

The Role of Hypotheses in Folkloristics¹

IN WHAT FOLLOWS I shall often be using the word "folkloristics." In adopting this term, I do not mean, at least for now, to refer to any particular school of folklore research. Rather, I use it as a generic term: it marks out anyone who assumes that the scientific mode of inquiry is workable and fruitful in studying folklore. The essence of folkloristics, as I see it, is an attempt to be scientific in the study of certain very roughly characterized phenomena commonly described as folklore—such things as spoken stories, informal singing, and conventional wisdom. In turn, hypotheses, properly employed, are an essential part of scientific method in folkloristics. To put the relationship negatively, if there is some reason why hypotheses cannot be employed in the study of folklore, then there will be no science of folklore. However, I believe that hypotheses can be employed properly in the study of folkloric phenomena. That is what my essay is about: it is an attempt to defend the thesis that hypotheses, used appropriately, are absolutely essential for folkloristics. I shall also be exploring the implications of such a thesis, both in terms of the impact of its truth upon procedures now in use and in terms of what might be fruitfully attempted in the future.

I shall initiate my discussion of the role of hypotheses in folkloristics by considering the account of the logic of scientific inquiry provided by Kenneth S. Goldstein in his distinguished book, *A Guide for Field Workers in Folklore*. This volume has been very influential in the attempt to apply scientific method in the study of folklore. That he intended the book to serve, at least in part, toward that end is seen in remarks he made at the start of the second chapter: "The preparation of a guide for folklore field work implies a concern with the status of the discipline of folklore. It is part of a larger effort to raise the discipline to the level of a science (a social science retaining close ties with the humanities, to be sure)."² Thus, I believe it is fair to say that Goldstein viewed his *Guide* as an attempt to describe,

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² Kenneth S. Goldstein, *A Guide for Field Workers in Folklore* (Hatboro, 1964), 13.

in moderately abstract terms, a way of scientifically accomplishing fieldwork in folkloristics. To that end, it was necessary for him to develop an account of the logic of scientific inquiry; otherwise it would be empty for him to claim that he is providing a description of scientific folklore field techniques.

It is doubly appropriate that the present article is concerned with a discussion of Goldstein's *Guide*. In the first place, the *Guide* is widely used as a textbook in folklore classes, so that the account of scientific inquiry it contains has been read by almost everyone who has seriously studied folklore. Second, since his view about the role of hypotheses in science is quite opposite from the one I shall defend, a consideration of his approach will be a good way for me to present my position so that it stands out with the maximum of contrast, couched in concrete terms relevant to the actual status of folkloristics at the present.

Goldstein on Scientific Inquiry

Goldstein holds that scientific inquiry involves the following stages:³

1. Problem statement: the setting up of a problem to be solved.
2. Analysis of the problem: the determination of the relevant data and the methods most appropriate for obtaining the data.
3. The collection of data.
4. Presentation of the research findings.
5. Postulation of hypotheses, based on the analysis and interpretation of the data.

Immediately after giving this list, he adds that "not all inquiry will proceed to the final stage." Clearly he is claiming that either an application of stages one through five or of one through four adequately characterizes the formal aspects of scientific inquiry. In the same paragraph Goldstein adds a third category to his list of the kinds of scientific inquiry—he notes that "where research has been undertaken to obtain data for the purpose of establishing an hypothesis, the fifth stage will be followed by the first stage again in terms of a new problem to test the hypothesis for the purpose of establishing laws." Let these three kinds of procedures be labeled A, B, and C, respectively. I shall argue that neither A nor B is descriptive of the logic of scientific inquiry.

Consider procedure A. The mistake lies in placing the formation of a hypothesis as the last step in an inquiry. It may sometimes be chronologically the last thing that occurs to one consciously, but it is logically prior to any other step. The problem with the approach Goldstein recommends is that there are too many things to experience in the world, an infinity of things, one could say. In order to avoid looking at them all, we require a hypothesis that suggests which area of experience might prove fruitful for an examination. Without such a hypothesis, every observation one could make would be purposeless and on a par with any other observation one might make. Thus, science would be reduced to aimlessness and, therefore, could not constitute an inquiry. Consider also that, in Goldstein's account of method, one "collects" the facts with no preconceived notions or hypotheses in mind; then one combs these facts in order to develop a hypothesis. If one were really serious about this, then one would have to admit that the best qualified "field" folklorist (or "collector"), a person meeting the criterion of having no

³ *Ibid.*, 16.

preconceived theories or hypotheses, would be someone who knew nothing about folklore. No matter which school of folklore study has one's allegiance, I think everyone would agree that this situation is literally impossible in a serious student of folklore.

The normal state of the human mind is roughly characterizable as belief. That state of belief tends to continue after the pattern of a habit unless disturbed in some way, usually through a frustration of action (or of imagined action) based upon such a belief. At such a point, concerning the particular matter at hand, a question or doubt arises. This does not mean that, when a doubt occurs, all of one's beliefs are thrown into question. Indeed, it can be easily shown that raising a doubt or asking a question necessarily presupposes many other beliefs that are not then in question. Thus, while such a doubt is the occasion for the initiation of an inquiry, inquiry does not arise in the context of total doubt (as Descartes, for example, might have thought), since it is impossible for an intelligent being to be in total doubt (to doubt all one's beliefs), although one might imagine a being, such as a rock or a stick, which is in that state. Thus, an inquiry is possible only if one has asked a question. That is, one simply does not have an inquiry unless one honestly feels puzzled about something, at least in some degree. Otherwise, one would simply continue in the state of belief as before. This is an extremely important point, for, without such a real doubt or question, one might go through the motions of inquiry; and no one should deceive himself by thinking that going through the motions of inquiry is the same as actual inquiry. Now, the question that logically initiates an inquiry leads one to formulate a tentative answer. Such an answer is basically what constitutes a hypothesis.

