

THE CULTURAL PROCESS OF EDUCATIONAL ANTHROPOLOGY:

TRADITION AND AUTHORSHIP

by

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("the subjects," "the natives," or "the participants" as I refer to them). Phenomenologically, the field experience and the overall research experience are a whole. They are really but one complex experience jointly controlled by the researchers, their professional peers and the participants. The great bulk of this experience is not reportable.

In this paper, I want to look at this activity holistically and stress the ties that bind all of us to a tradition in and out of anthropology that both limit what we can do and provide what we need in order to move on and, perhaps, progress. In particular, I want to investigate the processes that segment this activity into "data" production and analytic "reporting" of "the way it is." My goal is to discuss the questions we all have about the relationship of the different kinds of report we have about behavior in schools. We now have a large body of work on what happens there based on ethnographic or quasi-ethnographic observations. Without stretching the distinctive features by which ethnography recognizes itself, we can go back at least 50 years to the work of the Lynd's (1956 [1929]), for example. I am sure that we will also be able to use earlier reports. From the more "impressionistic" accounts of "everything," to the more focused analyses of sometimes apparently tiny interactions, from the more eclectic to the more theoretically dogmatic, we have an aggregate of studies which probably appear more miscellaneous in relation to the general knowledge which we seek than they actually are. It remains that translations have been rare and that there is justification for the scepticism of many.

To build, if not to "progress" in the old fashioned sense, we must understand the processes which lead us to focus on a particular subset of all the experiences we have in schools as "the" stuff on which we want to rely to present our analysis. Above all, we must realize that this understanding can come only if we replace the field experience in the context that makes it a "field" experience, that is in the context of the professional tradition that controls anthropology and the social sciences. No study, no field experience stands by itself. It is always a "next text" in the history of the discipline. It is a next "turn" in an ongoing conversation. Such a critique of our work should help clarify the relevance of our personal work to that of others who focused differently. Given the great variation in this focus, we must examine the grounds on which the following

questions can be asked:

- are we dealing with "the same" schools?
- are we dealing with "the same" processes?

Part of the impetus leading me to write this paper lies in the fact that the above is a general and pressing problem in the anthropology of schools and education. Part of the impetus also lies in the fact that I have just completed a work (Varenne, 1983) based on experiences in a high school. This work, in its final published form, looks very different from the usual monographs about school life, the category of ethnographic work to which it most closely belongs. It is an account of life in a school "in the round." It is also an account that looks so different as to raise serious questions of comparability or relevance. The issue is all the more important for me since I am convinced that "Sheffield," my school, is not inherently "different" from, say, "Middletown" (the Lynd's) or "Rome" (Henry's) or "Elmtown" (Hollingshead's) and the other Northern Illinois schools studied by Coleman.² I suspect that the Lynd's, Henry, or Coleman would have had experiences in Sheffield similar to those they had in their own high schools. In other words, I am convinced that, had they studied Sheffield, they would have written the same kind of book that they actually wrote (Coleman, 1961; Henry, 1963; Hollingshead, 1949). They would not have written the same book as the one I wrote. Conversely, I am convinced that I would have written the same kind of book as the one I did produce wherever I had had my experiences.

I am not talking here about "typicality" and temporal or regional differences. None of the schools I have mentioned are "typical" in the traditional sense. They are all heavily anchored in a certain time and space that is necessarily unique even if certain features of each schools seem to have a high probability of being found in other schools (e.g. size, S.E.S. of parent population, position of the school on the rural/urban continuum, dollar-per-pupil expenditures, etc.).³

I am not talking here either about "personal" style or theoretical point of view. These, of course, are relevant. To focus on them is also easily to loose sight of the historical frame within which the various "personalities" appear to surface. It is not simply that each work must be put back "in the context of its period." It is also that the ensemble of the works must be put in the proper historical sequence: my work comes

after much other work. It would not be what it is if some other work had not had the shape it has. My work comes next. As such "their" time is also "my" time.⁴

The Analysis of Action

It is commonly accepted that all social research is based upon more or less explicit theories of human action. It is less commonly recognized that these theories carry with them schemes for data production and a priori sorting of the ethnographic experience into types of moments. The theories I am thinking about (the various types of social structuralisms, marxism, symbolic and communicational analyses, etc) most often have the shape of hierarchies of activities where certain types are treated as somehow more fundamental than the others. They all assume that it is possible and altogether easy to separate, in the stream of the ethnographic experience, the postulated different kinds of activities. Whether the theory places material survival or interpretation as the "infrastructure," whether it emphasizes cognition, values or rationalistic common sense, most of the influential theories of the past century have established a center around which an undifferentiated "rest" is let to fall as it may. While this hierarchical tendency can be fudged in theoretical statements, it is glaring when we look at what happens in the "empirical" side of the researchers' work: Lévi-Strauss may write about the "primacy of the material infrastructure" but he has now spent most of his life studying myths. Marxists may have much to say about ideology but they are always better at identifying economic constraints in particular historical situations. This cannot be independent of the fact that, in the field, they spend so much more time collecting behaviors specifically marked for "economics" than they do with shamans and priests listening to exegeses of myths.

