LINGUISTIC ANTHROPOLOGY

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Ethnographic methods

This chapter and the next will present a critical review of the more common data-collection techniques and analytical procedures currently practiced by professional linguistic anthropologists.[1] With the exception of occasional references to practical questions, this chapter will emphasize the logic of research habits and procedures rather than the technical solutions needed to solve common research problems. In a few cases, I will briefly discuss what I consider some of the most innovative and interesting ways of documenting the role of communication in the constitution of culture. A more specific discussion of the practice of transcription will be done in chapter 5.

Linguistic anthropologists use traditional ethnographic methods such as participant-observation and work with native speakers to obtain local interpretive glosses of the communicative material they record. They also use elicitation techniques similar to those employed by typological linguists interested in grammatical patterns. Recently, these methods have been integrated with new forms of documentation of verbal practices developed in such fields as urban sociolinguistics, discourse analysis, and conversation analysis. The advent of new technologies for the electronic recording of sounds and actions has broadened the range of phenomena that can be studied, increased our analytical sophistication, and, at the same time, multiplied the number of technical, political, and moral problems that a fieldworker must confront. As we enter this new technological era, it is imperative to develop a discursive arena in which to examine the pros and cons of the new tools within a general discussion of methodology for the study of human communicative behavior.

4.1 Ethnography

If the goal of linguistic anthropology is the study of linguistic forms as constitutive elements of social life, researchers must have ways of connecting linguistic forms

[1] Within the related field of sociolinguistics, Stubbs (1983) is an introductory textbook to discourse analysis that is particularly sensitive to the methods used for collecting conversational data. See also Milroy (1987).

with particular cultural practices. Ethnography offers one valuable set of techniques for such a goal. For this reason, the integration of ethnography with other methods for the documentation of speech patterns is one of the most important distinguishing qualities of linguistic anthropologists as compared to other researchers interested in language or communication. In this section I will provide a brief discussion of the basic features of what constitutes an ethnographic inquiry and suggest ways in which such features can be an integral part of the study of language.[2]

4.1.1 What is an ethnography?

As a first approximation, we can say that an ethnography is the written description of the social organization, social activities, symbolic and material resources, and interpretive practices characteristic of a particular group of people. Such a description is typically produced by prolonged and direct participation in the social life of a community and implies two apparently contradictory qualities: (i) an ability to step back and distance oneself from one's own immediate, culturally biased reactions so to achieve an acceptable degree of "objectivity" and (ii) the propensity to achieve sufficient identification with or empathy for the members of the group in order to provide an insider's perspective – what anthropologists call "the **emic** view" (see section 6.3.2).

A few words should be said here about the use of the term "objectivity," which has been harshly criticized in recent writings about the ethnographic experience (Kondo 1986; Rosaldo 1989) and, more generally, in current debates in and about the social sciences (Manicas 1987). With respect to ethnography, the problems with the term "objectivity" arise from its identification with a form of positivistic writing that was meant to exclude the observer's subjective stance, including emotions, as well as political, moral, and theoretical attitudes. Such an exclusion, in its more extreme or "purest" form, is not only impossible to achieve, it is also a questionable goal, given that it would produce a very poor record of the ethnographer's experience (De Martino 1961). How would one be able to say what people are doing without at least a minimal identification with their point of view? One would end up saying things like "people squat on the floor, grab their food with their hands and bring it to their mouth – and this, they call 'eating'." As it is obvious from this example, rather than being "objective" and impartial, accounts of this kind can easily be read as implying a negative evaluation of local practices.

[2] What follows is by no means a full-scale introduction to ethnographic methods, but a brief discussion of what I consider some of the central issues pertaining to the process of practicing ethnography and producing ethnographic descriptions. For more informed descriptions of current ethnographic methods in cultural anthropology and related fields, see Agar (1980), Spradley (1980), Jackson (1987), as well as the critical appraisals in Clifford and Marcus (1986), Geertz (1988), Rosaldo (1989), Sanjek (1990a).

Equally implausible is a description that completely identifies with the native perspective and does not, in some fashion, reflect the researchers' perception of the described events, including their own sociohistorical awareness of pecularities (or, alternatively, predictability) of such events and hence their value for comparative purposes. What matters, however, is the attempt to control or put between brackets one's value judgment. Although this might be seen as a step that anthropologists share with phenomenological philosophers like Husserl and interpretivist sociologists like Weber, the practice of refraining from thinking the obvious is an important part of doing any kind of science. The problem, of course, is that it is not sufficient. A science of people, a human science, cannot but also exploit the researchers' ability to identify, empathize with the people they are studying. This implies that there exists in ethnography a certain playful element which consists of changing the familiar into the strange and, vice versa, the strange into the familiar (Spiro 1990) (see also section 2.1 on Hegel's notion of culture).

