ON CERTAIN SIMILARITIES BETWEEN THE PHILOSOPHICAL INVESTIGATIONS *OF LUDWIG WITTGENSTEIN AND THE OPERATIONISM OF B. F. SKINNER*[1]

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It is my purpose to point out certain similarities between the *Philosophical Investigations* of Ludwig Wittgenstein and the work of B. F. Skinner. In doing this, I hope to stimulate a somewhat deeper appreciation of Skinner's views than is generally found among psychologists at the present time. I hope also to influence the critical appraisal of Skinner's work, so that it might come to bear more cogently upon the position as it has actually developed. I feel that much of the current criticism (*e.g.*, Chomsky, 1959) misses its mark largely because it seems to take for granted that Skinner adopts philosophical perspectives which are in fact inimical to his views. It is my opinion that Skinner's position is more compatible with the later views of Wittgenstein than with other philosophical approaches more widely accepted among psychologists.

Ludwig Wittgenstein is acknowledged by some (*e.g.*, Warnock, 1958, p. 62) to have exerted an influence more powerful than that of any other individual upon the contemporary practice of philosophy.[2] The nature of this influence is suggested by the following comments of G. H. von Wright.

> It has been said that Wittgenstein inspired two important schools of thought, both of which he repudiated. The one is so-called logical positivism or logical empiricism, which played a prominent role during the decade immediately preceding the Second World War. The other is the so-called analytic or linguistic movement . . . [which] dominates the British philosophy of today and has spread over the entire Anglo-Saxon world and to the countries in which Anglo-Saxon influence is strong.

It is true that the philosophy of Wittgenstein has been of great importance to both of these trends in contemporary thought: to the first, his early work *Tractatus Logico-Philosophicus* and discussions with some members of the Vienna Circle; to the second, besides the *Tractatus*, his lectures at Cambridge and also glimpses of the works which he did not publish in his lifetime. It is also partly true that Wittgenstein repudiated the results of his own influence. He did not participate in the world wide discussion to which his work and thought had given rise. He was of the opinion—justified, I believe—that his ideas were usually misunderstood and distorted even by those who professed to be his disciples. He doubted that he would be better understood in the future. He once said that he felt as though he were writing for people who would think in a quite different way, breathe a different air of life, from that of present-day men. For people of a different culture, as it were. (von Wright, 1955, p. 527).

Wittgenstein died in April, 1951. His book *Philosophical Investigations*, which contains his later views principally of concern to us here, was published posthumously in 1953.

In what follows I shall list, and comment briefly upon, 10 specific similarities between the later work of Wittgenstein and the systematic position of B. F. Skinner. However, the preceding remarks of von Wright suggest at once certain preliminary similarities in the professional fortunes of Wittgenstein and Skinner. Both Wittgenstein and Skinner published an early book, each of which was striking in originality of thought and seriousness of purpose, and each of which was destined to exert a dynamic influence in its own field. The reference here, of course, is to Wittgenstein's *Tractatus* and Skinner's *The Behavior of Organisms*. Then also, both of these works bear an interesting relation to logical positivism, although to be sure they are related to that point of view in considerably different ways. The *Tractatus* played a conspicuous role in the very formulation of logical positivism, and although the position it as-

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sumes is more properly associated with the logical atomism of Bertrand Russell than to logical positivism, the work was accepted with only slight reservation by the Vienna Circle as a powerful and exciting exposition of their point of view (see Ayer, 1959, p. 4 ff.). *The Behavior of Organisms*, on the other hand, is related to logical positivism in that it was written by Skinner at a time when he was keenly interested in operational and behaviorist methodology, an interest which he shared at that time with many other experimental psychologists, and which was considerably nourished by notions of logical positivism that were more widely discussed among experimental psychologists than understood. Finally, both Wittgenstein and Skinner came in their later work to develop points of view which are markedly incompatible with what is generally taken to be the logical positivist position. For Wittgenstein, this development involved an explicit repudiation of earlier views. The *Philosophical Investigations* contains a powerful attack upon his own *Tractatus* (see Malcolm, 1954, p. 559). For Skinner, the development has involved principally an unfolding of various implications of his system, some of which would lead Skinner now to present his ideas in a considerably different fashion than in *The Behavior of Organisms*. There is little need for Skinner explicitly to repudiate the troublesome sections of *The Behavior of Organisms*, such, for example, as the discussion of static and dynamic laws. His verbal behavior is simply different now from what it was in 1938, the difference being undoubtedly accountable for in terms of his more extensive experience in the laboratory, his having worked through some of the implications of the analysis of verbal behavior, and his having become aware that writing books is hardly the most efficient technique for manipulating the scientific behavior of psychologists.

Antipathy to Logical Positivism

The preceding point that Skinner's system has developed into an approach fundamentally different from theories consonant with logical positivism is not widely recognized among psychologists, or among philosophers either, for that matter. In fact, it is the frequent practice of simply dismissing Skinner's perspective as a rather bizarre form of logical

positivism, when the philosophical implications of his work are brought up for discussion, that leads me to call attention in this paper to a number of striking similarities between his views and those of Wittgenstein. More specifically, I fear that the widespread influence of logical positivism still present in the outlook of many experimental psychologists leads even those who profess to be Skinner's disciples to misread his work, and hence to fail to understand it thoroughly.

It is true, one should perhaps not work too strenuously the udder of logical positivism in seeking similarities between Skinner and Wittgenstein. Although many of our current professional ills can undoubtedly be attributed to various habits of thinking about the nature of science and of psychology that have been picked up by psychologists over the past 50 years, such cultural influences are legitimately labeled logical-positivistic only for lack of a better word. It has been easy for psychologists to magnify out of all reasonable proportion the importance of logical positivism as a philosophical doctrine, either by accepting uncritically the writings of a number of logical positivists with whom they happen to be familiar, or by looking to the position as a kind of representation of Satan himself. Be that as it may, I offer as a first similarity between the later work of Wittgenstein and Skinner their inharmonious relation to logical positivism. The point merits elaboration.

With respect to Wittgenstein, it need simply be noted that philosophers of the linguistic movement are in general particularly careful to contrast their position sharply with that of logical positivism. Although both Ayer (1959, p. 5 ff.) and Gellner (1959, p. 86) have, for rather different reasons, stressed certain similarities in the two positions, the followers of Wittgenstein have been outspoken in their criticism of logical positivism. For particulars, one should consult Urmson's book *Philosophical Analysis*, which is devoted largely to a detailed criticism of both logical positivism and logical atomism from the perspective of linguistic analysis.

As for Skinner, his most explicit attacks upon logical positivism are to be seen indirectly through his almost bitter repudiation of what operationism was to become for psychology. To be sure, Skinner was strongly stimulated by the publication of Bridgman's

Logic of Modern Physics, and he has stated: "At that time—1930—I could regard an operational analysis of subjective terms as a *mere exercise in scientific method* . . . It never occurred to me that the analysis could take any but a single course or have any relation to my own prejudices" (1945, pp. 291-292). Skinner was apparently surprised to find that a professional interest in operationism was to lead quickly to the arena of intense philosophical debate, a debate with which he was totally unsympathetic. At the center of that arena, of course, stood logical positivism. In 1930 Herbert Feigl, the eminent logical positivist and member of the Vienna Circle, had come to Harvard, and according to Boring "it was he [Feigl] who introduced the Harvard psychologists to the ideas of their own colleague, Bridgman, to the work of the Vienna Circle, to logical positivism and to operational procedures in general" (1950, p. 656). S. S. Stevens at Harvard assumed leadership of the new approach, and he published ultimately in 1939 his paper on *Psychology and the Science of Science*, which Boring has called "the handbook of the new 'psycho-logic' ". Skinner's repudiation of this "new approach" is bitingly expressed in the following remarks.

> 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 United Sciences [note Skinner's mockery of the positivistic interest in Unified Science; see Ayer, 1959, p. 6], but the position taken is merely that of "methodological" behaviorism. According to this doctrine the world is divided into public and private events; and psychology, in order to meet the requirements of a science, must confine itself to the former. This was never good behaviorism, but it was an easy position to expound and defend and was often resorted to by the behaviorists themselves. It is least objectionable to the subjectivist because it permits him to retain "experience" for purposes of "non-physicalistic" self-knowledge. The position is not genuinely operational because it shows an unwillingness to abandon fictions (1945, pp. 283-284).