So far, what I have described is not very different from what any ordinary person who is not practicing science might experience in everyday life. Scientific method is different from nonscientific procedure, however, in that for the next step the scientist attempts to limit himself only to putting the question to nature, as opposed to clinging tenaciously to a prior belief in the face of this doubt or to putting the question to one's grandfather or one's esthetic sense of correctness or to the minister of one's church. The scientist interrogates nature by taking his tentative answer (the hypothesis) to a likely spot or into a likely situation to see if it is the correct answer, these things being "likely" because they are the sort of things that would be the consequences of the hypothesis if it were true. In addition, the scientist deals with these tests publicly, that is to say, intersubjectively, in order to ensure that the results represent reality rather than the scientist's bias or, as some instances in history have shown, his deceit. Furthermore, the scientist's work is usually characterized by great attention to detail, accuracy, and comprehensiveness. But all that care would come to naught if one were lacking a hypothesis, for without that one doesn't know which observation, not to mention which careful or comprehensive or detailed observation, will count toward solving the puzzling question or removing the nagging doubt. Thus, we see that, lacking a hypothesis, one simply does not have an inquiry.

This point can be stated in terms more pertinent to Goldstein's formulation A. It requires us to determine "the relevant data." But how can this be done, for one wishes to know, "Relevant to what?" Obviously the answer is not, "The facts relevant to this specific problem." For that alone makes no clear sense because, lacking

a hypothesis, there is no criterion for determining which facts or observations are relevant to an inquiry into this problem. For example, suppose my problem is to survey the folklore of Broken Toe, California. In looking up the schedule for transportation to Broken Toe, I see that a bus is available at nine in the morning. Am I to record this datum? How about the price of the hamburger I had while en route? Or the headache I developed because of the desert heat? These events and countless others are all relevant to my field problem in some sense, "practically" relevant, but it is clear that they are not logically relevant to any kind of inquiry into that problem. That is, given formulation A as a proposed logic of inquiry, I see no way one can reject such "facts" as being irrelevant to inquiry into this matter; but surely they are irrelevant to the logic of the case, although perhaps not irrelevant to the practical aspects of putting one into an appropriate locale in which one can conduct an inquiry. It would be more accurate, in describing the logic of inquiry, to say that we should determine what observations are relevant to a tentative answer to the problem. And such a tentative answer is precisely a hypothesis. In this case, the hypothesis under consideration probably is that there is some person or persons in Broken Toe who will exhibit folkloric behavior. Of course, since every normal human being will exhibit folkloric behavior at various times in his life, this is not a very surprising hypothesis. But, in light of it, we can reject such things as the bus schedule, because they are logically irrelevant to either a confirmation or disconfirmation of the hypothesis. In other words, an observation, or an event, is relevant to the solution of a problem if it is a logical consequence of a tentative answer (hypothesis) to the puzzling problem.

In view of my comments about Goldstein's A procedure, the desideratum for his B procedure is fairly clear. Since the B procedure contains no mention of a hypothesis, it cannot be an account of scientific inquiry. The B account assumes that one can be scientific by simply gathering data or by simply describing. Scientists do gather data and they do describe things, and they often do this very carefully and comprehensively, as I have noted. But they are scientific only insofar as they are doing this for a reason, that is, for the purpose of trying to confirm or disconfirm a hypothesis. Thus, it is incomprehensible that the final step of this second type of scientific inquiry (B) is "the presentation of research findings," for the use of a hypothesis is not mentioned at all in B. Research or inquiry is possible only after one has a hypothesis. If there is no hypothesis, there is no inquiry.

And one must not think that the way to get a hypothesis is to gather a large body of "data" in the hope that a usable hypothesis will spring forth from it. No doubt many students have eventually developed some usable hypothesis after looking over large bodies of materials that they regarded as data. But such a body of observations can be considered data only if one already has some vague hypothesis in mind that leads one to *these* observations and facts rather than to *those* observations and facts. That is, if one follows this procedure, one will of necessity already have an implicit hypothesis, for the canon used in selecting certain facts as data *is* a hypothesis.⁴ Much time and wasted effort could be eliminated by starting out from the first, before one undertook to amass reams of "data," with an expenditure of intellectual effort devoted to making explicit and precise the germinal hypothesis that leads one to regard a set of observations as data. If this plan is

⁴ See Abraham Kaplan, *The Conduct of Inquiry* (San Francisco, 1964), 133-134.

followed, fewer observations will be needed and many observations that would otherwise be unused will not need to be made. Furthermore, coming up with a hypothesis is a creative act that does not appear to be governed by any set of mechanical rules or procedures. There is no cut and dried way one can manipulate observations such that a hypothesis will come forward mechanically. There is no substitute for diligent and original intellectual effort at this point. Considering these factors, we thus see that large bodies of observations will not, in and of themselves, yield hypotheses, and, considered in isolation from hypotheses, they do not contribute to scientific knowledge.

A similar mistake is made if one urges that one cannot come up with an intelligent or worthwhile hypothesis until one has seen most or all the data relevant to some issue. First of all, someone attempting such a procedure will already have an implicit hypothesis that leads him to take one observation as a datum and reject another as not being a datum. Second, it is difficult to understand what is meant here by "all the data." Since what one wants to do in folkloristics is to test hypotheses, it would seem that for any given case all the data one requires are whatever are needed to confirm or disconfirm one's hypothesis.

A Revised Schema for Scientific Inquiry

Goldstein's C procedure comes closer to being an adequate description of the logic of inquiry. However, it is somewhat confusing since it makes it appear that steps one through three are logically prior to having a hypothesis. As we have seen, that order is not correct. The following schema, based upon comments made above, is a more accurate reconstruction of scientific inquiry.