It is not my intention to discuss the relationship of data-generation to theoretical framework. I want, rather, to emphasize the lack of experiential, observational, i.e. properly ethnographic, support for the hierarchization of action. While such a hierarchization often makes deductive sense in theoretical discourse, no one has been able to provide clear frameworks for the sorting out of action as experienced in field situations into the postulated kinds. No other criteria than our own cultural common sense allow us to distinguish an economic transaction from

the telling of a myth, an aspect of the "real" from an aspect of the "ideal," something that is "social" from something that is "cultural."

It is easy for someone like Marvin Harris to criticize Lowie for "failing to separate emic from etic data" (1968: 365). The implication is that he, Harris, has good (theoretical) grounds for distinguishing one from the other. But Harris, of course, cannot provide us with the coding scheme that would allow for the separation. No materialist can, on materialist grounds precisely, since one cannot imagine what non-material data would look like. Thus a participant's telling of an event is a material "etic" data at the same time as it might be said to be "emic" from a theoretical point of view. In fact the distinction emic/etic has no empirical value. At most it can lead people, as it does Harris, to reject the relevance of certain kinds of data. What such a stance forgets is that, by doing this, the whole enterprise of ethnography is subverted.

My critique goes beyond the traditional comments about the dangers of "misplaced concreteness." It is not simply that "social structure" is not a thing, it is also that no behavior is purely "social structural" in ordering or relevance. Part of the reason why so many researchers find it easy to write as if items of behavior could be only relevant to one theoretical category at a time probably lies in the fact that so few perform the kind of exercise that Bateson says he performed after finishing the writing of Naven:

I thought that there was one sort of phenomenon which I could call 'ethos' and another sort which I could call 'cultural structure' and that these two worked together--had mutual effect on the other... [But then] I began to doubt the validity of my own categories and performed an experiment. I chose three bits of culture: (a) a wau (mother's brother) giving food to a laua (sister's son); a pragmatic bit, (b) a man scolding his wife,; an ethological bit, and (c) a man marrying his father's sister's daughter; a structural bit... Then I forced myself to see each bit as conceivably belonging to each category. I found that it could be done. (1972 [1941], 84-5)

In other words, Bateson went back all the way to his field experiences. There, the distinctions between types of events evaporated. The distinctions did not cease to be useful for analytic purposes. They simply ceased to possess the referential quality that we generally assign to statements about "the way it is."

Very few social scientists go back that far when they begin

writing. If they are at all aware of the issue, the temptation is to try and organize the participants so that they will only perform the kind of behavior one is analytically interested in. Thus, if one is interested in "values," and values are understood at what organizes "selection" in "choice" situations, then the solution is to ensure that the informants find themselves in "pure" choice situations. As Kluckhohn wrote in a methodological discussion of social psychological research in "value-orientation":

'Real' values, then, can be discerned by careful analysis of selections made in 'choice' situation, many of which occur in the usual run of living. But the investigation can be supplemented and refined by hypothetical selections, projective techniques, questionnaires, and simple experiments (1951: 408).

The problem that he could not become aware of, starting as he did from a theoretical understanding and working on to "behavior" (the answers to the questionnaires) concerns the fact that the social situation of the setting-up of the questionnaire is not a separate event from the answering of it. In other words, the answering of the questionnaire is organized as much, if not more, by the situation in which it is given (who gives it and how this person is related to the person who takes it, where it is given, etc.) as it is organized by the psychological "event" (a person's "value-orientation") that appears to be performed.

It is not that we must get rid of all concepts and analytic tools. It is rather that we must be clear as to the place which they occupy in the overall scheme of research in the social sciences. It is appropriate for us to question the relationship between the "social organization" of American schools and their "ideology." It is appropriate too to wonder under what conditions these structure change. But such concerns must not blind us to the actual conditions of the ethnographic experience.

The Ethnographic Experience

So much has been written about the doing of ethnography that it is easy to forget the practical conditions of the activity. Before anything else, there is the continual flow of interaction with the participants that prepares and allows for the subsequent doing of more focused activities, the collecting of materials, the hours milling about the lounges and corridors, the attendance at miscellaneous events, the interviews, the

videotaping of a class, etc. In fact, all possible research will begin with, and be surrounded by, this interactional flow. The distinctiveness of ethnography--in contrast to surveys and experimental designs--lies in the assumption that essential knowledge is gained in such non-focused interaction. The ethnographer is told to "put down everything," "ça peut servir" as the French would say--it may be useful. The extent to which such knowledge is sufficient is a continual matter of debate in anthropology (and outside of it also of course). This debate is beside my point here since I am most immediately interested only with the "participatory" experience of the researcher, whatever is later to be done with this experience.

