

Editor's Note

The following two articles were presented at the Folklore Institute's lecture series entitled "Explorations in Folklore" conducted in the spring of 1976. We proudly present these articles and are pleased that we can bring them to a large scholarly audience. As folklorists, we know that there often can be a marked difference between oral and literary styles; bear in mind that these formal lectures are of the former.

THE RENASCENCE OF SOFT METHODS: BEING AHEAD BY WAITING

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When I proposed the title (among others) of my lecture I was quite satisfied: the heading sounded a bit mysterious, and I didn't even know exactly what it was about myself. By and by I drafted a design, but by and by I have learned that I was going to run the risk of just flogging willing horses. Now I think it is not too bad and will not offend anybody to break through open doors; but to do this by taking a run of about 5,000 miles is not usual even with the silent movie. Thus, just let me confess that I know that it was not a Tübingen fellow who gave a holistic account of bloodstoppers and bearwalkers (not to speak of his further studies), that it was an Indiana newcomer who gave comprehensive insights not only into Hungarian tales but into Hungarian tale-telling as communicative events--and I ought to add at least a handful more examples. But here I am--it is too late to take the night train to Solms--I'll try to make the best of it, and I will not mind if you will state afterwards that the only result of my visit is strong evidence for the theory which Richard Dorson regards as his own, the hemispheric theory saying that things are rather different in the Old and in the New Worlds.

I am going to tell you about some of the latest stages of development in European--or at least German--Volkskunde, folkloristics, folklife research, empirical culturology, European ethnology, and I must leave it to you whether or not there are any chances of transfer. I myself suppose, to be sure, that there are parallels or analogies between research over here and over there, as, for instance in the relations between folklore and folklife research on the one side, and sociology and other social sciences on the other side. In his survey on the different concepts of American folklore studies, Dorson points to the "younger American folklorists who admired the precision and data-supported generalizing of the social sciences." In Germany it was perhaps not so much admiration for the neighbors which led to new investigation methods, but a sense of discontent with what Volkskundler had achieved and not achieved, and also with the public image of Volkskunde; at best, the folklorists were looked at as sort of old-fashioned craftsmen among the scientists, working on nothing but nice irrelevances.

There were two remedies to cure that disease. First, folklorists could choose different objects, problems that are more relevant and under discussion. Some of them tried, but had to realize the danger of losing their ground: they sometimes felt they had deserted to domains of other sciences they couldn't cope with. The second way was to adopt and to adapt different methods--methods more rigid, better controlled, and more exact than the common sense approaches of collecting and arranging folk materials.

I think the methodological lag and lack was especially great with German folklorists. It is probably not by chance that Paul F. Lazarsfeld passed upon even German sociologists a very austere judgment: "There were German professors trained to make the most subtle conceptual distinctions; but when it came to writing questionnaires or presenting statistical findings they used crude and sloppy procedures."

Even the most sophisticated empirical works of Volkskunde were rather poor in methodology. Atlases and mappings which pretended to give a survey on what people said, thought, believed, which festivals they celebrated, and so on, were in fact generalizations of the attitudes of a few positively prejudiced informants; even the research of a very close community were not characterized only by ideological preconceptions, but also by the total lack of the idea of sampling.

Not all folklorists were eaten up with envy when looking to the research methods of other sciences; but some at least tried to catch up with sociologists. Sociology in this context meant empirical sociology--more exactly, an empirical sociology which limited field problems to those on which quantifiable data could be secured by the use of surveys or other means. In 1959 Nathan Glazer wrote: "The sociologist today--whether his field of interest is the community, criminology, marriage and family, world politics, social classes, housing--is a man who asks people questions and then statistically analyzes the answers to them . . ." Sociology--that meant cautious sampling, quantifiable data, exact exploration; and these were the standards that folklorists tried to make up for. The borderline quarrel and status battle between social sciences and sciences closer to humanities and arts could not be settled by this development in folkloristics; but at least one should no longer be able to mark the borderline by a clear difference in the reliability of methods.