1. REALIZATION OF A PUZZLING QUESTION OR DOUBT. Questions here typically take one of two general forms; either kind can serve to initiate an inquiry.

a. Unsaturated questions. These are the kinds of questions which begin with words like "what," "when," "where," "why," or "how." For the sake of brevity, I shall discuss only one of these cases, leaving it to the reader to fill in the rather similar details for the other instances. Consider the question, "What is S?" This is a request for (or an indication of the lack of) knowledge about S. This can be more easily seen if we transpose the question to the equivalent form "S is——?" In other words, the questioner desires that the blank be filled. Note also that there are usually a large number of things that could possibly fill this blank. This empty "slot" that the questioner wants "filled" is what characterizes unsaturated questions.

b. Saturated questions. These are questions of the form "Is S a P?" They can be viewed as instances of unsaturated questions that have the empty slot filled, although they remain as questions because the slot is not filled with confidence, as it were. That is, it is still a live issue whether S is P or S is not P.

2. DEVELOP ONE OR MORE HYPOTHESES.

a. If the inquiry begins with an unsaturated question, the first step will be to propose a hypothetical answer. As I stated above, there is no set of rules for accomplishing this since it seems to be essentially a creative act. The hypothesis is an answer to the unsaturated question in the sense that it is a proposed way to fill in

the blank spot in the question. At this point we should note an interesting difference between the use of unsaturated questions in interpersonal communication and their use in inquiry. In a communicative context, when an unsaturated question is asked, we usually tend to accept the answer we receive without further ado. But, in inquiry, often the questioner and the answerer are one and the same person, and in any case, no matter who the answerer is, we do not want to accept anyone's word that the proposed answer is correct; we wish to know for sure that it really is correct, and, in order to ensure that status for the answer, we go on to put it to a test. This test takes the form of asking a further question, although this time it is a saturated question. For example, if we had originally asked "What is S?" then considered that S might be P, since we want to put this matter to nature, so to speak, we go on to ask "But is S indeed P?" This readies us for the next step in inquiry, but, before discussing that, I must outline the case in which inquiry begins with the other kind of question.

b. Sometimes the inquiry begins with a saturated question. In that case, we already virtually have a hypothesis at hand, and we can then proceed to 3 and 4.

3. **DEDUCE CONSEQUENCES.** A hypothesis is in effect a saturated question. Perhaps if I put it in an alternate frame, the truth of this assertion can be more easily seen. We are probably more accustomed to thinking of a hypothesis as a statement that we think or suspect is true. Our suspicion means that we want to find out whether it is actually true. We think that it is perhaps probable that the statement is correct, but the fact that we suspect it to some extent, or the fact that we want to test it, reveals that we also think that it is possible that it is not true. Now what I have just said amounts to the following three conditions: S is probably P; but it could be that S is not P; and we desire to find out if S really is P. These three conditions taken together do constitute the questioning mode for "S is P." So a hypothesis is a saturated question that presents us with two contradictory beliefs, each of which is seen as possibly correct. In order to resolve this contradiction to secure a stable belief, we put the question to a test. The test proceeds in the following manner. We assume for the moment that it is true that S is P. Then we attempt to deduce consequences from that assumption, consequences that it would be possible to observe. Hypotheses from which it is logically impossible to deduce observable consequences (observable at least in principle) must be rejected since it is not possible to put such a hypothesis to test.

4. **TEST THE HYPOTHESIS.** For the next step, we endeavor to see whether the consequences of the hypothesis occur as expected under the prescribed conditions. If these consequences do not come to pass under the requisite conditions, then the hypothesis is disconfirmed. That is to say, if we had assumed that "S is P" was the correct answer, then deduced consequence T from that and discovered through a test that T did not occur as expected, that enables us to make the following valid deductive argument: if S is P, then T; but T is not the case; hence S is P is not the case. In other words, a disconfirmation shows us that the answer, S is not P, is correct; while a confirmation shows us that the answer, S is P, is correct. Either way, one gains some new knowledge. I should mention that, in the case of confirmation, the argument form used is the following inductive procedure: if S is P,

then T; T is the case; hence, probably it is the case that S is P. I should also add that, while disconfirmation gives us a new piece of knowledge (it tells us what will not work), we are usually not satisfied with that kind of truth. In that event we start inquiry over again, often going all the way back to an unsaturated question.

It is possible that Goldstein might accept my notion of an unsaturated question as being an account of what he means by "problem" in the first step of his summary of inquiry. If this identification is correct, the following rough reordering of his summary would be parallel to my schema.

1. Problem statement (Goldstein's 1)
2. Postulation of hypotheses (5)
3. Analysis of the hypotheses to determine relevant data so that tests can be made (2, 5)
4. Testing hypotheses, not in the sense of "collecting" pure data, but making observations in order to test consequences of hypotheses (3)
5. Presentation of research findings (4)

I see no possibility for preserving variations of this modification of Goldstein's procedure such that what we are left with is still some kind of outline of scientific inquiry (with the possible exception of the omission of the last step). That is, Goldstein had procedures A, B, and C. And, as I have indicated above, these procedures have shortcomings. But I see no way to modify my revision of Goldstein's summary such that I can propose two more variations on the logic of inquiry.