As participants in the informants' everyday life, the ethnographer is necessarily privy to a huge amount of information. We have but to imagine the amount of what we would have to learn if we entered schools the way anthropologists enter an Amazonian society to realize how much we always know and use when we enter an American school. None of us gain this knowledge by the route anthropologists generally take: the making of gross mistakes that make the informants roar in laughter and tag the anthropologist as some kind of overgrown four-year-old. Even when one phrases one's interest in schools in terms of a search for some kind of "reality" behind the "myths," one enters the school through its own ideological representations of the place of research and researchers. One knows, and one uses the knowledge, that one enters through the administrators, that one has to make assurances about confidentiality, that getting access to the students' file will be a delicate issue. Once one is in, one cannot escape participating in conversations about individualization and tax rates, bureaucratic requirements and love, cliques and universal brotherhood, segregation and democracy. One will hear references to God and church going. These conversations will be part of massive more-or-less ritual performances--from salary negotiations, to political campaigns for school board elections and increases/decreases in school taxes, to classes, pep rallies, basketball games, Proms and graduation ceremonies. All of this will be experienced, all of it is potential information, however opinionated one might be about the particular relevance of only some of these experiences for an understanding of the whole.

The issue of selectivity in ethnographic practice thus does not

have to do with the actual experience that researchers have. It resides rather in what is done with this experience: what gets written in the fieldnotes? What sort of focused activities does one then enter into? What is used in the analysis? What remains of all this in the published reports that constitute the researcher as an ethnographer (rather than a novelist, or a journalist, for example)? After having been told to "put down everything," the neophyte is then told "but you won't be able to." Depending on the tradition to which the teller of these aphorisms belongs, more or less attention is given to the selection process. There is now a large literature on what may impact the doing of ethnography. I do not feel the need to contribute to it. My concern is other. It lies with the analytic determination of the selection process.

Let us look briefly at the first moment in the transformation of experience: the writing of fieldnotes. After a day in a school, it would be typical for things such as the following to be "written down":

- "As I walked down the corridor, I saw John and Lisa talking animatedly to each other."
- "Paula told me that the coordinator was really mean to her. She thinks he wants her out of the school because of her independence."
- "As usual, the principal started the meeting with a joke that made the teachers wince."
- "I interviewed the assistant principal about his role in teacher evaluation. The following is a transcript of the tape: ..."
- "I went to the office and got a copy of the salary schedule and the school philosophy (they are attached to the notes for today)."

Depending on one's prolixity, such texts can eventually run into the hundreds of pages. Item by item, the "observations," "interviews," "documents," are always about as trivial as anything can be. What is not trivial is that all these items refer in some way to an actual behavioral performance by the participants. No ethnographer, however much he may be interested in myth, ideology, "what people say" and other apparently "ideal" (rather than "real") data ever experiences and--initially--reports on anything else but actually performed behavior.

Conversely, this behavior, however concrete it may be, cannot be preserved in the form under which it was experienced. It must be transformed to become data. This is itself a concrete activity, a behavior to be performed by the researcher. Most typically, it consists in writing.

Thus, all that we know about the relationship of language to experience, and particularly of written language, applies here. In particular, we must remember that no language can be transparent to experience. This is a general condition of all social science and not simply of general ethnography.

Thus, the practice of social science is defined by the activity of transforming experience into words that can then become "knowledge." This activity can be seen as one of "in-scription." If there is something to Derrida's argument (1967) that the original experience itself is such an in-scription--however temporary--on a social space using various media (vocalization, gesture, etc.), then there is justification for arguing that the production of fieldnotes, their "writing," is an exercise in transcription into a different social space using a new medium. Writing fieldnotes is not different from transcribing an audio or video tape. Indeed, if "cameras don't take pictures," as Byers reminded us (1966), the very act of taping (this rather than that, here rather than there, etc.) is itself a transcription using chemical or electro-magnetic media.

Transcription is not a free activity. To be recognized as such, and then meaningful in its new environment, it must conform to certain transcribing conventions, i.e., it must be "conventional." What gets written in field notes is less dependent on what was the experience of the writer than it is on the conventions for transcriptions that are used. All that has been written about the making of transcripts is relevant to fieldnote writing. In particular, it is important to remember that, as Ochs put it in relation to work in language acquisition (1979):

- 1) one never conducts one's analysis on one's initial experiences, but only one's transcripts of these experiences;
- 2) transcription is theory.

Ethnography as Transcription

If transcription is "theory" and convention, then it is not fully controlled by the text that is being transcribed. Rather, it is controlled by whatever it is that controls the researcher as researcher, particularly the social forces which make of him an "anthropologist" rather than a "novelist" or a "journalist." These are the forces that define "science" as a specific kind of activity within Euro-American cultures, with "ethno-

graphy" as a subcategory within this science. These are the forces that define for the ethnographer what is worth noting, what can be physically noted, and what can then be made of it in theoretical conversations with peers and critics.

McLuhan once said that "nobody can commit photography alone." Byers expanded this to argue that "photography is a social transaction among photographer, subject, and viewer" (1966: 31). One cannot commit ethnography alone either. It is a social transaction between a tradition (that defines "science" that defines "anthropology" that defines "doing ethnography"), an ethnographer, informants, a report, and the response to this report. All the elements are essential to the process.

