

Murphey Symposium

Replies

Murray G. Murphey

I am deeply grateful to the panelists who took time out from their own work to comment on my book, and to the editors of *American Studies* for publishing their comments and this response. I am also charmed to find that my colleagues, including my former students, consider the book perverse, but I am surprised by their surprise at its contents. I can only attribute that surprise to a confusion for which I am no doubt to blame. I have always held that the initial task in cultural analysis is to see a culture “from the inside”—that is, to understand the beliefs and actions of its members in their own terms (this is of course only the initial task but it is an essential one). For the purpose of understanding New England Puritans, it is irrelevant whether the doctrines of predestination, innate depravity, and visible sainthood are or are not true; what matters is that the New England Puritans believed they were true and took them as premises for their actions. But what people *believe* to be true is one thing; what *is* true is quite another. I have never said that all belief systems are equally true; quite the contrary, I have always held that some beliefs are true and others false. It is, I suspect, the emphasis I have laid upon the importance of emic approaches to the study of culture that has led my friends to attribute to me a cultural relativism I never held, and in fact denied in the book I published in 1973, *Our Knowledge of the Historical Past (OKHP)*. As will be clear below, my recent book is a sequel, or perhaps a prequel, to *OKHP* that embodies the same position I took there. I will therefore be referring to both in my replies.

Michael Zuckerman

Mike takes me to task on two basic points. First, he thinks that the parallels I find between science and history are mistaken. To make his point, he first refers to what I said about Fischer and Greene. The issue between Fischer and Greene that I referred to in the passage Mike quotes is the state of English culture at the time of the settlement of the colonies. If Greene is right, English culture was homogeneous and highly commercial; if Fischer is right, it was heterogeneous, with markedly different regional cultures. This is a question that will be settled empirically by English historians, not by Americanists. The fact that neither Fischer nor Greene has settled it does not therefore distress me. But Mike's broader point is that historians in general do not test their "interpretations"—what I call "theories"—the way scientists test their theories. Yes and no. He is right that historians seldom set out simply to test somebody's theory. But there is a process in history called "revisionism" that is a partial functional equivalent to the scientific process. What happens here? Most history books, like most scientific papers, are simply shelved and forgotten. But if an historian's theory commands wide interest and acceptance, it is fairly certain that in due course younger scholars, seeking to make names for themselves, will attack it, or "revise" it. Revision usually involves the presentation of new data that are supposed to prove the older theory wrong; very often it also involves a distortion of the prior theory to make it easier to attack. If the revision succeeds, then the new revisionist theory is enconced and the old one rejected; if it does not, "rerevisionists" will then show that the revision was flawed and will either offer a yet newer theory based on yet more data or resurrect the original one. Think for example of Robert Pope's attack on Perry Miller's declension model, Robert Brown's attack on Charles Beard's economic interpretation, the innumerable attacks on Frederick Jackson Turner's thesis, Eric Foner's attack on Lee Benson's theory of Jacksonian Democracy, etc. The examples are legion and well known. Whatever the motives for this process, it is in fact a process of testing the original theory designed to find its faults.

There is a further reason why the process of testing historical theories is not precisely parallel to the process of testing scientific ones—the extreme unpredictability of finding historical data. Once a scientist recognizes that his theory implies the occurrence of a particular phenomenon, he can—if the technology is adequate and the funding is available—design an experiment or observation to determine whether or not that phenomenon can be observed. But historians cannot generate their own data in this fashion. As I described in chapter 5 of *OKHP*, the finding of relevant historical data is often largely a matter of luck. Who in his wildest dreams could have predicted the finding of the Dead Sea Scrolls? Hence the process of bringing new data to bear on an historical theory to test its validity proceeds in a much more haphazard fashion than in science. But it does proceed! Where the parallel does break down is that historians do not usually attempt to prove an older theory true; if all you do is show that somebody else was right, no one will publish you.

Finally, Mike thinks I am unfair to narrativism. Perhaps. But my point is not really that historians don't write narratives—of course they do—but that narratives *per se* have no explanatory power. The explanatory power comes from the causal model. That model can be presented in narrative form, and often is, but it need not be, and in what is usually termed “social science history” increasingly it is not.

Mike's second point of attack is on my treatment of relativism. There are various sorts of relativism; what he finds wanting is my treatment of truth relativism. Of course if there is no agreement on what constitutes truth, one cannot prove to one's opponent that his theory is false. Nevertheless, I think that truth relativism *is* false. What has impressed me is the extraordinarily rapid spread of science and its standards around the world, crossing national and cultural boundaries with an ease shown by no other system of belief. I attribute this spread to the power and adequacy of science; people accept it because it is so useful to them, and having done so are forced to live with its standards. This, however, will not lead to the land of milk and honey. Controversy, as Mike rightly stresses, will continue between science and religion, science and ethics, etc. Where the belief systems opposed to science cannot be held true by *any* standard (and that I think is the case with ethics), this is not really a case of truth relativism. Where there are differing truth standards, I think that in those areas where there is conflict (as for example between science and creationism) science will win in the long run. The Christian Right has not been able to overthrow evolution. In the next twenty years, we will very likely have well confirmed cosmological theories explaining the origin and fate of the universe, and they won't fit creationism either. In the warfare between science and religion, the score is science 10, religion zip.