I can foresee that an important objection is likely to arise at this point. Someone is sure to say that all this insistence upon testing and confirming hypotheses is very relevant if one is studying something like physics or chemistry, but it is out of place in the study of folklore because folkloric phenomena are different from those studied in the physical sciences. This difference is often said to lie in the supposed uniqueness or nonrepeatability of social data, such as folkloric phenomena. Or sometimes it is urged that in regard to social data one is faced with a lack of "control" within a testing situation; that is, it is claimed that one cannot "manipulate" a social situation by holding some variables constant while studying changes in other variables. In any case, so the argument runs, one cannot employ hypotheses in folklore studies because the peculiar nature of the phenomena studied stands as a bar against any attempts to formulate or to confirm such hypotheses. In response to this position, it is important to admit that if there is some reason why the nature of folkloric phenomena is such that, if one wishes to study them, one is somehow deprived of the hypothetical method, then a science of folklore would not be possible. Upon this point I can agree. But I believe that the hypothetical method can be applied or that, at least, it would be a blockade of inquiry for anyone to arbitrarily stand in the way of an attempt to apply it to the study of folkloric phenomena. In any case, what is at stake in this objection is nothing less than the very possibility of a science of folklore.

To discuss this issue fully would require another essay of some length, but I shall try to indicate a possible line of defense.⁵ Concerning the claim that folkloric phenomena are unique and nonrepeatable, it is at least a *prima facie* counter to

⁵ A more detailed reply to some aspects of this objection can be found in Kaplan, chap. 4; also, Maurice Natanson, ed., *Philosophy of the Social Sciences* (New York, 1963), 158-180.

remind the objector that physical phenomena are unique too. For instance, we are not able to repeat earthquakes or hurricanes in order to study them, and we probably would not want to do so if we could. Yet we do have a science of geology and a science of meteorology with a body of confirmed knowledge in each. But, it will be said, what knowledge we do have, for example of earthquakes, derives from experiments performed in physics laboratories using weights and levers and the like, and these experiments are non-unique and can be repeated. Even if it were true that all our knowledge of earthquakes derives from physics (which I believe is not true), it is not the case that such experiments deal with non-unique phenomena. There must be more than one experiment that has been performed a great number of times in physics classes around the world; and these tests have differed among themselves in numerous small ways—the earth and other heavenly bodies were in different positions, small errors in the supposedly invariant variables occurred, and so on. Well, it might be said, the physical scientist abstracts from these unique events, creating classes of occurrences with which he then deals. I reply that the social scientist can do the same. As a matter of fact, in our everyday lives we do that sort of thing all the time. Everyday reasoning about social interaction, what is often designated as “horse sense,” depends upon one’s having at hand what might be called roughly confirmed generalizations about society. I have in mind here such things as: “Children avoid people who mistreat them,” or “If the supply is constant and demand rises, prices will go up,” or “Persons who have jobs do not like to be fired.” In our daily lives we all depend upon knowledge of low-level social science, which no doubt includes appropriate knowledge of phenomena popularly known as folklore, alongside knowledge of politics, economy, law, and all the rest. A good part of this informal social science consists of hypotheses that have in some sense been formulated and confirmed in our experience. All the social scientist wants to do is to try to improve upon everyday knowledge in the social sphere, much as the physical scientist, over the years, improved upon everyday reasoning about such things as mechanics.

On the matter of the putative lack of control in confirmatory situations in folkloristics, even if it is absolutely required, the only barrier to achieving it seems to be the complexity of social or communicative events. Of course, before one tries to mark out an event into abstract segments, any event will seem complex. The way to overcome this hurdle is to begin making stabs at abstractions (hypotheses, in other words) that appear to be possibly fruitful and then try them out with the hypothetical method.

Hypotheses and Folklore Materialism

Shortly after giving his five-point outline for the logic of science, Goldstein states that the “present work confines itself to the first three stages of inquiry; the first two stages to be discussed in this chapter and the third stage to constitute the bulk of the book.”⁶ In proceeding to discuss implementation of the first two steps, Problem Statement and Problem Analysis, Goldstein claims that “the field worker may be assisted by a knowledge of the kind of folklore ‘facts’ or data which it is possible to collect . . .”⁷ According to Goldstein, there are three kinds of folklore

⁶ Goldstein, 16.

⁷ *Ibid.*, 22.

data: folklore materials, processes by means of which these materials are transmitted from one person to another, and folklore ideas, which are the attitudes of tradition bearers about folklore materials.⁸ Notice that the meaning of the second of the categories depends upon the meaning of the first category, and there it is assumed that it is correct to treat folkloric phenomena in terms of an analogy with objects in that they are seen as isolatable items, entities, texts, or materials.

At this point, a question arises for me. Does Goldstein view this characterization of the nature of folklore data as an integral part of his proposed methodology for scientific "field" work in folkloristics, or is his reader to take it as a hypothesis, recommended by Goldstein, about the nature of folklore data? So far, I have found no passage in the *Guide* that clearly explains which of these two alternatives Goldstein might favor, although my guess is that, at the time he wrote the book, the former one was more attractive to him. Despite which option Goldstein might prefer (he might reject both), I shall argue that the first alternative is not logically viable, and, that being the case, we must prefer the latter.

In view of the comments I have made above about data and hypotheses, it should now be clear when someone tells us, "The nature of these data is thus and so," he is in effect proposing a hypothesis. This claim about the kinds of folklore data that it is possible to "collect" is basically a hypothesis about the existential nature of folkloric phenomena. It is a theory I shall call folklore materialism, since it insists that folkloric phenomena are to be understood as if they were objects, which means that folklore is seen as a static entity, a thing that can be transmitted from one person to another. The basic unit of study in this view is the text, which is seen as the object that is transmitted. In order to better see the kind of distinction I am indicating here, contrast folklore materialism with what I call folklore interactionism, a view which urges that folkloric phenomena are basically dynamic processes of interpersonal interaction.⁹

If someone takes the further step to say, for instance in the case of storytelling, that any given set of texts are really versions or variants of an idealized story, then he will be subscribing to the thesis I call folklore realism. Most materialists have been realists, although the former does not logically imply the latter. Materialism is a way of characterizing folkloric phenomena, and realism is a way of saying how they are interrelated. Materialism tells us that folklore is to be viewed on an analogy with an object. Realism, when added on here, claims that these objects (texts) are ordered, in that there is an ideal entity of which the actual objects are imperfect copies or versions. It is worth noting that realism is basically the logical theory of similarity associated with Platonic realism applied to the study of folklore, and folklore realism is subject to the same faults inherent in Plato's view.¹⁰

Since it is the case that Goldstein's characterization of the nature of the kinds of folkloric data is in actuality a hypothesis, namely folklore materialism, it would

⁸ *Ibid.*, 22-23.