The question for us then concerns our understanding of the requirements of the controlling conventions. Historically, it has been easy for social scientists to argue that, since experience cannot be directly transported into our observational records, the path to scientificity lies in the most extreme level of a priori control which we can manage over our experience in the field. It is appealing to think that, by narrowing the field of experience, we can apprehend this experience more completely. All experimental, survey and hypothetico-deductive research proceed on such altogether wishful thinking. What we have now learned is that, if I may say so, experience has a way of always being richer than we can impoverish it. Furthermore, as psychologists are coming to realize, too great a success in impoverishment and control produces results the relevance of which to everyday life is extremely tenuous (Cole, Hood and McDermott, 1978).

What those who wish to exercise control a priori forget is that our responsibility, precisely as it is defined by the tradition that makes us, is to capture everyday life as it is actually lived. In fact, the conventions, though they have a controlling power over our theories, are themselves pre-theoretical: on purely theoretical grounds our task is impossible, but, as all good cultural tasks, it must still be carried on. Experience, "the life of the people as they themselves live it," must remain our goal. Anything that does not take us there is necessarily flawed. The greatest problem with experimental methods lies in the fact that they transport the "biases" from a situation where it is plausible that the researcher will eventually be told by his informant that he is

wrong, to a situation where the informant is so controlled that he will never be able to give any guiding feedback. Experimental methods turn us back on ourselves. In this way they subvert our responsibility to our scientific duty. At its most general, what makes an activity scientific is the likelihood that it will lead us away from ourselves towards a new center.

In other words, to the extent that we wish to produce good anthropology we must stay with the broadest of the traditional definitions of the discipline. We must continue to grasp for "everything." We must continue to confront everyday life, the routine, the trivial and common place for this is where human beings live, what we as researchers experience, and what we must gain our knowledge from.

To make all this more concrete, let us think for a moment about something that is always of central interest to teachers in a school: their salary. It does not take long for an observer to learn that there is an uncomfortable tension in schools between the language of teaching as an almost sacred "calling," and the language of teaching as a job in exchange for which one receives some money. For a teacher, this money is never enough. It is always too much for the tax payers of the district.

It is usual for analysts of education and schooling to distinguish the two languages: one would be the "reality," the other the "myth" that "mystifies" the participants in not being aware of the reality of their situation. This transforms into a perennial tension among professional theorists of education between the "traditionalists" who like to see in education a quasi-mystical endeavor of spiritual development and the "revisionists" and other "de-mystifiers" who like to emphasize the ties between social structural, economic conditions and the delivery of education. The research temptation is to accept this dichotomy as an a priori given. It seems possible to define before hand what kind of behavior fits under either the "myth" of education, or the "economic infrastructure" of teaching. If the experiment is well designed and "controlled," then it seems possible to explore each side separately.

In fact, in the experience of teachers and ethnographers of schools, the two languages cannot be known separately. They are known together in a much broader setting than is implied by the statement of the dichotomy. What is experienced is a complex of conversations that one can

overhear or participate in. It is a life process that includes constant reference to something labelled "education" and to something else labelled "money." These references can be made in a great variety of contexts like:

- 1) public accounts of the motivations that lead some people to teach ("I love to teach." "Teaching is a calling and a vocation");
- 2) The public accounts of the value of a salary schedule;
- 3) the legal, restricted, accounts of its working as it might be written in contracts;
- 4) the economic implication of the actual figures (do they allow teachers to buy houses in the town they work in?);
- 5) the conversations teachers may have among themselves about the salary schedule;
- 6) the statements one teacher may make about this schedule in the privacy of an ethnographic interview;
- 7) anything that includes some reference to "salary."

In fact, what is striking during the ethnographic experience is the fact that, in some settings and for certain purposes, an altogether large number of participants can tell us about the "reality" which the "myths" supposedly prevent them from perceiving. Indeed, it is the participants who are the most vocal at telling the ethnographer that there is a "reality" in schools that is not "what people will tell you." They will then make certain that you can recognize events relevant to either side of the dichotomy that they themselves are making. They will specify "people will tell you X, but what is really going on is Y." In that sense myth and reality are both actual cultural performances. From a theoretical point of view they are both ideological statements. They are both myths. They are also "real" for both have actually been performed. These performances are experienceable and, if one is sensitive, both are reportable as "what the natives do." The analytic issue thus concerns the fact that we must preserve our interest in the interplay of ideology and social structure while taking into accounts that this interest is also such an overwhelming interest of our informants that we can easily be misled by the way they express their concerns. Above all, we can unwittingly adopt their language and loose the ability to express:

- 1) the ideological organization of the participants' way of distinguishing reality from myth;
- 2) the nature of the experiential process through which we have learned about the participants' ideological operations;
- 3) a theoretical understanding of the relationship between ideology and social structure that is ethnographically valid.