I do not say that everyone will come to love science any more than they have come to love the bomb. Controversies over nuclear power, Aids, etc. will of course persist. But these are not controversies over the *truth* of science; they are controversies over the uses to which science is put. Any theory that has power and adequacy can be misused; but if it were not substantially true it would not have power and adequacy.

Bernard Mergen

I find myself in basic agreement with Barney, and I am indebted to him for the marvelous line “History without truth is simply Disney.” I have always wondered why historians who claim not to believe in objective truth bother to do research. If we just make it up to suit our political creed, or our ethnic chauvinism, or our sexual preference, or our favorite fantasy, why bother to go to the library at all? Why not just sit home and spin out the prettiest, most dramatic tale we can think of? What would be the difference on that view between history and fiction? between sober scholarship and Disney? But this is not what historians do; they spend endless hours pouring over texts and objects, laboring to make sense of them and to devise theories that fit them. Their own practice is the most convincing refutation of historical relativism I know.

Barney's fascinating discussion of snow and ice taxonomies speaks to one of the fundamental problems of history—the translation of the language of one culture into another. I have tried to deal with this problem both in chapter 5 of *OKHP* and in the recent book. As Barney shows, despite the radical differences in language between the Eskimo, the Fort Norman Slave hunters, the snow scientists, and the historians, it is possible to say that certain answers to basic cross-cultural and cross-disciplinary questions are right. So far as I can tell, Barney and I are in agreement on these issues, and I welcome that confirmation.

Jay Mechling

Jay's comments strike me, as my book strikes him, as familiar and strange. Since I do not see my own position as having changed from the time I wrote *OKHP*, I am puzzled that he does. He is quite right that I reject what he calls "the postmodernist, postrationalist position"; I always did, and I am surprised that he does not.

It seems to me that Jay raises three important issues. First, he argues that every "claim" embodies the claimant's interests. I agree. So what follows? What the speaker's interests are has nothing to do with whether or not what the speaker says is true. The speaker may desire to deceive the auditor by telling him a lie, yet tell him the truth because the speaker's knowledge is faulty. As every reader of Watson's *Double Helix* knows, scientists are often very self-interested, yet that does not prevent their theories from being true.

Second, Jay argues that the aim in history should be to create an intelligible narrative. Let us leave the issue of narrative aside for the moment and concentrate on the issue of an intelligible interpretation, whatever its form. Jay seems to equate intelligibility with explanatory power and with truth. But what counts as "intelligibility" here? Apparently, a set of particulars is intelligible if it instances a pattern. Any pattern? Clearly no; if the analogy to clinical inference is taken seriously, it must be a recognizable pattern involving human beliefs and motives. Consider then as an example the standard male apparel regarded as proper in our society today—the white shirt, the coat and the necktie. Clearly, the coat is not a functional article of apparel; it is too warm in summer, and since it is worn either open or buttoned only at the navel, it leaves the upper chest unprotected and is thus an inadequate way to provide protection from the cold in winter. Then why is it worn? Why is it left open or so poorly closed? Apparently this is to allow the necktie to be displayed. And that makes intelligible why the shirt should be white; white provides the ideal background for showing off the necktie, particularly since the necktie is usually the gaudiest and the most highly decorated article of clothing worn. The necktie therefore seems to be the key article of apparel. The necktie has no evident utility; it constricts the blood supply to the brain, thus impairing cognitive functioning; it provides no warmth whatever and no protection against inclement weather; and it is a constant annoyance, dipping in the soup when one sits down to dinner, getting caught in machinery, and inhibiting

visibility when one bends over. It is, in other words, not only totally useless but an impediment. Its function must therefore be symbolic. But symbolic of what? Since it is worn by men rather than women, its function is obviously that of a phallic symbol—it is shaped like a phallus, it dangles like one, and it is the modern equivalent of the codpiece—a constant and flagrant reminder of and advertisement for what men do best. Is this an *intelligible* interpretation of these cultural artifacts? Yes. Is it *true*? No. Is it *explanatory*? Of course not. Intelligibility therefore is not equivalent to truth or to explanatory power. Indeed, anyone who has been trained, as I have, in New Critical methods for the exegeses of poetry must be aware that almost any interpretation can be tortured out of almost any poetic text if intelligibility is the only criterion. Intelligibility therefore is not an adequate criterion for choosing among historical interpretations. Nor, as noted above, need an explanation or a true theory be in narrative form. It can be presented in narrative form, but it can also be presented in non-narrative form.

