III

SIGNS ABOUT SIGNS ABOUT SIGNS

It is with some hesitation that I comment on some of the comments on *Signs, Language, and Behavior*. For commentaries piled on commentaries find an ever-narrowing circle of readers and the ladder of signs becomes more shaky the higher it ascends. Perhaps it would be wisest merely to appropriate the insights of my critics[1] in later writings. But we philosophers are gluttons for words and not as wise as our admonitions.

To keep our bearings we must recall the purpose of *Signs, Language, and Behavior*. The aim of the book was to further the development of "a comprehensive and fruitful science of signs". The book was to be a prolegomenon to a natural science of signs, and not the actual presentation of such a science. The development of the science itself was envisaged as a task for "many investigators working in many fields for many generations". It was felt that a philosopher could contribute to this development by attempting to clarify certain methodological issues, by striving for a more precise terminology, by bringing together material from many different fields of study, and by suggesting some of the ways in which semiotic might prove to be of high theoretical and practical importance.

The book was therefore written for a mixed audience – for those who wish to participate in constructing an experimental science, for those

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[1] In *Philosophy and Phenomenological Research*: the review by Daniel J. Bronstein, Vol. VII (1947), pp. 643-649; "The New 'Semiotic'", by Arthur F. Bentley, Vol. VIII (1947), pp. 107-131; "An Introduction to the Phenomenology of Signs", by John Wild, Vol. VIII (1947), pp. 217-233; "Logic and Semiotic", by Elaine Graham, Vol. IX (1948), pp. 103-114. In other journals: "Peirce's Theory of Linguistic Signs, Thought, and Meaning", by John Dewey, *The Journal of Philosophy*, Vol. XLIII (1946), pp. 85-95; "The Limitations of a Behavioristic Semiotic", by Max Black, *The Philosophical Review*, Vol. LVI (1947), pp. 258-272; "The Semiotic of Charles Morris", by Philip Blair Rice, *The Kenyon Review* (1947), pp. 303-311. To these may be added the reviews by David Rynin in the *Journal of Aesthetics and Art Criticism* (1947), pp. 67-70, and W. K. Wimsatt, Jr. in the *Quarterly Review of Literature* (1946), pp. 180-185.

who look to semiotic for its possible bearing on their own diverse fields of interest, for those who seek to advance the integration of knowledge. The attempt to nourish such a variety of needs influenced the language and the content of the book itself. It is too analytic for some readers, not precise enough for others, too dry for some, too speculative for others. In saying that it "sketches a program more than it records an achievement" (p. 246), the book clearly is presented as something to be modified, amplified, rectified, superseded.

I stress this point because some of its critics have measured the book by claims which it did not profess. Bentley banishes it as "a failure" because it does not justify the claim which he wrongly attributes to me, in his words, that of "building a scientific language, and thus creating a science". On page 247 of *Signs, Language, and Behavior,* it is stated that "a terminology is not . . . a science", and that further studies "may even make desirable drastic changes" in the present terminology. Black writes as if my statement that "technical semiotic must provide us with words that are sharpened arrows" is a claim that the present book is without terminological defects. The book is quite explicit in expressing a hope "to reduce for scientific purposes" the "vagueness and ambiguity" of current distinctions (p. 4). That it is in Black's words "a quiver brimful of sling shot" rather than a set of perfectly sharpened arrows is of course correct. But David used sling shot on Goliath with considerable effect. Precision is a matter of degree, as Black made quite clear in his 1937 paper on "Vagueness"' in *Philosophy of Science.*

1. THE GENERAL PROBLEM OF TERMINOLOGY

It is a truism that any inquiry about existing signs, or any proposal for the future use of signs, is itself carried on in part by signs which are not themselves analyzed in that particular inquiry. Let us distinguish these two classes of signs, relative to a given inquiry, by the terms *analyzed sign* and *unanalyzed sign.* The unanalyzed signs of a given inquiry may be those of an established science or they may be terms of the everyday language in which the inquiry is being carried on. Thus an English chemist may, in proposing a terminology for chemistry, use without analysis terms in physics and such words as 'it', 'no', 'thing', 'become', 'cause', 'produce', etc.

The question of the adequacy of the unanalyzed terms depends on the purpose for which they are used. Where the purpose is the scientific one

of getting warranted assertions it is important that the unanalyzed terms be such that those who cooperate in the inquiry are in wide agreement in the application of the terms under given conditions. Then on the basis of such unanalyzed terms the terms of the science in question are analyzed, either completely analyzed ("defined"), as when the necessary and sufficient conditions for their application are stated, or partially analyzed, as when sufficient but not necessary conditions for their application are stated by the use of the unanalyzed terms.

In *Signs, Language, and Behavior* it is suggested that the scientific unanalyzed terms of semiotic be drawn from the biological and the physical sciences, and that such terms as 'idea', 'mind', 'thought', and 'consciousness' be excluded from the *unanalyzed* terms because of the notorious contemporary disagreements in their application. The biological orientation of *Signs, Language, and Behavior* is therefore *primarily methodological* and does not involve a defense of "behaviorism" as against "mentalism" considered as psychological doctrines. Nor does it involve starting with rats or dogs as some of my critics seem to imply; the organisms of babies or philosophers would do just as well.

Signs, Language, and Behavior thus takes as unanalyzed such terms as 'organism', 'reaction', 'muscle', 'gland', 'sense-organ', 'need', 'energy', etc. There are of course many other unanalyzed terms in the book: 'behavior', 'produce', 'possible', 'condition', 'cause', 'influence', 'art', 'religion', 'situation', 'reader', etc., etc. Some of these latter terms are combined with the unanalyzed terms drawn from the biological and physical sciences in order to analyze (partially or completely) the terms proposed for semiotic, *but some are not.* Thus 'cause' and 'condition' are so used; 'behavior' is employed in the preliminary analysis of 'sign' (p. 7) but not in the attempt to give a more precise analysis (p. 10).