It seems clear that Skinner erred in feeling that his own interpretation of the *Logic of Modern Physics* would be shared by others. By 1945 he was to state, in commenting upon

the achievement of operationism, "Bridgman's original contention that the 'concept is synonymous with the corresponding set of operations' cannot be taken literally, and no similarly explicit but satisfactory statement of the relation [involved in definition] is available" (1945, p. 270). In fact, the paper from which the preceding quotation has been taken, *The Operational Analysis of Psychological Terms*, can be viewed throughout as a forceful indictment of logical positivism. Consider the following reference to Carnap: "The early [behaviorist] papers on the problem of consciousness by Watson, Weiss, Tolman, Hunter, Lashley, and many others, were not only highly sophisticated examples of operational inquiry, they showed a willingness to deal with a wider range of phenomena than do current streamlined treatments, particularly those offered by logicians (*e.g.*, Carnap) interested in a unified scientific vocabulary" (1945, p. 271). Or this reference to Feigl: "To be consistent the psychologist must deal with his own verbal practices by developing an empirical science of verbal behavior. He cannot, unfortunately, join the logician in defining a definition, for example, as a 'rule for the use of a term' (Feigl)" (1945, p. 277). In still other remarks Skinner's dissatisfaction with the vague ineffectuality of the philosophical reasoning of the period is made clear. Consider the following.

> The operationist, like most contemporary writers in the field of linguistic and semantic analysis, is on the fence between logical 'correspondence' theories of reference and empirical formulations of language in use. He has not improved upon the mixture of logical and popular terms usually encountered in casual or even supposedly technical discussions of scientific method or the theory of knowledge (*e.g.*, Bertrand Russell's recent *An inquiry into meaning and truth*). *Definition* is a key term but it is not rigorously defined. . . . 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 (1945, p. 270).

It is thus clear that Skinner came to view the relation between his own work and the common expression of logical positivism as

anything but harmonious. However, the specific ways remain to be identified in which the views of Skinner and Wittgenstein are jointly incompatible with that position. As a second point in common, then, I call attention to the fact that in their outlook both Wittgenstein and Skinner are essentially non-reductionist.

Anti-reductionism

There is little need to comment at length upon the non-reductionist character of Wittgenstein's later work. The *Philosophical Investigations* contains a painfully careful exposition of the defects of his own earlier atomism (see Strawson, 1954, p. 74 ff.). The strength of the opposition of the linguistic movement to reductionist analysis can be sensed from the fact that one of the significant chapters in Urmson's book is entitled *The Impossibility of Reductionism* (1956, p. 146).

Few seem to realize, however, the non-reductionist character of Skinner's position. Even so, the point has been clearly stated by Verplanck (1954, p. 269 ff., p. 302, p. 307 ff.) and by Wolman (1960, p. 127 ff.). Verplanck emphasizes Skinner's antipathy to "any explanation of an observed fact which appeals 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). For Skinner, facts are little more than what we observe to be the case, and they are generally to be explained by relating them to other facts, not by reconstructing them out of more primary sense-data, as in phenomenalism. Moreover, Verplanck has called attention to Skinner's affinities to non-reductionist Gestalt Psychology, as follows:

> Skinner's approach, then, bears no more than a terminological resemblance to Hull's or Pavlov's, but it is at least first cousin to Kantor's system, which explicitly rather than implicitly accepts a metaphysical position, naive realism, and rejects even the logical possibility of a reductionism. His approach has affinities to Tolman's. Tolman postulates that the so-called laws of perception, derived from phenomenological studies, apply to the rat; Skinner does what amounts to the same thing implicitly, by starting with what comes to him, to all other experimenters, and until proven otherwise, to the experimental animal. Hull, on the contrary, seems to wish . . . to derive 'perceptual' laws on the basis of his reductively stated postulates. Skinner wants to start with a point-at-able world, with point-at-able operations, and to carry on from there. (Verplanck, 1954, p. 308).

That Skinner's concepts have often been misunderstood and misinterpreted probably stems from his choice of a set of terms. . . . To the "Tolmanite", conditioned responses are *mere*, or *mechanical*. To the "Hullian", *expectancy* and *cognition* carry the suggestion of the capricious intervention of entities extraneous to behavior. Skinner has attempted to avoid such considerations, and to eliminate the preconceptions (about what organisms ought or ought not to do) that may flow from the use of terms with extensive connotations. He wishes to find out how animals behave and seeks a vocabulary that will let him talk about how they behave. Because of the existence in Sherrington and Pavlov of sets of data of the kind he believes are needed, he has adopted many of their terms and applied some of their laws in defining his area. As a consequence he has been misinterpreted. In his choice of terminology, Skinner has assured that his works and those of his followers will be read easily by the followers of Hull and Guthrie and only with emotion, if not with difficulty, by those who have selected the organismic-field-Gestalt-force family of words to work with. Skinner's conditioned responses seem to many readers just as *mere* as those of Pavlov or Hull, with the extraordinary result that he has been classed with Hull rather than with Tolman, with Guthrie rather than with Lewin, in his general position (Verplanck, 1954, p. 307).

Indeed, it is tempting to conclude that Skinner is more appropriately classed phenomenologist rather than behaviorist, from the argument implied by the following quotations. Verplanck states: "It is apparent that Skinner's positivism is closer to that of Mach and Pearson than to that of the more recent logical positivists and scientific empiricists" (1954, p. 269). Skinner himself supports this claim by specifically acknowledging his indebtedness to Mach several places in print (e.g., 1931, p. 427; 1945, p. 291; 1961a, p. 319). But Boring states: "Mach fits into the phenomenological tradition and can properly be regarded as a grandparent of Gestalt psychology" (1961, p. 200).

Actually, however, Skinner's position with respect to issues of reductionism is as different from conventional ones as Wittgenstein's, and it is properly understood only within the context of his system as a whole. As with Wittgenstein, the treatment turns about the analysis of verbal behavior, or of the functions of words, and particularly about issues involved when we talk about our own private experience. For Skinner, the treatment of reductionism is made indirectly through a consideration of abstraction and of what he calls "tacting" (see 1957, p. 109 ff.). He comments

specifically upon the traditional issue of re-
ductionism as follows:

> The fact that the process of abstraction appears
> to generate a world composed of general properties,
> rather than of particular events, has led, however,
> to inconsistent interpretations. On the one hand
> the particular event has been regarded as immedi-
> ate experience, while the process of abstraction has
> been said to *construct* a physical world which is
> never directly experienced. On the other hand the
> single occasion has been viewed as a momentary
> unanalyzed contact with experience (1953a, p. 277).

Yet Skinner's concern with the problems
that make reductionism philosophically sig-
nificant is more directly expressed by him as
a concern with dualism, to which topic I now
turn. I offer as a third similarity between
Skinner and Wittgenstein the fact that neither
position is dualistic. In fact, both views are
vigorously *anti*-dualistic, and for much the
same reason.

Anti-dualism

Skinner generally states his objection to
dualism in such words as these:

> It is usually held that one does not see the physi-
> cal world at all, but only a nonphysical copy of it
> called "experience". When the physical organism is
> in contact with reality, the experienced copy is
> called a "sensation", "sense datum", or "percept";
> when there is no contact, it is called an "image",
> "thought", or "idea". Sensations, images, and their
> congeries are characteristically regarded as psychic
> or mental events, occurring in a special world of
> "consciousness" where, although they occupy no
> space, they can nevertheless often be seen. We can-
> not now say with any certainty why this trouble-
> some distinction was first made, but it may have
> been an attempt to solve certain problems which
> are worth reviewing.
>
> There are often many ways in which a single
> event may stimulate an organism. Rain is some-
> thing we see outside our window or hear on the
> roof or feel against our face. Which form of stimu-
> lation *is* rain? It must have been difficult to sup-
> pose that any one discriminative response could
> identify a physical event. Hence it may have been
> tempting to say that it identified a transient
> but unitary sensation or perception of the event.
> Eventually the least equivocal form—stimulation
> through contact with the skin—became most closely
> identified with reality. A form vaguely seen in a
> darkened room was not "really there" until one
> could touch it. But this was not a wholly satis-
> factory solution. Stimulation arising from contact
> may not agree perfectly with that arising visually
> or audibly, and we may not be willing to identify
> one form with reality to the exclusion of the oth-
> ers. There still are psychologists, however, who

argue for the priority of one form of stimulation
and, hence, insist upon a distinction between ex-
perience and reality. They are surprised to find
that "things are not what they seem" and that a
room which looks square from a given angle may
be found upon tactual or visual exploration to be
askew. This difficulty offers no particular problem
here. It is obvious that a single event may stimulate
an organism in many ways, depending upon the
construction of the organism and its capacity to be
stimulated by different forms of energy. We are
much less inclined today to ask which form of
energy *is* the thing itself or correctly represents it.

