SPEECH MONOGRAPHS

Volume 42 August 1975 Number 3

LOGICAL EMPIRICISM AND THE STUDY OF HUMAN COMMUNICATION

DANIEL J. O'KEEFE

THE logical empiricist conception of the scientific enterprise has long dominated communication theory and research. But in recent years the criticism of this conception has been so incisive that Suppe can now call the logical empiricist approach "a view abandoned by most philosophers of science" and can speak of its "general rejection."[1] If communication theory and research are to be grounded in defensible philosophical underpinnings, an examination of those criticisms is called for. Hence this essay will adumbrate the logical empiricist view and note its dominance in the study of human communication (section I), explore some of the criticisms leveled at that view (section II), and discuss some implications of those objections for communication studies (section III).

Mr. O'Keefe is a Visiting Assistant Professor of Speech Communication and Theatre at the University of Michigan.

Wayne E. Brockriede provided extensive and especially helpful commentary on an earlier version of the manuscript; and David L. Swanson, Barbara J. O'Keefe and Jesse G. Delic gave useful criticisms of earlier drafts.

[1] Frederick Suppe, "The Search for Philosophic Understanding of Scientific Theories," in *The Structure of Scientific Theories*, ed. Frederick Suppe (Urbana: Univ. of Illinois Press, 1974), pp. 119, 62.

I

"Logical empiricism" (also sometimes called "logical positivism" or the "received view" of scientific theory) is thusly named because it stands at the confluence of two streams of philosophical work: a refurbished Humean empiricism and new developments in symbolic logic at the turn of the twentieth century.[2] Where Hume drew a distinction

[2] The account here presented of this approach is necessarily sketchy—as is the discussion of the criticisms. No one philosopher holds all the views attributed to the received view; my characterization of that view is rather a gloss of the general approach taken by, e.g., Percy Bridgman [*The Logic of Modern Physics* (New York: Macmillan, 1927)], Richard Rudner [*Philosophy of Social Science* (Englewood Cliffs, N.J.: Prentice-Hall, 1966)], Rudolph Carnap ["The Methodological Character of Theoretical Concepts," in *Minnesota Studies in the Philosophy of Science, Vol. I: The Foundations of Science and the Concepts of Psychology and Psychoanalysis*, ed. Herbert Feigl and Michael Scriven (Minneapolis: Univ. of Minnesota Press, 1956), pp. 38-76], Ernest Nagel [*The Structure of Science* (New York: Harcourt, Brace, and World, 1961)], and Carl Hempel [*Philosophy of Natural Science* (Englewood Cliffs, N.J.: Prentice-Hall, 1966)]. My prime concern throughout, however, is not so much with giving an accurate portrayal of the details of past and present work in the philosophy of science, as with conveying the general tenor of recent developments in the area and with indicating the import of that work for communication theory. As a result, the philosophical positions and issues discussed in this essay are treated rather superficially. The most comprehensive single treatment of the complexities surrounding the development

that this positivistic view of the scientific enterprise has been carried over into communication studies; such a view is in fact evidenced throughout the writings of communication theorists and researchers.[15] An indication of just how deeply ingrained the positivistic approach has become can be obtained by careful examination of David Smith's discussion of the "idea of process": while he calls for a movement away from "behavioristic methodology" he seemingly does not recognize the implicit acceptance of the positivistic philosophy of science that underpins that methodology, for his discussion of alternative research strategies fails to note that not all the "alternatives" reject the positivistic view.[16]

Marx, "The General Nature of Theory Construction," in *Psychological Theory,* ed. Melvin H. Marx (New York: Macmillan, 1951), pp. 4-19. An interesting sidelight on this point is offered by Brian D. Mackenzie, "Behaviourism and Positivism," *Journal of the History of the Behavioral Sciences,* 8 (1972), 222-31. He argues that behaviorism in psychology fails to meet the criteria for a Kuhnian paradigm, since in his view behaviorism amounts to little more than the institutionalization within psychology of a "positivist orthodoxy" without accompanying scientific substance; thus his view is that behaviorism does not merely rest on logical empiricism, but is in fact nothing but a positivistic philosophy of science.

15 See, e.g., John Waite Bowers, "The Pre-Scientific Function of Rhetorical Criticism," in *Essays in Rhetorical Criticism,* ed. Thomas R. Nilsen (New York: Random House, 1968), pp. 126-45; Robert S. Goyer, "Communication Process: An Operational Approach," in *Perspectives on Communication,* ed. Carl E. Larson and Frank E. X. Dance (Milwaukee: Speech Communication Center, Univ. of Wisconsin-Milwaukee, 1968), pp. 20-23; Jack M. McLeod and Steven H. Chaffee, "The Construction of Social Reality," in *The Social Influence Processes,* ed. J. Tedeschi (Chicago: Aldine-Atherton, 1972), esp. p. 60, n. 1; Gerald R. Miller, *An Introduction to Speech Communication,* 2nd ed. (Indianapolis: Bobbs-Merrill, 1972); Ronald L. Smith, "Theories and Models of Communication Processes," in *Speech Communication Behavior: Perspectives and Principles,* ed. Larry L. Barker and Robert J. Kibler (Englewood Cliffs, N.J.: Prentice-Hall, 1971), pp. 16-43.

16 See Lawrence Grossberg and Daniel J. O'Keefe, "Presuppositions, Conceptual Foundations, and Communication Theory," *QJS,* 61 (1975), 195-208.