The problem here is the claim that there is no “reality check” on historical theories. This claim seems to rest on the assumption that the data being explained constitute a closed fixed set, like the words constituting a particular poem. But the data rarely if ever have this character; there are always new data to be found, new ways of extracting information from known objects, new implications of the theory to be followed up, new theoretical linkages to be made. Certainly intelligibility is one criterion that one would want any historical theory to satisfy, but it is not the only criterion. Theories must integrate not only the currently known data but all the new data uncovered by future research; they must predict where to look and what to look for either in old data or in new; and they must be consistent with other theories dealing with related matters. There is a reality check.

But Jay’s basic point is, I take it, that he thinks intelligibility and truth are relative to a community—in other words, he seems to hold both to meaning relativism and to truth relativism. If he holds the former, I do not see how he can accept the eight propositions at the beginning of *PFHK*, which he says he does. If meaning relativism were true, if the systems of meanings of different “interpretative communities” were incommensurable, how could members of one culture understand the speech and thought of members of another? How would translation be possible? For that matter, how could we communicate with each other at all? Since I have argued at length in the book that meaning relativism is false, I will not repeat those arguments here. As to truth relativism, although I have dealt with that in the book and above, there is perhaps a further word to be said. Jay asks, true for whom? I would answer, for all human investigators. But only for humans? I am not sure of the answer to that. If there were another life form so constituted that its experience was totally different from ours,—if, for example, instead of seeing light (i.e., electromagnetic waves) it “saw” gravitational waves, would its theories converge to ours? I do not know the answer to that question.

Jay is right; I did not mention Rorty in the book. I hope I have made it clear why.

Karen Lystra

Karen is exactly right when she says that I am an antirealist, at least on questions of truth and meaning. As noted above, I do believe some theories are true and others are false; whether the earth is flat or spherical is not simply a matter of point of view. The fact that some “interpretative communities” have believed the earth to be flat just shows that some societies have held beliefs that are false. Similarly, human belief systems are not “incommensurable”, if that is supposed to mean that a believer in one cannot understand what the adherents of another believe. As I noted in the book, such claims of incommensurability rest in large part on misguided notions of translation.

She is also right that I think the “linguistic turn” has become a linguistic circle. Some philosophers who hold to empiricism (and I am an empiricist) have conceived a mortal terror of mental states such as concepts, and have sought safety in substituting talk of language for talk of thought. The safety is illusory. Having severed language from the mental states and processes that it expresses and that link it to the world, they have all too often ended in a form of linguistic idealism that betrays the very empiricism they sought to defend. That language is of the utmost importance for the understanding of human forms of life no one can deny; that it is all there is to culture is absurd.

Karen is suspicious of the criteria of knowledge used in studies of neonate cognition. I cannot review here the vast literature in this rapidly expanding field, but I strongly recommend that doubters examine this literature and talk to those working on this subject. As is so often true in science, my book was in these matters out of date before it was published, but subsequent work has, I believe, strengthened the case I made.

Karen is disturbed by my argument regarding memory, and she feels that I fail to treat the matter pragmatically. I am surprised by that criticism, since my treatment is based on that of C. I. Lewis, whose pragmatic credentials I would have thought impeccable. The argument for the *prima facie* credibility of memory is briefly this. Can we show that our memorial knowledge (i.e., what we know by memory) is true? Since errors of memory do occur, we cannot hold that all we remember is true. How then can we test which memorial propositions are true and which false? One can insist that true memorial statements must be consistent with each other, and indeed they must, but consistent systems are not always true. More is required. Can we then test memorial statements against observations? What observations? Think of any observation statement such as “I see a blue patch here.” To classify the patch as “blue” is to say it is like other patches I have experienced and learned to call “blue”—that is, the classification itself assumes the truth of *some* memories. Therefore one cannot prove memorial knowledge true on the basis of observation without assuming the truth of some other memories; one ends up chasing one’s tail. But if all observation statements assume the truth of some memories, one cannot deny the truth of memorial knowledge without landing in a total scepticism of the moment. Lewis’s answer

was to give memorial statements *prima facie* credibility. This permits any given memorial statement to be impeached (by inconsistency with other memorial statements, or testimony of witnesses, etc.) but it means we must assume our memorial statements in general to be true until they are proven false. This is in fact a pragmatic solution to the problem of memory; it lets us get on with the business of knowing without denying the fallibility of our knowledge.