For some time folklorists were very optimistic about their first and second steps towards a more exact science: they took large strides in putting the study of statistics into university curricula; dime novels and other folk or folksy literature were investigated by means of content analysis; explorations were not started until fair samplings were made; and for the first time, folklorists were really working on and not only with questionnaires. Elder folklorists (I say elder, but I should add that this was not at all only a matter of generation) felt uneasy with the development; younger ones were rather happy for some time. They had, as it seemed to them, jumped onto a train crowded with respected and even distinguished scientists.

If they would have been a little bit less occupied with what they supposed was a real revolution of their science, they would have noticed that on the next track another train had just started in the opposite direction. The train, then, was not yet packed with people, but there were at least some social scientists who held that the rigid methods applied by empirical research workers indeed prepared reality for scientific investigation, but did not positively prepare scientific investigation for reality.

Let me give an example to clear up the point. For the past several years, the investigations on language barriers have played an important role not only in sociolinguistics but in broader cultural research, for it was realized that these language barriers are cultural barriers and that they correspond to other cultural barriers. Linguists, headed by the Englishman Basil Bernstein and stimulated by Americans like William Labov, discriminated between two different codes within the same language: an elaborated code and a restricted code. These codes, characterized by different means and strategies of speaking, were attached to different social classes by means of empirical statistics: the elaborated code is characteristic of middle class people, the restricted code of the lower classes. For some years this concept of restriction and elaboration was elaborated further: hints for additional linguistic features were given and social classes were subdivided additionally into upward mobility lower class, lower class, and so on.

The problem, however, is still at hand: What does this separation by means of linguistic description and social statistics really mean, and how does it work to effect this correlation? There are, of course, a lot of conjectures and presumptions as to reduced linguistic turnover in lower class socialization or to reduction of communication and language by manual labor, and so on; but I spoke intentionally of presumptions and not of hypotheses, for presumptions could by no means really be confirmed, disconfirmed, or denied empirically. You can not manipulate a social situation by holding some variables constant while studying the changes in other variables; in other words, if you want to go beyond the mere description and striking demonstration of differences in quantified data, you have to take refuge in inter-

pretation and in methods which are (or come closer to) interpretation. With great amounts of statistical data you are able to find out the average--the average housewife, for instance, who was pinned down by a German study to be about forty-five, having two children and a husband who earns about \$800 a month. But I am rather doubtful about the use of that average or what to do with a woman who is fifty-five and has six children and a husband who is a disabled worker with about \$300 in benefits a month.

Another point which led part of the social scientists away from rigid methods or the exclusiveness of hard methods was the trouble with the alleged objectivity aimed for by these methods. I must cut this long story short: I restrict myself to point out that with all its bias, the subject of research, hidden by quantities of data, nevertheless influenced the direction of research in an often uncontrolled manner, and that the subject of the matter in question had almost no possibility of running the blockade built by the previously directed questions and observations. Thus, hard methods are scarcely compatible with action research, which tries to help and promote people by and during research work; nor are they compatible with interactionistic models for cultural research, on which Lauri Honko said: "It is important that the people who are being studied have an opportunity to influence the research process and to become actively engaged in it, or that they themselves take the initiative in starting a kind of research project which is acceptable to them."

Lauri Honko is an outstanding Finnish folklorist--here we are back again in the area of folklore research. As a matter of fact, folklorists didn't cling too long and too intensively to the use of the more rigid methods of questioning and investigation. They realized that they could not dispense with the softer methods of traditional fieldwork. I guess that not only the specific methodical dilemma indicated, but also special conditions of and in the field, was responsible for this shifting or reshifting.

One can only apply directed interrogation methods if one has distinct classifications and even clear expectations of the answers. This may be possible in homogeneous and rather static societies or cultures, but it is a problem with "folk societies in the process of critical transformation" (Dégh). Contexts of folklore become more informal (to use one of the classificatory terms of Kenneth Goldstein). In this process of transformation and erosion, even folk and fake fade into an ambiguous mixture; emasculation of folk cultures does not make them female but hermaphroditic, and supposed constants turn out to be mere variables of folklife.

