Murphey Symposium

Some Take the Linguistic Turn; Some Don't

Jay Mechling

I found reading Murray G. Murphey's *Philosophical Foundations of Historical Knowledge* both familiar and strange. It is easy to account for the familiarity. I was a graduate student in American Civilization at the University of Pennsylvania from 1967 to 1971, a period that was (I think) the most fertile and dynamic period for Murray's first putting together the elements I see in the *PFHK*. Put differently, the excitement of being his student at the time was seeing him come into class every session with some new set of notes around an idea fresh from his writing the night before (or so it seemed to this student).[1] We were witnesses to ideas-in-the-making, and it was dazzling.

By 1967 Murray had assimilated the anthropology of Penn colleagues A.I. Hallowell and Anthony Wallace fully into his thinking, and we read Kuhn under Murray's tutelage. Quine, Carnap, Peirce, and other philosophers show up in my graduate course notes from the period, and we knew that Murray was at work on a collaborative (with Elizabeth Flower) history of American philosophy.[2] Murray's *Our Knowledge of the Historical Past*[3] appeared soon after the end of my graduate study, and between the covers of that book lay ideas that I had heard in the classroom and in office conferences from 1967 through 1971. Michael Zuckerman was my dissertation director, but the considerable things I learned from Zuckerman seemed to reinforce what I had learned from Murray.[4]

0026-3079/95/3702-147$1.50/0

So I was launched on my own in 1971, carrying Murray's ideas into the relatively new American Studies Program at the University of California, Davis. I taught Kuhn's *The Structure of Scientific Revolutions*[5] and Anthony F.C. Wallace's *Culture and Personality*[6] to my students at UC Davis, but it was always Murphey I was teaching. I taught a methods course for American Studies majors and even, for a few years, tried to teach the undergraduates how to "read" (if not use) statistics rendered in social science research. In short, I had brought "the Penn approach" (in large part, the Murphey approach) into an undergraduate program run by three Ph.D.s (Robert Merideth, Brom Weber, and David S. Wilson) from the University of Minnesota American Studies program.

I read Berger and Luckmann's *The Social Construction of Reality*[7] right at the end of my graduate study, too late to use in my dissertation, but I saw in that "treatise in the sociology of knowledge" a useful elaboration of what I had learned as the Penn approach. So as the years and then decades passed, I plunged heavily into Peter Berger's sociology, Clfford Geertz's anthropology, Gregory Bateson's transdisciplinary ideas, and eventually back to William James, whose *Pragmatism* was the book at the center of my first term paper in college. Having used a postdoctoral year at the 1975-76 Yale Humanities Institute to read broadly in folklore studies, by the mid-1980s I had become interested in the uses of neopragmatism as the philosophical foundation for the study of common sense in everyday life. I was editor of *Western Folklore* by then and put together a special section of essays on William James and the study of everyday life. I had been reading beyond James to the neopragmatists, including Rorty. In Richard Rorty, I thought, I had found a statement of neopragmatism that fit well the trajectory begun with my graduate study under Murray.

Imagine my surprise on reading *PFHK* that Murray and I have landed in two different places.

My first clue came when, even before beginning the book, I checked the name index and found no reference to Rorty. In fact, Murray's treatment of Hayden White's arguments about the relation between narratives and history suggests a rejection of the postmodern, postrationalist position that I thought was the logical conclusion to the journey I had begun with Murray in 1967. Was this the same Murphey who had written "On the Relation Between Science and Religion" for the *American Quarterly* in 1971?

But I jump ahead here in my account of the combined familiarity and strangeness I experienced reading *PFHK*. I found I could agree easily with all eight premises Murray lays out in his introduction (x), and my allegiance to pragmatism stirred when I understood that Murray's main antagonist in this book is Quine, with Quine's failure lying in his inability to resolve the tensions between the two traditions—pragmatic naturalism and Logical Empiricism—he inherits (xii). Still, in the introduction, Murray signals that he believes Quine is correct in his pragmatic epistemology but errs in his adherence to a Logical Empiricism that leads to the faulty thesis of the Indeterminacy of Translation. The antidote, Murray hints early, is to keep the pragmatic naturalism (of Peirce, James, and Dewey) and to see "science as a process of inquiry."