Since students of behavior differ as to the terms 'stimulus' and 'response' these terms were defined (following Clark Hull). Four other terms were defined: 'preparatory-stimulus', 'disposition to respond', 'response-sequence', and 'behavior-family' ('behavior' is not employed in the definition of 'behavior-family'). With the help of these terms a semiotical terminology was proposed. 'Sign' was partially analyzed (not defined). Then there were given definitions of 'interpreter', 'interpretant', 'denota-tum', 'significatum', 'denotes', 'signifies', 'sign-vehicle', 'sign-family', 'signal', 'symbol', 'lansign', 'ascriptor', 'designator', 'appraisor', 'prescrip-tor', 'formator', etc. Such is in outline the terminological structure of *Signs, Language, and Behavior.*

It seems to me that the *method* used in developing this terminology

is scientifically correct. Some of the criticisms directed at the terminology are based on a confusion of the analyzed and the unanalyzed signs in my account, or on a confusion of unanalyzed signs which play a role in the development of the semiotical terminology and those which do not. These confusions make my terminology appear to be looser than the facts warrant. There are of course a host of legitimate and difficult problems which further advance in semiotic must meet. The term 'sign' itself may serve as an example.

2. WHEN IS SOMETHING TO BE CALLED A SIGN?

Our initial task in analyzing 'sign' is to state a set of conditions under which we will call something a sign. If in doing this we are not arbitrary, but keep one eye cocked on what often *are* called signs, the analysis proves to be very difficult. The earliest formulations by students of behavior – that a sign is any substitute stimulus or that a sign is any stimulus which influences a response to some other stimulus – were found to be too simple. The formulation in *Signs, Language, and Behavior* meets some of the difficulties of earlier formulations, but is itself rather complex and presents new problems.

Two partial analyses (not definitions) of 'sign' were given in the book, the first for preliminary orientation, the second as an attempt to move toward greater precision.

The first formulation (on p.7) was as follows: *"If something, A, controls behavior towards a goal in a way similar to (but not necessarily identical with) the way something else, B, would control behavior with respect to that goal in a situation in which it were observed, then A is a sign."*

It was held that this formulation is adequate for many purposes, but that for experimental purposes a more precise formulation was desirable in order to clarify the terms 'similarity of behavior' and 'goal-seeking behavior'. A second formulation, admitted to be tentative, was suggested (p. 10): *"If anything, A, is a preparatory-stimulus which in the absence of stimulus-objects initiating response-sequences of a certain behavior-family causes a disposition in some organisms to respond under certain conditions by response-sequences of this behavior-family, then A is a sign."*

This formulation, worked out after months of discussion with Clark Hull, Edward Tolman, and some of their co-workers, does not make

use of the term 'behavior', does not require that an organism react to a sign as it does to what the sign denotes, does not require identity of reaction in the situation in which the sign appears and the situation in which it does not appear, and does not require at the moment at which something is a sign that there be any response at all (in the sense of muscular or glandular reactions). In this, and in other ways, the new formulation seemed to me, and still seems to me, to mark a substantial advance.

But is pays for this by an unwieldy complexity. Critics have felt that the precision it introduced at some places is jeopardized by the lack of precision in such terms as 'disposition' and 'need' (a term employed in the definition of 'response-sequence'). Two years of work with the second formulation has shown me its inadequacy in certain respects. I agree with Philip Blair Rice that it may be better to stress reactions more and responses (as a specific kind of reaction) less. I agree with Max Black that the relation between goal-objects and signs is not clear in the second formulation; certainly we do not want to require of a sign (such as 'black') that it signify only goal-objects, and if my second formulation does this the term 'response-sequence' needs modification by dropping out the reference to goal-objects (the first formulation certainly does *not* require that signs signify only goal-objects). I agree with Arthur Bentley that the second formulation becomes unwieldy. The term 'sign' clearly needs a better grounding than I have given it, or than I know how to give it.

3. REPLY TO MR. BLACK

Black, an able critic, occasionally wastes a good arrow in the empty air. Thus he "rubs his eyes" at the vagueness of the word 'influence' in the phrase "influences a response to some other stimulus" and aims this verbal arrow: "It would be absurd to say that the stimulus to the morphine injection *denotes* a rabbit which is yet to be presented to the dog or *signifies* the presence of food. Nor is there the least difficulty in generalizing the point: *Any* stimulus which has some causal influence upon subsequent behavior will, if we insist upon the letter of Morris's definition, have to count as a sign." But the simple fact is that the phrase "influences a response to some other stimulus" is part of the definition of 'preparatory-stimulus' and *not* in itself a definition of 'sign'. *Signs, Language, and Behavior* makes clear that not *all* preparatory-stimuli are signs (p. 9) and uses on page 7 the very

example which Black uses of a drug influencing responses to later stimuli as a reason for *not* analyzing a sign as "any stimulus which has some causal influence upon subsequent behavior".

Black makes much of an alleged circularity and arbitrariness in my account. His argument is complex and I will not attempt to reproduce it in full. In terms of the argument he claims that my method is defective since the criterion of meaningfulness which I employ is not really an empirical one but depends upon a preference in the choice of a metalanguage. In Black's terms:

> If the denotata associated with a set of given preparatory stimuli are all describable by means of a predicate available in the English language (or whatever language the semiotician prefers), the stimuli will be "signs" and otherwise, not. Since there is no way to identify the "disposition to respond" or the "behavior-family" except by finding a set of associated denotata, verification of the alleged presentation of a sign reduces to this: A is a sign if and only if the circumstances which would remove the needs motivating certain associated response-sequences (or, roughly speaking, the circumstances to which the sign is applicable) *can be described in the English language*. But this leaves us helpless to discriminate sense from nonsense, or the empirically meaningful from the nonsensically unverifiable, in the case of signs belonging *to the English language itself*. Just at the point where semiotic might be expected to become useful, in ways other than that of speculative interpretation of the signification of animal responses to conditioned stimuli, the circularity of the entire procedure brings us to a halt (pp. 269-270).