> Another problem which the distinction between
> physical and non-physical worlds may have been an
> attempt to solve arises from the fact that more
> than one kind of response may be made to stimula-
> tion arising from a physical event. Rain is some-
> thing you may run to escape from, catch in your
> hands to drink, prepare crops to receive, or call
> "rain". Which response is made to "rain in itself"?
> The solution was to construct a passive compre-
> hension of rain, which was supposed to have noth-
> ing to do with practical responses. So far as we are
> concerned here, the problem is disposed of by rec-
> ognizing that many verbal and nonverbal responses
> may come under the control of a given form of
> stimulation (1953a, pp. 276-277).

Yet Skinner's position is not only specifi-
cally anti-dualistic. It is essentially in sympa-
thy with the following discussion of the rea-
sons which underlie Wittgenstein's opposition
to dualism. The remarks are taken from David
Pole's critical work, *The Later Philosophy of
Wittgenstein*.[3]

> The philosophical position Wittgenstein is seek-
> ing to break down might be roughly identified as
> Dualism; though at the risk of leaving the impres-
> sion that he proposes to replace it by something
> that would be called Monism. But Monism, men-
> talistic or materialistic, is for Wittgenstein only an-
> other, deeper error. . . . We ordinarily speak of
> people as perceiving physical things, tables and
> chairs and the like; the dualist speaks of the mind
> as perceiving inward entities, ideas, images or acts
> of will. . . . [It] seems . . . that two separate mistakes
> must be involved. . . . First, we suppose ourselves

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[3] I shall rely heavily in what follows on Pole's (1958)
analysis of the character of Wittgenstein's later
thought. Direct quotation from the *Philosophical In-
vestigations* is often awkward, especially when taken
out of context and addressed to an audience not
trained in philosophy. Wittgenstein's style in the *Phil-
osophical Investigations* is evocative rather than ex-
pository, so that one faces a certain problem in learning
how to read the work, as the quotation found below,
under *Anti-mentalism*, may well illustrate. Relevant
passages in the *Philosophical Investigations* are cited
by Pole.

to be dealing with two ontological realms, when we are in fact dealing with two parts of language; secondly we misinterpret the one, the language-game of inward experience, and force on it the grammar of the language-game of the public world of things. The language in which we speak of private experience is in fact part of a larger, public language, and is learnt in social contexts . . . .

The dualist conceives us as learning these concepts not in any social context, but privately, from our own experience. It follows from the picture he draws that one might live and move wholly in one's own private world; philosophers have often thought of this as our starting point, from which we progress to the discovery of outer things. And indeed it has puzzled them how in the circumstances we are able to make that discovery at any stage: and still more, how we penetrate the private world of any other person. This doctrine supposes the possibility of a private language—a language that will permit references to nothing but the speaker's own experiences. . . . Such a language will be necessarily private. . . . Wittgenstein denies that such a language is possible.

Such is the dualist picture, but it is unworkable; and struggling to free ourselves, we are only the more entangled. Dualism leads on to Behaviorism. For we have been told to think of a man's inner experience, his mental images, his feelings and the like, as forming a class of objects which lie hidden from the rest of us in some closed place; they are objects which only he can perceive. . . . If so they play no part in our lives; if his outward behaviour meets all our ordinary needs, and is all, so to speak, that we shall ever treat with, then the inward object drops out of consideration as irrelevant. . . . The behaviourist, therefore, rejects them as a fiction; and indeed a fiction would serve as well as an entity of which we can know nothing.

Yet Wittgenstein himself has been thought a behaviourist. For, one asks, if Dualism is rejected . . . what other alternative remains? But Wittgenstein does not mean to offer any alternative, any other or newer theory or picture (Pole, 1958, pp. 63-67).

Thus, Wittgenstein sees as the inherent error in dualism a faith in purely private language, which he maintains is impossible. Skinner is strongly in agreement. I shall return to this point shortly. But first, a fourth point of similarity between Wittgenstein and Skinner is suggested by the comments on behaviorism at the conclusion of the preceding quotation. Neither Wittgenstein nor Skinner rejects private events as necessarily meaningless or fictitious entities, in contrast to the usual behaviorist hypothesis. The question may well be raised as to how great the differences have to be between Skinner and what is generally taken to be behaviorism before he can no longer be considered a behaviorist?

The Significance of Private Events

Skinner contrasts sharply his position on private events with conventional behaviorism in the following remarks, which constitute in full the only reference to operationism indexed in *Science and Human Behavior*.

Operational definitions of sensation and image. Another proposed solution to the problem of privacy argues that there are public and private events and that the latter have no place in science because science requires agreement by the members of a community. Far from avoiding the traditional distinction between mind and matter, or between experience and reality, this view actually encourages it. It assumes that there is, in fact, a subjective world, which it places beyond the reach of science. On this assumption the only business of a science of sensation is to examine the public events which may be studied in lieu of the private.

The present analysis has a very different consequence. It continues to deal with the private event, even if only as an inference. It does not substitute the verbal report from which the inference is made for the event itself. The verbal report is a response to the private event and may be used as a source of information about it. A critical analysis of the validity of this practice is of first importance. But we may avoid the dubious conclusion that, so far as science is concerned, the verbal report or some other discriminative response *is* the sensation (1953a, pp. 281-282).

Skinner's avowed interest in private events is by no means merely lip-service. In fact, at the present time perhaps the most exciting of all Skinnerian empirical research centers around frankly perceptual problems, as in the focal concern with what children are actually looking at as they learn to draw an empirical inference or to match color to sample. Anyone familiar with the Hively study (1962) on matching to sample will recognize the relevance to Skinnerian research of the following remarks by Pole on Wittgenstein:

Now ostensive definition may seem to be a process whose significance is unambiguous and self-evident; a sound is simply correlated with an object. Children are taught this way. 'That', one says, pointing, 'is an orang-outang.' And Adam, we may suppose, simply uttered the sound when the first of the species was brought before him.

What we have here is a particular nexus of sounds, gestures and objects; it is false, however, that all this is of itself unambiguously significant. One might point to the door and say 'Go!' Here the same performance has a totally different function. This is not an ostensive definition—and yet it might serve as one after a fashion; for one way of teaching the meaning of 'go' in the imperative

might be this. If a mother points to the milk and says 'white' clearly the child may take 'white' to mean 'milk', and *vice versa*. There are different kinds of game to be learnt here which are by no means uniformly simple and self-evident. The child's error may be corrected, perhaps, by his mother's pointing in turn to the paper and the tablecloth, repeating the word—a fairly complex procedure whose significance has to be grasped. Again one might set objects in pairs as an ostensive definition of 'two'; one might even in certain cases find it the best way to give an ostensive definition to point to something strikingly different and say, 'That is *not* a such and such.'

From all this there are various things that may be learnt. The variety of ways in which words acquire their meanings is reflected in the variety of their uses; the ways in which the forms of language may be meaningful are no less numerous (Pole, 1958, p. 14).

The Impossibility of a Purely Private Language

Let us return now to what I offer as a fifth similarity between Wittgenstein and Skinner, namely, the care with which both argue the impossibility of a purely private language. For Skinner this argument is the central theme of his paper *The Operational Analysis of Psychological Terms*. In this paper he states:

> We have not solved the . . . 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?
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> There are at least four ways in which a verbal community which has no access to a private stimulus may generate verbal behavior in response to it (1945, p. 273).

Skinner then proceeds to elaborate upon what he conceives to be the ways in which the public community commonly teaches people to talk about their own private experience. As he concludes he is led to make the following comments:

> There is apparently no way of basing a response entirely upon the private part of a complex of stimuli. *A differential reinforcement cannot be made contingent upon the property of privacy.* This fact is of extraordinary importance in evaluating psychological terms.
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> The response 'red' is imparted and maintained (either casually or professionally) by reinforce-

ments which are 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. . . .