II

The logical empiricist conception of the scientific enterprise has been subjected to severe criticism. While alternative views have been presented,[17] I will not here be concerned with these alternatives, nor with the critical attacks on them.[18] Rather, this section will focus on the deficiencies in the positivistic view. I will not provide a complete accounting of all the criticisms, nor a thoroughly detailed treatment of any one of them, but rather attempt to convey the general lines of attack.

One locus of criticism of the received view is the nature of the connection between theoretical and observational discourse (the nature of operational definitions). The strictest positivistic line is that operational definitions exhaust the meaning of theoretical terms. Since theoretical terms can thus be explicitly defined in observational terms, theoretical discourse is in principle completely replaceable by observational discourse without loss or change of meaning (thus displaying the complete observational grounding of the scientific enterprise). This strict view was held by Bridgman

17 For example, Norwood R. Hanson, *Patterns of Discovery* (Cambridge: Cambridge Univ. Press, 1958); Paul Feyerabend, "Against Method: Outline of an Anarchistic Theory of Knowledge," in *Minnesota Studies in the Philosophy of Science, Vol. IV: Theories and Methods of Physics and Psychology,* ed. Michael Radner and Stephen Winokur (Minneapolis: Univ. of Minnesota Press, 1970), pp. 17-130; and Stephen Toulmin's *The Philosophy of Science* (New York: Harper and Row, 1953), his *Foresight and Understanding* (New York: Harper and Row, 1961), and his *Human Understanding, Vol. I: The Collective Use and Evolution of Concepts* (Princeton: Princeton Univ. Press, 1972). The most well-known alternative is, of course, the one provided by Thomas S. Kuhn, *The Structure of Scientific Revolutions,* 2nd ed., rev. and enl. (Chicago: Univ. of Chicago Press, 1970), henceforth cited as *SSR.*

18 For example, Israel Scheffler, *Science and Subjectivity* (Indianapolis: Bobbs-Merrill, 1967); Dudley Shapere, "The Structure of Scientific Revolutions," *Philosophical Review,* 73 (1964), 383-94; Shapere, "The Paradigm Concept," *Science,* 172 (1971), 706-09; and Shapere, "Meaning and Scientific Change" (cited above, n. 10).

(who coined the phrase "operational definition"): "the concept of length is . . . fixed when the operations by which length is measured are fixed: that is, the concept of length involves as much as and nothing more than the set of operations by which length is determined. In general, we mean by any concept nothing more than a set of operations; *the concept is synonymous with the corresponding set of operations.*"[19]

This extreme operationalist view has not fared well in the philosophy of science. It has long been recognized that theoretical terms have *a priori* meanings (i.e., meanings apart from any operational definitions) and that it is unreasonable to assume that some finite set of operational definitions can exhaust the meaning of any given theoretical term. Even Carnap long ago liberalized the linkage between the theoretical and observational languages, admitting that the strict operationist view is "too narrow."[20]

The necessity for a more liberal view can be clearly seen by examining Charles Taylor's critical analysis of Tolman's operational definition of the concept of a rat's expectation of food at a given location. Tolman's view is this:

> When we assert that a rat expects food at location *L*, what we assert is that *if* (1) he is deprived of food, (2) he has been trained on path *P*, (3) he is now put on path *P*, (4) path *P* is now blocked, and (5) there are other paths which lead away from path *P*, one of which points directly to location *L*, *then* he will run down the path which points directly to location *L*. When we assert that he does *not* expect food at location *L*, what we assert is that, under the same conditions, he will *not* run down the path which points directly to location *L*.[21]

Taylor argues that "Tolman's definition hardly gives the empirical sense which we would ordinarily attach to 'the rat expects food at location *L*.' We can well see that in certain conditions this could be the description of a test for the truth of the proposition defined. But there are other conditions in which it would not be. Thus, if there were a cat astride the path leading to *L*, we would not take the immobility or even retreat of the rat as evidence that he did not expect food at *L*."[22] But it is not simply that the theoretical statement has *a priori* meaning and that the operational definition is somehow incomplete. Rather, "there is no limit to the number of conditions in which the Tolmanian test for an expectancy would be invalid. . . . [The rat could be] thirsty and expect water at *M*, or sexually aroused and expect a mate at *N* or suffering from intense fatigue, or experimental neurosis, and so on and so on. The job of giving all the conditions in detail is endless, as is therefore the job of giving an operational definition."[23] As Wallach concludes, "it does not seem possible, then, that the meaning of an expectancy can simply be given by operational definition."[24]

Waismann puts the general point this way: "We can never exclude altogether the possibility of some unforeseen situation arising in which we shall have to modify our definition. Try as we may, no concept is limited in such a way that there is no room for doubt. . . . We can never fill up all the possible gaps through which a doubt may seep in."[25]

[19] Bridgman, p. 5; for a similar view about the nature of theoretical discourse, see Hempel, "Logical Analysis of Psychology," p. 380.

[20] Carnap, "Theoretical Concepts," p. 65.

[21] E. C. Tolman, B. F. Richie, and D. Kalish, "Studies in Spatial Learning, I: Orientation and the Short-Cut," *Journal of Experimental Psychology*, 36 (1946), 15.

[22] Taylor, pp. 79-80.

[23] Ibid., p. 81.

[24] Lise Wallach, "Implications of Recent Work in Philosophy of Science for the Role of Operational Definition in Psychology," *Psychological Reports*, 28 (1971), 602.