My position here is un-abashedly non-positivistic. One never experiences "raw" reality. Neither the "myths" nor the "realities" can be accessed except through indirect symbolic reference. What is to be transcribed is not the parameters of an object which was hidden and has now been uncovered. Neither "culture" (the myths and symbolic representations of a people); nor "social structure" (the classes and patterned relations into which the people find themselves) possess the kind of object-ive existence that they would have to possess in order to be dis-covered singly and separately. As Radcliffe-Brown told us a long time ago, there is something in human populations which remains stable even as every single member of the population has been replaced. A stability in interpersonal relations, however, is not substantive since, precisely, all the "substances" (the people and the objects that the structure organizes) can change without any change in the relationships. Social structure, like culture, is a performance that is continually reconstructed, reproduced by the actual, living members of the population. It is less accurate to say that behavior is determined by external (social or cultural) constraints than it is to say that behavior creates its own constraints within a set of pre-existing, "always-already-there," constraints.

In other words, "to deal with everything" is to look after the organization of a creative process that is probably best understood as a complex conversation. Clearly, the process itself is not "there," as an object, anywhere in time or space. It can only be reconstructed through the remains of its operations, the physical traces in some medium, the "inscriptions" that are left over. These inscriptions are the only physical entities which anthropologists can "transcribe." They are the "stuff" out of which our science is made. As left-over inscription from an improvised creative process, this stuff is now "dead." The first task of anthropology is to maintain this stuff in a kind of limbo somewhat akin to those "cell lines" which biologists and medical researchers use to develop their knowledge of life.

The Fate of Observation

Transcription itself is a "live" process. It is something which the researcher performs in a far from haphazard manner. As Lévi-Strauss would write, this process has a "concrete logic" (1966). It has, as Kaplan

would put it a "logic-in-use" (1964: 3-11). In no event can it have the rationalistic, controlled or "reconstructed" (Kaplan, 1964: 3-11) logic which social scientists too often believe can only establish their scientificity. The "liveliness" of this process, however, is precisely what concerns us and what we must better understand. In brief, the transcriptive act transforms the original experience in:

- 1) preserving a small bit of historical development that would otherwise have been radically obliterated;
- 2) decontextualizing it by cutting it from all subsequent events that were, and still are, being produced by it;
- 3) creating artificial beginnings and ends;
- 4) being made to fit in a new context ("anthropology") into which the people who originally produced it would never have made it fit;

In other words, the transcriptive act pumps a radically "new" life into the moment than it would otherwise have had. This act, however, is not free from the original experience to which it is a possible response within the anthropological community.

In fact, the fate of the transcriptive act which has transformed an experience into an observation and then data is the same kind of fate as it would have had among the participants. Had there been no anthropologists in Sheffield in 1972-73, the moments in the everyday life of the high school which became "observations" in our notes, and "data" for my analysis, would still have died and been reborn in the subsequent moments which the people had to improvise to conduct their life. This improvisation would itself have had a "logic-in-use" that would have made something out of something else. This thing could not have been anything in so far as the community of interactants would have held each other accountable for it being a particular kind of thing internally marked both for its explicit tie with the original event and for its participation in the system of "next events."

The community of anthropologists perform the same function for "analysis." If we accept the fact that what has been inscribed in the past is always constraining on future behavior, even though it is not absolutely determining, then we can also accept the idea that the transcriptive activity of anthropology is never itself absolutely creative in relation to the field experience itself. The field experience was a live interactional event. Some of it will always transpire though only as much as the new

environment of the inscription, the "theory" that transforms it into "data," lets transpire.

Take a salary negotiation for example. It is a moment in the life of the school. It follows various other events (teachers feeling they cannot maintain a certain standard of living, administrators realizing they cannot ignore the teachers' protests, etc.) and leads to the production of other events (threats of strike, creation of unions, tensions between the administration and the school board, non-reelection of certain members, etc.). While the negotiation itself was a well-marked event with an internal organization that makes it stand out, it is not by itself either the "beginning" or the "end" of any larger sequence in an absolute sense (the participants may eventually constitute it either as "the end of a period of trouble that we have thereby resolved," or the contrary. But this is simply evidence for the ability of the participants to segment history into ever larger chunks than the negotiation itself. It is not evidence for the end, or beginning, of history).

The same salary negotiation, obviously, does not fit at the same place within the sequence of anthropological research. The negotiation is "data" constituted as such by the fact that it comes after the proposal, is explicitly tied to "what I (the researcher) wanted to find out," and eventually leads to a published analysis. This sequencing of the negotiation is equivalent to the quote marks which indicates that a sentence, in a text, is intended as a report of direct speech. Such a sentence however continues to be a moment in the actual text within which it is placed. Its tie to the speech purportedly reported remains tenuous. The quote marks, as such, do not ensure the adequacy of the transcription. At most, they increase the verisimilitude of the quote.⁵ Data is never data by itself. It is only data because it is made to fit within a sequence at the place where "data" appears.

