

Hypotheses Fingo

Perhaps it is too easy to think of interchanges between researchers—replies and counter-replies—as if they were some kind of combat. Surely that is an inappropriate kind of model for scholars to adopt in their discussions. I am pleased that Norm and Anne Cohen, in their comments [*JAF*, 87 (1974), 156–160] on my article “The Role of Hypotheses in Folkloristics,” [*JAF*, 86 (1973), 114–130] have chosen what I take to be the proper mode of scientific discussion, the dialogic or conversational attitude. In combat, one seeks the opponent’s demise, while in a dialogue, especially a scientific one, one hopes to be guided, by good evidence and argumentation, in the presence of fellow researchers, toward the truth, and not toward personal victory. Such an enlightened approach is what one can expect from good scholars such as the Cohens.

This should not suggest, however, that scientific dialogues are mere polite chit-chat or mutual back slapping. Such discussions are frank and as rigorous as possible. Persons not familiar with dialogues of this kind might feel that combat is occurring, but one should not mistake vigorous and healthy pursuit of the argument for warfare.

Norm and Anne suggest (and at one point, on page 157, openly declare) that the intent of my essay was to *prescribe* this or that to folklorists in regard to method. I do not expect that any careful reader of my essay will sustain them in that claim, for that was not the spirit of my article (nor is that the spirit of these remarks), and that spirit is well expressed at several points. On page 114, in the introductory paragraph, one finds a repetitive mention of the attitude of the paper in comments such as: “[this essay] is an *attempt to defend* the thesis that hypotheses, used appropriately, are absolutely essential for folkloristics”; or “I shall be *exploring* the implications of such a thesis”; or “[I am concerned with] what *might* be fruitfully *attempted* in the future.” (Italics added.) Of course, in cases like this, one tries energetically to present the arguments that are at hand—perhaps rigor (or at least, vigor) has been mistaken for prescription which would be a form of dogmatism. But, that issue aside, another problem is possibly present here, a problem which the foregoing comments do not adequately cover. It is this: even if I had prescribed something, that would have no relevance to the correctness or incorrectness of the arguments I presented. To think that such considerations ought to count would be to fall into a form of the *ad hominem* fallacy; that is, it would be to argue that the soundness of an argument is affected by the special circumstances or personality of the person advancing the argument. Or, in other words, even Satan can present valid arguments (that isn’t to say that she would, of course, but she could if she chose to do so). Hence, clearly I did not prescribe anything. And even if I did, that would have no effect upon the correctness of the arguments I advanced. Such arguments should be considered independently of the personality or motives of their presenter.

If I have not prescribed, it follows that I have not prescribed using the physical sciences as a model for a prescription, as my friends seem to suggest. But perhaps they

mean to imply that I have attempted to read off from physical science recommendations concerning method for students of the science of man. The latter also is not true. Nowhere in my essay will one find a statement that such is my intention. And if it is there to be found in a tacit form, then I now repudiate it. But I do not disclaim the position that any science, physical or otherwise, offers useful data for understanding the nature of science in general. Seeking data for understanding the nature of science by considering chemistry, geology, literature, biology, psychology, and the like, is a far cry from picking, for example, physics, and saying that all the other sciences *must* be strictly identical to it. Amazingly, it seems to me that Norm and Anne, through their note, are the persons who have introduced this idea into our dialogue. They seem to be suggesting that their account of science as given in their discussion of ninety-percenters and ten-percenters (and that account is plainly arising from something like physical chemistry, as they understand it) is normative for science (for every scientific inquiry), and anything not conforming to it is considered unscientific. This is not to say that such an account (see page 157) is thereby wrong; one should consider the arguments advanced for it. Yet, we do here seem to have a confirmation that my critics have been the prescribers instead of me. Moreover, their distinction between science and scientific inquiry appears to be a distinction without a difference, unless the difference be that until relatively recently the study of man did not bear the title "science," while the study of matter, or of the stars, are projects that did bear such a title. If that is proposed as the distinguishing difference, it is of no logical consequence. It seems to me to be better to describe a discipline as a science if it follows the method of scientific inquiry (the method of hypothesis). So, I find the following statement (page 158) to be puzzling: "To reiterate our own position, however, we see no need for qualifying folklore (or folkloristics) as a science. It is sufficient that the discipline distinguish itself by following the path of scientific inquiry. . . ." If the method of scientific inquiry (method of hypothesis) is rigorously followed by a group of scholars, I would say that those scholars are scientists, and that their discipline is a science. Thus, the term "science" is not reserved for, or pre-empted by, the physical sciences, as my friends seem to believe.