[25] Friedrich Waismann, "Verifiability," in *Logic and Language: First and Second Series*, ed. Anthony Flew (New York: Doubleday Anchor, 1965), p. 126.

Theoretical terms and statements are meaningful quite apart from an operational definition; no set of operational definitions completely exhausts the meaning of a theoretical term or statement.

A second area of criticism of the positivistic approach focuses on the nature of verification and falsification in the scientific process. Put in its crudest form, the logical empiricist view is that a theory allows the derivation of a (large) number of observational statements, and to the extent the observation statements are found to be true, the theory is correspondingly verified. But it has been persuasively argued that no conclusive verification is possible, since (e.g.) the results of experiments depend on an unlimited number of conditions, not all of which can be specified in advance; empirical descriptions are indefinitely extendable (i.e., never logically complete); there are an unlimited number of tests for the application of a particular description; and so on.[26]

Recognizing this, Popper has suggested that falsification, not verification, lies at the heart of the scientific enterprise: "only the falsity of the theory can be inferred from empirical evidence, and this inference is a purely deductive one."[27] Since theories can "clash with observations," it is "possible to infer from observations that a theory is false."[28] The primary difficulty with Popper's suggestion is that theories do not *directly* lead to observational consequences.[29] As Hempel makes clear, "auxiliary hypotheses" (including a *ceteris paribus* clause) are required for the derivation; this, of course, means that a falsification of the observational consequence will imply not the falsity of the theory, but rather the falsity of the theory-plus-auxiliary-hypotheses manifold—and one can never be certain which part of the manifold is the culprit.[30] Conclusive falsification of a scientific theory is thus not possible.[31]

A third topic of criticism in the logical empiricist program is the theoretical-observational distinction. The argument here is that observations are inherently "theory-laden," that "facts" are not facts independent of a conceptual (theoretical) framework, and thus that there is no theory-independent observation language.[32] As Hanson puts it, "seeing is a 'theory-laden' undertaking" and thus

[26] An excellent discussion is provided by Waismann. See also Pierre Duhem, *The Aim and Structure of Physical Theory*, trans. Philip P. Weiner (New York: Athenum, 1962), esp. pp. 162, 187-90.

[27] Karl R. Popper, *Conjectures and Refutations* (New York: Harper and Row, 1965), p. 55. See also Popper, *The Logic of Scientific Discovery* (New York: Harper and Row, 1959).

[28] Popper, *Conjectures and Refutations*, p. 192.

[29] See Duhem, esp. pp. 187-88.

[30] See Hempel, *Philosophy of Natural Science*, pp. 23-31 (and esp. pp. 26-27).

[31] Hempel's discussion relies on Duhem's classic treatment of the topic. Now Popper sometimes (e.g., *Conjectures and Refutations*, p. 112, n. 6) denies that Duhem's analysis shows strict falsificationism to be untenable. Popper's discussion is phrased, as is Duhem's, in terms of "crucial experiments" (tests that distinguish two competing theories such that one is conclusively refuted and the other confirmed); Popper's argument is that the two theories are compared together with some "background knowledge," which knowledge the two theories have totally in common. It is thus Popper's view that the crucial test now decides between "two systems which differ *only* over the two theories which are at stake" (p. 112). But it seems improbable that two theories would in a given experimental situation employ exactly the same auxiliary hypotheses, let alone have in common the entire background knowledge against which the theories are formulated (cf., *SSR*, pp. 77-81, 99-103, and 146-50 on falsification and incommensurability). To be fair to Popper, it must be said that he sometimes acknowledges that conclusive falsification is not possible (e.g., *Logic of Scientific Discovery*, p. 50); but if that is the case, then the falsification program has no particular advantage over the verificationist one, at least with respect to the issues addressed here.

[32] See, e.g., Hanson (cited above, n. 17); Grover Maxwell, "The Ontological Status of Theoretical Entities," in *Minnesota Studies in the Philosophy of Science, Vol. III: Scientific Explanation, Space, and Time*, ed. Herbert Feigl and Grover Maxwell (Minneapolis: Univ. of Minnesota Press, 1962), esp. pp. 7-8, 14-15;

"there is more to seeing than meets the eyeball."[33] In Aune's words, "experience . . . always requires interpretation; and it is always some conceptual scheme, however rudimentary, and not virgin reality, that supplies the criteria by which an interpretation of experience is to be appraised,"[34] and hence "the meaning and acceptability of an observation claim are . . . ultimately determined by a system of background assumptions."[35]

Of all the lines of attack on the positivistic view that have been considered here, this is the most important, for it strikes at the heart of the logical empiricist program: at the assumption that there is a special observational vocabulary which is suitable for all scientific theories, which is neutral with respect to the claims of competing theories, and which forms the rock-bottom certain foundations upon which scientific knowledge can be erected.[36] Thus the denial of the theoretical-observational distinction has implications for the positivistic treatment of scientific knowledge and scientific progress. The positivists' claim that knowledge-claims are justified by reference to the foundational observation-statements can no longer be allowed to stand. The cumulativity of scientific knowledge at the levels of data-sentences or systematized data-sentences is questioned. And the occurrence of Nagelian intertheoretic reduction is minimized: later theories cannot always deductively absorb their predecessors, for later theories are often incompatible with earlier ones;[37] and even where the same terms are used by the two theories, there is no guarantee that the meaning of the terms will remain invariant.[38]