In this argument Black has perhaps scalped an Indian, but unless my fingers deceive me, my own scalp is quite intact. For on my account "a sign may, of course, signify without there being a formulation of what it signifies" (p. 20); I specifically distinguish between a significatum and a formulated significatum. Perhaps Black as a logician is so accustomed to work with signs whose signification is already formulated in other signs that he forgets that a theory of signs must provide us with ways of finding out empirically what a sign means.

He is in effect saying that unless we are able to formulate the signification of a sign the sign cannot have signification – hence the supposed relativity of my criterion for signification to a metalanguage rich enough to formulate the signification. And hence the supposed circularity and arbitrariness, since the signs in the chosen metalanguage would not have signification unless we could in turn formulate their signification in some other preferred metalanguage. But on the account given in *Signs, Language, and Behavior* it is quite possible for a sign

to have signification without our being able to formulate the signification, either because our own language is not rich enough or because our empirical investigation of the signification of the sign has not gone far enough. The procedure outlined gives us a way to seek the signification of any sign, including the signification of signs in a given metalanguage, provided we do not attempt to formulate the signification of all signs at once. The alleged vicious circularity collapses into the harmless fact that at any moment of inquiry there are some un-analyzed and unformulated significations.

It is true that in *Signs, Language, and Behavior* 'condition' plays a crucial role, and is unanalyzed. It is also true that it might be well to analyze this term. But the fact that it is not analyzed does not mean that is is without signification. Black in his own account uses 'circumstance' without batting an eye: the English language which we both employ often uses both terms under similar "circumstances" or "conditions". We may and often do ask under what conditions or under what circumstances a person becomes President of the United States, or dynamite explodes, or philosophers write books. I see no reason why we "must" analyze these terms in talking about signs. Analysis and reconstruction of our language inevitably proceeds piecemeal.

A final and somewhat minor point. Black holds that " 'response-sequences' and 'needs' are fictions" – at least in connection with human signs. But sequences of responses are admitted in the approaches of both Tolman and Hull. Black talks as if psychologists recognize only such "general human 'needs' as food, sex, play, and the rest". Actually "the rest" is a large class, as reference to Murray's *Explorations in Personality* will show. And while it may be well to attempt a reformulation of semiotic in which 'need' is not a basic term, it is significant that both Tolman and Hull stress the importance of intervening variables in psychology, and use need as such a variable. Even the elementary laws of learning are stated by Hull in connection with the reduction of need. If response-sequences and needs are fictions, I at least have not been responsible for their introduction in the study of human behavior.

4. THE CASE OF MR. BENTLEY

Although I do not see that Black has vindicated his charge that my method is at fault, his criticism is at least pointed. Of Bentley I can only say that his criticism is at least long. I will first show the basic

inaccuracy of what he presents as a "report" of *Signs, Language, and Behavior,* and then comment on his general remarks in the second part of his paper.

In his second footnote Bentley says that he will use the word 'define' "casually and loosely" as I am said to do. In *Signs, Language, and Behavior* 'define' is quite precise: it is synonymous with giving the necessary and sufficient conditions for the application of a sign; 'sign' itself is expressly said not to be defined (p. 250). Bentley's casual and loose use of 'define' is really essential to his strategy: it obviates the necessity of distinguishing the defined terms in the book, the partially analyzed terms, the unanalyzed terms taken from biology and physics, and the unanalyzed terms in general use in English. By breaking down these fences he has a romping field-day indeed, exciting but without eventuation.

Take the term 'behavior'. In *Signs, Language, and Behavior* this term is part of the unanalyzed vocabulary of the English metalanguage. It is not an analyzed term in the language proposed for semiotic and it is not employed in defining any of what Bentley likes to call my "official" terms (it is used in the preliminary analysis of 'sign' but not in the second partial analysis of 'sign' to which Bentley mainly refers). Bentley obscures this fact by a typical inaccuracy in his reference to *Signs, Language and Behavior.* He says 'behavior' *is* used in the book's definitions; and on page 115 of his article he arbitrarily replaces in the definition of 'response-sequence' the term 'response-sequences' by the term 'behavior'. This gives the reader the impression that 'behavior' is a key defined term, and that it is restricted to "muscle-gland action". Such carelessness on his part misrepresents the book he is said to be "reporting".

Or consider on page 111 of his article the fun he has at my use of 'or' and 'um' endings (to distinguish what signifies and what is signified). The example he uses to show my ineptness is a straight misquoting of the text: he says that for me a locatum is a sign, whereas the page 67 of *Signs, Language and Behavior* he quotes makes clear that only the *word* 'locatum' is a sign. This is the same order of confusion as would be involved in claiming that someone held that man is a noun because he said that the sign 'man' was a noun.

Such careless reporting occurs throughout his article. On page 125 he says that formators are signs about signs; this is specifically denied on page 157 of *Signs, Language, and Behavior.* On page 113 he gives a supposed definition of 'reaction', referring to page 355 of my book;

but there 'stimulus' is defined and not 'reaction'. On page 116 he says that muscle-gland action in itself is held to be purposive; *Signs, Language, and Behavior* says nothing of the sort. On page 117 he states that 'behavior-family' is itself "defined with respect to behavior"; a glance at pages 10 and 346 of the book will show that this is not so. On page 119 he says that the distinction between 'sign' and 'sign-vehicle' is "the ancient difference between 'meaning' and 'word', rechristened but still before us in all its ancient unexplained crudity"; p. 251 of *Signs, Language, and Behavior* points out that the distinction intended is that made by Peirce in terms of 'type' and 'token'.

