internal processes. Instead of S-R relations going on inside the organism we now had R-Rf relations going on between the whole organism and the environment – where they could be observed, in principle, by other organisms.

A question that arises in the extension of Skinner's science of the whole organism to human behavior and human problems is how do you analyze specific responses with neither an external stimulus nor an external reinforcer? The alcoholic's drinking, for example, is clearly reinforced by the drink. But what about the alcoholic's (occasional) refusal of a drink? What reinforces that? There are (at least) two ways in which a behavioral theory can accommodate such behavior; one molecular, one molar. The molecular behaviorist, finding no specific external reinforcer (immediate or delayed) of a specific drink refusal, looks inside the alcoholic for the reinforcer (in the form of internal reinforcement: internal pats on the back, so to speak, or internal gratification, or internal guilt reduction, and so on). The molar behaviorist, also finding no immediate external reinforcer of drink refusal, may expand the concept of response. The alcoholic's drink refusal is considered not as a particular act (reinforced by a particular environmental event) but as a pattern of acts strung out over a wide temporal extent. Such a pattern may be reinforced by a contingent pattern of events (such as social approval) or may itself be of high value (despite the possible negative value of every individual act that makes up the pattern).² Because *B of O* was confined to analysis of a relatively simple response in a relatively simple environment, there was no need at that point to confront this issue. But, as behavioral analysis was extended to important human concerns, the latter view of behavior—the molar, or teleological, view—was rejected by Skinner.

It may well have been Skinner's original roots in reflexology, his adherence to the concept of the operant as a form of reflex, a narrowly defined, physiologically specified response, that prevented him from standing back and adopting a wider temporal outlook on behavior, from seeing operant rate not as the instantaneous slope of a cumulative record but as extended over weeks or months and interleaved, like an alcoholic's rate of drinking, into behavioral patterns with other responses.

Reflexology may thus have led Skinner, and many of his followers, to concepts of internal, essentially private, unobservable entities—exactly what *B of O* warns against. The fact that the internal entities were called discriminative stimuli, makes them no better—no more aspects of “the organism as a whole”—

² The individual act is performed *for the sake of* performing the pattern just as the pianist plays the individual notes *for the sake of* playing the melody. This view of reinforcement is thus teleological as well as molar.

than Hull's r_g - s_g 's or Tolman's cognitive maps of which Skinner complained in 1966, and from which the Skinner of 1938 (when he published his first and best book) might well have shrunk in horror.³

The Operational Analysis of Psychological Terms:⁴ The September 1945 issue of *The Psychological Review* published a series of articles by prominent psychologists on *operationism*, then a dominant concept in the philosophy of science. The series was organized by E. G. Boring, a prominent historian of psychology, who provided a list of questions for the participants to discuss. *OAPT* is Skinner's contribution. Skinner does not address Boring's questions specifically, but one presumes they are addressed in passing. Skinner does discuss (and dismiss) Boring's version of operationism. Skinner says, "...the position taken [by Boring] 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" (p. 284). Skinner's main argument in the article is that private events are legitimate objects of psychological study.⁵

Boring (and his colleague, S. S. Stevens) had been philosophical dualists, following in the footsteps of Fechner and Wundt. They had believed that introspective reports offered a window into the mind (to "private events"). However, the success of operationism in physics convinced them that the only way psychology could become a science was to relate overt behavior (including verbal behavior) to measurable stimuli. The "meaning" of psychological terms (that is, mentalistic terms) was transferred by this sort of operationism from non-physical "ideas" in the head to physical "operations" in the environment. The word, 'red' was no longer held to mean a red idea in an internal, non-physical mind, but a red object in the environment. Skinner argued that although this sort of operationism seems like behaviorism, it fails to ask, let alone answer the critical question, *why* do people say and do the things they say and do? He says,

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.... It is simply not true that an organism reacts to a sign 'as it would to the object which the sign supplants.' Only in a very limited area (mainly in the case of autonomic responses) is it possible to regard the sign as a simple substitute stimulus in the Pavlovian sense. (p. 273)

³ Parts of this section were translated and published in an online journal, *Metapsicologia.com*. (2004). The journal is no longer available online.

