

THE ETHNOGRAPIC TRADITION IN SOCIAL SCIENCE: BETWEEN THE BOUNDARIES OF THE CLASSIC SCIENTIFIC IDEAL AND PERSONAL EXPERIENCE

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My focus in this article will be on the place and development of personal experience in the transition from fieldwork to the written text. I will discuss the boundaries between what I have defined as the classic scientific ideal and the position of personal experience within the various directions of social science that have examined field work as a central method for studying culture and ethnicity. I am thinking here of the ethnographic tradition within anthropology and some sectors of sociology. As a common denominator I would call this the ethnographic tradition in social science.

My starting point for an examination of the classic ethnographic tradition in social science is the tradition of textual criticism. This is a tradition that, in my opinion, has made a significant contribution toward reformulating fieldwork and the written literature in ethnography. An ethnographic text is based on fieldwork, but it is the written text that must represent this fieldwork. Thus, the text has a certain amount of independence from the fieldwork on which it is based. How the culture is portrayed in a text is partly independent of the manner in which the researcher became familiar with — came to know — that culture through a fieldwork project (Van Maanen: 4).

What, then, does the classic scientific ideal in ethnography entail, seen from the perspective of textual criticism? The study of culture and ethnicity as a scientific project had its beginnings in this century with, among others, the Polish-British anthropologist Bronislaw Malinowski. As a result of fieldwork that, for various extraneous reasons, lasted several years longer than anticipated, Malinowski (2002/1922) wrote an extensive monograph: "Argonauts of the Western Pacific." With Malinowski, personal experience through an extended stay in the field became a key element in data collection. The ethnographer had to live with and share subsistence with the ethnic groups he wished to describe. Thus, during the 1920s the trend in eth-

nography was toward more extensive fieldwork. Researchers who wanted to study foreign cultures had to leave the university and conduct fieldwork, which meant that they gained firsthand experience of the society or lifestyles they wished to describe later in monographs. This had not been the case before that time. The ethnographic method had consisted of travel descriptions, interviews, conversations and reports in accordance with various rigid patterns.

Toward the end of the 1920s American sociologists also turned to field work — personal experience in the field — as a suitable method for studying social phenomena at home in their own context. Many see what is called the "Chicago School of Urban Sociology" as the beginning of a tradition in sociological fieldwork. In this tradition, which Jules Henry has called "passionate ethnography," the near and, in a certain sense, known city was studied as if it were a remote and exotic place (Van Maanen: 17). Researchers were to be direct participants in groups and cultures and they were to look for the particular, the distinctive. Anthropologist John Van Maanen says of the Chicago School that it placed great importance on the difference between knowledge about something and acquaintance with the phenomenon. He went on to point out that, particularly in its early phase, the Chicago school eagerly supported a form of presentation that was similar in many ways to the genre of journalistic documentation. Social phenomena were frequently described in a straightforward manner, as if their significance were self-evident. Researchers were supposed to dig up data and use it to tell the "real story." Social facts were the events to which the field worker was eye- or ear-witness and which he or she wrote down or recorded on tape. The researcher was then supposed to present this data in a text. The facts were to speak for themselves through this text (Op.cit. 18 - 21). The important point here is that representation of a social world through a written text was not seen as problematic from the standpoint of methodology and scientific theory. In this tradition, which many call realistic ethnography (I will return later to this term), fieldwork, and the personal experience and involvement it entailed, was established as a key element of the scientific method. In the written monograph, however, descriptions and analyses of the epistemological (i.e. related to the theory of knowledge) significance of this experience were often lacking.

The classical realistic monograph is the most familiar, popular, and widespread form of ethnographic writing (Van Maanen: 45). What, then, characterizes realistic ethnography as text and what are the frameworks or conventions within which the researcher can develop as a writer? The author of the text is often presented in an explanatory foreword, along with the personal motivation behind his or her work. This is followed by a description of the arrival in the field, the frustration involved in learning the language and becoming accepted, and the feelings related to departure from the field. Textual analyst Mary Louise Pratt sees these conventional opening stories as an important part of realistic ethnography. She believes they play an important role by creating a link between the realistic narrative and what she calls the ethnographer's intense, authority-giving personal experience of field work." (1986: 32). The opening narrative gives the ethnographer the personal authority he or she needs in the realistic narrative. The author or narrator is then either totally absent or pushed far into the background in the main ethnographic narrative. Conventions relating to the textual composition and style were strict and there was little room for experimentation or for adapting the form of the monograph to the reality the text was intended to represent. The author's style must not be prominent. Clifford Geertz has discussed these relationships among text, form, style, and literary models in the classic ethnographic texts, for example, in his book "Works and Lives, the Anthropologist as Author" (1988). Still, classic realistic ethnography places its main focus on the subjects, on those who are studied, in their own distinctive cultural circle. It is what the research subjects say, do, and perhaps think, that is visible in the text. A view of cultures as separate entities, separated by cultural boundaries, is the main metaphor behind classic realistic ethnography.