> 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 expression like '*I see* red' or '*I am conscious of* red'? Here 'red' may be a response to either a public or a private stimulus without prejudice to the rest of the expression, but 'see' and 'conscious' seem to refer to events which are by nature or by definition private. This violates the principle that a reinforcement cannot be narrowed down to a specifically private event by any known method of differential reinforcement. . . . To say 'I see red' is to react, not to red (this is a trivial meaning of 'see'), but to one's reaction to red. 'See' is a term acquired with respect to one's own behavior in the case of overt responses available to the community. But according to the present analysis it may be evoked at other times by *any private accompaniment* of overt seeing. Here is a point at which a non-behavioral private seeing may be slipped in [by others] (1945, pp. 275-276).

The Wittgenstein argument is outlined by Pole as follows:

> But if some one uses the word 'pain' and so doing, speaks to and communicates with other people, it must be the ordinary word, with its public meaning, not the word we gave meaning to by a private reference, that he uses. If we still cling to the dualist picture, and, allowing a public meaning to the word 'pain', yet claim that there is something, even if its nature is inexpressible, that remains behind, we shall still find that the words we are using, so long as we use any at all—here the word 'something'—belong to public speech.
>
> In all this the privacy of sensations is not in doubt; for that is a part of the language-game. 'Sensations are private' is for Wittgenstein a grammatical proposition that we might use in teaching the word. Only one who feels pain can report it, and if he sincerely says that he is in pain he cannot be wrong. Somewhat similarly, someone who has learnt the five-times table can repeat it; but we have seen that in order to repeat it correctly, it is not necessary that he should be at the same time glancing or gazing mentally at a private image. He does not look inward and proceed accordingly. These two cases, that of reporting one's feelings and that of repeating what one has learnt, are very different in other ways; but in both what must be rejected is the notion that a correct report or performance can only follow on an act of inward inspection. . . .
>
> Here we have the crux of Wittgenstein's argument. To speak a language, on his view, is to take part in a certain form of social activity which, moreover, is governed by rules. Hence conduct may be condemned as wrong or irregular; the procedures of an individual may diverge from accepted procedures. We have a standard to which to refer

it. In the case of a private language no such appeal can be possible; there can be no such things as divergent or irregular practice, and hence the notion of such a language is nonsensical (Pole, 1958, pp. 73-75).

It might be helpful to add that when Wittgensteinians speak of the necessity for public "criteria of identification," for public "means of determining right and wrong", or for "condemning conduct as wrong or irregular", they are pointing up what Skinnerians consider to be the function of differential reinforcement in establishing verbal discriminations.

The Behavioral Nature of Language

However, it is not only in connection with the verbal reporting of private experience that Wittgenstein and Skinner are similar in their approach to language. As a sixth similarity, it is to be noted that in both perspectives language is viewed as something natural, with an emphasis on the *effects* of verbal behavior and on the situation in which verbal behavior occurs. Wittgenstein's emphasis upon effects is analogous to Skinner's emphasis upon reinforcement. For Skinnerians, "effects" or "consequences" are much better words for "reinforcement" than is "reward", the popularly used substitute. There is clearly no need to illustrate this point from Skinner. Yet the following quotations from Pole on Wittgenstein are relevant.

> Language, we may say, is the instrument of human purposes and needs; thus, very broadly, Wittgenstein thought of it. . . . It is easy to think of human language as if it were some kind of gift of the gods, like Promethean fire; to give it a status that sets it apart from all the rest of our doings and concerns. Wittgenstein saw it differently. Language is part of the social behavior of the species; it belongs as much to our natural history as walking, eating or drinking. It is created, or evolves, like an institution. Parliaments and the party-system, social and religious ceremonies, cricket matches and competitive examinations are forms or functions of social life; and it is on these analogies that language is to be thought of. And they, in turn, may be compared to the hiving of bees and the nesting and migration of birds. Language presupposes, therefore, a non-linguistic context. It operates against a background of human needs in the setting of a natural environment. These together determine its character. And we must see it and understand it in this way, as involved in a pattern that goes further, if we are to understand it at all (Pole, 1958, pp. 2-3).
> The uses of words are infinitely various: there are orders, questions and reports, prayers and reci-

tations. We lose sight of their variousness and seek to assimilate them. Every word must have a meaning, we say; and then we suppose that this meaning is in all cases some sort of object related to the word, as St. Paul's is related to the name 'St. Paul's'. But in effect we have said no more, Wittgenstein urges, than if we should say that every tool in the tool-box has its use; the use in each case is different. . . .
> Wittgenstein often bids us consider the situations in which our words were first learnt; we shall see then what setting they belong to, what part they play in our lives. One may ask what difference the first introduction of a given word made to the pattern of activities it was brought into. . . . Here we touch on a last important point: for Wittgenstein there is no compulsion about the use of words. We suppose that their existing use obliges us to apply them in such ways in other contexts; we think that the meaning itself demands it. But we may use them as we like; and further they have no more meaning than we have found work for them to do. In face of a philosophical 'must', of some statement that we seem compelled to adopt, we shall ask to be shown its application—what connexions it makes and allows, what language-game it belongs to. 'What can I do with this?' is Wittgenstein's question. . . . In all this Wittgenstein's general aim is to break up the rigidity of our terms of thought. But he disclaims any thesis of his own, he offers no doctrine. He merely describes the various workings of language and lays them before us (Pole, 1958, pp. 27-28).

Opposition to Reference Theories of Language

As a seventh point of similarity, let me call attention to the opposition of both Wittgenstein and Skinner to correspondence or reference theories of language. In developing their views on language, both Wittgenstein and Skinner take as their point of departure objections to the common belief that the chief function of words is to stand for, to name, or to *refer* to objects. Pole on Wittgenstein has the following to say:

> The view Wittgenstein is attacking is that which sees the working of language generally in terms of the function of naming. A name stands over against an object; in some such way, it is supposed, all significant language must be related to some independently existing entity. It is a picture which widely dominates philosophical thinking. It leads us to see the relation of language to reality as essentially uniform, as a relation of correspondence or confrontation. . . . [This] view Wittgenstein . . . repudiates (Pole, 1958, p. 10).
> Wherever non-natural qualities, subsistent entities and the like, are invoked to vindicate the meaningfulness of forms of discourse, the same model is at work: we have a notion, Wittgenstein said, that the meaning of a word is a sort of object

—that to every word there corresponds a meaning, related to it much as St. Paul's is related to the name 'St. Paul's'. . . . It will suffice to recall Wittgenstein's recommendation to seek, not for objects corresponding to words and sentences, but for their function in human life as parts of language. . . . There are other views or assumptions concerning language that those of Wittgenstein's replace or dispense with. [But the] outcome . . . is condensed in a single saying of Wittgenstein's: 'For a large class of statements—though not all—in which we employ the word "meaning" it can be defined thus: the meaning of a word is its use in the language.'

The equation of meaning and use, in the light of our previous discussion, will, I hope, not need much further explanation. If we are asked the meaning of any word . . . we must answer it by exhibiting its function; we must show the kind of work that it does (Pole, 1958, pp. 15-19).

As for Skinner, much of the introduction to his book *Verbal Behavior* is devoted to a discussion of this point. Consider the following:

The existence of meanings [as substantive psychological entities] becomes even more doubtful when we advance from single words to those collocations which "say something". What is said by a sentence is something more than what the words in it mean. Sentences do not merely refer to trees and skies and rain, they say something about them. This something is sometimes called a "proposition" —a somewhat more respectable precursor of speech but very similar to the "idea" which would have been said to be expressed by the same sentence under the older doctrine. To define a proposition as "something which may be said in any language" does not tell us where propositions are, or of what stuff they are made. Nor is the problem solved by defining a proposition as all the sentences which have the same meaning as some one sentence, since we cannot identify a sentence as a member of this class without knowing its meaning—at which point we find ourselves facing the original problem.