⁹ For some examples of folklore interactionism see the work of Georges cited below, plus Kenneth Laine Ketner, "Superstitious Pigeons, Hydrophobia, and Conventional Wisdom," *Western Folklore*, 30 (1971), 1-17; Michael Owen Jones, "Chairmaking in Appalachia," Ph.D. thesis, Indiana University, 1970.

¹⁰ I discussed folklore realism in some detail in the following presentation: Kenneth Laine Ketner, "What Is The Story?" read before the American Folklore Society meeting in Los Angeles, November 1970. On this topic, see also Paul Henle, ed., *Language, Thought, and Culture* (Ann Arbor, 1965), 25-30; P. L. Heath, "Concept," in *Encyclopedia of Philosophy*, vol. 2 (New York, 1967), 177-180.

be a mistake to claim that it is a crucial part of the logic of scientific methodology for folkloristics in general, although it would obviously be of basic importance for anyone who wishes to follow a program based upon the materialist hypothesis (for example, diffusionism or survivalism). But, since it is a hypothesis, and not part of the regulative logical machinery by means of which hypotheses are tested, it is disconfirmable. Materialism cannot be a part of the general logic of science for folkloristics, because materialism is testable by that logic, just as any other hypothesis advanced by any other school of folklore study might be tested with the hypothetical method. Thus, since materialism cannot be part of the general scientific logic for folkloristics, I presume that it would be best to interpret Goldstein's use of it in the *Guide* as a recommendation or statement of preference on his behalf, being basically an indication of a predilection for studies based upon or subordinate to the materialist thesis.

Intrinsically, there is no harm in studying folklore on the basis of the materialist hypothesis. However, one should recognize that there are many indications that folklore materialism is false; in fact, one is led to say that it has been virtually disconfirmed as a viable hypothesis in folkloristics. The least one could say is that it has not been a very fruitful hypothesis.¹¹ As an example of one kind of disconfirmation of materialism, consider the following. If folkloric phenomena are to be considered in terms of materials, then it should make sense to speak about them with the language we apply to materials. For example: "Hand me that —," or "How much does — weigh?" or "There is a stack of — four inches high on my desk," or "There is a — on page 29." For the most part, however, such language does not apply to folkloric phenomena, although it does apply to transcriptions of folkloric phenomena. So, materialism is thereby disconfirmed. Someone might be inclined, at this point, to urge that, despite the above argument, transcriptions represent the folkloric phenomena so well that, for practical purposes, there is no difference between the phenomena and their transcription. But, no matter what purposes a scholar might adopt, there is a large difference between the actual occurrence of the great majority of folkloric phenomena and transcriptions of them. This can be readily observed by anyone. In effect, transcriptions, at least the kind the folklore materialist uses, isolate only a part of the complex folkloric phenomenon. To isolate parts of a phenomenon for purposes of a study is a perfectly sound procedure; but to then infer, as the materialist seems to do, that the transcription is the total of the phenomenon, or that it exhausts all aspects of the phenomenon, is a faulty inference because its conclusion is false—a fact that can be discovered by careful observation of the actual occurrence of almost any folkloric phenomenon. Many materialists, because of their typical interest in comparing the "content" of folkloric phenomena, were led to make transcriptions of these occurrences. To this point, there is nothing wrong. It is perfectly legitimate for one to compare the message contents of folkloric interchanges, although this far from exhausts the possibilities for study in folkloristics (contrary to what some scholars seem to believe). The mistake occurs when it is said that such transcriptions, or "texts," are virtually identical to the folkloric phenomenon; or, stated

¹¹ See Robert A. Georges, "Toward an Understanding of Storytelling Events," *JOURNAL OF AMERICAN FOLKLORE*, 82 (1969), 313-328; Melville Jacobs, *Patterns in Cultural Anthropology* (Homewood, Illinois, 1964), 319-344.

alternatively, a faulty inference is made to the effect that, because such texts can be discussed with the language of materials, the folkloric occurrence is therefore to be thought of in materialistic terms.

One might object to the foregoing comments by saying that materialism is actually more like a philosophical theory and should not be regarded as a hypothesis. However, if one wants a social science of folklore, it has to be all or nothing in respect to one's commitment to scientific inquiry and the important role of hypotheses in such inquiry. If one reserves a certain portion of conjecture from being tested or from the possibility of disconfirmation on the grounds that such a thesis is immune from unfavorable evidence (as a religious faith is in some views), one has given up one's commitment to practice science and is reverting to what Peirce called "the method of tenacity," whereby one simply clings to one's old beliefs in response to real doubts that occur rather than proceeding with a scientific inquiry.¹² It is true that such hypotheses as materialism are quite "large"; that is, they serve as a foundation for many other hypotheses built upon that foundation. But the matter of size does not make such hypotheses immune from disconfirmation—it only makes their disconfirmation more difficult, and, according to some scholars, of a different mode but rejectable nonetheless. For example, Thomas Kuhn¹³ argues that during what he calls scientific revolutions, which occur when there is a radical shift from one paradigm to another, the mode of change is different from the process of what he calls normal scientific inquiry, which is executed from within a single paradigm. Kuhn is not alone in advancing this kind of thesis; such philosophers as P. K. Feyerabend, N. R. Hanson, and S. Toulmin propose similar views. This approach, however, suffers from some serious difficulties.¹⁴ Thus, I prefer to reject the notion of paradigms in favor of what I have called large hypotheses. Of course, I have noted the function of such hypotheses in a very compressed manner since that topic is not the main concern of this essay.