I did use some examples from the physical sciences in one part of the essay (pages 120-121), but not for purposes of prescription. I had quite another notion in mind, which is summarized in the following argument outline:

1. Some persons advance certain statements, for example the claim that human events are unique and unrepeatable, with the intent of showing that social science is logically impossible.
2. But arguments in support of these statements are applicable in a parallel way to physical science. In the example given one finds that the same argument structure could be employed to conclude that physical phenomena are unique and not repeatable.
3. But, physical science is possible in the face of such objections.
4. Therefore, these considerations do not show that social science is logically impossible, for the same arguments would show that physical science is impossible, which would be an absurdity.

This does not make the physical sciences into models for the social sciences to emulate; it simply shows that persons who would forestall social scientific inquiry, through tactics such as the "repeatability" gambit, must be aware that such arguments are applicable in a parallel way to sciences acknowledged as being logically possible.

It follows then that I am not guilty of the suggestions contained in the complex questions on page 158 of the note. "We are intrigued by the search by folklorists (and behavioral scientists before them) for a justification for calling themselves scientists and their discipline a science. Why the great reverence for the physical sciences; why the attempt to emulate their methods?" Those questions would be logically similar to asking some friends if they are going to stop beating their dog when the issue at hand is whether such beatings have occurred at all. I would be among the vanguard of persons assailing the position that man ought to be studied, as Auguste Comte (for example) thought, through some kind of physics of society. Indeed, I have already made such a criticism of Leslie White's version of social physics in an earlier issue of this *Journal* (84 [1971], 444-446), and my critiques of materialism and realism are further logical attacks upon other aspects of social physics. Yes, superorganicism, as explicated in my accounts of materialism and realism, is a version of social physics, for it is a way of taking out the human element, while concentrating upon "items" or "materials" that are studied as if no human beings were involved, thus providing the social physicist with the desired "mechanical" system devoid of human choice and creativity or other such "contaminating" elements. Interactionism, as proposed in my paper in outline form, is a way of combating this version of social physics, for it is a way of conceiving the phenomena to be studied as being concrete activities, and interactivities, of actual individual human persons—individuals who have freedom of choice, creativity, mean streaks, and all the other undeterministic, un-mechanical features which are present in activities of the human species. It seems to me that the superorganicists and historic-geographicists and materialists and realists, who are the social physicists in our discipline, are the persons to whom my friends should address these comments.

Hence, I am puzzled and dismayed by what I take to be a strong reaction on the part of the Cohens to defend the "literary" or humanistic approach, as if I had questioned it. I would have thought that it needs absolutely no defense. How could I? As a philosopher I am obviously a humanist, even a literary being. If one were to read my paper again, especially pages 122, 123, and 125, one would find that I do not say bad things about a literary approach to our subject. One can easily be a student of literature and not be a materialist or a realist (or a social physicist). One can surely deal with texts and not regard them as things, or not regard them as being the totality (or even the most important aspect) of our studies. Dealing with "content" is a valuable enterprise, and perhaps can only be accomplished within a literary framework (although of this I am less than sure). What I *was* attempting to criticize was a dogmatic literary approach that would demand, among other things, that texts are the only aspects in the phenomena worth studying, or that content is the only significant feature of the phenomena at hand, or that social physics provides an appropriate basis for undertaking literary studies. I am not alone here, for one of the most prestigious books in literary theory is Wellek and Warren's *Theory of Literature* (New York, 1942), and they suggest in Chapter Twelve ("The Mode of Existence of a Literary Work of Art") a position for novels, for example, that is rather parallel to the one I suggested in my proposed replacement for materialism (a position I named interactionism). Here, will it seem that I am prescribing to scholars in folkloristics using the discipline of literature as a model for prescribing? I concur that scientific inquiry (the general logic of science) is available to literary scholars as well as to behaviorists, and even hinted at that

on page 125 of my essay. And surely if a literary scholar is committed to inquiry, such a person will take seriously honest arguments, such as those I advanced in my paper, and if such arguments stand, will wish to change appropriate views, so as to return to scientific literary inquiry without the hindrance of unhappy assumptions, such as those associated with realism and materialism.