But Karen is most disturbed by my claim that Cotton Mather has the epistemological status of a quark; that is, that historical entities are postulates in our theory about the past. But consider what can serve as historical evidence. Nothing can be evidence of the past that is not observable now (including memories which are our *present* ideas of the past). If that is so, then all I can know about Cotton Mather is what I can learn from present data. I have certain books, papers, etc. now that mention "Cotton Mather" or are signed "Cotton Mather", etc. I account for these by postulating that a person named Cotton Mather really existed at a particular time and place and did certain things. Since I cannot now observe Mather, this is the only basis I can have for believing he existed. Karen objects that others saw Cotton Mather in the seventeenth and eighteenth centuries. But how do I know that others—for example Samuel Sewell—saw Cotton Mather? All I know of Samuel Sewell is that I have certain documents that I see now that mention the name "Samuel Sewell"; I postulate Samuel Sewell as I do Cotton Mather to explain present evidence. The same is true of all the others who saw Mather. The whole set of historical figures and events in which we now believe consists of posits in our theory of the past, a theory the evidence for which can only be presently existing objects and documents.

Some of my readers have objected to this theory because they think the evidence for the existence of Cotton Mather is so overwhelming that it is absurd to say he is a posit in a theory. In part I think this reaction stems from confusing epistemological questions with ontological ones; epistemologically Cotton Mather is a postulate; ontologically he is real if the theory in which he is postulated to exist is true. But in part also this reaction is due to the sheer mass of data we have about him. Consider, however, Samson and Moses. Did Samson exist? We have but one document that says so. I think most scholars now doubt that there ever was such a person. Did Moses exist? The Bible is the *only* existing evidence for Moses; there is no known Egyptian record of Moses or the exodus. One may be inclined to say Moses did exist because it is hard to imagine how many other events we think happened could have come about if he did not. Yet here I think the postulational character of Moses (from an epistemological point of view) is quite clear. And if it is clear here, simply increasing the amount of present data will not change the postulational character of the entity.

I am aghast to discover that my book interfered with Karen's love life. But anyone stupid enough to break up with her would of course have been too dumb to understand the book!

Stanley Bailis

Stanley is quite right that the crucial question for the position I hold is the issue of confirmation. In *OKHP*, I devoted the last two chapters to that problem; I did not pursue it much further in this book because so far as I can see not a lot of progress has been made on this problem in the last twenty years. But one should not conclude from this that historical theories are never confirmed. All historians seek to confirm their theories by matching them against all the data they can find, and as I have noted the process that historians call “revision” is one by which increasing amounts of data are brought to bear on particular theories. The point is rather that we have for history no *theory* of confirmation, comparable to, say, the Neymann-Pearson theory of hypothesis testing in statistics, and our methods of confirmation are imperfect, as the problems of sampling, measurement, and all the others I dealt with in *OKHP* show very clearly. The best I think we can do at present is to try to integrate as much data as possible and particularly to use databases with offsetting biases. Such methods are obviously better than nothing, and they are very often effective; the problem is that we cannot tell how effective they are. While I do not want to give the impression that there are no means of confirming historical theories at present, it remains true that our current methods need a great deal of improvement.

I agree wholeheartedly with what Stan has to say about interdisciplinarity. One can think of a “discipline” as an orientation which specifies certain variables as the ones characterizing the subject area and develops methods to study the interactions of those variables. As Stan notes, these separate “disciplines” reflect the lines of academic bureaucracy and have little relation to real distinctions in the world. It follows that the variables of one discipline are rarely related to those of another; thus the “missing data” to which Stan refers. There is some awareness of this problem in the groves of academe; hence the lip-service constantly paid to “interdisciplinary study.” But what this means in practice is interdepartmental study, which amounts to little more than getting a few people from several departments to talk to each other. The blockheads who run my university, and I suspect most others, never get beyond that point. It does of course sometimes happen that when people from different disciplines talk, they get interested in relations among their variables and something constructive results, but the usual result is that each protects his/her own turf and nothing happens. Hence the need for departmentally based enterprises like American Civilization that not only seek to integrate the various disciplines but that ignore disciplinary boundaries entirely in seeking to understand the human reality that is our subject, and that exhibit a healthy independence in appropriating and combining from all fields those methods that can be usefully applied to understanding human life and in inventing new and better methods wherever they are needed. Not only is it necessary to study the relations among variables from the diverse disciplines, but there are often aspects of human life that are ignored by all the usual disciplines. A typical example of this is material culture, which (with the partial exception of

archaeology) has been largely neglected by the social sciences and the humanities alike. If disciplines are often useful for the comparison of certain aspects of human forms of life across cultures, enterprises like American Civilization are equally vital for the study of how all aspects of a human form of life are interrelated in a particular society or group of societies over time. I need not stress how resistant established departments are to this enterprise, nor how stupid administrators are—we all know that. But this is a mission for those in American Studies/American Civilization that could pay very rich rewards for those with the courage to undertake it.