The absence of the individual ethnographer from the text also indicates an implicit understanding that the conclusions presented in the text are independent of the field worker's personal background. What any trained field worker sees, hears, and writes down at a given place will be the same that any other field worker sees, hears, and writes down at that same place, given the same academic training (Van Maanen: 46). Thus, the realistic tradition in ethnography relies heavily on its institutional authority. It is the impersonal, analytical instrument of the profession and method that legitimizes access to a

culture and the possibility of research within it. In the realistic monograph it is the fieldwork, the personal experience in the field that is primarily responsible for establishing the authority of the ethnographer. But these same personal experiences are not presented in the text. The subjectivity of the researcher is separated out of the objectively descriptive style of the text; the subjective researcher is almost invisible or at least pushed far into the background in the written text. The researcher's experiences in the field are presented only in accordance with an inflexible convention: the story of the authority-establishing arrival, negotiation, incorporation, and departure. Fieldwork as a journey, in which the outsider comes to new places, makes himself known, experiences things, and then leaves as a trusted friend, is present as a main metaphor in the text itself. The narrative presented in the written monograph is clearly delimited with respect to the experiences in the field. Complications, strong emotions, episodes of violence or love, noteworthy slipups or failures, and crucial changes in course are basically excluded from the published text. John van Maanen has pointed out how ironic it is that authority is established by removing the first person, i.e. the field worker, from the ethnographic text. Thus, the realistic monograph is organized on the principle that it is the field worker's (and not the subjects') conceptual and disciplinary orientation that is crucial to what and how field material is presented. The text states with a matter-above-fact objectivity what the research subjects think, do, and believe. The fact that it is actually the researcher or ethnographer who sees, hears, thinks, and writes about the research subjects is not often brought to the fore in the realistic tradition. Thus, realistic ethnographies often exhibit an absolute certainty in their interpretations, since alternative perspectives are not presented and the author does not use such expressions as "in my opinion, from my standpoint, etc." The realistic text is closed off to alternative or competing perspectives. There is no room in the realistic text for uncertainty, ambiguity, or failures in the field. This has a disturbing effect on the reader and undermines the authority of the text (Op.cit 45-48). One corollary to the classic scientific ideal in ethnography is a view of culture as comprising well-ordered and structured units. Cultural spheres were described in dichotomies between the others and us. It was the scientific rationality of the Western world

that was in a position to describe and it was the colonies that were described. And the ethnographic researcher stood more or less outside the textual framework. But this world, the foreign culture, was objectified and, in a certain sense, "laid out" by the ethnographer-as-author to be read.

During the 1960s and 70s, the realistic tradition lost its absolute hegemony in ethnography. Ethnographers began to write their field experiences into the text in ways that broke with the classic norms. George Marcus has called the period following the break with realism as the only ethnographic textual form, as "an experimental moment." Attention is now directed toward ethnography as text, i.e. the rhetorical foundation for the production of the ethnographic text and the manner in which it is read (Marcus & Fisher 1988). James Clifford links the changes in the rules to what he calls the "game of ethnography" to the fall of the major colonial powers during the post-war period (Clifford & Marcus: 9-10). But, imperialistic and colonialist power and dominance relationships, both formal and informal, that had formed a fixed framework around ethnographic practice, were in a state of dissolution. This situation, Clifford says, with shifting ideological tendencies, changed rules and compromises, created pressure in which anthropology found it necessary to reposition itself with respect to the object of its investigation. Anthropologists no longer spoke with automatic authority on behalf of others who were defined as being incapable of speaking for themselves (Ibid.). It was no longer possible to paint cultures in still life. This development has helped accelerate the discussion on the possibilities and limitations of ethnography as the production of text. Historical studies on hegemonic patterns of thought, such as Foucault's studies on discursive practices, as well as various directions in textual criticism and sectors of feminism, point out that what appears as "real" in history and social science can always be seen and analyzed as expressions that are controlled and delimited by social codes and conventions (Ibid.). Reality is socially constructed, as are the social sciences. Rhetorical construction of the text is central to the construction of truths in social science and ethnography. This is not to say that ethnography is a fiction, that the boundaries between the novel and the ethnographic monograph are unnecessary. What I am saying is that in many social science departments the precise definitions of analytical terms that we need for

modeling or grasping social reality are given the highest priority, while textual construction, in which all social scientists are involved, is not subject to the same consideration