Bentley has two other favorite devices. One is to list a number of unanalyzed words used in the book ('substitute for', 'because of', 'initiates', 'controls', 'affects in some way', 'determines', etc.), and then say without explanation (as on page 122) that these are instances of the "linguistic insecurity" of *Signs, Language, and Behavior* — presumably because they are not analyzed. Is the English language really so insecure?

The second and more common device is to restate the language of the book in such a way that it seems to involve an endless congeries of disconnected and reified entities. *Signs, Language, and Behavior* defines a response as an action of a muscle or gland; because the word 'action' rather than 'reaction' is used, Bentley says this "has the effect of pushing it off to a distance and presenting it rather "on its own" than as a phase of semiosical process" (p. 114). Because organisms are said to be disposed to various types of behavior we are told that "dispositions and behaviors are thus set over against each other" (p. 144). I am said to make behavior "a compartment of the organism", to make sign "a glistenly transmogrified denoter or signifier", to make "muscles and glands all on their own" purposive (p. 123). Such distortions are passed off as honest reporting. Every distinction made in *Signs, Language, and Behavior* is seen by Bentley as a reification, an unnecessary multiplication of disconnected entities.

By these various strategies Bentley gives the reader the impression that *Signs, Language, and Behavior* is a terminological chaos. In the second part of his article, thus prepared, he moves in for the kill. The book is dismissed as an unholy alliance of "goal-seeking psychology at the rat level", set up "with little change", and "the cobwebs of the older logics and philosophies", a mixture of the outworn traditions of Newtonian mechanics with philosophic formalism and dualism.

As for the first member of this supposed monstrosity, it is hard to

see how an attempt to use and extend the behavioral analyses of Mead, Tolman, and Hull is to employ a rat-level psychology: all of these men have endeavored to develop a general theory of behavior which will embrace the behavior of rats and men and yet account for their differences. As for 'mechanism', *Signs Language, and Behovior* has an explicit discussion of freedom (pp. 197, 275). Perhaps Bentley's reading of the book is blocked by the mere use of the sign-vehicles 'stimulus' and 'response'; perhaps he thinks every stimulus "mechanically" calls out a response. But this is not the position of the book. On its terminology there can be stimuli without responses, and when there is a responce to a stimulus this response may be partly due to the way in which the stimulus has been signified. There can thus occur responses-sequences which have never before appeared in the behavior of the responding organism. Dewey's position in the article on the reflex-arc concept in psychology to which Bentley refers as out-moding my view is nowhere contradicted by the book -- nor by the work of Tolman or Hull.

As for "the cobwebs of the older logics and philosophies", symbolic logic is presumably an "older" logic, a strange use of terms; I will return later to the relation of symbolic logic and a behavioral semiotic. The book is supposed to "maintain the older mentalistic view of thought" by making a sign a third "thing" that intervenes "between" the organism and the world, and thus "to bring back *sub-rosa* the very thing that Peirce – and James and Dewey as well – spent a good part of their lives trying to get rid of". Now this claim that I am "fearful of getting objects and organisms into direct contact", and put the ghost of "ideas" between them, is without foundation. The fact that how organisms respond to something is *sometimes* in part a function of how that something is signified, does not imply that the direct contact of organism and world is ever lost, or that signs always occur, or that when signs do occur some mental or quasi-mental "thing" has intervened. When behavior is a function of a number of variables, no one of these variables need be made into a mental substance standing between the organism and the world. There is no warrant whatsoever for interpreting *Signs, Language, and Behavior* as an attenuated Cartesian dualism. My own cosmology – not defended in the book itself – is a version of objective relativism. A brief sketch of it is presented in the article, "Multiple Self and Multiple Society", in *Freedom and Experience: Essays Presented to Horace M. Kallen* (Cornell University Press, 1947, pp. 70–77).

Semiotic is so undeveloped today that a plurality of various constructions are desirable, even within a behavioral framework. I wish Mr. Bentley well on his own enterprise. He and Dewey have chosen to begin their terminological analysis at a lower level than did *Signs, Language, and Behavior*. But in their construction they will either have to develop an alternative science of behavior that will convince scientists, or they will have to show how the basic terms of existing behavior theory can be defined in their own terms. No mere insistence on "transactions" will be sufficient. That what happens happens only in contexts does not free us from the obligation to speak precisely about what does happen in various contexts. When they have elaborated their new semiotic sufficiently to deal with existing behavior theory, with signs in animals, with signs in the sciences, logic, arts, and religions, and with signs in personality disturbances, it will be instructive to assess their results. In the meantime no useful purpose is sustained by a wholesale distortion of *Signs, Language, and Behavior*.

5. INTERPRETANT AND SIGNIFICATUM

In footnotes 57, 60, and 61 of his article, Bentley summarizes Dewey to the effect that my version of semiotic is "a complete inversion of Peirce". *Signs, Language, and Behavior,* however, claims to be "an attempt to carry out resolutely the insight of Charles Peirce that a sign gives rise to an interpretant and that an interpretant is in the last analysis 'a modification of a person's tendencies toward action' ". Something is wrong somewhere.