It has been tempting to try to establish the separate existence of words and meanings because a fairly elegant solution of certain problems then becomes available. Theories of meaning usually deal with corresponding arrays of words and things. How do the linguistic entities on one side correspond with the things or events which are their meanings on the other side, and what is the nature of the relation between them called "reference"? Dictionaries seem, at first blush, to support the notion of such arrays. But dictionaries do not give meanings; at best they give words having the same meanings. The semantic scheme, as usually conceived, has interesting properties. Mathematicians, logicians, and information theorists have explored possible modes of correspondence at length. For example, to what extent can the dimensions of the thing communicated be represented in the dimensions of the communicating medium? But it remains to be shown that such constructions bear any close resemblances to the products of genuine linguistic activities.

In any case the practice neglects many important properties of the original behavior, and raises other problems. We cannot successfully supplement a framework of semantic reference by appealing to the "intention of the speaker" until a satisfactory psychological account of intention can be given. If "connotative meaning" is to supplement a deficient denotation, study of the associative process is required. When some meanings are classed as "emotive", another difficult and relatively undeveloped psychological field is invaded. These are all efforts to preserve the logical representation by setting up additional categories for exceptional words. They are a sort of patchwork which succeeds mainly in showing how threadbare the basic notion is. When we attempt to supply the additional material needed in this representation of verbal behavior, we find that our task has been set in awkward if not impossible terms. The observable data have been preempted, and the student of behavior is left with vaguely identified "thought processes".

The impulse to explicate a meaning is easily understood. We ask, "What do you mean?" because the answer is frequently helpful. Clarifications of meaning in this sense have an important place in every sort of intellectual endeavor. For the purposes of effective discourse the method of paraphrase usually suffices; we may not need extraverbal referents. But the explication of verbal behavior should not be allowed to generate a sense of scientific achievement. One has not *accounted for* a remark by paraphrasing "what it means".

We could no doubt define ideas, meanings, and so on, so that they would be scientifically acceptable and even useful in describing verbal behavior [as in conventional operationism]. But such an effort to retain traditional terms would be costly. It is the general formulation which is wrong. We seek "causes" of behavior which have an acceptable scientific status and which, with luck, will be susceptible to measurement and manipulation. To say that these are "all that is meant by" ideas or meanings is to misrepresent the traditional *practice* [i.e., for Wittgenstein, *use*]. We must find the functional relations which govern the verbal behavior to be explained; to call such relations "expression" or "communication" is to run the danger of introducing extraneous and misleading properties and events. The only solution is to reject the traditional formulation of verbal behavior in terms of meaning (Skinner, 1957, pp. 8-10).

The chapter on "The Tact" in *Verbal Behavior* contains a section specifically devoted to "The Problem of Reference," and it begins as follows:

Semantic theory is often confined to the relation between response and stimulus which prevails in the verbal operant called the tact. Words, parts of words, or groups of words on the one hand and things, parts of things, or groups of things on the other stand in a relation to each other called "reference", "denotation", or "designation". The relation may be as empty as a logical convention or it

may provide for the "intention" of the speaker. But how a word "stands for" a thing or "means" what the speaker intends to say or "communicates" some condition of a thing to a listener has never been satisfactorily established. The notion of the verbal operant brings such relations within the scope of the methods of natural science. How a stimulus or some property of a stimulus acquires control over a given form of response is now fairly well understood. The form of a response is shaped by the contingencies prevailing in a verbal community. A given form is brought under stimulus control through the differential reinforcement of our three-term contingency [i.e., a discriminative stimulus in the presence of which a response is followed by reinforcement]. The result is simply the probability that the speaker will emit a response of a given form in the presence of a stimulus having specified properties under certain broad conditions of deprivation or aversive stimulation. *So far as the speaker is concerned*, this *is* the relation of reference or meaning. There would be little point in using this formula to redefine concepts such as sign, signal, or symbol or a relation such as reference, or entities communicated in a speech episode such as ideas, meanings, or information (Skinner, 1957, pp. 114-115).

I suggest that it is the difficulty that most psychologists face in overcoming their tendency to view language as a process of referring to things that stands most in the way of their proper understanding of Skinner's analysis of verbal behavior.

The Nature of Meaning

However, two sentences in the preceding quotation suggest an eighth, and for Wittgensteinians a most important, similarity between Wittgenstein and Skinner. Wittgenstein and Skinner are very much alike in their analysis of the nature of meaning itself. For both, there are no such *things* as meanings, where meanings are taken to be mental entities somehow focally involved in communication. For both, a search for meaning can lead only to the study of word usage, to the analysis of verbal behavior as it is actually seen to take place. For both, the meaning *is* the usage. There is little need to illustrate the point from Wittgenstein, other than perhaps to refer the reader again to the quotations from Pole offered above in illustration of Wittgenstein's views on reference. Indeed, it is largely because of the lengths to which Wittgenstein has teased out the implications of his famous dictum "the meaning is the use" that his views are accorded their current philosophical importance.

However, with respect to Skinner, consider first the relevant sentences from the preceding quotation. "The result is simply the probability that the speaker will emit a response of a given form in the presence of a stimulus having specified properties under certain broad conditions of deprivation or aversive stimulation. *So far as the speaker is concerned*, this *is* the relation of reference or meaning." The first of these sentences describes what is seen as the usage of words when verbal behavior is analyzed in terms of the Skinnerian language-game. Thus the second sentence states directly 'that meaning is usage.' Consider the following quotation taken from *The Operational Analysis of Psychological Terms:*

> 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], p. 250). Only in a very limited area (mainly in the case of autonomic responses) is it possible to regard the sign as a simple substitute stimulus in the Pavlovian sense. Modern logic, as a formalization of 'real' languages, retains and extends this dualistic theory of meaning and can scarcely be appealed to by the psychologist who recognizes his own responsibility in giving an account of verbal behavior. . . .
>
> A considerable advantage is gained from dealing with terms, concepts, constructs, and so on, quite frankly in the form in which they are observed—namely, as verbal responses. There is then no danger of including in the concept that aspect or part of nature which it singles out. . . . 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). . . .
>
> What we want to know in the case of many tradiional psychological terms is, first, the specific stimulating conditions under which they are emitted (this corresponds to 'finding the referents') and, second (and this is a much more important systematic question), why each response is controlled by its corresponding condition. The latter is not necessarily a genetic question. The individual acquires language from society, but the reinforcing action of the verbal community continues to play an important role in maintaining the specific relations between responses and stimuli which are es-

sential to the proper functioning of verbal behavior. How language is acquired is, therefore, only part of a much broader problem (Skinner, 1945, pp. 270-272).

When Skinner says, "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)", or "What we want to know in the case of many traditional psychological terms is . . . the specific stimulating conditions under which they are emitted (this corresponds to 'finding the referents')", he is not holding a brief for ostensive definition. He is demanding an analysis of word usage to establish the meaning of a term.

Skinner's interest in the relation between meaning and use is much more for him than a mere statement of theoretical perspective. The relation as he sees it is brought into daily use in the laboratory as a working tool of considerable practical importance. In connection with his practical interest in programmed instruction, Skinner is very much concerned at present with the meaning of such terms as "thinking", "inductive reasoning", and "reading". He must know, for example, what is generally meant by "a knowledge of French" in order to know how to teach French effectively. In this, of course, there is no single entity that can be identified as the referent of "a knowledge of French", nor does a constructed referent emerge out of a complex variety of instances. The method Skinner uses to clarify one aspect of the meaning of "a knowledge of French" is to describe whatever observable situations act as discriminative stimuli to control usage of the phrase in identification. That is, he attempts to describe whatever stimulation we might respond to with such verbalizations as "That is an illustrative instance of a knowledge of French", or "That is the sort of thing we normally take to be evidence of a knowledge of French." To do this is to employ the technique referred to in the above quotation as one which "corresponds to 'finding the referents' ".[4] Consider the following.

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[4]Needless to say, emission of the phrase "a knowledge of French" is not restricted to verbalizations which function as identifications, and descriptions of the discriminative control of verbal identifications are only a small part of the analysis of verbal behavior in general.

We can define terms like "information", "knowledge", and "verbal ability" by reference to the behavior from which we infer their presence. *We may then teach the behavior directly.* Instead of "transmitting information to the student" we may simply set up the behavior which is taken as a sign that he possesses information. Instead of teaching a "knowledge of French" we may teach the behavior from which we infer such knowledge (1961b, p. 383).