⁴ Quotations from *OAPT* are from the version reprinted in Cumulative Record, (1961). This version differs from the original *Psychological Review* article, by containing Skinner's comments on material presented by other contributors.

⁵ He says, "...while Boring must confine himself to my external behavior, I am still interested in 'Boring from within.'" (p. 285).

In other words, red objects are generally not conditional stimuli; when they affect behavior, they are discriminative stimuli. The crucial question is, *why* does the person say, “red”? What is the reinforcer?

Skinner claims, that behaviorists had been the true operationists in psychology. He says,

It is not surprising that the broad empirical movement in the philosophy of science...the background of operationism, should have had a vigorous and early representation in the field of psychology—namely behaviorism.... behaviorism has been (at least to most behaviorists) nothing more than a thoroughgoing operational analysis of traditional mentalistic concepts. (p. 274)

However, he adds, behaviorism, up to that point “...stopped short of a decisive positive contribution...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 [mental] terms” (p. 274).

The difficulty faced by operant analysis when it comes to mental terms, as Skinner sees it, is that:

This scheme presupposes that the stimulus act both on 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* [emphasis in original] stimuli. (p. 275)

Skinner is referring here to private discriminative stimuli. Any operant—a rat’s lever press, for instance—is a response to private stimuli: the neural impulses that activate the muscles. As I indicated in the previous section, the great contribution of *B of O* is that those stimuli are irrelevant to a strictly behavioral analysis.

...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. (p. 275)

This is a crucial point. What is the private stimulus that acts when a person says, “My tooth aches”? Is it the (diseased) tooth or is it the ache? If we understand the private stimulus to be the diseased tooth, there is no problem with public access. A child cries, her mother says, “Where does it hurt?” The child opens her mouth and points to her tooth (or points to her stomach, or her ear, etc.) The mother says, “Oh, you have a toothache.” As experience is gained the child learns the vocabulary of pain.

A toothache is no more problematic for learning the language of pain than a splinter in a finger. It would not be strange if a child were to say, at some point,

before these discriminations were mastered, “I have a toothache in my finger.” The discriminative stimulus may be wholly internal, as with a diseased tooth or visible on the surface, as with a splinter. There is a commonality between the two as between different colored stimuli, and there may also be some neural commonality. But the *basic* commonality, the thing that makes them both pains, is not the physiological one—it is the behavioral one. In both cases, the child is harmed; in both cases the parent (i.e. society) needs to do something about it. That is the basic commonality; that is what makes both of the child’s acts pains. There is nothing in Skinner’s extended discussion in *OAPT* of how terms for internal events may be socially learned, to suggest that, when a person says, “I am in pain,” pain itself is the discriminative stimulus. Moreover, as Skinner says, “A similar analysis could be made of all terms descriptive of motivation, emotion, and action in general, including...the acts of seeing, hearing, and so on” (p. 279). Skinner goes on with emphasis: “A *differential reinforcement* [hence scientific understanding] *cannot be made contingent upon the property of privacy*. This fact is of extraordinary importance in evaluating traditional psychological terms” (p. 279, emphasis in original). For scientific understanding, there must be some public correlate of the internal event.

So far so good—or at least so consistent with *B of O*. But then Skinner makes an oblique reference to Pavlov and Watson. He says, “The original behavioristic hypothesis was, of course, that terms of this sort [“psychological” or mental terms] were descriptions of one’s own (generally covert) behavior” (p. 280). I believe he refers here to Watson’s conception of thought—that a person thinking of hammering a nail is moving her muscles as she would have done if she were overtly hammering a nail, except covertly. People have wasted their professional lives trying to measure such covert behavior and have failed. Seen from the viewpoint of *B of O*, it is a ridiculous idea and does not stand scrutiny. Suppose the muscle was isolated and stimulated on a lab bench. Is it thinking? Can it possibly be thinking? Of course not. Perhaps what Watson really meant was that the thought resided in the proprioceptive nerves emanating from the muscle, or perhaps in the motor nerves going from the brain to the muscle, or perhaps in the brain itself. If the whole muscular circuit were isolated, could it think? If a whole human brain could be isolated and stimulated without a body, could it think? An affirmative answer to this question is given by some modern philosophers—neural identity theorists. According to the web-based Stanford Encyclopedia of Psychology:

The identity theory of mind holds that states and processes of the mind are identical to states and processes of the brain.... Here I take identifying mind and brain as being a matter of identifying processes and perhaps states of the mind and brain. Consider an experience of pain, or of seeing something, or of having a mental image. The identity theory of mind is to the effect that these experiences just *are* brain processes, not merely *correlated with* brain processes.⁶

⁶ <https://plato.stanford.edu/entries/mind-identity/>

This is the theory that Skinner demolishes in the last chapter of *B of O*. The main problem with neural identity theory is that if the mind were *identical* with the brain (or the muscles or the proprioceptive nervous system) it would be essentially a prisoner in the body, forever out of contact with the world. Introspection would be meaningless because there is no other entity inside of us through which the brain may be viewed and no internal mirror by which the brain might view itself. Even if there were such an entity inside of us, if you are a physical monist, it could only consist of more neurons—another, higher set of neurons, again supposed to be *identical* with consciousness—and so on ad infinitum.

An alternative to neural identity theory presented in *B of O* (however briefly), is what might be called, behavioral identity theory (see also Rachlin, 2014). According to this theory, mental terms are abstract conceptions of overt behavior; that is what they just *are*; they have no other reality inside the head or out of it. The great advantage of behavioral identity theory is that it holds mental states to be observable—directly by other people, and indirectly by the actor through reflection from the environment, including the behavior of other people. One does not need to invent a homunculus. Such a theory implies that other people may know our mental states as well as or better than we do. To give a trivial example: A 5-year old child asks me, “What’s your favorite color?” I say, “red.” My wife of some 57 years says, “Are you kidding? All your pants are green, all your shirts are green, all your underwear is green. Your favorite color is green.” Of course, she is right, and I am wrong. The relevant data to answer the child’s question, as for all my mental states, are not in my head, subject to introspection, but in the patterns of my behavior—past, present, and future, subject to direct observation by anyone close to me over a length of time. These patterns *are* my mental states; they are not just effects or outputs of my mental states. The dualist idea that overt behavior may be merely the observable effect of an internal, non-physical mental state, which can be scientifically studied only through analysis of its overt effects, is the methodological behaviorism of Boring and Stevens, which Skinner rightly rejects in *B of O*.

In *OAPT* Skinner never considers the behavioral identity theory of *B of O*. Instead he proposes a modified form of Watson’s covert, neuro-muscular identity theory. He considers (p. 280) the response, “red.” As we have seen above, the discriminative stimulus for that response could have been a question from a 5-year-old child. The discriminative stimulus for the response “red,” as Skinner discusses it in *OAPT*, could have been understood to be part of a psychophysical experiment—say the presentation of an ambiguously colored red-orange circle together with the question: “Is this circle closer to red or orange?” The situation does not demand introspection. Skinner points out that *the discriminative stimulus for the verbal response, “red,” is the physical red object and not an interior image*. I emphasize this because it is just after this (valid) point where he crosses a line.

In considering the response, “I see red,” as opposed to the response, “red,” instead of asking, “Why would a person say, ‘I see red?’” and finding social discriminative stimuli and reinforcement for such a statement (perhaps the

presentation of a very pale red disk and the question: “Do you see any color here?”) he says: “To see red is to react, not to red...but to one’s reaction to red.... According to the present analysis it may be evoked ...by *any private accompaniment* of overt seeing” [emphasis in original] (p. 281).

