

I am taken to task for not having accomplished all of this yet. Schutz never contended that an interpretive social science could be built in one fell swoop. In my earlier response,<sup>8</sup> I indicated how coding categories were the product of the everyday social actors who produced the communicative behaviors subsequently coded. I then used Markov chain models to formulate second-order constructs of everyday communicative action. The model is in the process of being tested in the social science community. When that task is more satisfactorily completed, the propositions generated must be reformulated into first-order propositions to be validated in the everyday social world where they originated.

Specific points of apparent disagreement aside, however, it was never my intent to demonstrate exegetically that Schutz and Brodbeck were saying the same things. My objective was to mine the neutral analogies uncovered by juxtaposing certain of Schutz's ideas to certain of Brodbeck's ideas. Any work of a fertile mind, such as Schutz or Brodbeck, can be interpreted in several ways. Certainly, a fertile mind can be expected to change over time—as did Schutz's. O'Keefe and Grossberg, and Natanson for that matter,<sup>9</sup> are entitled to informed opinions of what Schutz "really" meant in his many works. I find it more challenging to use the thinking of the most fertile minds to stimulate my own empirical and theoretical activity.

*in Sociology* (Englewood Cliffs, N.J.: Prentice-Hall, 1966).

<sup>8</sup> Leonard C. Hawes, "A Response to Grossberg and O'Keefe: Building a Human Science of Communication," *QJS*, 61 (1975), 209-219.

<sup>9</sup> I am referring here to the as yet unpublished work of Natanson which O'Keefe and Grossberg cite in their rejoinder: Maurice Natanson, *Missouri Symposium on Phenomenology and the Social Sciences*, ed. Joseph Bein (Columbia: Univ. of Missouri Press, forthcoming).

I am not suggesting that exegetical scholarship is without merit. I am suggesting that unless it is tied to theoretical and empirical scholarship, its full impact and utility cannot be realized. I suggest that Grossberg and O'Keefe add to their exegetic interests some empirical and theoretical concerns and thereby assist more broadly in our collective efforts to understand and explain human communication.

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#### THEORETICAL CLARITY AND INTERPRETIVE SOCIAL SCIENCE

We hope the following brief points illuminate certain issues in our dialogue with Professor Hawes, to the end of a clearer picture of our differences.

(1) When Hawes wrote that "'behaviors' and 'meanings' are coextensive" we took him to mean that the two terms had the same logical extension (i.e., referred to the same objects); our reading followed the usual philosophical sense of the term (see *OED*) and dovetailed with Hawes' claim that the "patterns of communication *constitute* the 'because' and 'in-order-to' motives." The rejoinder makes it clear that by "coextensive" was meant something very different and quite idiosyncratic, and that whatever "constitute" meant, it did not mean "compose." Careful word choice, however, is essential to the clarity and precision the rejoinder urges.

(2) Our linkage of Brodbeck's objectivism with behaviorism is characterized as a "force-fit argument," "guilt by association" that "simply will not wash." The charge unfortunately ignores Brodbeck's explicit statement that "objectivism" is her broader term for a view

"known as behaviorism" in psychology.<sup>1</sup> A careful reading of an author's work is essential to a full and fruitful understanding of that work.

(3) We are charged with separating Schutz's conception of action from his prescriptions for the study thereof, but here we follow Schutz's own procedure. His discussions of the concept of action, projected and completed acts, the two types of motive—all were conducted "while our study was as yet limited to the stream of consciousness of the solitary Ego. . . . We then turned to an analysis of the social world," of ideal types and the methods of social science.<sup>2</sup> And in avoiding the details of Schutz's phenomenology we are one with Hawes; we all avoid the phenomenological fine-points (polythetic Acts, internal time-consciousness, the Ego-ray, prephenomenal experience) in chapter two of *PSW*—after which, as Hawes notes, Schutz abandons "the strictly phenomenological method."<sup>3</sup>

(4) Hawes and we hold different views of the import of interpretive social science; his reading rests heavily on the "Concept and Theory Formation" article, an essay described by Maurice Natanson (in the *Missouri Symposium on Phenomenology and the Social Sciences*, ed. Joseph Bien [Columbia: Univ. of Missouri Press, forthcoming] as a singularly misleading guide to Schutz's views. This essay seemingly supports Hawes' construal of interpretive social science as beginning with "subjective data" and ending with "objectively valid theories." But this construal over-

looks important subtleties in Schutz's work and the interpretive view.