For example, in a very revealing article, Muriel D. Schein did not ask, What is an ethnic group? but rather, When is and ethnic group? realizing that ethnicity is "intimately involved with other structures such as class and trading systems," and that ethnicity and ethnic identity vary their meaning and importance in time and space, and even at a time according to the specific situation.

In other words (and applied to the problem of methods), by harder methods of inquiry you'll get, as a rule, only a snapshot, a synchronic cross-section, or at most--by panelling--two cross sections; but you are hardly able to manage with processual developments of both the field and the investigation. The field, being dynamic and sometimes turbulent, requires one to operate with a great deal of "free ranging" observation: one must be able not only to fit findings in a given frame, but to allow the frame to change through unexpected findings. If you work with hard material only, analysis needs to await completion of data gathering. I've heard research fellows, when asked for the results of their investigations, say, "I haven't yet any idea, but I fixed next September as the date with the computer." Some of them still have no idea what they detected--they finished their work struck with the frustrations of not really understanding what they were doing for months or even years. This is of course an extreme, referring to a caricature of rigid methods, but cartoons are rarely totally wrong.

Let me now give you a few examples--inevitably only anecdotal--to illustrate why I think that soft (and that means more tentative, more open, more inventive) methods can hardly be done away with in folkloristics.

My first example refers to the time of carnival, which is a culmination not only for popular fun, but also for traditional folk customs. There are fools' parades, fools' trees, fools' dances, and even fools' courts of justice--sometimes in the formal sense of the word--with public prosecutor, lawyer, and judge, sometimes in different forms of functional equivalents. In many a village carnival sermons are held; sometimes they are performed as a parody of ecclesiastical sermons, sometimes as just a speech, an oration in funny rhymes on what people did (whether right or wrong) during the past year--a foolproof channel of more or less critical community communication.

This custom, assigned to the field of "people's justice," has met with the very vivid interest of folklorists. One can distinguish three stages of perception and conception: in the first, highly ideological one, folklorists didn't look too much at the real contents of the sermons, but were starting from and going in search of an emphatic image of Gemeinschaft, community. The sermons were understood quite generally, on the one hand, as a means to express and secure community by attacking deviation and deviators from the rules and, on the other hand, as an outlet for criticism against the most influential people--and that means as a favorable democratic correction of community power structures. That these interpretations (reproof of deviators and criticism of influentials) were in a certain contradiction to each other didn't bother those folklorists too much, for they understood themselves as collectors more than as analysts.

In the second stage, folklorists came to a closer investigation of the contents of the sermons. In some researched villages, the sermons dealt more with the women than with the men, more with the unmarried than with the married, more with the poor than with the rich, and so on. Suspicion was aroused that the foremost function of the sermons was the stabilization and consolidation of given power and influence structures.

In the third stage, folklorists, starting somehow from the point of this suspicion, tried to get hold of the context. You cannot get to the context by mere interviews, but only by (additional) observation and--best--by what community researchers called total immersion. By means of participant or non-participant and above all close observation, researchers realized the double meaning of the custom: of course, in many of the villages newcomers lived who had moved in from neighboring towns during the last years. Moreover, such customs of gay display and ostentative demonstration are visited by the people of neighboring villages and by tourists from the urban surroundings. In regard to them the custom is part of the "cult of the folksy" (Dorson)--for them the content of the verses in general is nothing but cheerful stories.

This does not preclude, however, that these stories, which often are not even true but are invented, have their special aim and target understandable only to insiders. To illustrate with a real example, in a village near Tubingen, which we investigated thoroughly and perhaps even excessively for years, there was a woman who was asked whether she might be "brought"--that means mentioned in the sermon. Her answer, reluctant but resigned to what she thought inevitable, was "Do whatever you want; it's all the same to me; I don't care." In their sermon, the young men did what they wanted: in their sermon they held that the woman once went to the cellar for potatoes, but instead of these fetched highly alcoholic hard cider apple juice. This seems to be quite innocent and inoffensive, but you can only get the real meaning if you know that her husband is a drunkard from whom she suffers all the time.