In saying such things here and in my original essay, am I emphasizing a "rift" between literary scholars and behavioral scholars, as Norm and Anne seem to think (see page 160 of their note)? I believe that isn't what I have done. I have attempted to present some arguments and to criticize other arguments. That seems to me to be what Professor Wilgus did in his distinguished essay, "The Text Is the Thing" [JOURNAL OF AMERICAN FOLKLORE, 86 (1973), 241-252], to which the Cohens refer: he criticized some arguments and presented others. I find some of his arguments compelling; others he advanced appear to need further evidence, and discussion focusing upon the latter will no doubt continue. I think it is best, as scientific inquirers, to proceed in terms of arguments, instead of lamenting "rifts" (or by means of *ad hominem* appeals). And surely, in contrast to what is also claimed in this same paragraph, at least an entire section of my essay was devoted to giving a careful proposed characterization of what constitutes understanding of the kind needed (see pages 126-127). Moreover, such an account of understanding is as accessible and usable for literary scholars as it is for anyone else.

In the critique, a particular illation is advanced (pages 156-157) which is a good example of what I would call a "know-nothing argument."

In connection with Ketner's paper, we should note that, in general, it is not necessary for a person to be a good philosopher of science in order to be a good scientist, any more than it is necessary to know Peano's postulates for the natural number system in order to be able to add and subtract. Likewise, a scholar can be a good folklorist without being committed to (or even without understanding) a meta-folkloristic underpinning to his discipline. We are not saying that the need does not exist for such a theoretical groundwork; it does, and Ketner's article is an important one. But what may be of primary logical importance need not be of methodological import and may not really be relevant in a guide to folkloric fieldwork, which is a laboratory manual, not a *Principia Folklorica*.

This argument seems to offer one either the option of having more knowledge or that of not being concerned about having more knowledge, even if it is suddenly available in some form. Then, the suggestion is made that the latter alternative is preferable. Or, in terms of the above quote, it isn't necessary for a good scientist to be a philosopher of science. But, I would add, it might help; or occasionally it might be helpful for a good scientist to converse with a good philosopher. Why throw away knowledge if it is relevant and helpful? The analogy which mentions Peano's postulates is simply inappropriate, for the issue at hand concerns scientists, and persons who add and subtract are more numerous than scientists who add and subtract. Perhaps the analogy intended might be one like the following: It isn't necessary to know Peano's postulates in order to be a scientist with regard to adding and subtracting. Again, it isn't necessary, but it might help. Furthermore, it isn't even necessary for one to be a scientist, but many folklorists, both literary and behavioral, appropriately claim that title. Now, consider the assertion that a scholar can be a good folklorist (here read "good scientific inquirer concerning folklore," which are words the Cohens would surely accept) without understanding her (or his) discipline's methodological and theoretical underpinnings. Yet, wouldn't it help? Imagine a folklorist applying for a job while saying

the following: "I know nothing about my discipline's foundations." And finally, look at the suggestion toward the end of this quote that "laboratory manuals" are logically independent (or could be thusly independent) from theoretical considerations. I think I have amply shown that Goldstein's manual is not free of a certain theoretical position, and I think it might even be possible to defend the position that no *scientific* "laboratory manual" would be devoid of theoretical foundations. Didn't "laboratory manuals" change when the phlogiston theory of heat was replaced by the kinetic theory; or, doesn't one find differing "laboratory manuals" in psychology, for instance, when there is more than one theoretical school of thought present?

I am pleased to read of Norm and Anne's suggestion that the Baconian method should be rejected, but I cannot take their statement to that effect (page 157) as meaning the same thing that I meant when I offered arguments against the feasibility of such a method. The reason is that their account of the nature of scientific investigation (page 157) is strictly Baconian. Perhaps what they reject in the Baconian approach is some kind of necessity to submit research reports in a particular format. The cases they cite appear to justify this interpretation of the phrase "Baconian method" as they use it. But that is not what I meant, although that might be a part of what I meant. One of the crucial points of Bacon's approach, as I was discussing it, is that one assumes that something can be a datum in the absence of any kind of hypothesis. And here I must state parenthetically that Norm and Anne have put their finger on a weakness in my paper: namely, I confused the terms "theory" and "hypothesis" and indeed often used them as synonymous, a relation which does not hold between them. I am grateful to my friends for correcting me on this point. A theory is composed of a multiplicity of hypotheses, and this set is organized internally in some way. Of course, a theory as a whole is like a "large" hypothesis, in that the theory is open to disconfirmation just as a single hypothesis is.