Dewey's article in which he makes his charge ("Peirce's Theory of Linguistic Signs, Thought, and Meaning", *Journal of Philosophy,* Vol. XLIII (1946), pp. 85–95) dealt only with my earlier monograph, *Foundations of the Theory of Signs.* For this and other reasons it did not seem fair to reply to Dewey's analysis in terms of the fuller treatment of *Signs, Language, and Behavior.* But since Bentley's note shows that Dewey has not changed his position because of *Signs, Language, and Behavior,* and since he (Bentley) claims that I have "evaded" the issue Dewey has raised, some discussion of Peirce is in order. This may serve to clarify somewhat 'interpretant', and 'significatum – terms which have bothered many critics, and understandably.

I should like to make clear that the position developed in *Signs, Language, and Behavior* did not start from Peirce. George H. Mead first stimulated me to think about signs behaviorally. *Signs, Language,*

and Behavior is in many ways a further development of Mead's *Mind, Self, and Society*. I never heard Mead in lecture or conversation refer to Peirce. Only later did I work earnestly at Peirce, Ogden and Richards, Russell, and Carnap, and still later at Tolman and Hull. All of these persons influenced in various ways the formulation of *Signs, Language, and Behavior*. Nevertheless, in historical perspective, it seems to me that the position of *Signs, Language, and Behavior,* though its orientation was not derived from Peirce, is in effect "an attempt to carry out resolutely" his approach to semiotic.

But Dewey says no. His argument that I have inverted Peirce's position centers around the use of the terms 'interpreter' and 'interpretant'. Dewey states that the crucial error is my "gratuitous introduction of an interpreter" (p. 86), and then the "translation of 'interpretant' into a personal user" (p. 88), that is, into an interpreter. According to Dewey the interpretant for Peirce is a sign which in a process "supplies meaning to the earlier ones" (p. 89). It is perhaps well to quote Dewey's position at more length:

The misrepresentation in question consists in converting *Interpretant*, as used by Peirce, into a personal user or interpreter. To Peirce, 'interpreter,' if he used the word, would mean *that which interprets*, thereby giving meaning to a linguistic sign. I do not believe that it is possible to exaggerate the scorn with which Peirce would treat the notion that *what* interprets a given linguistic sign can be left to the whim or caprice of those who happen to use it. But it does not follow from this fact that Peirce holds that the interpretant, that which interprets a linguistic sign, is an "object" in the sense of an existential "thing". On the contrary, the interpretant, in Peirce's usage, is always and necessarily *another* linguistic sign – or, better; set of such signs. The following passage is strictly representative: "On the whole, then, if by the *meaning* of a term, proposition, or argument we understand the entire general intended interpretant, then the meaning of an argument is explicit. It is its conclusion; while the meaning of a proposition or term is all that that proposition or term could contribute to the conclusion of a demonstrative argument" (p. 87).

In *Signs, Language, and Behavior* an interpreter is an organism for which something is a sign, an interpretant is a disposition in an interpreter to respond in certain ways because of a sign (p. 17), and a disposition to respond is "a state of an organism at a given time which is such that under certain addional conditions the response in question takes place" (p. 9). Finally, those conditions which would permit the organism to respond as it is disposed to respond because of the sign are the signification (the significatum) of the sign.

The question now is whether the position of *Signs, Language, and Behavior* is in substantial agreement with Peirce's doctrine or whether it is, as Dewey contends, a falsification of Peirce's doctrine. To answer this we must bring Peirce's words into the argument.

Peirce himself does employ the term 'interpreter'. The interpretant – whether emotional, energetic, or logical – is an effect of a sign on an interpreter. Thus Peirce inquires into "the nature . . . of the essential effect upon the interpreter, bought about by the semiosis of the sign, which constitutes the logical interpretant" *(Collected Papers,* V, §484).

The answer as to the nature of the logical interpretant which he gives is as follows *(ibid.,* §476): "Shall we say that this effect may be a thought, that is to say, a mental sign? No doubt, it may be so; only, if this sign be of an intellectual kind – as it would have to be – it must itself have a logical interpretant; so that it cannot be the *ultimate* logical interpretant of the concept. It can be proved that the only mental effect that can be so produced and that is not a sign but is of general application is a *habit-change,* meaning by a habit-change a modification of a person's tendencies toward action". Peirce is somewhat bothered by the mentalistic terms in this account and "to clear the sign of its mental associations" be points out that "habit is by no means ex- clusively a mental fact" *(ibid.,* §492). A habit is a readiness "to act in a certain way under given circumstances and when actuated by a given motive" *(ibid.,* §480). A habit is "a tendency . . . to behave in a similar way under similar circumstances in the future". He even speaks of a habit as a "disposition to respond to a given kind of stimulus in a given kind of way". Man by "fancied reiterations" of a desired kind of con- duct can *"produce habits,* just as do reiterations in the outer world; *and these habits will have power to influence actual behavior in the outer world" (ibid.,* §487).

In terms of these quotations I can only conclude that the position of *Signs, Language, and Behavior* is very close indeed to that of Peirce and not an inversion of his position. Both talk of effects of signs on inter- preters, both describe interpretants in terms of such effects, both regard the interpretant as an effect on an interpreter such that the interpreter tends to act in a certain way under given circumstances when actuated by a given need. *Signs, Language, and Behavior* uses the sign-vehicles 'disposition to respond', 'respond', conditions', 'need'; Peirce uses the sign-vehicles 'tendency to act', 'act', 'circumstances', 'motive'. Both use the terms Bentley finds so objectionable: 'person',

'behave', 'produce', 'influence', 'modify', 'effect', 'constitutes'. Where is the essential difference in the two accounts? Where does *Signs, Language, and Behavior* invert Peirce?