If the above is correct, it will allow us to go beyond the radical theoretical scepticism that often goes with any critique of positivism. The scientific value of anthropology lies in the fact that the field experience is such that it constrains what will then be said outside of it. The evidence that what we are saying is so constrained is the non-positivistic version of "objectivity." To draw the full consequences of all this for scientific "progress" in our understanding of schools and educa-

tional anthropology, we must also be clear about the role of the anthropological tradition. It is this tradition which makes it possible for us to be constrained by the field. It is this tradition which is also responsible for keeping us on the "right" track and, sometimes, for getting us lost.

The Cultural Construction of Anthropology

Garfinkel once made the following statement in a symposium on ethnomethodology (1968):

Now, let us say you want the term ethnomethodology to mean something. Dave Sudnow and I were thinking that one way to start this meeting would be to say, "We've stopped using ethnomethodology. We are now going to call it 'neopraxiology.'" That would at least make it clear to whoever wants the term ethnomethodology, for whatever you want it for, go ahead and take it. You might as well since our studies will remain without that term. I think the term may, in fact, be a mistake. It has acquired a kind of life of its own. I now encounter persons, for example, who have acquired a professional responsibility for methodology. They wonder what it is all about and then begin to imagine, "Ethnomethodology must be something like this." They talk to other persons. They have trouble getting access to the papers. They want, after all, to know and they begin to tell each other, and then the rumor mill gets under way. Pretty soon you have a machinery that is generating attitudes and questions about this work that we are now expected to take and to address ourselves to, even though these are not our attitudes and questions (1968: 10-1).

I like this quote because:

- 1) I have framed this article in a language which I consider related to Garfinkel's;
- 2) the quote is an excellent account of the social life of social scientists among themselves;
- 3) it underlies the difficulty of escaping the same dominant myths about social interaction which Garfinkel has made it his life to challenge. These myths are:
 - a- the myth of authorship ("I think," our attitudes and questions");
 - b- the myth of real meanings detached from their social contextualization ("these are not our attitudes and questions);
 - c- the myth of control of one's meanings ("our studies will remain without that term").

The point of all this is that it can help us outline a solution to the problem of control in anthropology which the preceding deconstruction of the "externally real" may seem to have created. If the "real" is, in

fact, a construction, then it cannot provide for the eventual touchstone allowing us to settle our analytic disputes. I am rather certain that even physicists never absolutely resolve their disagreements about the number of electrons uranium has "by recourse to objective evidence." There must first exist an agreement about the location of "objective evidence." This agreement is, as Watzlawick et al. would say, a "relationship" issue that can only be solved by a recourse to a symbolic power which defines the location of evidence. To quote the quote Watzlawick et al. use at this point:

"'The question is,' said Humpty Dumpty, 'which is to be master--that's all.' (last italics ours)" (1967: 82-3)

The touchstone for our arguments is the tradition itself--anthropology as it has been defined by itself, and by its external popular and intellectual critics and apologists. Once the field experience has been had, the anthropological tradition "is master."

This is not the place to outline the properties of anthropology as a tradition that are used to make anthropologists accountable. It is certain that these properties and criteria are neither a literal consensus nor a jumbled amorphous aggregate. To say that anthropology is a tradition is not to say that all anthropologists "share the same values," or have "the same cognitive frame of reference." As Garfinkel unwittingly reminded us, single, unique and "different" authorship is a central feature of the tradition. So is the rhetorical externalization of "real meanings" and "real objects" that are to be related. So are the disputes about the relative importance of 'x' aspects of life vs. 'y' aspects. The tradition cannot present itself to itself as harmonious unison. It must construct itself as a cacophony of individual voices. It has to be experienced as the Suya akia style of singing so well described by Seeger (1979) is experienced : a concert of voices singing together different individual songs at the top of their lungs.

The Suya akia, however, is a well circumscribed event marked by its style. The Suya know when the parts of rituals which require akia start, what range of behaviors belong to akia singing, and when it must stop. Similarly, anthropologists--however diverse they may present themselves to be to each other at such times as the Annual Meetings, however much they may listen to calls for rethinkings, reinventings and other

recreations of anthropology--also know, pragmatically if not literally--when a text ceases to be anthropology to become something that the "anthropology" section of the National Science Foundation or the Wenner-Gren Foundation for Anthropological Research should not support. Even if Geertz is right when he writes about the "blurring of genres" in social science work (1980), it is clear that this blurring is an event within the tradition that is controlled by the tradition--and by the other traditions which in fact control us through their institutional representatives--deans, N.S.F. review boards, journalists, politicians, etc.

All this is to say that a text, to be considered within anthropology, to be recognized as an "analysis" of "data" collected using the "method" of "ethnography," cannot have any form without rousing reactions that can range to radical rejection. Indeed, subtraditions within anthropology define each other in terms of what they accept and reject as "anthropology." Certainly, no work will be acceptable as "descriptive/analytical" (rather than "theoretical/philosophical") unless it convincingly establishes a tie between itself and some experience in some "field." To do ethnography is to link some events experienced "among the natives" with a long conversation in anthropology and beyond about point of view--and values, beliefs, characters, ideologies, social structures, ecological constraints, and so on and so forth.