Given that there are different degrees of distance from or closeness to a given ethnographic reality, descriptive adequacy for most ethnographers lies somewhere in the middle. Geertz (1983) adopted the psychoanalytic contrast between "experience-near" and "experience-distant" to illustrate this point:

> An experience-near concept is, roughly, one that someone – a patient, a subject, in our case an informant – might himself naturally and effortlessly use to define what he or his fellows see, feel, think, imagine, and so on, and which he would readily understand when similarly applied by others. An experience-distant concept is one that specialists of one sort or another – an analyst, an experimenter, an ethnographer, even a priest or an ideologist – employ to forward their scientific, philosophical, or practical aims. "Love" is an experience-near concept, "object cathexis" is an experience-distant one. "Social stratification" and perhaps for most people in the world even "religion" (and certainly "religious system") are experience-distant; "caste" and "nirvana" are experience-near, at least for Hindus and Buddhists ... The real question ... is what roles the two sorts of concepts play in anthropological analysis. Or, more exactly, how, in each case, ought one to deploy them so as to produce an interpretation of the way a people lives which is neither imprisoned within their mental horizons, an ethnography of witchcraft as written by a witch, nor systematically deaf to the distinctive tonalities of their existence, an ethnography of witchcraft as written by a geometer.
>
> (Geertz 1983: 57)

The "balance" between being insensitive and turning into a witch is simply the realization that writing ethnography implies the understanding of several, sometimes contradictory, sometimes complementary points of view. A successful ethnography, then, is not a method of writing in which the observer assumes *one* perspective – whether "distant" or "near" –, but a style in which the researcher establishes a dialogue between different viewpoints and voices, including those of the people studied, of the ethnographer, and of his disciplinary and theoretical preferences. This is indeed the style of the best ethnographies we have. They are a composite of a number of viewpoints, including the observer's and the observed. They combine the sense of awe at what the ethnographer might see or notice for the first time with a genuine attempt at finding out how such practices are made "ordinary" for the participants – or, conversely, how something that is taken for granted by the ethnographer appears exceptional or incomprehensible to the people being studied.

What is, however, often missing in most ethnographies is an explicit discussion and documentation of the dialogical practices out of which descriptions are born. As Dennis Tedlock (1983) points out, despite the fact that most of what we learn in the field is the product of live dialogue – between ourselves and the "natives" as well as among the natives themselves –, one sees very little if anything of that dialogue in published ethnographic accounts. Tedlock's criticism of what he calls **analogical anthropology** and his proposal for a **dialogical anthropology** articulates the contribution of linguistic anthropological methods to the study of culture. Rather than replacing native discourse with the observer's monologic narrative (whether in the first or third person), as typical of analogical anthropology, dialogical anthropology promotes native talk to the position of prominence so as to give readers more direct access to how members represent their own actions as well as how they deal with fieldworkers and comply with their demands.[3] The practice of **transcription** (see chapter 5) and its embedding in ethnographic description is an essential element of this process as investigators make explicit the sources from which they derive their understanding of a given cultural phenomenon.

The criteria for identifying a community as suitable for an ethnography can be quite varied, including political, geographical, racial, theoretical, and methodological considerations. The complex of features required for thinking about a

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[3] "In the classic ethnography, the informants, collectively, speak occasional isolated words in a totally exotic language; in confessions or reflections, on the other hand, where contact between individuals and between cultures is an undeniable reality, informants are allowed occasional complete utterances, but these are likely to contain or even to consist entirely of words from contact languages. In any case, the dominant mode, even of the confession, is the monologue" (Tedlock 1983: 326).

number of individuals as forming a "community" also vary, ranging from shared living space to affiliation with the same political, religious, or educational institution. We have thus ethnographies of people who live or work in the same town, village, island, building, and factory, and ethnographies of those who spend a certain period of time together, such as the participants in a class, a political confrontation, a religious movement, a ceremonial exchange.