Where, then, is personal experience in this picture? First of all, what is personal experience, and what role does it play in fieldwork that is subject to the requirements of science? Back in the 1950s sociologist William Foote Whyte wrote concerning personal experience as related to the scientific method and field work, that any researcher who lives for any extended period of time in the society he is studying cannot prevent his personal life from becoming intertwined with his research. Like all other people, the researcher is a social animal and his basic personal needs must be covered if he is to function. According to Whyte (279-280), a real explanation of how the research was conducted must include a rather personal examination of the researcher's life during his time in the field. This is because the personal experience gained from living in a society is an indispensable help in analyzing and interpreting the data. Whyte points out that it is by living with both informants and the data that we begin to see patterns we have not seen before. Personal experience gained from fieldwork, together with systematic interpretation, is what brings these patterns to the fore. Since such a great part of the research process is dependent on personal experience and actually occurs on an unconscious plane, the research process itself will never be in harmony with formalized methods (Ibid).

In my subsequent discussion of the boundaries between personal experience and the scientific ideal, I will rely in part on my personal experience growing up in a mixed Sámi-Norwegian milieu in northern Norway. Personal experience with respect to Sámi culture and ethnicity will be different for a researcher who grew up in northern Norway and for one who did not grow up here. A researcher from the outside will not have the same personal experience from the historical context. Due to his different background experience, the researcher from the outside will see things from a different standpoint than the researcher from the inside. But neither can claim that his own view and his own perspective are more valid than those of the other. They will see different things and tell different stories, however. They must tell different stories in order to make their own experiences fit.

And they must be aware of this scientific theoretical condition, i.e. that they see with different eyes, from a different perspective. What specific problems related to scientific theory does the northern Norwegian social researcher face when studying the culture and ethnicity of northern Norway? The non-Sámi researcher from northern Norway is on both the outside and the inside when studying Sámi social relations. (Here I am making a distinction between Sámi/non-Sámi that may be too sharp since many people have a mixed background, but it may be tolerated for analytical purposes.) The researcher is outside the inner Sámi sphere (Eidheim 1971). He may be able to visit it, but will seldom achieve the full confidence of those on the inside. He differs in this regard from the Sámi researcher who is studying his own culture. But the northern Norwegian social researcher is on the inside in the sense that he has personal experience of the oppressive conditions to which Sámi society was subjected during the latest era of Norwegianization. For my own generation, the post-war generation, this means the 1950s and 60s. After all, this oppression affected Northlanders as well, but Sámi society was affected in the culture's most important medium; from 1898 up to 1962 Sámi children by law were not allowed to use their native tongue in schools.¹ In this context the northern Norwegian social researcher is on the inside. He experienced the concrete results of this policy. He may not have been aware of these conditions while growing up; the world was what it was and no one could do anything about it. As adults, however, we look back and what we see in retrospect today is a policy and social relationship that produced a great injustice in these regions against our fellow Sámi schoolmates, villagers, and neighbors. And in many ways — perhaps actively, perhaps passively — we all were a part of this power game.

Thus, when personal experience finds its way into a study on culture and ethnicity in northern Norway, it must stand out clearly from the historical background. However, experiences cannot be viewed as an objective description of a past time, but are given meaning by the context in which these experiences exist today. Researchers on both the inside and the outside cannot pretend that this historical background does not exist and expect to act solely in the role of researcher in a field situation. But while the outside researcher lacks per-

¹ This bill was called the "Wexelsen – bill", named after the Cabinet Minister for Schools when it was passed.

sonal experience with the concrete consequences of the Norwegianization policy, thus risking a slipup due to a shortage of credibility, the inside researcher must remain aware of his experiences and take them into account when meeting with his informants, and carefully examine his own understanding and reaction during this meeting. Neither the outside researcher nor the inside researcher can ask that the other recognize his precedence, but rather they are seeing this world from different perspectives and they must take this aspect of scientific theory into account when meeting with their informants.

For the northern Norwegian researcher this meeting, in a sense, is characterized by a feeling of guilt. This feeling of guilt stems from the policy of Norwegianization and one's own unawareness of the suppression of Sámi culture during one's youth. What does this guilt situation entail when non-Sámi researchers conduct fieldwork in regions where Sámi people live? Researchers must accept that their view of history, and their view on the position of Sámi society today and in the future will be challenged. The researcher must take a stand on the well-known question: which side are you on? Who do you work for? What are your motives? What will your research mean for us? (Becker 1971). The researcher must enter discussions with her or his informants on history, identity, origins, and power. Researchers must be willing to enter into a dialogue in a field situation - a dialogue not only concerning the rules to which importance is to be attached in that situation, but also a dialogue on what information is to be passed on by the researcher. The informants, those who are being studied, can no longer be seen as passive suppliers of information. They understand the importance research has and they are actively involved in deciding what parts of their own culture and history are to fall into the hands of the researcher and how this knowledge is to be communicated further. They want codetermination and participation in processes involving their own collective identity and history. They write and interpret their own history and identity. These negotiations can be seen as attempts to create symmetry or balance in an initially unbalanced relationship related to power between researcher and research subject. Power in research is closely linked to institutional relationships and researchers represent the institutional power. Through conversation or dialogue, however, this balance of power can be made