A fourth area of criticism focuses on the requirement that (e.g.) psychological discourse be translatable into physicalistic discourse. This criticism centers more directly on the applicability of the positivistic view to the realm of human phenomena. It is generally conceded that only material entities exist; that is, Cartesian dualism (a dualism of substances) is rejected. But granting this, there are yet two different conceptual frameworks one might use in approaching human beings and their activities. The first is a purely physicalistic scheme in which persons are construed as nothing but physical organisms; the alternative is an action-scheme, in which persons are construed "as agents, as beings who can act and who have intentions, motives, reasons, desires, and so forth."[39] Thus one might describe a person's behavior (where "behavior" is taken as a term neutral between the two approaches) in action-talk or in movement- (physicalistic-) talk. But there is a logical gap between the two accounts, for action-talk is not translatable into movement-talk. In part this is because a given action (e.g., mailing a letter, opening a win-

Peter Achinstein, "The Problem of Theoretical Terms," *American Philosophical Quarterly*, 2 (1965), 193-203; Kuhn, *SSR*, pp. 125-29, 145-46.

33 Hanson, pp. 19, 7.

34 Bruce Aune, *Knowledge, Mind and Nature* [New York: Random House, 1967) p. 83.

35 Aune, *Rationalism, Empiricism, and Pragmatism*, p. 148.

36 The logical empiricist program is a continuation of the Cartesian search for the ultimate bedrock foundations of knowledge. The "foundations view of knowledge" has increasingly been called into question [see, e.g., Aune, *Knowledge, Mind, and Nature;* Ludwig Wittgenstein, *On Certainty*, ed. G. E. M. Anscombe and G. H. von Wright, trans. Denis Paul and G. E. M. Anscombe (New York: Harper and Row, 1972)]; and the attack on the theoretical-observational distinction is where this larger attack finds its focus in the controversy surrounding the logical empiricist philosophy of science.

37 See Paul Feyerabend, "How to Be a Good Empiricist: A Plea for Tolerance in Matters Epistemological," in *Philosophy of Science: The Delaware Seminar, Vol. II*, ed. Bernard Baumrin (New York: Wiley-Interscience, 1963), p. 13.

38 Ibid., pp. 13-19; *SSR*, pp. 102, 149; and Ernest Cassirer, *Substance and Function* and *Einstein's Theory of Relatively*, trans. William C. Swabey and Marie C. Swabey (Chicago: Open Court, 1923), p. 365.

39 Charles Landesman, "The New Dualism in the Philosophy of Mind," *Review of Metaphysics*, 19 (1965), 329.

dow) can be accomplished by "an indefinitely large range of movements";[40] and in part because a given movement may in different circumstances and on different occasions instantiate different actions. Thus the choice between the two frameworks comes down to the question of "the most fruitful forms of explanation of behavior."[41] There is, argues Taylor, "no *a priori* way of deciding the issue."[42] That is, it is a question of trying the frameworks out and seeing which ultimately provides the most coherent and useful account. Thus the *a priori* requirement of physicalistic translatability is unjustified.[43]

In surveying criticisms of the received view, some rather important objections have been bypassed: the denial of the analytic-synthetic distinction,[44] general problems with the deductive-nomological model of explanation,[45] specific doubts about the applicability of the deductive-nomological scheme to human affairs,[46] and the problem of intentionality[47] come readily to mind. But hopefully the sorts of attacks outlined here have at least been sufficient to show that the received view has been on the receiving end of some rather damaging criticisms—sufficiently damaging for it to have been "abandoned by most philosophers of science."[48]

III

In this section focus is shifted to communication studies per se, and to the way the difficulties with the logical empiricist philosophy of science reflect on the conduct of communication theory and research. The recommendations, observations, and comments that follow are not all directly tied to particular criticisms of logical empiricism. Rather, ele-

40 Theodore Mischel, "Scientific and Philosophical Psychology," in *Human Action: Conceptual and Empirical Issues*, ed. Theodore Mischel (New York: Academic Press, 1969), p. 8.

41 Charles Taylor, "Mind-Body Identity, A Side Issue?" in *The Mind/Brain Identity Theory*, ed. C. V. Borst (New York: Macmillan, 1970), p. 240.

42 Taylor, *The Explanation of Behavior*, p. 100.

43 Those familiar with the work of Kenneth Burke may have noted that Burke's distinction between action and motion is related to the action-movement distinction discussed here. For Burke's distinction, see *A Grammar of Motives* (Berkeley: Univ. of California Press, 1969), pp. 14, 61; *Language as Symbolic Action* (Berkeley: Univ. of California Press. 1966), p. 67; *The Rhetoric of Religion* (Berkeley: Univ. of California Press, 1970), p. 40. But Burke's distinction is drawn "ontologically"; that is, Burke holds that there are two different sorts of things in the world, actions and motions. The distinction drawn here is a logical or conceptual one: a person's behavior may be approached through either of two different conceptual frameworks (an action framework as reflected in action-talk, or a movement framework as reflected in physicalist discourse). See George Sher, "Causal Explanation and the Vocabulary of Action," *Mind*, 83 (1973), 22, for a negative appraisal of ontological attempts to draw the distinction.

44 See Suppe (cited above, n. 1), pp. 67-80; and Willard Van Orman Quine, *From a Logical Point of View*, 2nd ed. (Cambridge: Harvard Univ. Press, 1961), pp. 20-46.