Village people know that, but the researchers ought to know it, too. The problem they are faced with is not only that of "ethnographic dynamite" (Goldstein), but that of the range of "context." Perhaps (I hope I am not misled only by language difficulties) the problem might be explicated by the semantic differential of "performance." It is

no longer revolutionary to claim that performance should be included in folklore research. But what is performance? In structural linguistics (which in Europe is still devoted in greater part to St. Chomsky), performance means the contrary and complement to competence: not the ideal ability to speak, but the real speech acts in their real meaning and environment. Speaking of folklore performance, however, one moves within the semantic horizon of theatre; one thinks of a stage or an equivalent to a stage, where performances are produced and shown. Even if the context is informal, the folklorist tends to crystallize what he or she calls performance. I think that the "breakthrough into performance" ought to be followed by a breakthrough through performance, to learn to know the whole background of how and what is performed. By the way, I am rather sure that this is far less opposed to Dell Hymes, whom I just quoted, than to many of his predecessors and followers.

But let me give you a second example. In the same village the members of our research group, concentrating on social structure and cultural tradition in that village, heard a story several times about a family who went to Hungary long ago and returned by a coach drawn by very small horses. Elder people in the village remember the harness and bridle of these horses; it was kept up to the last year of the First World War, when the leather was needed for boots and shoes. This is a story without belief motifs, without any tension, and without funny traits. Folklorists who are strictly bound to look at convention-governed situations would not even have realized or taken note of this story. Why was it preserved? Why do people--some people, at least--still tell about that family and their horses and the horses' harness?

Of course, the village culture is no longer a nonliterate society; but oral tradition and communication are still important parts of the culture. One may suppose, therefore, that the "system of elimination," which Barnes called "structural amnesia," is still of some importance in that society. That means that such a story would have long been forgotten if it made no sense at all for the present time. There is no really "functionless trait," and the self-defense of a tale is at most, for the smaller part, "built-in"; it cannot be fully derived from the formal structure of the tale, but must be coordinated with the interests and the attitudes of the people and with the structure of society in a comprehensive sense.

Now the tale itself, to be sure, and our general knowledge about the background, may lead to certain assumptions. There are reasons to suppose that the particular period of time mentioned is not by chance. If there were a type index for such reminiscence tales (and I argue that such an index ought to be worked out), one could possibly assign to this story the heading, "I gave gold for iron"--a sentence or saying which was written on the receipts people got when they abandoned (or let's say sacrificed) golden rings and valuable jewelry in order to support German war industry. The misery of the wars is often remembered in terms of material wants and lacks. A second assumption concerns the exoticism of the story. The harness some people are still reminding themselves of was different from average harnesses: it indicated that the horses were smaller than average horses and that they were of foreign origin. Thus the harness was a symbol of strangeness and as long as strange things are really strange they are worthwhile to be noticed and to be remembered.

But these are nothing but general assumptions. The ethnomethodological or ethnography of speaking folklore approach might be supposed to lead somewhat further: What do people telling and hearing the tale think about it? How will they interpret what they tell or were told? But even an interview which is cautious and penetrating at the same time will give only very little additional information. The researcher is forced to outrun his informant because the informant is not conscious of every motive and reason and thus cannot verbalize what it is that diverts his thoughts into just this channel and puts the story in his mind. However, speaking of the unconscious does not mean taking refuge in spurious mysteries or even in questionable human archetypes--essential parts of the unconscious and subconscious--but rather in what people are

doing as a matter of course, often stubbornly and without thinking of alternatives. The passive, not to mention suffering, aspects of identity--are the results of former conditions of social life and socialization and thus may be traced to local social history.

To return to the Hungarian horses, I'll go some distance in that direction, but only by way of intimation. Working on the files in the community archive for weeks and months, the research group came across some facts which might be relevant at least for the story and its preservation. There was, indeed, a family that left their home village and went to Hungary. But they didn't sell their land and their house: rather, they had their eldest son stay in the (Suebian) village. Twelve years later they came back, probably with the small horses. But this was not the only thing strange and therefore noticeable thing about them: they no longer conformed to the norms and values of the old village; they did not acknowledge the unwritten laws of customs any more, such as harvest and other working traditions; they didn't fear quarrels with their neighbors and often had recourse to the official court with its written, positive laws. They were the first people in the village who noticed the upheaval of industrial revolution, and took courage to found the first factory or manufactory there--a brickyard--which was a start for wealth and reputation in the village.