It is still necessary to explain how Dewey was led to his interpretation of Peirce. I think an ambiguity in Peirce's term 'interpretant' gives the explanation: 'interpretant' sometimes, as above, names a certain kind of effect of a sign on an interpreter (a tendency to action), but often it signifies what I have called the signification (significatum) of the sign (the circumstances under which one could respond as he is disposed to respond because of the sign). It is this second use of 'interpretant' that Dewey refers to whem he says that for Peirce the interpretant is not something existential but a set of signs. A better instance perhaps is this sentence from Peirce: "the logical interpretant is general in its possibilities of reference (i.e., refers or is related to whatever there may be of a certain description)" (*ibid.*, §486). To formulate this "whatever" in other signs is to give what Peirce calls the meaning of a particular "logical interpretant" – and this process can go on indefinitely. When Dewey singles out only this aspect of Peirce's terminology he writes that "signs as such are connected only with other signs". It does not follow even then that reference to an interpreter is "gratuitous", for if one asks – as Peirce also does – what is the effect on an interpreter that is *not* a sign when a sign has a certain signification, one can hold without contradiction with Peirce – and with *Signs, Language, and Behavior* – that this effect is a tendency of actual persons to act in a certain way under certain conditions. Dewey himself has written in a similar vein. In his *Logic*, pages 48–49, he says of the reading of a scientific treatise, "so far as definite grounded agreement or disagreement is reached, an attitude is formed which is a preparatory readiness to act in a responsive way when the conditions in question or others similar to them actually present themselves".

The distinction between Peirce's two uses of 'interpretant' is made in *Signs, Language, and Behavior* by distinguishing 'interpretant' and 'significatum'. I think the above account explains – and explains away – Dewey's contention that I have foisted an Ersatz version of Peirce on an unsuspecting public.

I may add that this distinction also cuts the nerve of John Wild's somewhat similar charge that the position of *Signs, Language, and Behavior* is "subjectivistic". Wild says that on its analysis "we cannot signify anything which is not a complex involving our own subjective

responses and purposes". But this confuses interpretant and significatum. *Signs, Language, and Behavior* admits each "involves" the other (p. 18); Wild wrongly quotes this as saying each "belongs" to the other. The fact that a sign only signifies when there is a disposition to respond by the interpreter for whom it is a sign does not mean that it is the disposition (the interpretant) which is signified. The book – as on page 19 – is quite explicit on this point.

6. THE STATUS OF DISPOSITION TO RESPOND

Many criticisms of *Signs, Language, and Behavior* center around the term 'disposition'. In previous formulations I did not use this term. It was introduced in order to make it possible to say that a given item is a sign and has signification even when no responses of the organism are made. Suppose for instance a passer-by asks me the whereabouts of the Pennsylvania Station, and I say to him 'straight ahead to 32nd Street and then two blocks to the left'. He does not at the time of hearing the words turn to the left, yet we would ordinarily say that the words are "signs" to him. *Signs, Language, and Behavior* expresses this situation by saying that the passer-by is "disposed to respond" because of my words in a certain way, i.e., that the words change the state of his organism in such a manner that given a need to get to the Pennsylvania Station he will walk straight ahead and turn left when he sees the street sign with the letters '32nd Street' upon it.

The common criticism are twofold: one set centers around the notion of 'disposition' itself, the other set around the question as to whether we can get evidence as to the existence or non-existence of a disposition to respond.

Disposition-predicates are of course very common in everyday speech. Paper is inflammable, innoculation makes persons immune to some diseases, the rubber band on the table is elastic, the sugar in the bowl is soluble, dynamite is explosive, a certain person is phlegmatic, a certain bar of iron is magnetic, and so on. In each case something is said to be at a given moment in such a state that it will under other stipulated conditions react in such and such a way. I see no difficulty in distinguishing 'disposition to response x' and 'response x' that is not present in distinguishing 'inflammable' and 'burning'. I think none of my critics suggest throwing out all dispositional terms, and I do not see that any of them have given an argument why the particular disposi-

tional term 'disposition to respond' is objectionable if other dispositional terms are not. If some of my readers have a general doubt about dispositional terms, I suggest they consider Carnap's analysis of such terms in "Testability and Meaning" *(Philosophy of Science,* Volumes III and IV, 1946 and 1947), Stevenson's analysis of 'disposition' in *Ethics and Language,* and the discussion of the place of intervening variables in psychology given by Tolman and Hull.

There is an occasional complaint that I have not told *what* the state of the organism is when it is disposed to respond to something because of a sign, i.e., what the interpretant is as an organic state. The fact that I made no attempt to do so was deliberate. I do not know. The problem is an empirical one and its solution awaits the development of semiotic as an empirical science. This does not mean, however, that a scientific semiotic cannot make progress until this problem is solved. Centuries of fruitful study of magnets went on before a molecular explanation of the magnetic state was griven. The case may well be the same with semiotic.

Since I did not give a biological description of the state of an organism for which something is a sign, it is not correct, as Rice does, to assume that I must mean by this state something static. Whatever it is, it is undoubtedly immensely complicated and dynamic. Rice thinks it may be primarily a circular neural process, a position which has been suggested by a number of theorists in recent years and which is being explored in an eagerly awaited work by the neurologist and psychiatrist Dr. Warren S. McCullough. I am inclined to believe that this is a rich vein of ore. But the problem is one for experimental science, and it would have been out of keeping with the character of my book to anticipate the answer. In this field we are just beginning to roll balls down inclined planes.