4.1.1.1 Studying people in communities

The initial assumption that the people studied form a "community" must be sustained by systematic observations. This means that ethnographers expect to find certain commonalities among the members of the group, certain shared or mutually intelligible habits, social activities, ways of interacting and interpreting social acts. Language is of course an important indication of membership in a community; variation in linguistic patterns such as a frequent switching between languages, dialects, or registers (see chapters 1 and 9) is an index of possible internal subdivision within the same community. In general, the focus on one group should not be seen as implying cultural homogeneity in the group. The more we study different societies and especially complex multiethnic, post-industrial societies like the US the more we realize that the homogeneous community where everyone speaks the same language (or dialect) and knows everything there is to know for daily survival is either a romantic idealization of small-scale societies or a collective construct that is at the heart of nationalism (Anderson 1991). Despite this recognition, however, ethnographers are still in constant search for **patterns**, that is, recurrent configurations in people's behaviors, descriptions, interpretive procedures, uses of natural resources, and production and handling of tools and artifacts. Whether or not an ethnographer will be attracted more by similarities than by differences among members of the community will be, in large part, determined by his theoretical preferences. This is why the notion of culture he adopts is so very important in the process of producing an ethnography. If the ethnographer assumes, following Wallace's (1961) suggestion, that a culture is an organization of diversity, she will look for the ways in which members are able to coordinate their actions and goals, *despite their differences* (see section 2.1.2). In other words, the ethnographic account will try to describe not only how a particular group of people are kept together by their similarities but also how they are united despite or on account of their differences. If, on the other hand, the ethnographer is oriented toward a view of culture as something shared more or less in equal measure by all members, he will concentrate on commonalities and will tend to ignore differences, claiming that they are irrelevant variations of a basic, underlying pattern.

Ethnographers assume that the information they need is somehow available

through particular types of data-collection techniques. In this sense, ethnographers do not differ from other human scientists, such as psychoanalysts, for instance, who believe that it is possible to arrive at hidden psychological conflicts through the examination of overt behavior such as oral narratives, drawings, or physical reactions. What differentiates ethnographers from other students of human conduct is that they try to come as close as ethically appropriate to their subjects' cultural experience (the American Anthropological Association has guidelines that can be consulted). Rather than acquiring knowledge of the reality they want to study from oral or written reports, ethnographers live for an extended period of time with the people whose way of life they want to understand, watching them work, eat, play, talk, laugh, cry, be angry, sad, happy, satisfied, frustrated. The **observation** of a particular community is not attained from a distant and safe point but by being in the middle of things, that is, by **participating** in as many social events as possible. It is this often difficult but necessary combination of modalities of being with others and observing them that is referred to as **participant-observation**, a building stone of anthropology's contribution to our understanding of human cultures (Malinowski 1935, vol. 2: 3–4).

In this sense, before being a product, that is, a written text, ethnography is an experience or a process (Agar 1980: 1). It is the experience of participating in the social life of a given group as a way of understanding how they constitute each other into a collectivity, what makes them at the same time unique and predictable.

As it becomes obvious from the exemplary anecdotes that ethnographers like to tell about their fieldwork, their experience is for them rich with meanings that go well beyond the satisfactory completion of the research project as originally envisioned. Fieldwork has important consequences for the ways in which a researcher will, from that point on, think about his work and, at a more personal level, his own personal life. For the apprentice, however, all of the talk about transformations and understanding is often too vague. For anyone who has never tried it before, it is difficult to imagine exactly how one engages in ethnographic work. The first questions anthropology students ask are about the kinds of phenomena they should look for once in the field. Answers such as "an ethnographer is interested in everything" or "anything can be the object of inquiry for an ethnographer, it depends on his or her interest" are not much help to the novice. Non-exhaustive but extensive lists like the one in table 4.1 might be more useful as a first approximation.

Table 4.1 *Topics of ethnographic study*

Ethnographers are interested in:

- what people do in their daily lives (e.g. the activities they engage in, how they are organized, by whom and for whom)
- what they make and use (artifacts)
- who controls access to goods (land products) and technologies
- what people know, think, feel
- how they communicate with one another
- how they make decisions (e.g. what is right or wrong, what is permissible, what is strange, unusual, what is true)
- how they classify objects, animals, people, natural and cultural phenomena
- how the division of labor is organized (across genders, ages, social classes, ranks, etc.)
- how the life of the family/household is organized, etc.

The general issue behind these themes is a concern with the **constitution of society and culture**. Ethnographers gather information in order to answer two basic questions: (1) how is social order constituted (created, managed, reproduced), that is, what makes this particular group of people a functioning unit of some sort? and (2) how do individuals make sense of their way of living, that is, how do they explain (to themselves first) why they live the way they do and differently from others (sometimes even their neighbors)?