Hawes' view of the origin of interpretive social science as "subjective data" overlooks the reason *why* interpretive theories begin with the perspectives of naive social actors: the everyday world is meaningful to those who live and act in it, and their actions are based on that meaningfulness.<sup>4</sup> This oversight leads Hawes to misconstrue the sense in which the destination of interpretive theory is objective theory formulated according to formal logic. Subtleties are again important. First, Schutz's special sense of "objective" should be recalled (see section II of our essay). Second, Hawes' characterization of the endpoint of interpretive investigation omits the *key* feature of Schutz's sought-after theory. Given the meaningfulness of the social world, the observer's theory must be brought *back* to that world. Schutz stressed that when "the social scientist observes human interaction patterns . . . he has to interpret [them] in terms of their subjective meaning structure lest he abandon any hope of grasping social reality."<sup>5</sup> The key question thus is, do the "patterns of behavior" revealed by Markov (or similar probability) analyses have any meaningful status in the everyday world? Hawes' research merely reports the patterns without considering their meaningfulness to everyday social actors. Never are the Markov patterns brought back to the life-world, to the level of commonsense typifications of *patterns* (as opposed to typifications of *actions*, which on Hawes' account are obtained through stimulated recall). Starting from the "subjective data" of stimulated recall is thus not the same as giving the patterns a subjective in-

<sup>1</sup> May Brodbeck, "Meaning and Action," *Readings in the Philosophy of the Social Sciences*, ed. May Brodbeck (New York: Macmillan, 1968), p. 59.

<sup>2</sup> Alfred Schutz, *The Phenomenology of the Social World*, trans. George Walsh and Frederick Lehnert (1932; Evanston, Ill.: Northwestern Univ. Press, 1967), p. 217.

<sup>3</sup> *Ibid.*, p. 97.

<sup>4</sup> Alfred Schutz, *Collected Papers* (The Hague: Nijhoff, 1962, 1964), I, pp. 5-6; II, p. 85.

<sup>5</sup> *Collected Papers*, I, p. 40.

terpretation; instead the patterns are treated in the first sense of "pattern" noted in section V of our essay, not the second (interpretive) sense.

(5) Differences in interpretation are always difficult to resolve in print, since only short citations from an author's work can be presented. Our reading of the complete programs of Schutz and Brodbeck has then holding widely divergent stances and fundamentally opposed epistemological orientations; as indicated in our earlier essay, we do not

feel the advancement of theoretical issues best served by playing free with selected concepts drawn from theorists of radically contradictory perspectives. While we are naturally inclined to think our reading exegetically preferable, it is a question readers can best decide for themselves through careful examination of the relevant works.

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## A REPLY TO METAPHOR AND LINGUISTIC THEORY

### I

Current directions in linguistic theory relative to the vexing problem of describing and explaining metaphor are certainly of interest to communication theorists. It is because of such significance that Paul Campbell's problematic article "Metaphor and Linguistic Theory" (hereafter cited as MLT) must be confronted.<sup>1</sup>

The crucial conceptual issued is that MLT is *not* an argument. First, MLT should have acknowledged the well recognized fact that the Katz/Fodor position (hereafter cited as KF) did not include figurative language nor even claim to account for metaphor.<sup>2</sup> Second, Dwight Bolinger in "The Atomization of Meaning" (1965) responded directly to the structural faults in the KF position

and convincingly argued that a semantic theory must include metaphor.<sup>3</sup> Third, a short time later (1966) Uriel Weinreich continued the argument and thoroughly discredited the KF position. Weinreich also opened the way for significant theoretical advances in Section 3, "A New Semantic Theory," of the same article cited in MLT.<sup>4</sup> Fourth, the conceptual position of MLT is 10 years out of date and would commonly be described as some variety of that infamous academic strategy—the "straw man."

Therefore, MLT presents no substantive argument. Curiously, however, the basis for an argument appears as an "appendage" to be found when one reads the specific *articles* referred to in MLT footnotes 11, 25, and 28. In fact, the interested reader might take the

<sup>3</sup> Dwight Bolinger, "The Atomization of Meaning," *Language*, 41 (1965), 555-573.

<sup>4</sup> Uriel Weinreich, "Explorations In Semantic Theory," *Current Trends In Linguistics*, ed. Thomas A. Sebeok (The Hague: Mouton, 1966), pp. 395-477. MLT cites a reprint in *Semantics: An Interdisciplinary Reader In Philosophy, Linguistics and Psychology*, ed. Danny D. Steinberg and Leon A. Jakobovits (London: Cambridge Univ. Press, 1971). This reader uses only part of Weinreich's article and so acknowledges on p. x. MLT is innocent of Sections 3 and 4 in Weinreich.

<sup>1</sup> Paul Newell Campbell, "Metaphor and Linguistic Theory," *QJS*, 61 (1975), 1-12.

<sup>2</sup> Jerrold J. Katz and Jerry A. Fodor, "The Structure of a Semantic Theory," *The Structure of Language: Readings in the Philosophy of Language*, ed. Jerrold J. Katz and Jerry A. Fodor (Englewood Cliffs, N.J.: Prentice Hall, 1964), p. 479, 497-498.