45 See, e.g., Michael Scriven, "Explanations, Predictions, and Laws," in *Minnesota Studies in the Philosophy of Science, Vol. III: Scientific Explanation, Space, and Time*, ed. Herbert Feigl and Grover Maxwell (Minneapolis: Univ. of Minnesota Press, 1962), pp. 170-230.

46 See, e.g., Michael Scriven, "Truisms as the Grounds for Historical Explanations," in *Theories of History*, ed. Patrick Gardiner (New York: Free Press, 1959), pp. 443-71; William H. Dray, "The Historical Explanation of Actions Reconsidered," in *Philosophy and History*, ed. Sidney Hook (New York: New York Univ. Press, 1963), pp. 108-10; Louis O. Mink, "The Autonomy of Historical Understanding," *History and Theory*, 5 (1965), 24-47; and Thomas P. Wilson, "Normative and Interpretive Paradigms in Sociology," in *Understanding Everyday Life*, ed. Jack D. Douglas (Chicago: Aldine, 1970), pp. 57-79.

47 This refers not to questions of purpose or intent, but rather to the fact that certain sorts of sentences (called "intentional sentences") fail to meet the positivistic requirement of extensionality (which roughly holds that scientific discourse must permit the substitutability *salve veritate* of coextensive expressions). This topic figures centrally in contemporary work in the philosophy of mind. See, e.g., D. C. Dennett, *Content and Consciousness* (New York: Humanities, 1969), and Dennett's "Intentional Systems," *Journal of Philosophy*, 68 (1971), 87-106. A useful collection of papers on this topic is provided by Ausonio Marras, ed., *Intentionality, Mind, and Language* (Urbana: Univ. of Illinois Press, 1972).

48 Suppe, p. 119.

ments of those criticisms are woven throughout the discussion, to the larger end of presenting a coherent treatment of the conduct of communication studies.

The first implication I see issuing from the criticisms of the logical empiricist view is this: maximally productive research involves the systematic extension, elaboration, and defense of a theoretical framework. As long as one holds the positivistic view that observations are theory-free, one can afford to go about randomly doing whatever experimental study comes to mind and strikes one's fancy. The researcher jumps from study to study, producing more and more "data" without any clear theoretical program underlying the activity. The "findings" all too often end up as a string of unconnected results, devoid of significance for want of clear connection to some conceptual framework that indicates their importance and utility. This picture is, I think, a not entirely unfair portrayal of a good deal of contemporary communication research.[49]

But when it is recognized that there is no theory-free observation language and that research findings have significance only within a larger theoretical framework, then the character of the research enterprise will be seen to change. Research can no longer be conceived as merely the production of "more data" as grist for the mill of some future theorist who will "put it all together." Rather theory and research must be seen as much more closely tied. Now it has of course always been acknowledged that "theory and research go hand in hand"—but research is usually conceived to lead the way. What is being argued here is that theories have, implicitly or explicitly, *always* led the way. Communication researchers are guided by theoretical frameworks in their selection of a particular area of study, in their conceptualization of problems, in their choice of relevant variables, in their methodological stance, and so on; "no experiment can be conceived without *some* sort of theory."[50]

If one of the prime virtues of the scientific enterprise is its publicness, these frameworks that give direction and significance to research must be made explicit. If it is theories that will ultimately provide a comprehensive understanding of communication, then research should be clearly related to a general theoretical perspective (not merely to "previous research"); and thus research reports should show how the research extends the framework to new areas, or how it elaborates and fills out the theory, or how it tests the strength of the theory's claims. If, in sum, a satisfactory theory of communication is what is wanted, then the researcher should embrace that theoretical view he finds best and undertake programmatic research under its aegis.

The significance of these sorts of considerations can perhaps be more clearly seen by considering Robert Sanders' seemingly Kuhnian discussion of the study of "speech-using behavior." Sanders suggests there are two primary alternative paradigms (the "classical" and the "grammatical") for the investigation of such behavior. The question, as he sees it, is which paradigm to invoke. His answer is this:

> . . . since what is wanted is a way to discover which paradigm to invoke, it is initially necessary to undertake research outside both paradigms. . . . what is crucial in research of this kind is that hypotheses are avoided which

[49] Phrased in Kuhnian terms, my argument could be put this way: research is typically pursued as though communication theory were in a period of normal science, when in fact communication theory is faced with multiple paradigms.

[50] *SSR*, p. 87, italics added.

assume the reality of variables that are peculiar to only one of the paradigms. Although [this] method of locating the appropriate paradigm might seem tortuous, the only alternative would be to invoke arbitrarily one of the paradigms and then examine the fruitfulness of study within it. Although this [latter] method offers the possibility of an immediate (though conceivably false) sense of progress, it is far less tenable; even if there were some way to know whether the paradigm had been tried sufficiently, in the event that results were not fruitful it would be impossible to tell whether the problem was with the paradigm or the quality of study within it.[51]

While Sanders employs Kuhn's terminology, his suggestions for research are undercut by Kuhn's whole program. On Kuhn's view there are no paradigm-free facts; there is no possibility of paradigm-free research that will allow us to "discover which paradigm to invoke."[52] Research that is undertaken "outside both paradigms" is (1) of problematic relevance to the choice between the competing paradigms; and (2) itself necessarily conducted within a third paradigm which will now compete with the original two.