Traditionally, for example, something called a "knowledge of French" is said to permit the student who possesses it to do many things. One who possesses it can (1) repeat a French phrase with a good accent, (2) read a French text in all the senses of reading listed [earlier in the paper], (3) take dictation in French, (4) find a word spoken in French on a printed list, (5) obey instructions spoken in French, (6) comment in French upon objects or events, (7) give orders in French, and so on. If he also "knows English", he can give the English equivalents of French words or phrases or the French equivalents of English words or phrases.

The concept of "a knowledge of French" offers very little help to the would-be teacher. As in the case of reading, we must turn to the behavioral repertoires themselves, for these are all that have ever been taught when education has been effective. The definition of a subject matter in such terms may be extraordinarily difficult. Students who are "competent in first-year college physics", for example, obviously differ from those who are not—but in what way? Even a tentative answer to that question should clarify the problem of teaching physics. It may well do more. In the not-too-distant future much more general issues in epistemology may be approached from the same direction. It is possible that we shall fully understand the nature of knowledge only after having solved the practical problems of imparting it (1961b, p. 391-392).

Anti-mentalism

Many people may well feel that Skinner's search for the meaning of "knowledge" is destined inevitably to be a superficial one, even when it is restricted to an interest in the "knowledge of French" as in the above illustration. It may seem to many that Skinner's method is limited to a concern with trivial indices of knowledge, or with what may be judged as mere behavioral representations of a deeper knowledge within. It is true that Skinner might make his survey of the signs of knowledge arbitrarily broad or increasingly subtle, as more and more careful analysis is made of conditions that govern the usage of the word in identification. Nevertheless, even though the very most subtle signs of knowledge were to be described, would they constitute in sum or in part a picture of knowledge

in itself? Is there no difference between *evidence* of knowledge, no matter how subtle that evidence may be, and knowledge itself as it *really is?*

It is Wittgensteinian to reply that questions such as those derive "from our interpreting the language of experience in terms of the language of public objects; for the private realm we [seek] is an attempt to see mental life in the image of the world of common things" (Pole, 1958, p. 65). A Skinnerian might reply, "But after all, knowledge consists only of whatever it is that makes us think we know anything." Even so, Wittgenstein himself responds to questions of this kind, as in the following remarks taken from the *Philosophical Investigations.* The quotation will also serve to illustrate a ninth similarity between Wittgenstein and Skinner, namely, their essentially compatible approach to issues of mentalism.

303. "I can only *believe* that someone else is in pain, but I *know* it if I am."—Yes: one can make the decision to say "I believe he is in pain" instead of "He is in pain". But that is all.—What looks like an explanation here, or like a statement about a mental process, is in truth an exchange of one expression for another which, while we are doing philosophy, seems the more appropriate one. Just try—in a real case—to doubt someone else's fear or pain.

304. "But you will surely admit that there is a difference between pain-behavior accompanied by pain and pain-behavior without any pain?"—Admit it? What greater difference could there be?—"And yet you again and again reach the conclusion that the sensation itself is a *nothing*."—Not at all. It is not a *something*, but not a *nothing* either! The conclusion was only that a nothing would serve just as well as a something about which nothing could be said. We have only rejected the grammar which tries to force itself on us here.

The paradox disappears only if we make a radical break with the idea that language always functions in one way, always serves the same purpose: to convey thoughts—which may be about houses, pains, good and evil, or anything else you please.

305. "But you surely cannot deny that, for example, in remembering, an inner process takes place." What gives the impression that we want to deny anything? When one says "Still, an inner process does take place here"—one wants to go on: "After all, you *see* it." And it is this inner process that one means by the word "remembering".—The impression that we wanted to deny something arises from our setting our faces against the picture of the 'inner process'. What we deny is that the picture of the inner process gives us the correct idea of the use of the word "to remember". We say that this picture with its ramifications stands in the way of our seeing the use of the word as it is.

306. Why should I deny that there is a mental process? But "There has just taken place in me the mental process of remembering. . . ." means nothing more than: "I have just remembered . . . .". To deny the mental process would mean to deny the remembering; to deny that anyone ever remembers anything.

307. "Are you not really a behaviourist in disguise? Aren't you at bottom really saying that everything except human behaviour is a fiction?"—If I do speak of a fiction, then it is of a *grammatical* fiction.

308. How does the philosophical problem about mental processes and states and about behaviorism arise?—The first step is the one that altogether escapes notice. We talk of processes and states and leave their nature undecided. Sometime perhaps we shall know more about them—we think. But that is just what commits us to a particular way of looking at the matter. For we have a definite concept of what it means to learn to know a process better. (The decisive movement in the conjuring trick has been made, and it was the very one that we thought quite innocent.)—And now the analogy which was to make us understand our thought falls to pieces. So we have to deny the yet uncomprehended process in the yet unexplored medium. And now it looks as if we had denied mental processes. And naturally we don't want to deny them.

309. What is your aim in philosophy?—To shew the fly the way out of the fly-bottle (Wittgenstein, 1953, pp. 102ᵉ-103ᵉ).

Wittgenstein is pointing out that the difficulties faced by psychologists or philosophers in their concern with mental processes arise from habitual ways of talking about, of conceptualizing, of thinking about mental events as objects of study. He notes that the language in terms of which we conceptualize much profitable inquiry, as for example in physical science or in practical technology, often breaks down when it is brought to bear upon an interest in mental experience. He calls attention also to our learned aspirations and expectations, which lead us to think it is even possible for us to have knowledge of mental processes and to hope that we shall sometime know more about them in pursuing conventional modes of inquiry. Thus we bear in our very behavior what is easily viewed as an intellectual commitment.

In this, Wittgenstein resists ontology.[5] That is, he resists making claims about the nature of reality, or about the ultimate constituents of which reality is composed. What then is

[5]Whether or not Wittgenstein succeeds in avoiding ontological commitment has been argued (Pole, 1958, p. 18 n.1, p. 100 ff.; Gellner, 1959, p. 103 ff.).

Wittgenstein doing? He is trying to help—flies out of fly-bottles, philosophers out of puzzles (Pole, 1958, p. 98). But what is the status of this reply? In saying that Wittgenstein is trying to help, are we making an ontological claim? Is "trying to help" what Wittgenstein is *really* doing, as opposed, say, to enjoying the display of his own brilliance? Of course, to say that someone is trying to help can be viewed not as a statement about the inherent nature of his motivation but rather simply as a *description*. "Trying to help" may be taken as only one of presumably a variety of descriptions that are applicable.

At the core of much interest in psychological explanation is the hope that an account can be given of the way in which certain ultimate constituents of our psychological nature are interrelated. With sufficient sophistication this would be theory, and it is ontological in character. Such an account Wittgenstein resists. When pressed for an account of the nature of what he is doing, Wittgenstein gives, in the end, only the broadest possible description: he is behaving. "There comes a point, Wittgenstein writes, where he is inclined to say, 'This is simply what I do.'" (Pole, 1958, p. 51).

> 217. "How am I able to obey a rule?"—if this is not a question about causes, then it is about the justification for my' following the rule in the way I do.
> If I have exhausted the justifications I have reached bedrock, and my spade is turned. Then I am inclined to say: "This is simply what I do." (Wittgenstein, 1953, p. 85°).

What has all this to do with mentalism? Skinner frequently attacks an outlook in psychology which he calls "mentalism", and at times he contrasts his position with that of conventional behaviorism on the basis of this issue (see, *e.g.*, Skinner, 1964, p. 106). It is less important here to give evidence of Skinner's antipathy to mentalism than it is to clarify what he means when he speaks of it. For Skinner there are two sides to the coin of mentalism. The first of these is the dualistic separation of the physical and the mental into two ontological realms, an issue which we have already discussed. The second side of the coin, and the one which bears the similarity to Wittgenstein now under consideration, has to do with the practice of reifying terms generally thought to refer to psychological or behavioral processes. For Skinner, it is mentalistic to look at such words as "attending", "inferring", "observing", "trying", "deciding", "remembering", *etc.*, as identifying psychological acts, states, or processes which correctly map the underlying structure of our psychological nature. It is here that he resists ontology. For Skinner these terms are viewed as part of the language with which we ordinarily make sense out of behavior, and if we are to account for the behavior to which they are relevant we must first analyze the control of these terms as aspects of verbal behavior. Wittgenstein captures very nicely the spirit of Skinnerian anti-mentalism on paragraphs 305-306, quoted above from the *Philosophical Investigations*. It will suffice to give only one illustration of Skinner's efforts to avoid the ontological implications of mentalism. Consider the following discussion of "tendencies" and "readinesses" to behave.