It is nevertheless possible to obtain evidence concerning the occurrence of interpretants even if we cannot describe them neurologically and physiologically – just as magnetism was studied before the magnetic state was described. In *Signs, Language, and Behavior* a number of types of evidence are discussed (pp. 13–15), and since none of my critics has specifically considered the analysis there given, I feel no obligation here to reopen the matter. Perhaps one point can be made. Peirce writes: "How otherwise can a habit be described than by a description of the kind of action to whicht it gives rise, with the specification of the conditions and of the motive?" *(Collected Papers,* V, §491). Insofar as this is so, evidence for the presence of a habit is found by

observing whether under the given conditions and motive the kind of action in question is performed. This is in fact the first kind of evidence discussed in *Signs, Language, and Behavior*. But we can go much further. Once we have this kind of evidence we can begin to study the state of the organism which does so act under such circumstances, and as we get such knowledge it gives us a new source of evidence for the presence of habits even when no overt behavior is performed. This has been the course of development in the study of magnetism, solubility, and immunity. A semiotic which starts with overt responses need not forever remain at that descriptive level. But for the present macroscopic description is our necessary anchorage.

7. LOGIC AND SEMIOTIC

Elaine Graham's paper, "Logic and Semiotic", will serve as the basis for a few remarks on one of the most difficult (and most interesting) problems that a biologically grounded semiotic must face: Can it include within itself formal or symbolic logic? Chapter VI of *Signs, Language, and Behavior,* in an admittedly tentative and explorative manner, suggested a positive answer. This means that logical constants and logical sentences set up dispositions to respond as do non-logical signs. It does not, of course, as Bronstein's criticism of this position assumes, require that an actual response always be made when one interprets a logical (or a non-logical) sign.

   Graham's paper first considers the relation between the behavioral analysis of certain formators in *Signs, Language, and Behavior* and the analysis of their signification given by the truth-tables of the symbolic logician. She aims to show, and I think does show, that when some ambiguities in the book are resolved, the "two formulations are in a real sence equivalent". My only question at this point is whether her interpretation of formators in terms of "patterning of denotation" (or "patterning of interpretants") can be extended to apply to all formators (such as 'not' and 'some') or whether it holds only for such connectives as 'and' and 'or'.

   The second part of her paper concerns the criterion proposed in *Signs, Language, and Behavior* for formative ascriptors (analytic and contradictory sentences). This criterion was stated as follows: "A *formative ascriptor* is a compound ascriptor such that the denotation of one or more of the component ascriptors (called the *antecedent ascriptors)* is, because of the signification of the ascriptor, a sufficient

condition for the denotation or lack of denotation of the remaining component ascriptor or ascriptors (called the *consequent ascriptors),* and hence for the denotation or lack of denotation of the compound ascriptor itself" (pp. 162, 163).

Graham's analysis shows that this formulation is unsatisfactory. Some of the difficulties she finds come from only one possible reading of the criterion, namely, where 'antecedent' is taken to mean 'before' and 'consequent' to mean 'after'. Thus she says that 'John is my son and I am John's father' would be on this criterion an analytic formative ascriptor, since if 'John is my son' denotes then the conditions are fulfilled for 'I am John's father' to denote, and hence the conditions for the compound sentence to denote – and yet this compound sentence is not what is normally meant by an analytic sentence.

While this reading of 'antecedent' and 'consequent' is somewhat forced, the criterion as formulated was certainly defective in not specifying that the relation intended was that of implication (see page 22 of *Signs, Language, and Behavior* for the term 'implicate' and page 165 for the view that analytic sentences involve an implicate relation). The examples given in the book show, however, that the defect in formulation was not accidental but the result of inadequate analysis on my part. So this arrow of Elaine Graham hits a vulnerable spot.

Rather than attempt to sharpen the previous criterion (though this might be done successfully), I now think the simplest way out would be to take criteria of analytic and contradictory sentences formulated by symbolic logicians, and attempt to define their terms on the basis of the biologically grounded terms of semiotic. If this were done, the whole of symbolic logic would be brought within a "behavioral" semiotic. And I see no theoretical difficulty in doing this. Carnap, for instance, in *Meaning and Necessity,* page 10, defines an 'L-true sentence in language S_1' in terms of 'holds in every state-description in S_1.' A state-description is "a class of sentences in S_1 which contains for every atomic sentence either this sentence or its negation, but not both, and no other sentences" (p. 9). He then gives (on p. 10) rules for determining whether a given sentence "holds" in a state-description. If we could define 'sentence', 'atomic sentence', 'negation', and 'holds' in the vocabulary of *Signs, Language, and Behavior,* and equate 'L-true' and 'analytic formative ascriptor', the whole of Carnap' terminology and method of analysis would become part of semiotic as developed in the book.

This brings us to Elaine Graham's third question, whether analytic

formative ascriptors necessarily denote (and so are necessarily "true"). She answers yes; *Signs, Language, and Behavior* answered no on the general ground that as semiotic is there constructed signification as such never involves denotation. What is now clear is that the problem is terminological, and depends upon how 'denote' is defined. In the Graham construction 'analytic formative ascriptors denote' is, as she admits, itself an analytic formative ascriptor, just as in Carnap's terminology 'an L-sentence is true' is itself L-true. In my terminology, however, to say 'X denotes' implies 'there is or was or will be some y such that . . .'; hence I argued that 'x is an analytic formative ascriptor' does not imply 'x denotes'. Graham questions whether I am right because of the unique "patterning of interpretants" which occurs in analytic formative ascriptors. She may be correct on this point. The moral to be drawn is that we must be cautious not to confuse various constructions of semiotic, and in any given construction not to confuse formative ascriptors with other kinds of ascriptors. That is why the axiomatization of semiotic must be carried out with much greater care than is done in *Signs, Language, and Behavior*. Here semiotic needs the help of formal logic.