In reply to my denial of the methodological primacy of materialism for folkloristics, an objection was voiced at a recent gathering of folklorists. I was told that it is true that folklore is actually neither static nor an object, but for purposes of study one must abstract texts or materials from the entire event. The first step in analysis, I was told, is to describe an event adequately, and, since only a three-dimensional sound movie could even begin to approximate the ideal of adequate description, one must fragment the event in some way. I was told that texts, treated objectively, provide us with the way to fragment that event in order that we may study it. This has often been used as a way of insisting that materialism is a methodological necessity for folkloristics while denying that it is a hypothesis; for often a person presenting this kind of argument goes on to make a version of the inference I have already noted—namely, that, since it is possible to "fragment" most folklore phenomena as texts, these phenomena are therefore to be seen as materials. There is an element of truth in this line of thought: in order to initiate an inquiry, one must fragment reality in some way. Doing that is pre-

¹² See Charles Sanders Peirce, *Collected Papers*, ed. Charles Hartshorne and others, vol. 5 (Cambridge, 1934), 233–247.

¹³ Thomas Kuhn, *The Structure of Scientific Revolutions* (Chicago, 1962).

¹⁴ Several of these problems are noted by Carl R. Kordig in "The Theory-Ladenness of Observation," *The Review of Metaphysics*, 24 (1971), 448–484, and by Israel Scheffler in *Science and Subjectivity* (New York, 1967).

cisely what is involved in making a hypothesis. Any hypothesis abstracts from reality in some way. But it does not follow, as my objector seemed to think, that because the materialist theory, with its emphasis upon texts, fragments reality, it is therefore correct. It is a hypothesis like any other. And, as I have suggested, the materialist way of fragmenting reality has not stood up very well and has not been a very fruitful hypothesis.

This point is important enough to deserve further illustration. As an example, consider the following "fractured fairy tale." A traveler comes to Murca, a land where he is a stranger, and he asks a Murcan storekeeper if he has a map the traveler can use in order to find his way around. The storekeeper replies that there is only one map available in that country and it is on a scale of one foot to one foot, is three-dimensional, and has been draped over the land so that everyone might have access to it. The storekeeper states that, as a matter of fact, the traveler is standing on the map now, looking at its perfect detail as represented in the storekeeper's house and grounds and all the surrounding landscape. The traveler might be justified in telling the storekeeper that such a map is useless to a traveler, for it doesn't help a stranger to get around, it doesn't serve the purpose that maps are supposed to serve. And he would be correct. Now, hypotheses resemble maps constructed on a scale similar to those commonly found in the roadmaps we have all used in traveling by auto. They are certainly not the same thing as the reality they represent. Only a hypothesis as complete as the Murcan map would be so close to reality that there would be hardly any difference. But, like the huge map, such a hypothesis would be of little value, since it would be so cumbersome that we could not find it useful. Thus, every hypothesis must abstract from reality, and it is neither an asset nor a criticism of any hypothesis if someone reminds us of this. What makes one hypothesis better than another is its degree of confirmation, plus whatever other usefulness it has over and above this confirmation.

Here one might object by saying that the foregoing discussion of folklore materialism is simply a veiled polemic against those who wish to engage in literary studies of folkloric phenomena. But this does not follow from what I have said, nor has that been my intention. As I mentioned above, the disconfirmation of materialism shows that folkloric phenomena are not correctly seen as objects, items, or materials. The downfall of materialism also has the effect (among others) of destroying the unsupportable inference from the nature of texts to the nature of folkloric phenomena. These results should have no adverse effect upon those who wish to make use of texts in studying folkloric phenomena from within a literary framework; for, in disconfirming materialism, we have not done away with that aspect of the phenomenon that has been the object of transcription and study by students of literature—we have only come to see that this aspect is just a small part of the total phenomenon. Indeed, a realization of the mistakes of materialism should enable one to be more accurate in one's statements about the nature and significance of the content of such transcriptions within the total structure of the occurrence from which they were abstracted. Such a result should be equally welcome to literature-centered scholars as well as to behavior-centered scholars.

By the way, given the foregoing, one can see how to clear away another related problem. It has been standard practice among students of folklore to describe

proposed hypotheses for the nature of folklore-in-general as being definitions of folklore. These definitions are often proposed with the tacit assumption that they are somehow immune from inquiry because of their status as definitions. But, in view of what has been said above, most of the talk by folklorists concerning definition has been in reality disguised talk about hypotheses for the nature of folkloric phenomena, what I have just called "large" hypotheses.¹⁵ Thus, the famed and often noted "twenty-one varieties" of definition in the Funk and Wagnall's Dictionary¹⁶ are, by and large, sketches of such hypotheses. One might say that folklorists have been the victims of a "definitions fallacy," a procedure in which one describes hypotheses with the kind of discourse favored by lexicographers, followed by the tendency to see such definitions as not subject to disconfirmation, because (so it is suggested) they are definitions and not hypotheses and it would be wrong to try to disconfirm definitions. That problem can be overcome if one realizes that, even though a theoretical framework is expressed in the form associated with dictionary entries, it is logically a hypothesis if it is used for guiding empirical research. The recent rise of scientific folkloristics will probably eventually diminish the "twenty-one varieties," because no doubt many of them will be disconfirmed through empirical research or dropped because of a lack of empirically testable implications.