> A science must achieve more than a description of behavior as an accomplished fact. It must predict future courses of action; it must be able to say that an organism will engage in behavior of a given sort at a given time. But this raises a special problem. We want to believe that a prediction is in some sense a description of a condition at the moment—before the predicted event has taken place. Thus, we speak of tendencies or readinesses to behave as if they corresponded to something in the organism at the moment. We have given this something many names—from the preparatory set of experimental psychology to the Freudian wish. Habits and instincts, dispositions and predispositions, attitudes, opinions—even personality itself—are all ways of representing, in the present organism, something of its future behavior.
> This problem cannot be avoided in any scientific account, but it can be expressed much more rigorously. We are dealing here with a question of probability—specifically, the probability that an organism will emit behavior of a given sort at a given time. But probability is always a difficult concept, no matter in what field of science it arises. What is a probability? Where is it? How may we observe it? We have tried to answer these difficult questions by giving probability the status of a thing—by *embodying* it, so to speak, within the organism. We look for neurological or psychic states or events with which habits, wishes, attitudes, and so on, may be identified. In doing so we force extraneous properties on behavior which are not supported by the data and which may be quite misleading (Skinner, 1953*b*, p. 69).

Interest in Description

A tenth similarity, and one that is the last to be mentioned here, is that both Skinner and Wittgenstein have viewed their work as

essentially descriptive in nature. Skinner is widely held among psychologists to regard his system as purely descriptive, as opposed to hypothetical or theoretical, in approach (*e.g.,* Hilgard, 1956, p. 101). Concerning the basically descriptive character of his system, Skinner has had the following to say in *The Behavior of Organisms:*

> So far as scientific method is concerned, the system set up in the preceding chapter may be characterized as follows. It is positivistic [in the Machian sense]. It confines itself to description rather than explanation. Its concepts are defined in terms of immediate observations and are not given local or physiological properties. A reflex is not an arc, a drive is not the state of a center, extinction is not the exhaustion of a physiological substance or state. Terms of this sort are used merely to bring together groups of observations, to state uniformities, and to express properties of behavior which transcend single instances. They are not hypotheses, in the sense of things to be proved or disproved, but convenient representations of things already known. As to hypotheses, the system does not require them—at least in the usual sense (Skinner, 1938, p. 44).

However, a number of psychologists have found it difficult to conclude that Skinner's system is as purely descriptive as one might hope. Consider, for example, the following comments by Chaplin and Krawiec:

> Skinner's willingness to grapple with the age-old problems of human behavior demonstrates the lure of theory. Despite his "antitheory" bias, Skinner, in company with every psychologist who has tried to organize and systematize the data of behavior, finds it necessary to bridge the all-too-frequent gaps in our knowledge of human behavior by appealing to theory. We do not mean to imply that Skinner has fallen into what he himself considers the traditional "errors" of seeking explanations in the nervous system, on the one hand, or appealing to intervening variables, on the other. Rather, Skinner's theorizing takes two forms: First, he accepts the ready-made skeleton of conditioning theory as the structural framework of his system, and, second, by a process of logical reasoning, he is willing to extend or extrapolate the principles of operant conditioning to everyday problems of human behavior. This, we submit, is theory (Chaplin and Krawiec, 1960, p. 250).

Hilgard appears to feel that even were Skinner to succeed in formulating a purely descriptive system, the system itself would then not genuinely mediate practical application. It is as if he believed that predictions can legitimately be made only through the deductive implications of theory. Is it the case that Skinner's system is hopelessly dependent in its utility upon some such factor as the experimenter's ingenuity, his insight, or what Chaplin and Krawiec identify above as "logical reasoning"? In the influential 1956 edition of his *Theories of Learning,* Hilgard concludes his analysis of Skinner's position in the following way.

> The practical use of the system is based on the complementary principles of control through presenting and withholding food reward for a hungry animal. The supplementary principles of stimulus discrimination and response differentiation suffice to inaugurate the method of successive approximations. Beyond that, all that is needed is the experimenter's ingenuity. It is not necessary to worry about anything precise in the way either of experimental data or of correlated principles. From the point of view of a theoretical achievement this is really a pretty modest extension of Thorndike's law of effect. Whether or not a child can be taught to write a limerick by the same methods can only be known when the limerick gets written. The theory does not propose to predict[6] (Hilgard, 1956, p. 119).

Professional usage of "logical reasoning", "extrapolation", and "analogy" in commenting upon Skinner's practices of prediction and explanation has possibly been stimulated by Verplanck (but see also Skinner, 1953a, p. 39).

> Does the system mediate the application of knowledge to new situations? Does it predict?
> Some systems or theories of behavior lay great stress on their ability to predict the outcome of planned experiments—often taken to be *experimenta crucis.* It is not surprising, however, that a theory of restricted empirical basis, "informally stated", and "inductively" developed does not generate rigorous predictions about the behavior to be observed in novel situations. In fact, such statements as these must be qualified. Several aspects of Skinner's view of the problem of prediction and extrapolation must be treated individually. Although one of our more conservative theorists when he is making statements *about* prediction, Skinner is more willing than most to extrapolate his concepts from the situations in which they have been developed to some of the more intricate cases of human behavior. This willingness is clearly indicated in the title of the treatise, *The Behavior of Organisms,* which deals with the white rat in the Skinner-box.
> Two situations may be distinguished in which a systematist may wish to make predictions. The first

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[6]Hilgard would possibly attempt to substantiate this cryptic remark by reference to Skinner's challenging remarks on prediction in *The Behavior of Organisms* (1938, p. 10 f.).

is that in which a logical or operational analysis shows that the "same" variables that have already been isolated and studied in the laboratory are operative elsewhere in the "same" relationships that have been investigated. The data are in, and the theorist simply asserts the genotypicality of the situation, applies his theory, and "predicts" the course of events. In the other situation, familiar variables may be encountered, but in novel configurations, the theorist is required to generate statements that go beyond those he has already made. Or, again, new variables may be encountered, and again, prediction may be called for. While Skinner will predict, or rather extrapolate freely where a logical analysis reveals familiar variables acting in familiar ways, he will not predict at all under other circumstances. As a consequence it is possible to find no predictions at all of the behavior of rats, or of pigeons, when novel combinations of stimuli are presented to them in the Skinner-box, and many predictions among Skinner's writings with respect to human behavior in a social environment. The great difficulty is that these predictions are usually unverifiable, because of the complexity of the situation and the consequent impossibility of meaningful experimental test. Prediction, then, is represented by extrapolation, by analogy; its use for the generation of propositions that may be put to experimental test is avoided (Verplanck, 1954, pp. 310-311).

It is clear, then, that various psychologists have been unable to view Skinner's system as one that successfully avoids theory and is purely descriptive in nature. Moreover, the imputation has been made that in certain ways practical application of the system is not free from dependence upon extra-systematic, unspecified, and vague mental operations. Very much the same kind of criticism has been directed at Wittgenstein's attempt to avoid metaphysical involvement by what purports to be simple description. An explicit charge of mysticism has been made by Bertrand Russell (1959, p. 14; see also Ayer, 1959, p. 5). A similar view by Gellner (1959) is elaborated into an accusation of general intellectual inadequacy. Even Pole, whose remarks have been quoted here at length in exposition of Wittgenstein's views, is led to the following critical conclusion.

Wittgenstein disclaimed any intention of propounding a philosophy of language [*i.e.*, theory of the nature of language]. To me it seems that he has done so whether he intended it or not. To tell us that languages resemble games; that the use of language is as much part of our natural history as eating or walking; that it is a particular form of public activity, interwoven with others and subject to

rules; that language consists of an infinity of different parts, like so many tools, each working in its own way, in its own context—all this, I suggest, is to do as much in the way of a general characterization as one could ask of any philosopher who had made that his avowed object. If so much is granted, a metaphysician will naturally wish to know why language should be the only subject matter that lends itself to such treatment. Surely there may be other fields of inquiry where no less illumination may be got from a non-empirical, reflective investigation. But that is a line of argument I shall not pursue. My concern here is rather to ask whether the present picture of language, upon which, for Wittgenstein, everything turns, is not itself inadequate and vulnerable to criticism.