To return to the original issue, can a datum exist as such independently of some hypothesis, as suggested in the critique? No. Consider the account proposed on page 157. We are told that the first step in a typical cycle of scientific investigation is for an experimentalist to invent a new experimental technique that permits the measurement of a property or phenomenon never before measured. But ask yourself why this particular experimentalist would seek to invent a new experimental technique, and why would just this experimental technique, if invented, be taken by its inventor as significant in some way? Without these kinds of factors, we would have strange sights such as that of a student of astronomy composing an experimental technique for measuring the amount of protein in cacti. Without a guiding hypothesis (not necessarily a theory in all cases, however) in the mind of the experimenter or a supervisor, either before the invention of an experimental technique, or provided after the unintentional notice of a phenomenon, the experimental technique could not be invented or discovered. Someone might witness the technique, or allied phenomena, but it would have no significance to such a person without a *logically* prior hypothesis as to its significance and relevance. That is why persons with a blank slate cannot be scientists. Or in other words, to know something new, one must already know, or at least suspect, something old. Or, in order for an experience (experimenting, for example) to be significant, there must be logically prior means for giving significance. And this has nothing to do with one's motives: having hypotheses and having motives are logically separate. Someone could stumble onto a new phenomenon, but that person would fail to notice

it in a scientific way unless equipped with the means for understanding it as potentially scientifically significant. Roentgen stumbled onto X-rays apparently with no motive or intention of doing so. But, he saw the unplanned event as significant because he had hypotheses awaiting (in the sense of logical priority) such an experience. A bricklayer who saw the same accidental event would have merely been puzzled. Because he was a scientific inquirer, Roentgen was able to transform an accident into a step in the method of hypothesis.

And why the insistence that typically an experiment must *measure*? As a humanistic scientist, here I object most strenuously. Norm and Anne obviously believe that our discipline is a kind of scientific inquiry. This coupled with the emphasis upon measurement would mean that we could not be (or would most likely not be) scientific inquirers, on their account, if we do not have experiments that measure. Now we do not seem to be measuring much in our discipline, yet often we are using the logic of scientific investigation. The best inference in view of these difficulties, seems to be that there is a flaw in the account of the nature of scientific investigation presented in their note.

This contention could also be supported through an examination of Anne's distinguished recent work, *Poor Pearl, Poor Girl: The Murdered-Girl Stereotype in Ballad and Newspaper*, American Folklore Society Memoir Series, No. 58 (Austin: University of Texas Press, 1973). This work is organized around a hypothesis which is expressed in the first chapter, the subsequent chapters being in effect a presentation of evidence (data) that tend to confirm the original hypothesis. Anne recognizes this, and the reviewer of her book (*JAF*, 87 [1974], 172) has also recognized the presence of such a hypothesis. It is this hypothesis that enabled her, for example, to consider the data that she did consider, and not the average number of pages in the newspapers she examined, to give but one possibility. One could say that her book is a good example of the use of the hypothetical method (scientific inquiry) in literary study, as is (for another example) much of the distinguished work of D. K. Wilgus. Yet Anne seems to undertake few measurements (if any) in her scientific treatise.

In regard to the Cohens' comments upon my proposal for disconfirming materialism (pages 159-160), their way of rescuing it seems to me to fail. It does make sense, at least in principle, contrary to what they urge, to ask someone to hand me an electron. Of course we may not now be practically able to honor such a request, but that is not the point. We were not practically able, until a few years ago, to hand someone a moon rock. But that didn't mean that before that time it was not in principle sensible to ask for such an object. Indeed, now I can hand you several electrons by simply handing you a pencil or a drop of water; the difficult part might be in handing you just one electron. But it is intelligible, in principle, to make that request. The case of weighing a rainbow is more difficult because it involves a possible ambiguity. That is, if one means by "rainbow" someone's perceptual experience, then it would not make sense in principle to weigh that. But this is not a counterexample to my point, being instead a parallel example to "please pass the ballad," or like statements. On the other hand, if by "rainbow" one means water droplets in the sky, or some other such collection of physical objects, then it makes sense to inquire after their weight. The above being true, this leaves my critics with the following accurate statement in this paragraph of their note: "[Ketner] is certainly correct that we do not say, 'Hand me that tale,' or 'How much does that ballad weigh?'" All that one needs to add here is that this statement does indeed disconfirm the materialistic hypothesis, an aspect of social physics.