A final point on the relation of formal logic and semiotic. Formal logic is not a *theory* of signification but an analysis of given or proposed significations. (This does not imply of course that a formal logician may not also have a theory of signification.) As such, formal logic is compatible with various theories of signification – with a Platonic as well as a behavioral analysis. The formal logician, therefore, has nothing to fear in attempts to define the basic terms of formal logic on the basis of terms in a biologically grounded semiotic. Peirce himself was the their of a long tradition which conceived of logic as part of semiotic. He himself, and rightly, saw no conflict between his work as formal logician and his analysis of signs as involving tendencies to behavior. And semiotic, as developed in *Signs, Language, and Behavior* provides a place within one system for the logical analysis of the significations of signs and for the investigation of what happens to interpreters for whom signs have signification.

I will not attempt now to climb still higher on the semiotical ladder. I had intended to say something about esthetics in the light of comments made by Philip Blair Rice, W. K. Wimsatt, and David Rynin. And I had intended to discuss John Wild's Platonic "phenomenology" of signs, and the relation of pragmatism to semiotic. But these dispositions to respond had best be checked for the moment.

Our discussion has been limited to foundational problems. My critics, as philosophers, have quite properly concentrated on such problems, and they have given *Signs, Language, and Behavior* a very through analysis indeed. I thank them for this. Undoubtedly I have not done full justice to their arguments in my attempt to defend the general soundness of *Signs, Language, and Behavior*. The many verbal arrows we have drawn in attack and defense are evidence that we at least agree upon the basic importance of semiotic in the reconstruction of contemporary thought.

APPENDIX

Since writing the above comments, I have seen the manuscript of L. O. Kattsoff's "What is Behavior?" *(Philosophy and Phenomenological Research*, Vol. IX, (1948), pp. 98–102), and read two analyses which I had overlooked: George Gentry's paper, "Signs, Interpretants, and Significata", *(Journal of Philosophy,* Vol. XLIV (1947), pp. 318–324), and Virgil C. Aldrich's review in the same issue (pp. 324–329).

Mr. Kattsoff holds that to admit "something inside the interpreter" is to introduce "ideas". Since he does not expliciate what he means by 'idea', it is not clear what he wishes to add by this term to 'interpretant' as defined in *Signs, Language, and Behavior*. His claim that we can only find out whether for a human being there is an interpretant or idea, and the purposes for which someone uses a given sign, "by asking him and listening to his reply", seems to me wrong. We do of course use verbal reports as evidence of the signification of signs and the purposes for which signs are produced. But persons do lie and do have signs whose signification they cannot precisely formulate and do use signs for purposes of which they are unaware; to recognize this is to admit that there is evidence other than verbal reports by which the reliability of verbal reports themselves can be controlled. Kattsoff's third claim – that metaphysical discourse is lexicative and not formative – cannot be discussed since he did not state what he means by 'metaphysical discourse'.

The preceding pages have made clear that I am not, as Mr. Gentry suggests, "committed to a motor theory" of the interpretant in contrast to a cortical theory. I do not believe, however, that scientific work in the study of signs is impossible until one determines what is the state of the organism when something is a sign; much good work in linguistics,

for example, has been accomplished without that question being answered. His second main thesis – that the significatum of a sign is itself a cortical process – arises from his attempt to answer the questions "What sort of entity is a significatum? What does the term refer to?" I believe that this formulation of the problem is misleading. It is true that I stated that "a sign must signify" and that "a sign signifies a significatum". But these ascriptors are in the language of *Signs, Language, and Behavior* formative and not lexicative ascriptors; "a sign has signification" and "a sign signifies a significatum" are equivalent formative ascriptors. Hence to search for an "entity" which is "referred to" in the ascriptor "the sign x signifies a significatum" – whether the entity be a cortical process or not – is to run the risk of confusing within semiotic itself lexicative and formative discourse. If we avoid this danger, the problem is merely that of how to describe what differentiates a given sign from other signs. I do not see that this description must be given solely in terms of the cortical processes which occur. Hence I see no need to identify the signification of 'spiral nebula' with whatever cortical processes occur when the term is a sign to someone, or to say that the term "refers" to such processes. It is for such reasons that I have not identified 'interpretant' and 'significatum' ('signification'). Perhaps Mr. Gentry has in mind a simpler semiotic in which fewer basic terms are involved. Such a semiotic would be welcome; I have not been able to construct one that did not seem to blur important distinctions.

Mr. Aldrich singles out in his review the problem of the differentiation of the modes of signifying. I agree with Aldrich that my account leaves much to be desired, but I do not think that he has put his finger on the main difficulties nor that his own suggestion for a basic distinction between evocative and cognitive signs provides the way out. His charge that I do not actually analyse the ways of signifying but only the different things signified – so that I am left with only one mode of signifying, the designative – is unconvincing on two points: the modes of signifying are distinguished in fact in terms of different kinds of interpretants (so that we may signify the *same* object in different ways, designatively, appraisively, etc.), and secondly, Aldrich uses 'designates' in a wider way than I do, perhaps as equivalent to my term 'signifies'. I would not say in 'x is good' that 'good' designates x, even when x is good, since 'designates' is restricted to signs which signify casually efficacious characteristics of objects. 'X is good' is appraisive only if someone is disposed to accord preferential

behavior to x because of the sign 'good'; to say that "y likes x" is not appraisive of x but designative of y in relation to x. So in my terminology the appraisive (and prescriptive and formative) mode of signifying does not reduce to the designative. There is on my account, to use Aldrich's terms, an "evocative" and "cognitive" aspect to signs in all modes of signifying; to pull these apart and make them the basis for a classification of modes of signifying is in my opinion to return to the state of analysis represented in the old dichotomy of "emotive" and "referential" meaning. My account certainly needs to be improved, but I think it indicates a direction in which appraisals and prescriptions have a "cognitive" character which is controllable by evidence and yet remain distinct in signification and in empirical control from designative statements. If this lead can be developed satisfactorily we can eat the cakes offered to us both by the pragmatists and the logical empiricists. I for one am hungry enough to want both.