Now, I can skip over the chapters on "Meaning and Reference," on "Other Minds and Intersubjective Knowledge," on "Causation and Explanation," on "Action," and on "Rules," as much of this material is familiar from his past work and I agree with Murray in large part. My one quibble would be with Murray's reliance on so many laboratory-based studies of children's cognitive development and acquisition of language skills. As I have come to work more on children's folk cultures in natural settings, I have come to be increasingly suspicious of the validity of studies based in laboratory rather than natural settings. But what Murray draws from these studies are the sorts of principles that seem to stand up across several sorts of research settings, so I can put aside my suspicions and grant that there are universal processes of cognitive development grounded in our biology. Indeed, the more I did fieldwork with male adolescents at a Boy Scout camp and the more I have examined male adolescent cultures across time and space, the more I am willing to temper my strict social-constructionist views and to make room for the power of developmental processes beyond the effects of culture (though culture may shape the behavioral outcomes of the processes). Hormones have their reasons.

Besides, Murray takes us through these studies toward substantiating the first premise that "There is a real world of which true knowledge is possible," a premise I accept. Berger and Lucklmann begin at somewhat the same place. "It will be enough for our purposes," they write in their Introduction to *SCR*, ""to define 'reality" as a quality appertaining to phenomena that we recognize as having a being independent of our own volition (we cannot 'wish them away'), and to define 'knowledge' as the certainty that phenomena are real and that they possess specific characteristics" (1). The meaning of Murray's phrase, "true knowledge," must await chapter 6's discussion of "Truth and Reality."

Chapter 3, on "Causation and Explanation," did begin to raise some suspicions I held, as Murray nowhere in that chapter acknowledges that explanations need to be persuasive to a community to whom the explanation is offered, a point I thought was central in Kuhn, and Murray's critique of Wittgenstein in Ch. 5, on "Rules," made me begin to worry that Murray was not going to follow the path to the postmodern condition.

It is in chapter 6 on "Truth and Reality" that I became most alarmed about the absence of Rorty from Murray's discussion. I thought I could be comfortable with Murray's brand of realism—in this case, that "True knowledge of the real world is possible even though complete knowledge of it is not" (213)—so long as we are talking about a pragmatic understanding of "true." Indeed, Murray defines "confirmation" as

> a process of testing a theory against alternative theories, and that theory is the better confirmed that makes all the known

> evidence more probable than do any of its rivals. The best theory may be defined, then, as that theory that always beats all challengers. . . . Of course, we could never know that a given theory was the best theory, since further tests would always be possible in the future. If we could know that our present theory was the best, we could have certain knowledge and certainty is what we shall never have. (232)

This seems fine to me, though I have to assume that Murray agrees that "beats all challengers" means that a theory looks "best" to a specific community that is the audience for the claim. And Kuhn tells us to look at all the criteria and interests an audience might invoke in preferring one theory over another.

Still on the crucial issue of confirmation, which Murray takes up again in the all-important chapter 7, "Knowledge of the Past," he poses the key question for readers in American Studies—namely, whether history is like the sciences in terms of confirmation. Acknowledging how history is not like the social sciences, Murray still holds out for a sort of confirmation. "The best we seem able to do at present," he writes, "is to develop theories that integrate as much of the existing data as possible" (300).

Now, this is a Kuhnian solution and is quite consistent with another formulation I like, this time from Clifford Geertz, who argues that interpretive anthropology is "scientific" in the same sense that we would feel comfortable calling clinical inference "scientific." It is worth quoting Geertz in full on this matter:

> To generalize within cases is usually called, at least in medicine and depth psychology, clinical inference. Rather than beginning with a set of observations and attempting to subsume them under a governing law, such inferences begin with a set of (presumptive) signifiers and attempt to place them *within an intelligible frame*. Measures are matched to theoretical predictions, but symptoms (even when they are measured) are scanned for theoretical peculiarities—that is, they are diagnosed. In the study of culture the signifiers are not symptoms or clusters of symptoms, but symbolic acts or clusters of symbolic acts, and the aim is not therapy but the analysis of social discourse. But the way in which theory is used—to ferret out the unapparent import of things—is the same.[8]

Put differently, the anthropologist or historian uses a method resembling clinical inference in that the culture critic creates a *narrative* that makes "best sense" of the array of seemingly unconnected symptoms or symbolic acts.