Skinner speculates on some ways that external reinforcement may act on private behavior (without specifying what form, muscular or neural and, if neural, where in the nervous system such private events take place). These speculations are vague and almost wholly based on adventitious contiguities between internal and external events. I could understand them only by relating them to adventitious reinforcement as it may occur in overt behavior. For Skinner, it seems, the private event is reinforced in the same way that the idiosyncratic *style* of an overt response is reinforced—for example, the batting style of a professional ballplayer. Elbow up or down, bat held vertical or horizontal, etc. If the response as a whole is reinforced (if she’s a good hitter), idiosyncratic aspects of her style would be reinforced as well and would persist. So might internal hitting (or hammering) conceivably be reinforced. As Skinner states (p. 280), this is only a “slight modification” of “the original [Watsonian] behavioristic hypothesis... that terms of this sort [mental terms] were descriptions of one’s own (generally covert) behavior.” Indeed, it is very much like Watson’s conception of these terms. And, it has all the problems of that conception. As Skinner himself notes (p. 283), “...Watsonianism was, in fact, practically wrecked in the attempt to make [private behavior as a stand-in for mental terms] work.”

What does it mean then to say, “I see red”? I am a passenger in a car, sitting next to the driver. There is a red light ahead, but the car isn’t slowing down. I say, “There’s a red light.” The driver says, “Yes, I see it.” The car slows down and stops. *Question*: What is the discriminative stimulus for the driver’s braking, and for the driver’s verbal response? *Answer*: Some combination of what I said and the red light—nothing inside the driver, no internal image of a red light. Alternatively, the driver says, “No you imagined it.” The car does not slow down. There was no red light. What is the cause of my statement that I saw a red light? Nothing inside me. Maybe something in my reinforcement history causing both a general lack of trust of this driver, or a very low threshold for seeing red lights, or a combination of the two. But for sure, not a red light shining in my head. And even if somehow there were a red light shining in my head, who is there to see it? No one, as the author of *B of O* would have said.

Behavior Analysis and Mental Terms: There is, of course, a problem in the use of mental terms in a science of behavior. As in the popular vocabulary, they are often undefined. Dictionary definitions of mental terms almost always refer to mental processes understood as private. From Google:

Perception: the ability to see, hear, or become aware of something through the senses.

Imagination: the act or power of forming a mental image of something not present to the senses or never before wholly perceived in reality.

These definitions are not helpful in a behavioral analysis. However, as I quoted from *B of O* previously:

In spite of the conceptual nature of many of our terms we are still dealing with an existent subject matter, which is the behavior of the organism as a whole. Here, as elsewhere in the experimental sciences, a concept is only a concept. Whether or not it is fictitious or objectionable cannot be determined from its conceptual nature. (p. 441)

In deciding what kinds of concepts are subject to behavioral analysis one may note that in physics the terms: *force, pressure, volume, mass, power, etc.* (to cite only those that come quickly to mind) were common in English long before they were given operational definitions in physics. The earlier meaning of these terms remains in the language along with their scientific meaning. Imagine a physicist who claimed that *force* could not be studied in physics because its dictionary definition was useless, and that everything to which the popular term referred was out of bounds for physical study. It is up to the physicist to determine the referent of *force* within his science, and it is up to the behaviorist to determine the referent of mental terms within *her* science. As examples of how this may be approached, let us consider two such terms: *perception* and *imagination*.

Perception: Perception is *identical* to a correlation over time between a person's overt behavior and an identifiable pattern of events in the environment. *Question:* What is the difference between two people (say John and Marcia), one of them (Marcia) stone deaf, both sitting stock still while a Mozart quartet is playing? *Answer:* John is hearing (i.e., perceiving) the music whereas Marcia is not hearing it. *Question:* What does it mean to hear? *Answer:* To discriminate by overt acts, over time, among sounds and between sounds and silence. That is, a non-zero correlation exists between John's behavior and sounds (unsignaled through other senses) whereas there is no correlation (a zero correlation) between Marcia's behavior and such sounds. In other words, sounds are discriminative stimuli for John, and not for Marcia. During the past, in the presence of sound signals, John's and Marcia's behavior (perhaps including taking audiometric tests) differed and will differ in the future. (Consider their differing reactions to someone rushing into the room behind them yelling, "Fire!") Their identical behavior during the Mozart quartet is merely one congruent point in two drastically different correlations (over time) between behavior and sound.