To amplify the first point: any research Sanders might conduct (in trying to determine which paradigm to choose) might very well not be accorded the status of "fact" by one or both of the original two paradigms, and in that sense would not be relevant to deciding the worth of a particular paradigm. To the extent that the two paradigms differ over the appropriate description of what happens in one of Sanders' investigations (i.e., to the extent that they differ over the "facts"), any research of the sort Sanders urges is, on a Kuhnian view, predestined to fail to indicate the superiority of one of the paradigms.

The second point is more important, however. Given that the proposed research will necessarily be conducted under the auspices of *some* conceptual framework, where do those concepts come from? It seems that on Sanders' view concepts peculiar to one of the original two paradigms ought not be employed. One is apparently left with concepts that appear in neither or both of the original competitors. Where concepts are employed that play a role in neither view, however, the relation of Sanders' proposed research to the original two paradigms is, as just noted, problematic. And where concepts are employed that seem to play a part in both paradigms, there is no guarantee that an apparently "shared" concept will in fact be shared. Concepts have meaning, on Kuhn's view, only within an entire conceptual (theoretical) framework. As the framework changes, the stability of the meaning of the concepts is problematic. And so any term, any label, that occurs in both paradigms cannot be guaranteed to unchangingly retain its meaning when one shifts back and forth between theoretical perspectives. As Kuhn points out, terms like "spatial position," "mass," and "time" occur in both Newtonian and Einsteinian physical theory, but "the physical referents of these Einsteinian concepts are by no means identical with those of the Newtonian concepts that bear the same name. (Newtonian mass is conserved; Einsteinian is convertible with energy. . . .)"[53]

[51] Robert E. Sanders, "The Question of a Paradigm for Speech-Using Behavior," *QJS*, 59 (1973), 10. Cf. Robert E. Sanders and Larry W. Martin, "Grammatical Rules and Explanations of Behavior," *Inquiry*, 18 (1975), 78-79, for a somewhat different formulation of this point.

[52] See, e.g., *SSR*, pp. 125-29. Kuhn's unfortunate locution, "pre-paradigm research," is thus quite misleading, as Kuhn himself has pointed out. See Kuhn, "Reflections on My Critics," in *Criticism and the Growth of Knowledge*, ed. Imre Lakatos and Alan Musgrave (Cambridge: Cambridge Univ. Press, 1970), p. 272, n. 1; and esp. Kuhn, "Second Thoughts on Paradigms," in *The Structure of Scientific Theories*, ed. Frederick Suppe (Urbana: Univ. of Illinois Press, 1974), pp. 460-61, n. 4.

[53] *SSR*, p. 102; see also p. 149.

In sum, Sanders seems to have fallen prey to the positivistic philosophy of science implicit throughout communication theory. He failed to realize that a key element in Kuhn's attack on the positivistic view is the denial of a theory-free observation language. If Sanders is to be true to his seemingly Kuhnian colors, then the only way to proceed *is* to "arbitrarily" invoke *some* theoretical view—either one of the original two, or Sanders' third, albeit implicit and perhaps ill-formulated, paradigm. ("Arbitrarily" is enquotated, since I argue below that, even in the absence of direct research evidence, there can be good reasons for adopting or rejecting a theoretical perspective). There is no atheoretical stance from which data can be collected. Data must instead be seen as intimately wed to conceptual frameworks; and research should be conducted accordingly.[54]

There is a second implication I draw from the criticisms of the received view: theoretical and conceptual analysis should be recognized as a productive—even necessary—element in the achievement of a satisfactory theoretical account. As a way of approaching this implication, consider the hard-line positivistic view that operational definitions exhaust the meanings of theoretical terms, that there is an "upward seepage" of meaning from the observational to the theoretical level. Some serious philosophical objections to this view have already been noted. But entirely apart from such objections, it can be shown that communication researchers have long operated with the implicit presupposition that theoretical terms and statements are meaningful apart from operational definitions. This is most apparent in the emphasis laid on establishing the validity of operational definitions.

If Bridgman's strict operationalist stance is taken, then there is no question of the "validity" of an operational definition. The meaning of the theoretical term just *is* "the corresponding set of operations," and there is no "other meaning" of the theoretical term against which the operational definition is to be judged. As Wallach puts it, "if one assumes that operational definitions are the source of empirical meaning, real justification of operational definitions is impossible."[55]

But if theoretical terms are *a priori* meaningful, then the operational definition must somehow match up to this meaning; the operational definition must be a *valid* measure of the concept. Williams' definition of validity will here serve as well as any: "the degree to which the researcher measures what he claims to measure."[56] Now if the operational definition exhausts the meaning of the theoretical term, then the researcher necessarily measures what he claims to measure. But if theoretical discouse is meaningful apart from operational definitions, then the question of validity can arise.

Thus the recurrent concern of communication researchers for the validity of measures evinces an implicit acceptance of the *a priori* meaningfulness of theoretical discourse.[57] Recognition of

[54] I should perhaps emphasize that I do not see anything I have said here as providing grounds for concluding that the scientific enterprise is "irrational," or that the process of scientific change is a capricious one. While Kuhn's critics frequently attribute such conclusions to him, I believe this does Kuhn an injustice; see, e.g., Kuhn, "Reflections on My Critcs," pp. 235, 259-66.

[55] Wallach (cited above, n. 24), p. 604.

[56] Frederick Williams, *Reasoning With Statistics* (New York: Holt, Rinehart and Winston, 1968), p. 22.