In collecting information that might help them answer these questions, ethnographers are expected to respect analytical, methodological, as well as ethical standards that have been established over the years by a long series of documented individual experiences. Here are some of these rules as seen by British anthropologist Raymond Firth, one of Malinowski's most acclaimed successors:

> Over the last fifty years social anthropology has developed a fairly sensitive technique of fieldwork. Rules have been worked out for securing as accurate information as possible. The fieldworker is encouraged to have maximum contact with the people he is studying, as by living in their midst. He is expected to use the vernacular, not only to avoid the misconstructions of an interpreter, but to be able to reinforce his set questions with material picked up by listening to ordinary conversation between the people themselves. He is expected not to rely on single informants for all significant data, but to indulge in a thorough process of checking. The opinions he obtains from individuals are not to be taken as

objective statements of the social reality, but as reflections of the position and interests of the people who give them. Above all, generalizations about local institutions are not expected to be framed solely upon verbal data collected from informants, but to be backed up at every turn by the field-worker's own observations of the actual behaviour of the people. (Firth 1965: 3)

As revealed by this eloquent and succinct statement, a major preoccupation for ethnographers is the reliability of the information they collect. They must not only develop ways of ascertaining the accuracy of what people tell them but also ways of assuring their readers that their descriptions are accurate. This means that ethnographers have to deal with two types of interlocutors: the subjects of their studies and their future readers. The recognition of these two, often conflictual, allegiances unveils a profession that is constantly dealing with issues of "power, resistance, institutional constraints, and innovation" (Clifford 1986: 2) during fieldwork and after. There is no way of turning away from these questions and responsibilities. There are, however, ways of incorporating into the research and its public (re)presentation the tension created by the ethnographer's intrusion into the world of Others who (by definition) have different ideas and standards from the ethnographer's. This means that in addition to the issue of access (to people, resources, information), ethnographers have become sensitized to the question of their role in the community where they work. More and more have ethnographers become concerned with how they are perceived, what they are expected to do, and the extent to which their individual research agenda as well as their representation of such an agenda is the by-product of several, sometimes complementary, sometimes conflicting forces and allegiances.

4.1.2 *Ethnographers as cultural mediators*

Ethnographers thus have started to recognize that they operate as **cultural mediators** between two traditions: one established by their discipline and their particular theoretical orientation and the other represented by the people they study and live with, who have their own understanding of what the fieldworkers should be doing and how they should conduct themselves. In recent ethnographies, the role of members in influencing the ethnographer's research agenda has been made more explicit. Here is an example from the introductory chapter of Fred Myers's ethnography of the Pintupi, an Aboriginal people from the Western Desert in Australia:

> As Margaret Mead once said, anthropology has informants, not objects of study. People teach us. The condition of my living in Pintupi communities has always been my participation as a

"relative." Their acceptance has never been based on my research, which they have never been much interested in once they decided I was a friend (despite my sincere and lengthy attempts to explain my work). Rather, what they expect from me is my human commitment to them as fellow people. This condition has set the tone of my whole research. Since the Australian government's policy of "self-determination" began, the Pintupi have insisted that those who live in their communities must "help Aboriginal people."

Their willingness to provide me instruction in Pintupi culture has followed a similar course in making me part of their lives. The Pintupi I know have emphasized my learning through participation and have been reluctant to submit to the sort of "white room" formal sessions of inquiry of which, in frustration, I have occasionally dreamed. It is neither polite nor productive to ask a lot of questions. When individuals have sponsored me with their help, we have worked by my spending a day in participant-observation, waiting for opportune moments to ask questions. In this way I learned gradually to identify certain Pintupi symbolic constructs with realms of action, not just as objects of analysis, but also in making myself understood. My experience of Pintupi culture, then, conforms to Wittgenstein's dictum not to ask what a thing means, but to look to its use. (Myers 1986: 15)

As implied by Myers's remarks, being an ethnographer means first of all learning to look and to listen. While in the field, there are all kinds of interactions and transactions around us, the majority of which is (fortunately) not just caused by our mere presence. In order to describe these interactions, we must first learn to recognize them as of the same "kind." This means that the repetitiveness of everyday life is a crucial element in our ability to learn to detect patterns. As participant-observers, we acquire expectations and learn to make predictions about what a given act (including words) produces and where or how it might have originated. In the process of learning to make these predictions, we must locate ourselves in time and space. We must choose *where* to sit (or stand) and *when* to be present. Such choices are not without consequences. We know this and, as Myers reminds us, the members of the group we study know it too. People often have strong ideas about where an outsider/visitor/guest (plus or minus other identities we might have acquired during our stay) should be and what he or she should be doing. They also have strong ideas about which public persona should be presented to the fieldworker. For these reasons, fieldwork is nothing but a long series of negotiations and compromises between our expectations and stan-

dards and those of our hosts. An emblematic example of such negotiations is provided by Elinor Ochs in the introduction to her ethnographic study of language acquisition and socialization in Western Samoa:

When I first began recording Samoan children and their caregivers in the summer of 1978, I encountered a serious methodological problem. Instead of engaging in the usual range of everyday household activities and interactions, the children would sit very properly on mats near my own mat and either wait for me to tell them what to do or perform at the command of an older sibling, parent, or other relative. Worse for the poor researcher, instead of conversing in the register typical of most social interactions in the village (the register Samoans call "bad speech"), caregivers and children appeared to use only the register Samoans call "good speech," characteristic of written Samoan and of Samoan spoken in school, church, and certain business settings and to foreigners who know Samoan. "Please," I would say over and over to members of the household, "just go on doing what you usually do and do not pay attention to me." I hoped somehow that this formula would magically create the context for the "spontaneous" talk of children and caregivers that is characteristic of longitudinal studies of child language in other societies. How else would I be able to bring back "comparable" data? The failure of my magic and the prospect of loss of face in the world of developmental research led me to a full-scale analysis of the basis of this problem. (Ochs 1988: 1)

Ochs's solution to her problem was to readjust her intellectual focus and reframe her interest in language development within a larger setting that included, among other features, the social organization of space in a Samoan household. In her case, the behavior of the children and adults she was observing and recording forced her to reconsider not only the effect that her presence in the house might have but also the boundaries of her analytical framework. If, as she discovered, people's verbal behavior changes in different parts of the house and depends on where the researcher is sitting, the very notion of "language" as the object of inquiry must be reconsidered to include in its scope the interplay between sounds and spatial orientation, speech acts and bodily acts (see chapters 3 and 6).

Myers's and Ochs's experiences illustrate how the process of ethnography always involves ways of learning from the people one studies (Spradley 1980: 1). This learning is often seen as part of the ethnographer's strategy "to grasp the native's point of view, his relation to life, to realize *his* vision of *his* world,"

according to Malinowski's now classic definition of the goal of ethnography (1922: 25). But this view is only partly accurate. In the Malinowskian tradition, the ethnographer is portrayed as a novice, treated by the natives as a grown-up child who still needs attending as well as constant reminding of what is appropriate and what is inappropriate in any given situation. Ethnographers routinely sustain this perception by putting themselves in situations in which they are clearly incapable of competent behavior. This is done sometimes unknowingly and other times strategically, to see how people react to one's blunders, given that error-corrections may offer an opportunity to hear explicit definitions of social norms and rules of etiquette.

Beyond the representation of ethnographers as naughty children or culturally impaired adults lie other sometimes complementary sometimes contrasting realities. Ethnographers' relationships with the people they study are by no means simply those of subordinate novices to superordinate experts. Their humility to be detected in some of their attitudes is part of a professional posture that, whether or not subjectively intentional, is expected to pay off in the long run. The ethnographer's interest in people's lives and their problems is often similar to the lawyer's interest in his clients' complaints and the therapist's interest in his patients' conflicts. It is sympathetic but detached. In listening to people's stories, especially the more dramatic ones, the ethnographer's interest is often not only for the tellers and their personal drama, but for the plot behind their stories, not for the individuals involved in those dramas but for the dramatis personae they represent, not necessarily for the ways in which a conflict might be resolved but for the logic implicit in that conflict. In their conversations with their subjects, ethnographers have an awareness of professional goals that projects them beyond the here-and-now and into the realm of academic writing and professional quests. This does not mean that real interest in human dramas or real friendships is not there to start with or cannot develop during or after the field-work experience. It just means that as ethnographers we cannot pretend to be what we are not: one of "them." There is a need for honesty with others as well as with ourselves in terms of our very special forms of participation in people's lives and dramas. As suggested by Narayan (1993: 672), "what we must focus our attention on is the quality of relations with the people we seek to represent in our texts: are they viewed as mere fodder for professionally self-serving statements about a generalized Other, or are they accepted as subjects with voices, views, and dilemmas – people to whom we are bonded through ties of reciprocity and who may even be critical of our professional enterprise?"