It could be that Marcia's hearing mechanism is entirely normal, but she is nevertheless unresponsive to sounds. In that case we would say she was "psychologically deaf." Is psychological deafness real deafness? Yes, it is. What counts for deafness as for all psychological (or mental) states, is Marcia's behavior in the long run. If she were faking deafness, then her subsequent behavior would reveal what her state really was. If, despite her normal hearing mechanism, she continued to behave all her life as a deaf person behaves, the question: "Was she

faking deafness or psychologically deaf?” would be entirely non-pragmatic and thus meaningless.

Imagination: Aristotle says, “Imagination must be a movement resulting from an actual exercise of a power of sense” (*De Anima*, Book III, chap. 3, 429a). As far as the overt speech and actions of a person are concerned, imagination is the same as perception. If I am doing a good job of imagining that I smell a rose, I will behave, for a moment, just as I would behave if I smelled a rose. The difference between perception and imagination is that the object is present in the world during perception (the rose is there when you are sensing it), whereas during imagination the object is not present in the world (the rose is not there when you are imagining it). It is not necessary to infer that the rose I am imagining (that would be present in the world if I were perceiving it) is present inside me (as a representation, an internal image, a neural discharge, a muscular twitch, or anything else) when I am imagining a rose. When I imagine a rose, my overt movements with the rose absent are the same as those I would make if a real rose was present. In other words, all is the same in perception (or sensation) and imagination except that when I imagine the rose it is not present.⁷

If you generally behave one way in the presence of, and another way in the absence of, red lights, you are perceiving red lights. However, if, on occasion, you behave in the absence of a red light as you normally do in its presence, you are on that occasion imagining a red light. Imagining is acting and not dreaming: Vividness of imagination is not vividness of interior image but of overt behavior. Suppose two people are asked to imagine a lion. One closes her eyes and says, “Yes, I see it, a mane and a tail, that’s right, it’s walking around,” and so on. The other runs screaming for the door. Although they are both imagining, they are imagining different things. The first person is not imagining a lion, but a picture of a lion. The second person is imagining the lion. The location, intensity, orientation, or even the existence of an image in the head of either of them would be entirely irrelevant to the imagination of either. Neuroscience bears this out. There are no pictures in the head for either of the two imaginers to look at and, even if there were, as Aristotle pointed out, there are no sensory receptors in the head with which to see them. A good imagination is not just an aid or a tool in good acting. Rather, good acting *is* good imagining.

Mental terms are not just a loose way of talking about behavior, nor do they refer to covert events. As *B of O* implies, there are no inner *psychological* causes at all. It is not that the organism is in any way empty, but that the molecular substrate of behavior (its set of inner efficient causes) is the domain of neuroscience, not of psychology. As Skinner emphasized in *B of O*, behavior analysis does not rely on neuroscience but stands on its own as a separate discipline. When some aspect of voluntary behavior is unexplained by current observation, a behavior analyst should look for its explanation, not more deeply in the nervous system but more

⁷ The difference between sensation and perception is a difference in the complexity of the object perceived and not in the discriminatory behavior.

broadly in time—in the reinforcement history of the individual or, if still not found, in the evolutionary history of the species.

A behavioral analysis of mental terms may exist alongside the everyday-life use of mental vocabulary to refer to hypothetical inner states. (In the great majority of everyday-life references to the relative motion of the earth and the sun, the sun is said to be moving. Yet we all believe that the earth is really rotating on its axis.) One morning, we observe a piece of behavior—a person smiles. We say that he is happy, but we do not observe his inner happiness. We observe only the smile and harmlessly ascribe its cause to a state of happiness within his body. For most everyday-life purposes, this is sufficient. But that smile is only one part of his verbal and non-verbal behavior all morning. If that pattern is not there, he was not *really* happy (implies *B of O*; perhaps his wife and children would agree) regardless of his inner neural or muscular state, or his own introspective report.

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