more visible and in the next turn re-negotiated. In fieldwork and in writing up the fieldwork as text, the researcher must remember that he himself is part of a historical context that controls the questions he asks and the approaches he uses to gain experience and understanding of an ethnic group. But he must also remember that the information he receives from his informants also comprises interpretations that, in the same fundamental way, are tied to a historical context. Thus, interpretations are always part of a historical context, but they also exist in a relational context in which informants and researcher interpret each other's interpretations.

Anthropologist Judith Okeley (1992) maintains that incorporating the personal experiences of both the researcher and the research subject politicizes both the fieldwork and the text. The underlying political themes that any ethnographer deals with become more readily visible. As I have shown with my examples from northern Norway, personal experiences are linked to the historical context, to relationships between researcher and informant, and to their understanding of these processes. Placing oneself and one's informants into the ethnographic text through personal narratives also means providing a personal frame of reference to a discussion of power and knowledge. This means that both researcher and informants must take into account the power network that was active, for example, through the policy of Norwegianization, a network that encompassed us all, and that worked through each one of us, whether we passively or actively supported this policy or whether we opposed it. Power's mode of operation in multicultural northern Norway is not only through institutions and various "agencies," it was — and is — written in each one of us. Power's mode of operation is engraved in our personal biography and it works even today through the roles we play and in the ways we shape these roles, even as researchers. Capturing in text the personal experiences of ourselves and of our informants also means an attempt to capture the way in which power operates on us.

How then can personal experience in the study of culture and ethnicity in northern Norway be expressed? What are the possibilities for textual representation? Classic ethnography in its traditional form is closed to personal experience. During the period between 1920 and 1970 personal experiences had to be presented under a pseudonym, such as Elenore Smith Bowen's (pseudonym of Laura Bohannen)

"Return to Laughter," (1954), or put in the drawer, such as Malinowski's diary, published in 1967 as "A Diary in the Strict Sense of the Term." The realistic monograph has been greatly reformed in the past thirty years, and the first person narrator can be placed more conspicuously into the text. Some new niches have also been established alongside the realistic monograph that provide an opening for personal experience. John van Maanen (1988) has divided them into two categories: the confessional tale and the impressionist tale. Although they both provide possibilities for personal experience, they have their limitations. The confessional tale has arisen in response to the pressure created by having to keep so much personal experience outside the realistic text. By now, this genre has almost become a tradition. Lengthy introductions or entire books now present the difficulties and personal trials the ethnographer has experienced in his field work: doubts concerning his own project, the frustration of being rejected by his informants, a guilty conscience, information that just slipped out, or trust, personal friendship, and informants as co-interpreters. Thus, this genre provides room for presenting personal experience, but this experience must be closely linked to the fieldwork as a process. In the impressionist tale, the ethnographer is featured more as author or narrator. The tale itself, with its inner drama, is given top billing. The researcher who tells the tale draws the readers into the empirical data and tries to recreate the field conditions in such a way that the readers feel they, too, were there. The analytical points and theoretical discussions are unobtrusive. In the impressionist tale the ethnographer does not rely on large, unified theoretical systems. The text is just as hesitant and open to differing opinions and different interpretations as the concrete, experienced reality it represents. Both these ethnographic textual genres provide a much greater opportunity for expressing personal experience. At its best, the text is characterized by detailed descriptions of the setting in which the fieldwork takes place. It contains the research subjects' own stories, dialogue and conversations with the informants hold a central position, the ethnographer as author or narrator is placed in the text, and the ethnographer explicitly describes feelings, reactions, and crucial events in the fieldwork.

It is implicit in my presentation of personal experience in research that one's own experience and the research method must be seen as having a reciprocal relationship. A text on culture and ethnicity in northern Norway based only on personal experience would surely not pass as a scientific text. But a text in which the research method takes precedence and all personal experience is lacking will be a text in which much will have to be read between the lines. It will be a poor text. A researcher in culture and ethnicity in northern Norway must know the method — the craft, but he must also know when to allow his personal experience to enter the text. Eventually, dealing with personal experience may become a part of the method. We simply need new tales from the boundaries between personal experience and scientific ideals.

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