Understanding and Scientific Explanation

Now I wish to consider the part played by hypotheses when they have reached the stage of being confirmed. In their confirmed phase, they have an essential function in scientific explanation which is a principal means by which one gains an understanding of that which is the object of inquiry.

There has been a feeling among some practitioners of folkloristics that the discipline has been seriously flawed in its failure to provide an understanding of the phenomena it considers. In a discussion of games, Georges has explicitly captured this notion.

Collections . . . which have appeared sporadically during the past fifty years add significantly to the corpus of texts and comparative notes but contribute nothing new to our *understanding* of traditional play activities. . . . Those folklorists, then, who are no longer satisfied with merely collecting and annotating texts and studying them within diffusionist and survivalist frameworks must propose new directions for research if they are interested in gaining meaningful insight into traditional games and recreation.¹⁷

I believe that Georges' remarks are easily applicable to other folkloric phenomena besides games. Since hypotheses have a large part in attaining this kind of understanding, I shall briefly describe their place in scientific explanation.

The first prerequisite for scientific explanation is the possession of confirmed hypotheses that provide a statement of general lawlike relationships between phenomena. These are the sorts of things one might reasonably expect to obtain

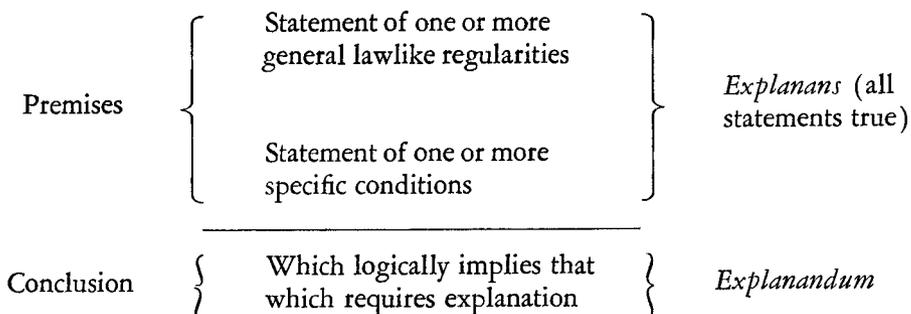
¹⁵ See Roger Abrahams, "On Meaning and Gaming," *JOURNAL OF AMERICAN FOLKLORE*, 82 (1969), 268-270.

¹⁶ Maria Leach, ed., *Standard Dictionary of Folklore, Mythology, and Legend*, vol. 1 (New York, 1949-1950), 398-403.

¹⁷ Robert A. Georges, "The Relevance of Models for Analysis of Traditional Play Activities," *Southern Folklore Quarterly*, 33 (1969), 1-23; and Georges, "Toward an Understanding of Storytelling Events," 319-344.

through the techniques of testing outlined earlier in this paper. Such statements assert that a uniform connection exists between phenomena as a matter of observable fact. This last requirement brings out a difference between some statements that are lawlike general statements and some that are not. Some general statements are universal conditional sentences that express merely tautological truths, such as "All unicorns have horns," and thus do not qualify as lawlike generalizations of the kind here envisioned. Those which do qualify are said to express a "true connection in nature" rather than a connection that relies simply upon meanings in language or logic for their truth.¹⁸

The second requirement for scientific explanation is a set of statements describing the specific conditions, the relevant circumstances that are associated with the *explanandum*, or matter to be explained. The *explanans*, or material used in explaining, will be composed of a statement of one or more lawlike regularities plus a statement of these specific conditions. This brings us to the third requirement: the statements that make up the *explanans* must be true, and they must either inductively or deductively imply the statement that is the *explanandum*. Thus, a scientific explanation is basically an argument of the following general form.¹⁹



Thus, we see that hypotheses are essential not only in guiding inquiry; but in their confirmed phase they also provide a necessary element of the very understanding one is seeking as a scientist.

Conclusion

At the beginning of this discussion I promised that I would trace a few consequences and implications of the view I was presenting. The time has come to fulfill that obligation. I ask you now to perform a thought experiment with me concerning the nature of archiving. I should add that in what follows my comments are directed toward examining the assumption that an archive would be useful for a folkloristic (social scientific) approach to the study of folklore. I am interested in discovering what kind of archive might be consistent with the account of the logic of scientific inquiry I have presented. Nothing I have to say here

¹⁸ See Ernest Nagel, *The Structure of Science* (New York, 1961), chap. 4.

¹⁹ See Kaplan, chaps. 5 and 9; Richard S. Rudner, *Philosophy of Social Science* (Englewood Cliffs, N.J., 1966), chap. 3; George C. Homans, *The Nature of Social Science* (New York, 1967), *passim*; Nagel, chaps. 2 and 3; Robert A. Manners and David Kaplan, eds., *Theory in Anthropology* (Chicago, 1968), essays 6 and 7; Peirce, vol. 6, 319-323; Carl G. Hempel, *Aspects of Scientific Explanation* (New York, 1965), essay 9; May Brodbeck, *Readings in the Philosophy of the Social Sciences* (New York, 1968), chap. 5.

precludes the usefulness of archives other than the kind that I recommend if one is not following a social scientific approach to folklore study.

Most contemporary folklorists, at one time or another, assume that some kind of folklore archive is essential to the practice of the discipline. Many seem to agree that an archive is a collection of texts arranged in some system of classification so that future workers may examine this factual matter in order to develop hypotheses about folklore materials and processes. This sounds like a repetition of the view of folkloristic methodology I have been criticizing throughout this paper. That is, the reason an archive is seen to be valuable is that it preserves many "objects" or pure data that would otherwise be "lost," objects that are seen as useful in the absence of hypotheses about specific issues. Also, it should be apparent that an archive conceived in this manner typically presupposes that large hypothesis which I have called folklore materialism.