To do ethnography is to participate in these conversations. But a conversation never consists in the constant restatement of the same words. While a conversation is always well organized by turn-taking rules, it must also progress. However brief, it must move from a before to an after. A conversational turn is always a next turn. We generally speak of the doing of anthropology as of something which one anthropologist performs by having extremely personal experiences in some "field." We speak of a personal "style," of a unique moment in history. In the process, we forget that the same persons is, also, taking a turn in a conversation. This conversation started before him and will continue after him. He does not control this conversation. If we are not to lie to transform ourselves into Promethean heroes, we must see our responsibility as consisting in making sure that our contribution really is a next step for the discipline. It cannot be a simple recycling of previously produced work. It cannot present itself as something radically new. It must be a response to the texts that consti-

tute this work. It must include them and go beyond them.

Next Texts in Educational Anthropology

I could easily have cast this article as a call for "new beginnings," for a recasting of the work to be done. In a way, this is what I have done by criticizing the objectivist and positivist stances which make of "data" an external event to our operations. However, I have preferred to argue that literal attempts at redefinitions are themselves misguided: we do not have the freedom singly to transform anthropology. The paper is a call for a return to the tradition, for the need to write in terms of the old categories though obviously not in the way one used to do so. We must go beyond the opposition culture/society but it remains with us as categories to which we are held accountable as we converse with each other and with "outsiders" (e.g. school personnel) who expect something from us in exchange for what they give us. To refuse to participate in these conversations is not to re-direct the discipline. It is to resign from it.

What then can we do next? Re-construct. We have no choice but to stay with the traditional intuitions which define anthropology. As a logic-in-use, these intuitions cannot be formalized. But they clearly entail the recognition of the need to look at human beings acting in every aspect of their everyday lives--even the esoteric ones--to learn about them scientifically. From the beginnings, those among the social scientists who have been convinced of this need have produced what we now recognize as anthropology. It is not that we must accept the work these have produced "as is." On the contrary. Our task is continually to confront this work with the intuition that is our operating legitimating principle. In other words:

- 1) what makes some work "anthropological" is its ability to claim that the process of its production has involved a time when the author has directly experienced the everyday life of the people he is writing about;
- 2) what makes some work "good" anthropology (or at least work that is directly useful at the time of its publicizing) is its ability to convince a community of anthropologists that it speaks to their concerns by being a "next turn" in the over-all conversation;
- 3) what allows for the production of "new" anthropologies is the developing awareness that the work that has come before has not, in fact, been controlled by the ethnographic experience.

To take an example from my own work, I recently made the point (1982) that the work which made of high school "cliques" objective social organizational events (i.e., aspects of the "real" of high schools) in fact blinded us to processes actually performed by the students that made it possible for the cliques to disappear on occasion. The work I criticized is work that is generally seen as falling within the broader boundaries of anthropology: work by Hollingshead, Henry, Coleman, etc. Whatever the final form of the published version of this work, it presented itself as having involved at least some "ethnography." It is work that has been useful. Collectively, it represented "what we know about life in high schools." It was what I had to build upon. How could I do this?

Within positivistic paradigms, it would seem necessary to "duplicate," to find the "same" school and to perform the same operations in order to check whether the "same" results were obtained. Anthropology, however, cannot use positivistic paradigms since these require a priori definitions of the parameters that define sameness both in the purported object and in the operations. If anthropology is in fact defined by its refusal to accept any other a priori than the need to learn through experience, then, of course, we cannot ever settle the question of sameness. What we can try to do is reconstruct the pragmatic operations of the researcher in order to account for the process which made someone like Coleman certain that "cliques" were so objectively real that they could be counted. What did he experience? What did he make of this experience, i.e., how did he transcribe it? What did he consider irrelevant? What did he do next?

I have sketched my analysis of this process elsewhere (1982).⁶ Methodologically, my suspicion is that Coleman was caught by the manner of his asking students about their interactions with peers. At those logical moments when statements became "data" for him, he could only get the kind of statements that identify groups as "cliques." I am convinced that Coleman also experienced those statements which I use to demonstrate the either/or effect of cliquishness. For him, these statements only came at moments when the statements could not become data, at moments when, so to speak, his camera was "turned off." My own analysis or, as it should be put, the analysis that the tradition assigns to "me," is the interactional product of what Coleman (and a very great number of others) have already

written about American schools, and my experiences in one school, all this within still broader contexts that define what research in schools should look like.

This analysis cannot be fully convincing since Coleman has not published the data one would need to conduct the comparative analysis: he gives us almost nothing of his fieldnotes. There is just enough for me to recognize that parts of my fieldnotes contain statements and observation very similar to the ones he quotes. There is also enough for me to be quite certain that, after half a century of research into American schools, we can say that the issue is not to decide whether there "are" or "are not" cliques in American schools in general (or whether they were "stronger" in Northern Illinois than in Sheffield, or stronger in the early 50's than in the early 70's). There is enough evidence that de Tocqueville experienced clique-like things during his voyages in the United States in the 1830's to be certain that they are an extremely general type of event linked to deep structural characteristics of American culture. The dissolving of cliques in certain rhetorical contexts is just as general a process.