Wittgenstein characterizes his own work as descriptive. He explains nothing, he says, he merely lays before us the different parts or segments of language, and points out the actual use of different terms. He thus establishes for himself something like a Socratic immunity from criticism; he professes to know nothing—or nothing beyond what other people can see for themselves. But the claim, or rather the disclaimer, may be subject to suspicion in the one case no less than the other. What Wittgenstein seems in fact to require of us, when he sets out these things, is to understand the working of a mechanism; for he tells us time and again to think of a word as a tool—there is no saying of his that occurs more often. Wittgenstein's crucial concept is of a language-game, a complex or system of linguistic activities; and every such game must be understood individually, for each works to its own end and in its own particular setting. Our endeavour in each case must be to see or grasp a unique pattern, a system of relations; and language contains at least as many such games as—to take not too remote an analogy—there are concepts to be unfolded in the Hegelian Dialectic.

Now it seems that it would be possible to describe the action of a machine, to specify all its movements correctly, without understanding its working principle. And again one might describe all the procedures followed by the various players in a game without in any way appreciating the pattern. Yet that is what above all Wittgenstein requires us to appreciate. For consider the great purpose of all this—this descriptive setting-forth of language-games. It is to bring us to see that some particular move which we took for a move in the game has no proper place in it. Such a move is to be shown as failing to connect with the rest of the pattern. Wittgenstein compares it to a wheel spinning idly, disengaged from the machine it should belong to. Here we have a luminous metaphor—and yet no more than a metaphor. For there can be no way of testing whether this or that linguistic wheel has failed to engage, except to grasp the pattern in each case; to arrive at some sort of insight into that unique set of relations which it professes but fails to form a part of. . . . [It must be] agreed that Wittgenstein in fact requires something more than a mere description, or the acceptance of a description, [in order for his views to find application] (Pole, 1958, pp. 79-82).

In what follows I shall comment briefly upon the preceding criticisms of Wittgenstein and Skinner. To consider Skinner first, it seems clear that a particularly troublesome aspect of his published work centers about the way in which his system mediates prediction. However the problem of prediction is but one part of the even more sensitive problem of how his system mediates explanation in general. The above quotation from Chaplin and Krawiec misses its mark by speaking as if any specialized use of language in interpretation and explanation must be taken to be theoretical. Ontological properties are attributed not only to theory, presumably as distinguished from description, but also to such entities as logical reasoning and extrapolation, possibly taken either as mental processes or as *a priori* forms of knowing. Verplanck's intelligent remarks on prediction and explanation can easily be read from two different points of view: on the one hand that of Chaplin and Krawiec, and on the other hand that of Skinner. In a Skinnerian reading, Verplanck's statements about prediction are taken as having been emitted by him chiefly under the control of his own observation of practices of scientific prediction; in other words, these statements are read as descriptions. "Logical analysis", "extrapolation", and "analogy" are read in the Skinnerian fashion as loosely distinguished classes of responding, verbal or otherwise.

The issue here is whether explanations and predictions are properties of scientific systems in themselves or whether they are aspects of human functioning. Are predictions about what is to be observed properties of formally organized words and symbols or are they varieties of human behavior? For neither Skinner nor Wittgenstein is the problem in this regard to determine which one of these alternatives is *really* the case. We have only to decide which of the two language-games we wish to play. For Skinner, the preference is to view explanations and predictions as aspects of human behavior. Their nature can be understood only after examining the variety of factors controlling usage of the words "explanation" and "prediction". To raise questions concerning how a particular explanation happens to have been given is to inquire about the behavior of the person offering the explanation. To raise questions concerning how predictions happen

to be made is to inquire about the behavior of making a prediction. To raise questions concerning how predictions *should* be made is to invite behavioral control in the form of advice. To raise questions concerning the *adequacy* of an explanation is to inquire about the effects of the explanation upon the behavior of persons who entertain the explanation. Of course, to adopt such a perspective as Skinner's does not diminish general interest in such questions as whether or not professional psychologists happen to agree with, to be stimulated by, or to like Skinner's explanations, or whether or not a study of his work enables them to predict and control more effectively those human activities in which they are interested.

The critical comments by Hilgard quoted above, while relatively unappreciative of the breadth and originality of Skinner's thought, are nonetheless not unperceptive. They reflect at heart a concern with Skinner's obvious affront to the time-honored canons of respectable procedure in psychological science. Thus, Hilgard stands as the champion of those who find distasteful the apparent untestability of many Skinnerian explanations. Yet for Skinner, his explanations are after all no more than instances of his own verbal behavior. If what he has to say, either by way of what is likely to be called explanation or not, poses a problem to psychologists, then he sees only one avenue of interesting and effective comment as open: an analysis must be made of the variables that may be considered to have controlled his emission of the troublesome verbal behavior in question. This is, of course, to take a uniquely Skinnerian course of action; but it is also to bring intellectual activity to bear upon the problem as it exists as an aspect of the observable world. How would one approach the task of attempting to determine the variables which control, for example, that aspect of Skinner's verbal behavior that appears to be explanatory? It is immediately seen that no one actually knows very much about how to give an empirically oriented accounting of what people happen to say, whether the particular verbal behavior is generally accorded intellectual significance or not. Nor do we know very much by way of fact about what scientists do when they succeed in effecting a new measure of control over natural events, or when they come to feel

they have attained a more adequate understanding of observed phenomena, or when they conclude that an explanation that has been offered is a satisfactory one.

It is distinctively Skinnerian to urge the empirical study of whatever intellectual activities happen to characterize successful scientific behavior. It is true that many scientists, and notably psychologists, *think* they know what science is (to speak in terms of the name-relation), or *think* they know what is involved in legitimate scientific explanation and in the application of scientific method. However, their *thoughts* in this respect have still to be accounted for just as much as Skinner's even if the value in trusting such conventional views were not to be questioned. It is hardly necessary to elaborate upon how sharply conventional attitudes towards scientific explanation have been questioned, even recently and by the most competent of scientists (see Polanyi, 1958). To fail to view the problem of explanation in this fashion, as *inescapably* an empirical and behavioral problem, is perhaps to miss the force of what well may be Skinner's major contribution to psychological thought.

However still, asks the philosopher, how is it that some of Skinner's statements work upon us as explanations whereas others do not? This is the kind of question that plagues Pole when he reads Wittgenstein. When Wittgenstein says "This is simply what I do," he stops talking. Wittgenstein can say no more; yet the listener to whom he speaks remains transfixed by the meaning and significance of his remark. How is it that he can stop talking? Will he not take the next step and inquire *how* the miracle of language has taken place? Pole itches to know, as do many others, the how and what of language. Very much as a psychologist he asks, in effect, "What are the properties intrinsic to human functioning that enable effective communication to take place?" Effective communication seems to *depend upon* certain conditions.

Wittgenstein would certainly contend that to raise questions of this kind is to play a particular type of language-game. To use language of this sort involves us in speaking of a world broken apart into effects and their antecedent conditions. Of course, this particular game is one that interests Skinner, unlike Wittgenstein. In contrast to a view popularly

attributed to Hume, Skinner holds that certain events are *seen* to have their effect upon other events. One can *see* effects and dependencies in nature. For Skinner, natural controlling contingencies are observed to take place. Yet the perception in this is not totally trusted: the disposition is there to regard whatever is seen as dependent upon a previous history of reinforcement. What is seen in observation is simply a part of the behavior-game a man plays, a behavior-game that is often critically and dangerously linguistic. Thus, he considers his own perceptions of natural contingency to be under the control of the same natural processes he hopes through observation to be able to understand. He hopes, moreover, that the observation of natural contingency will have, in turn, its own effect upon the success with which he is able to understand and control other events. For Skinner, an individual whose behavior is controlled more by the *observation* of natural contingency than by any other kind of factor is, in an interesting way, properly called a scientist. To my view, such a statement of simple hopes, and fears, and faiths, even when coupled with strong conviction, is not ontology. Such a statement is a verbal description of behavior. With this comment I shall rest my case.

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