In the meantime, economic conditions changed. But possession--property, landed property--is still the foremost basis for influence and esteem. Today there are different branches of the "Hungarian" family, but it is not only by chance that some of the members of these branches distinguished themselves as candidates for the position of burgomaster or as functionaries in the more important local associations.

Thus, the story (to speak once more about it) is not only a faint document for a certain exoticism, but sort of an etiological legend with a real historical background. Richard M. Dorson once pointed out that in Scottish Highland stories "precise historical details" of the famous history "fade into an empty never-never land." In our example, never-never land is not yet reached, and we have to ferret out the history ourselves through comprehensive observation and archive investigations.

I will present the last example to outline different implications of my general pleading for soft and wide methods. A postgraduate of our institute is working on photography as an example of popular esthetics. I do not want to engage in the problem here of whether popular culture is sort of a monstrous equivalent to folklore and whether folklore remains folklore in the mighty realm of popular culture. But I should admit that I join with Herbert J. Gans, who will not "subscribe to the notion that popular culture is simply imposed on the audience from above," and perhaps not even from outside. There is some support for this statement in the argument that popular culture is not only a matter of "audiences," but also a matter of active creativity which is, to be sure, framed and patterned by special agencies of popular culture (photography for example).

The research fellow wanted to know why, what, and how people photographed, and he began his investigation with very severe methods of content analysis, referring to pictures. He'd gotten a few dozen albums with thousands of pictures, and had made very precise statistics on the most favored motifs of the pictures taken, on the ages of the persons photographed, on their countenances, gestures, clothing, and so on. Through this he got a lot of data, but he didn't rid himself of the feeling that the data was not substantial enough. Thus we decided to extend the methodical approach. When he returned the albums to the owners after having analyzed them, he took a tape recorded with his and had people tell about their lives and the lives of the men, women, and children in the pictures. These interviews were extremely nondirective, and lasted for hours and sometimes even days. What people said could not always be assigned exactly to different pictures. Often the pictures were just the starting point, the provocation for long chains of associative reports and tales.

I don't want to indulge in this at great length, but I pick out just one commentary. In one of the albums there was a picture showing a couple in Sunday dress, accompanied by a written reference: holidays--vacation. In this album, and in other albums of this family, it was the only picture with this label. For our researcher, "vacation" was one of his numerous categories for quantitative analysis, for people are never as broadminded in taking pictures as during their vacation. The single picture in that album didn't add substantially to the amount of holiday pictures, to be sure, but the category could be checked off with this family, too. It was only by the additional commentary of the people that the researcher learned what vacation meant for this couple: they used to go to the old parents of the woman, and there they worked for weeks, cleaning and plastering the house, splitting the firewood, and so on.

They were not capable of taking real holidays until today, the man's wages being too low and the financial obligations for their house too high. Vacation as displaced working--here again one meets with special semantics, misleading as long as one sticks to the familiar meanings but revealing and informative when one has the correct translation.

Both sides of the investigation are important and complement each other. In the commentaries and tales there is strong evidence for alienating conditions and situations, but the pictures seem to be one way to reconcile people to their lives. One might, of course, as van den Haag said about the media, speak of "substitute gratifications," but I think esthetic activity and experience is always somewhat substitutive. I contend that there is not a true biography (that of the commentaries) or a wrong or mendacious one (that of the pictures); for these pictures and the occasions for these pictures are also subjective realities of the people and thus are important contributions to the biographies. One could say perhaps that there are soft lines in the life history of the people besides the more stern and rigid ones, and this leads me in a somewhat metaphorical way to emphasize the advantage of a similar reconciliation of soft and hard methods, and double strategy of research.