This way of conceiving the function of an archive suffers from some serious weaknesses, despite whatever advantages it might have. First of all, because it is based upon the materialist's thesis, to the extent that materialism is disconfirmed, that portion of the concept of an archive will have to be modified. Second, since the content of such an archive is seen to be records for the most part, usually very skimpy records at that since a "text" is just a small part of a very complex phenomenon, it contains no knowledge in the form of confirmed hypotheses that could be used in future explanations. Third, when one does have a hypothesis in mind and decides to consult an archive hoping to find a relevant observation, more often than not one finds that the "collector" failed to take note of the kind of phenomena in which one is interested. This is the case because, typically, the collector, following what was once taken to be sage advice, purposely avoided having any "preconceived ideas" (hypotheses) in mind as he elicited songs and stories from informants, carefully recording every word verbatim. What we are often left with in an archive is a mass of "texts" of limited usefulness. Melville Jacobs has concisely summarized this state of affairs. His remarks are directed toward studies in oral literature, but I believe they are equally applicable to archiving in general, as it has usually been known: "Oral literature is . . . backward as a science, although it possesses extraordinary amounts of descriptive, catalog, and comparative items. Their utility for construction and validation of a system of theory about oral literature is no longer a moot question. There is no doubt that they are largely waste effort."²⁰ Robert Jerome Smith, in a recent review of two published collections of folktales, has made a similar observation: "We are still bound by the Baconian idea that if you gather enough data and present it neatly enough, sooner or later something of theoretical value will make itself apparent. But a large number of collections have been made by now and cross-references and comparative notes fatten the indexes, but precious little comes of it."²¹ The view I have been criticizing throughout this essay is essentially the same as the Baconian approach.²² Of course, in regard to the remarks by Jacobs,

²⁰ Jacobs, 320, emphasis added.

²¹ JOURNAL OF AMERICAN FOLKLORE, 85 (1972), 84-86.

²² For a concise compilation of Bacon's work, see *The Complete Essays of Francis Bacon*, ed. Henry LeRoy Finch (New York, 1963); note especially pp. xi-xii.

oral literature, if not treated as a social scientific mode of study, might not be backward at all.

With these matters in mind, I ask you now to exercise your imagination to consider what kind of archive might be compatible with the way a science of folkloristics has been conceived in this paper. As I see it, a piece of folkloristic research basically involves a test of one or more hypotheses. Therefore, the type of material one might find in an appropriate kind of archive would include hypotheses considered, means adopted for testing, the observations employed in such tests, results concerning whether a hypothesis was confirmed or disconfirmed, what possible explanatory force that hypothesis might have if verified, plus remarks describing how the work contained in that report might be suggestive for future research. This is not intended as an exhaustive enumeration, but simply as an indication of the kind of material that would be valuable for an archive based upon the guiding principle that folkloristic research should be directed by reference to overt and clearly stated hypotheses. Such an archive could be organized according to a self-generating classification scheme based upon the key words in each hypothesis. This kind of heuristic classification technique could easily be computerized in the fashion of the key-word lists found in information retrieval systems. A body of research of this kind would form a valuable set of materials and would no doubt lead to bigger and better hypotheses for future workers to check out. This would in turn lead to an increased understanding of folkloric behavior, the ultimate goal of any of our procedures.

One other matter is worth mentioning since it has a connection with the approach I have been defending. A hypothesis that was once widely entertained among folklore scholars was the view that folklore was something to be found in some special place (often called the "field"), which was populated by a special kind of people, the "folk," the peasantry—the simple but noble kind of person whose ways were worth study because of the quaintness of his customs or because one wished to discount the errors of his crude and inept practices or techniques. It would be very difficult nowadays to find a scholar of folklore who would disagree with me when I say that this hypothesis has been rather resoundingly disconfirmed. Most scholars would probably now agree that this kind of behavior is perfectly natural and normal among all people of every race, social class, religion, and cultural background. Yes, even professional folklorists should be added to this list. Serious students no longer view folklore as some kind of curiosity or error that is congenitally practiced by certain stereotyped "lower" peoples or "folk." To capture the essence of this notion in a nutshell, one could say that folkloristic research having folklore graduate students (or professors of folklore) as subjects would be as valid as folkloristic research having as subjects persons from an urban ethnic enclave, or a backwoods community, or from an isolated island culture in the Pacific Ocean. In other words, the term "folk" in "folklore" is that sense of the word which means "human," not that sense which means "a particular kind of group or society." Thus, adopting the interactionist thesis as a way of giving sense to "lore," folkloristics is the scientific study of certain universally distributed kinds or modes of human interaction processes. Here I am going beyond (but not dropping) the original sense for "folkloristics" I announced at the beginning of this

essay. But this has been accomplished in a manner that allows the reader to distinguish between the two ways of using the word; furthermore, it can easily be seen with which specific school of thought (within the broader, initial sense of "folkloristics") I am affiliated.

The extent to which this hypothesis (that folklore derives from certain kinds of societies or groups known as *a* or *the* folk) has been disconfirmed can be taken as a consequence of a fairly recent realization among contemporary folklorists that this was the hypothesis that had motivated a good deal of the research of their predecessors. Once this hypothesis was raised to full consciousness, its disconfirmation came to pass rather quickly, for the evidence is overwhelmingly against it. Its rejection, however, had to await the time when it came to be explicitly verbalized. There is some poetic justice in this, for one is often told how inhumane a social science would be. Nothing could be more humane than this important finding, established with the aid of a scientific mode of inference: that folkloric behavior is not a curiosity, not a symptom of inferiority and ineptitude, not a mass of error, not the exclusive property of the stereotyped "folk," but a sign of one's humanity.

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The Role of Hypotheses in Folkloristics

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