[57] See, e.g., Ralph R. Behnke and Larry W. Carlile, "Heart Rate as an Index of Speech Anxiety," *SM*, 38 (1971), 65-69; D. Thomas Porter and Gerald P. Burns, Jr., "A Criticism of 'Heart Rate as an Index of Speech Anxiety,'" *SM*, 40 (1973), 156-59; Larry W. Carlile and

the untenability of the extreme operationalist view can also be seen in Campbell and Fiske's well-known discussion of validation. They suggest that the researcher should generate from the theoretical construct "not one operational embodiment, but two or more, each as different in research vehicle as possible."[58] But they note that in many cases of multitrait-multimethod matrix validation "no relationship may be found between two methods of measuring a trait." They suggest that when this occurs the investigator should entertain several alternative explanations for that result, including "neither method is adequate for measuring the trait" and "one of the two methods does not really measure the trait."[59] Both of these proposed possible explanations make it clear that Campbell and Fiske implicitly acknowledge the meaningfulness of theoretical terms and statements apart from any operational definitions.

But if theoretical claims are meaningful apart from operational definitions, then purely theoretical discussions are (at a minimum) justifiable—if not often genuinely productive. If such discussions are to be possible, theoretical frameworks must be publicly formulated and hence open to critical public scrutiny. The public formulation is, of course, required if (as suggested above) the relations between research efforts and the conceptual frameworks in which they are embedded are to be clear. The more important facet here is the "critical public scrutiny"—and it is just this careful, reflective conceptual analysis that communication theory typically lacks.

One wishes, for example, that a critical eye had early been turned on the notion of "ego-involvement." The concept is vague and confused, quite apart from the search for a suitable operational definition. Wilmot's insightful discussion is especially relevant here:

> Based on Sherif's work, ego-involvement may be a function of (1) the willingness to join a social group that is concerned with a topic, (2) the amount of social support one has for his position, (3) an unknown personality variable associated with joining extreme groups, (4) the amount of information one has on an issue, (5) the strength of emotional feeling on an issue, or (6) how publicly one is committed to his position on an issue. Even the extremity of one's attitude may in some situations have an influence on the degree of ego-involvement. To further complicate the choices, ego-involvement might even be the combination of any or all of the above elements.[60]

Those familiar with the current status of social judgment-involvement theory will doubtless concur that the concept of ego-involvement is amorphous and vague. To correct this situation, Wilmot makes nine recommendations, all of which involve doing *more* research (and indeed are called "research guidelines").[61]

What I am suggesting is that more research can only compound, not rectify, the difficulties Wilmot so skillfully identified. One really cannot hope to find a clear and acceptable "operational definition" of a vague and incoherent concept. Now Wilmot suggests that "the various operational definitions of ego-involve-

Ralph R. Behnke, "A Retort to the Criticism of 'Heart Rate as an Index of Speech Anxiety,'" *SM*, 40 (1973), 160-64; Harold Nichols and Raymond G. Smith, "Perception of Intensional and Extensional Meaning Domains in a Semantic Differential Application," *SM*, 40 (1973), 322-25.

58 Donald T. Campbell and Donald W. Fiske, "Convergent and Discriminant Validation by the Multitrait-Multimethod Matrix," *Psychological Bulletin*, 56 (1959), 101.

59 Ibid., p. 104.

60 William W. Wilmot, "Ego-Involvement: A Confusing Variable in Speech Communication Research," *QJS*, 57 (1971), 434-35. See also Ruth Anne Clark and Roy Stewart, "Latitude of Rejection as a Measure of Ego-Involvement," *SM*, 38 (1971), 228-34; and R. Harré and P. F. Secord, *The Explanation of Social Behaviour* (Oxford: Basil Blackwell, 1972), pp. 300-02.

61 Wilmot, pp. 435-36.

ment should be explored for their possible relation to concepts other than involvement," since, for example, the latitudes may not be indicative of levels of ego-involvement.[62] But how can one possibly tell whether latitude measures are related to ego-involvement if one has no clear idea of just what ego-involvement *is*? More careful thinking and less research is what is wanted. And if in the case of ego-involvement that critical scrutiny had occurred, there might not be the conceptual confusion Wilmot identifies and research time might have been spent more productively.

Research methods, being theory-laden, must not be exempted from this critical analysis. As an example of the significance of unexamined method, consider Martin Fishbein's well-known treatment of attitude as a function of beliefs and their evaluative aspects.[63] In Fishbein's theory, predicted overall attitude is obtained by multiplying the evaluation of a given belief by that belief's belief-strength score, and then summing the products across beliefs. To measure belief strength and evaluation, Fishbein and Raven developed the seven-step AB scales.[64] Now the two most common methods of scoring seven-step scales are from 1 to 7 and from -3 to $+3$. In most cases the choice of scoring method makes no difference. But in Fishbein's theory the two methods provide different results (because of the possibility of multiplying by zero in the -3 to $+3$ method) and embody different theoretical assumptions (about, e.g., the nature and operation of evaluatively neutral beliefs). Yet investigators employing Fishbein's approach have generally not reported which method was employed, nor recognized that substantive claims were implicit in their choice of method.[65]