I have tried to outline the necessity of applying the more soft methods. In addition to the above mentioned development of the social sciences in general, there is a certain practicableness of these methods which might be called, not exclusive, but somehow typical and specific for folklore research. I want to repeat some of these traits abstracted, derived, and condensed from the concrete examples:

1. I hold that folklore research turns and has to turn more and more to rather unobtrusive facts of everyday life. Matters of daily routine become more important for research, and even if you start to prove the stupendous tenacity of (at least some) folklore, you can do that only by comparison with the less striking cultural elements of change and modernization. For unobtrusive objects unobtrusive methods of investigation are more appropriate than the more compelling and rigid methods.
2. In highly complex cultures--and perhaps it would be even more precise to speak of cultural complexity instead of complex cultures--most cultural items are ambiguous, multiform, and multivalent. In such a complex situation, the investigator cannot make it his task to avoid or ignore people's biases, but must get hold of and analyze them. Soft methods, once more, are more appropriate to this problem.
3. Soft methods are profitable with reference to the biographical accentuation and frame of men's culture. Biographical accentuation not only means that people refer to or reflect upon their biographies but that every cultural item, including traits of folklore, has a special meaning which depends on life history. This will not necessarily lead to the atomization of culture into innumerable individual elements, since common, collective cultures and subcultures are often patterned by comparable life histories.

4. It seems to be necessary to trace back utterings, attitudes, and interactions not only to what people consciously intended, but beyond this to those things of which they are not at all conscious. Whether or not you take a psychoanalytic view in a strict sense, I think one has to admit that the unconscious is a very important agency in our lives. Perhaps I should add once more that "unconscious" does not refer only to crude fantasy complexes evoked by repression, but also to the highly stereotyped actions and interactions of everyday life.
5. Folklorists' interest in history is a crucial factor differentiating them from most other social scientists. More and more, however, history is not taken for the ample reservoir of fragmentary relics but is understood as the real basis for very real traits of present everyday life. This also means different approaches to history, and I think that it makes sense to speak of soft historical methods as well.

In this lecture I did not give an all-embracing survey on German or European folklore research; I just tried to hint at an important tendency in folklore research. To be sure, there are a lot of folklorists who never changed their view and never quit using the way of traditional folkloristic methods. Some of them laughed in their sleeve when they saw the young, adventurous scientists fall back to what they thought to be honest, traditional fieldwork methods. Ironically, they held that the best way to get ahead was by waiting-- just as in the Grimm tale about the hare and the hedgehog or tortoise (Aa 275).

But fortunately one cannot win a race by deception in sciences, and I ought to have put a thick question mark following the subtitle of my lecture. As a matter of fact, folklorists who turned to soft methods did not really "fall back" to what had been done ever before. A hard core of rigid methods was not at all banished by them; most of them adhere to a combination of more soft and more rigid methods. There is a saying by Theodore Roosevelt which is said to be an African proverb (or, there is an African proverb which is said to have been quoted by Theodore Roosevelt): "Speak softly and carry a big stick, you will go far." That's a rather martial statement, but perhaps one might turn it into advice for fieldworkers: Research softly and carry a big stock of stern data and hard methods.

Traditionalistic folklorists are, moreover, wrong not only in their insinuation that the methodical area of social sciences has been absolutely left; they also did not realize that the soft methods applied now are somewhat different from most of the ways of the old fieldwork investigation. If it is characteristic of rigid methods that "the respondent is led to restrict his discussion to the questions posed," or at least to the items in question, traditional folklore research was in general rather rigid, but often without making use of the advantages of rigid methods. Informants were asked questions about preconceived objects, but often there was neither any sampling nor any chance for the informant to evaluate the object and thus help the researcher to functional connections.

The soft methods developed and applied now are aiming at a sympathetic understanding of people and folk culture. Soft, as I see it, doesn't mean weak, but rather means tender. But I feel I ought to be more reserved with semantics over which I have no real control. Let me try to explain by saying that in past years folklorists have tried to come within the range of rather exact investigative methods on the one hand, and the refinement (or, to say it in a paradox, the sharpening) of soft methods on the other.

As to the question of whether to choose soft or rigid, for my part I would not decide once and for all. It is worthwhile to reflect upon different methods, but I agree with D. K. Wilgus: "I have yet to find an approach to folklore from which I have learned nothing; I have yet to find one whose dominance is not dangerous."