Now, there are at least two interesting features of this formulation, both of which seem (to me) to be compatible with Murray's approach (but also showing Murray's gap). First, clinical inference in medical diagnostics has a "reality test" of the sort Murray says is at work in the sciences. That is, hypotheses in clinical inference may have "power and adequacy" to the extent that a patient might actually improve after a given therapeutic intervention, though we never know for sure whether the intervention was the single variable changing. In any case, in medicine we might judge one explanation better than another to the degree that the patient improves with one treatment and not with another.

But, as Geertz says, neither the anthropologist nor, by implication, the historian has the same "reality test" available. In place of nature, Geertz offers the "intelligibility" of the narrative as the criterion for judging one explanation better than another. The question in this passage, of course, is "a frame intelligible to whom?" As in Kuhn's formulation, Geertz assumes that there is an *interpretive community* with criteria (some explicit, some implicit) for judging one narrative explanation superior to another. And those criteria, William James would suggest, amount to the community's criteria for judging the "truth" of an explanation. So Geertz offers a formulation of culture and of the interpretive method quite consistent with Kuhn and, I would argue, with Murray.[9] So why does Geertz play no role in *PFHK*?

Murray does articulate a criterion for comparing explanations, one that Kuhn and Geertz would embrace easily enough. If the better explanation is one that integrates "as much of the existing data as possible" (300), then, reasons Murray, "it is important to combine types of data which have different and offsetting biases. . . . A theory that can integrate a wide range of data types therefore has a better claim than one that is based on only one type of data" (300-301). Sometimes the data types must include inferred data, as a solution to the problem of missing data is the creation of inferred data from existing data (301-302).

This faith in the diversity of data types underlies, no doubt, Murray's confidence that historians will find a way to sort out the contradictory claims of Jack P. Greene's *Pursuits of Happiness*[10] and David Hackett Fischer's *Albion's Seed*.[11] "It should be emphasized that there are major differences in the theories proposed," writes Murray, "*and* that they are differences that can—and will—be settled by empirical research. These are not matters of the scholars's values somehow distorting their vision, of 'prejudice' or 'alternative and incommensurable interpretations.' They are matters of evidence and of testing contradictory theories against that evidence" (287). I doubt it.

Everything I have learned in thirty years of reading and writing culture criticism, including what I learned from Murray, makes me believe that *every* rhetorical act, every claim (including the one I'm making now) embodies the rhetor's interests. I have no faith that persuasion about a "best theory" rests only on "evidence." I have made " the linguistic turn," I have abandoned "the Enlightenment project" and moved to post-structuralist and other post-rationalist critical positions, which Murphey has not. And I still cannot account for that.

Note

1. I still have my notes from his graduate courses, including the typescript notes he used in teaching quantitative reasoning to the first year grad students.

2. Elizabeth Flower and Murray G. Murphey, *A History of Philosophy in America* (New York, 1977), 2 vols.

3. Indianapolis, 1973.

4. In retrospect, I know that I did not appreciate then what differences there were between Murray's and Mike's evolving ideas, though it was complicated by Mike's having been Murray's student as an undergraduate at Penn. Since then I've come to understand better the distinctive, powerful nature of Mike's ideas and approaches.

5. 2nd ed.; Chicago, 1970. Orig. 1962.

6. 2nd ed.; New York, 1970. Orig. 1961.

7. Garden City, NY, 1966.

8. Clifford Geertz, *The Interpretation of Cultures* (New York, 1973), 26. Emphasis added.

9. Geertz's notion of "culture" as "best seen not as complexes of concrete behavior patterns—customs, usages, traditions, habit clusters—. . . but as a set of control mechanisms—plans, recipes, rules, instructions (what computer engineers call 'programs')—for the governing of behavior" (*Interpretation*, 44) should suit Murphey. As I was revising this essay for publication, Clifford Geertz chimed in again with a review, entitled "Culture War," of two books—Gananath Obeyesekere's *The Apotheosis of Captain Cook: European Mythmaking in the Pacific* and Marshall Sahlins's *How "Natives" Think, About Captain Cook, for Example*—that take the same "evidence" and draw very different "truths" about the meanings of Captain Cook to the Hawaiians. Geertz's review, among other things, is an exercise in expressing the criteria that make one explanation more "persuasive" than another to one anthropologist, the reviewer. See *The New York Review of Books*, Nov. 30, 1995, 4-6.

10. Chapel Hill, 1988.

11. New York, 1989.