It should be clear from these examples that careful reflective analysis can bring conceptual difficulties to light, can induce the clarification of a theory's claims, concepts, and methods, and can lead to maximally productive research efforts. But it can also allow reasoned non-arbitrary choices between competing views, even in the absence of direct research evidence. This was Sanders' problem: how to choose between competing paradigms. I have already argued that his solution ("undertake research outside both paradigms") is inadequate. What one *can* do is carefully inquire into each theory's concepts and claims. To the extent that unclarities, confusions, and contradictions are uncovered in a theory, one should be wary; an incoherent theory will surely not provide a clear understanding of human communication.[66] And this critical analysis of theoretical frameworks should not be conducted in the solitude of one's study or in the limited forum of the seminar room; it should be public inquiry, itself open to criticism. What must be encouraged is public discussion of the conceptual foundations of theoretical approaches to communication. Without such critical analysis communication theorists will doubtlessly be lost in an increasing mass of confused and unconnected "empirical findings," still vainly hoping that "more research" will provide all the answers. What is striking

[62] Ibid., p. 435.

[63] See, e.g., Martin Fishbein, "A Behavior Theory Approach to the Relations between Beliefs about an Object and the Attitude Toward the Object," in *Readings in Attitude Theory and Measurement*, ed. Martin Fishbein (New York: Wiley, 1967), pp. 389-400.

[64] Martin Fishbein and Bertram Raven, "The AB Scales: An Operational Definition of Belief and Attitude," *Human Relations*, 15 (1962), 35-44.

[65] In the case of the AB scales there are in fact four (not two) scoring methods, since the A-scales can be scored using one method and the B-sales scored using the other method.

[66] Examples of the kind of analyses I have in mind can be found in Noam Chomsky, "Review of Skinner's *Verbal Behavior*," *Language*, 35 (1959), 26-58.

is that communication researchers have implicitly recognized the justifiability of theoretical discussions, yet nevertheless been blind to the possibility that such discussions might advance the understanding of communication.[67]

These two implications may jointly provide the basis for dispelling some of the discomfort many have voiced concerning the apparent fragmentation and lack of unity within our field. There are at least two central facets to the idea of a unified discipline: agreement on disciplinary goals and agreement on which conceptual framework is best suited to the achievement of those goals.

Regarding agreement on goals: it is surely apparent by now that a theorist who adheres to a positivistic philosophy of science will in some sense have goals very different from one who rejects the received view. Yet if the statement of goals is put sufficiently generally, commonalities may emerge. Supose, for example, that our field took "the understanding of human communication" as its goal; Campbell has in fact recently suggested something like this.[68] Recognition of this goal might both allay fears of fragmentation and sharpen the discussion of theoretical and philosophical differences. Mink, for example, notes that the logical empiricist view treats deductive-nomological explanation as the only genuine and legitimate form of understanding; he argues that this approach is unjustified.[69] Insofar as differences like this (over the appropriateness of the deductive-nomological scheme) separate theorists who study communication, common goals can be recognized and yet fruitful discussion of differences can be pursued.

Regarding agreement on the best available conceptual framework: students of communication are presently faced with a plurality of theoretical perspectives. No one approach has won wide-spread allegiance. But the present plurality of theories might be replaced by a more-or-less unitary viewpoint; that is, one particular theoretical view might come to hold sway by virtue of its substantive accomplishments and its "promise of success."[70] Now Becker has argued that if we are to have a unified field "we must have some modicum of agreement on a logically consistent set of related concepts that can be applied to communication phenomena and that help us explain or make theoretical statements about their relationships"—in short, we must have a unitary theoretical approach.[71] In contrast, the view offered here is that there is a useful sense of "unified discipline" which does not turn on adherence to a common theoretical framework, but rather on common goals (even if phrased generally). One advantage to the present approach is that it allows debates over fundamentals to emerge, whereas Becker's line either presupposes agreement over fundamentals (which surely is lacking presently) or submerges such differences (and such hidden disputes have a way of viciously reappearing). Now a single theoretical framework may come to deservedly enjoy wide admiration because of its past

[67] Along the same lines, Wallach (cited in n. 24) gives a good summary of the arguments surrounding operationism, yet merely concludes that operationalizations must be justified by reference to the independent meaning of the theoretical concept (a conclusion, as just noted, not at variance with contemporary practice); thus she too fails to recognize the implication I have drawn out. Cf. Fritz Heider, *The Psychology of Interpersonal Relations* (New York: Wiley, 1958), for an approach that stresses "theoretical analysis and conceptual clarification" (p. 4); a similar emphasis can be found in Harré and Secord.

[68] Karlyn Kohrs Campbell, "Criticism: Ephemeral and Enduring," *ST*, 23 (1974), 12.

[69] Mink (cited in n. 46, above).

[70] See *SSR*, pp. 23-24.

[71] Samuel L. Becker, "Presidential Message," *Spectra*, 10 (Feb. 1974), 2.

achievements and expected successes. But such admiration is, I submit, likely to be had only by the hard work of the theorist in elaborating, extending, and defending a single framework.

This sort of approach is all too rare. Most communication researchers seem obsessed with "doing studies" at the expense of theoretical explication and analysis. Both Sanders (faced with choosing between competing paradigms) and Wilmot (faced with conceptual confusion) seem to see no alternative but more research. And even Smith only urges a rejection of behavioristic methodology. But simply changing the *methods* of research is not enough. Rather what is required is the renunciation of the positivistic philosophy of science, to clear the way to a reconceptualization of the study of human communication: a reconceptualization that stresses conceptual analysis and programmatic research.