Our task, then, is to provide an account of our practical operations in terms of someone else's operations. My analysis is relevant to Coleman's because I can assume that he experienced some of the same things I experienced and because I can also assume that we are both interested in what constrains behavior in American school. Neither he nor I control this interest: this interest is controlled by our own culture, a culture which requires that "scientists" look at schools within the United States in terms of each other. This culture expects us to find that things have "changed" between "then" and "now" (whenever that is) and to demonstrate "differences" between "here" and "there" (wherever that is). This cultural framework of definitions and expectations is, happily, open enough to allow for the making of statements that are, in fact, "new" within it. Our responsibility is to use this openness in such a way as to understand better what it means to live an American school, what it means to "act," to improvise the whole of one's everyday life within the constraints imposed on oneself by America and its schools.

FOOTNOTES

Acknowledgements: The research mentioned in the paper was supported in part by the National Institute of Education, the Ford Foundation and the Horace Mann-Lincoln Institute at Teachers College. The paper itself is a moment in the more restricted conversation (than the one mentioned in it) which I have been having with many good friends and colleagues, particularly Clifford Hill, George Spindler and above all Ray McDermott who read a late draft of the paper and helped me sharpen it. This paper should also be seen in tandem with another one (Varenne, forthcoming) that expands further upon my understanding of the conversational process at the cultural level. And if there is something of phenomenology in this paper, it has much to do with the conversations Susan, my wife, has gotten me into.

¹ It is important to realize that the field experience is precisely only a logical moment. It is constituted by the external conventions that tell us when to begin and when to end "observing." For example, the year I conducted the fieldwork on which American School Language (1983) is based, I was also teaching full time as an assistant professor. This was my first job after graduate school. None of my experiences from this part of my life got entered into the fieldnotes. And yet if, later, I was so taken by the plight of young, non-tenured high school teachers having to banter in the corridors with their principal, it cannot be unrelated to the fact that, at the same time, I also had to banter with the genial president of my institution... But that was not "data." It was "personal life."

² While Coleman's work is certainly far from "ethnographic" in the process of its transformation of an experience into reportable data, the phrasing of the results is in fact closely related to the kinds of phrasing that are found in ethnographic monographs. This is a challenge which anthropologists must take up.

³ I do not think there is any theoretical justification for the kind of common sensical statement Coleman made in the introduction to The Adolescent Society:

It is likely that there are more similarities between Executive Heights, a well-to-do midwestern suburb, and Scarsdale, New York, ... than there are between Executive Heights and Green Junction [a farm town or 5000]. The results of the study are ... intended to apply to all schools encompassed within the range of community composition exhibited by these schools (my emphasis. 1961: ix).

There are no more reason to generalize on the basis of "community composition" than there is to generalize on any other basis. The issue is not one of a priori generalization but of the knowledge that can be gained from comparing relative difference and sameness.

⁴ While there is much in this paper that can be related to Kuhn's seminal work on scientific paradigms ([1962] 1970), I want to go beyond his statements to emphasize the processual element in scientific thinking. It does make sense to talk of paradigms and revolutions, culture and moments of change. It must also be recognized that the revolution is, precisely, a revolution in that it is tied to the paradigm it is transforming. In this sense a revolution can be seen as a part of the preceding paradigm, some-

thing that it already contained in an incipient fashion.

⁵ There is something telling in the fact that, in the representation of reported speech, directness marked by quote marks always seem more "real" than indirect forms. It is as if:

- He said: "I am happy."

was closer to the primordial event than:

- He said that he was happy.

This may explain why it is easy to think that videotapes of events are better evidence for the organization of the event than monographic reports written according to the traditional conventions in anthropology. In fact the quote marks, and the presentation of visual material in the context of a telling, are rhetorical devices which are both very useful and dangerous in their blinding qualities.

⁶ In brief, I argued in that paper that cliques were complex symbolic performances upon sociological constraints. Given a large enough group that is not differentiated by status (e.g. "the senior class," or "the teachers"), it will necessarily happen that some of the people in this group will get to meet more regularly with each other than they meet with others of the larger group. Some of these smaller groups, in some settings, will be symbolically identified. The "insiders" will speak of this group as of an "open, loose group of friends." The "outsiders" will speak of it as of a "closed, tight clique." Both insiders and outsiders are aware of the dual possible identification of the event; both can only talk of clique in a symbolic distance: Cliques only exist in the past or in the someone else's present "they are a clique" ("though we both know that others see me as a member of this clique"). The difficulty, for social scientists is that they mostly engage the participants in situations where the "distancing" modes are normal (e.g. in interviews or questionnaire answering). Furthermore, the reports themselves are written in the same distancing mode as the participants used ("The people in the school... they"). In other words, most descriptions of schools produced by American social scientists are in fact little more than barely edited versions of the participants' own statements.

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