The view of the ethnographer as the child-novice is inaccurate because ethnographers are professional adults who usually come from powerful foreign nations and institutions that have economic and military superiority over the people

they are studying. These researchers act and are usually perceived as wealthy and powerful individuals who have only a temporary and in many respects very limited interest in the community they study and live in. Beyond ethnographers' intentions, motivations, or awareness, there are political and global processes that enter into the relationships they establish in the field. Anthropologists have just started to investigate these relationships and their potential and actual consequences, especially now that a new generation of ethnographers have gone to study their own community or the community of their parents (e.g. Abu-Lughod 1991; Appadurai 1991; Kondo 1990; Mani 1990; Narayan 1993; Said 1989). At the same time, one should not overestimate the power of researchers over their subjects or informants. As pointed out by Harvey (1992: 75), "the relationship between researcher and researched cannot be depicted as a straightforward hierarchical one in which the researcher simply imposes an agenda." It is simply patronizing or racist to think of the people we study as innocent victims of our own academic and scientific plans. They have their own ideas, plans, and goals. We must fit into their lives just like they need to fit into ours.

The view of ethnographers as cultural mediators emphasizes the fact that no matter how "close" or "distant" ethnographers act, feel, or think, their interpretations as well as their actions are always embedded in larger processes and more complex dialogues. Part of the work done in and through ethnography must thus include an understanding of such dialogues, regardless of the extent to which individual researchers decide to devote their research and publications to a discussion of such an interpretive process. Just as it would be naive to characterize ethnography as always a genuine and selfishless quest for knowledge, it would also be misleading to see it as an unavoidable and unmediated act of domination where ethnographers and the people they study simply act as puppets on the stage of a human theater totally controlled by more powerful and hidden agents. An ethnography is an interpretive act and as such should be turned on itself to increase the richness of descriptions, including an understanding of the conditions under which description itself becomes possible. Linguistic anthropologists' contribution to the ongoing definition of ethnography, its goals, conditions, and outcomes is an emphasis on the need to let our subjects speak, as much as possible, with their voices and their bodies, to tell the stories they normally tell in their daily life. The process of transcription discussed in the next chapter must be understood in the context of such an enterprise.

4.1.3 How comprehensive should an ethnography be? Complementarity and collaboration in ethnographic research

When Malinowski started to promote ethnography in its modern sense, that is, as participant-observation, he was thinking of ethnographies as total,

comprehensive accounts of a given people. The ethnographer had one or two years to become acquainted with the language spoken in the community and (at the same time!) describe every possible aspect of social life and material and symbolic culture he or she could document.

> An Ethnographer who sets out to study only religion, or only technology, or only social organisation cuts out an artificial field for inquiry, and he will be seriously handicapped in his work.
>
> (Malinowski 1922: 11)

This condemnation of partial descriptions and endorsement of *total* ethnographies produced some remarkable accounts but also well-known oversimplifications. There were always certain aspects of the culture that were either ignored or taken for granted, sometimes with the assumption that they were either fairly straightforward or in no need of special investigation. Language was often one of those cultural aspects placed in this residual category. Ethnographers could not do without it, but they would rarely give it the necessary systematic attention. It was an instrument for other, theoretically more important topics such as the social organization, the kinship system, and, in some cases, the interpretation of myths and legends. The sixth edition of the *Notes and Queries on Anthropology* produced by the Royal Anthropological Institute of Great Britain and Ireland (1951), for instance, dedicates a chapter to "Language"; its best advice to the prospective ethnographer is either to get hold of already existing linguistic descriptions or get trained in linguistics. In eleven pages, the reader is introduced to gestures, sign language and spoken language, including sections on phonology, grammar, and semantics. The next chapter is on material culture, which occupies one hundred and eighteen pages!

Contemporary anthropologists have come to accept the fact that one person cannot cover the culture of a group *in all its aspects*, as originally prescribed by Malinowski (1922), and that different researchers will emphasize different aspects, according to their expertise and theoretical interest. We have now ethnographies of particular groups (e.g. weavers, tailors, drug addicts, doctors), activities (classroom interaction, musical performances, spirit possession, rites of passage), events (trials, political meetings, marriage ceremonies, gift exchanges), and social processes (socialization, acculturation, hospitalization, marginalization, institutionalization of certain practices). The ethnographic description of languages is no exception. Linguistic anthropologists adopt ethnographic methods to concentrate on the ways in which linguistic communication is an integral part of the culture of the groups they study. While participating in the broader social life of a community, the linguistic anthropologist documents communicative behaviors across a range of interactions (including casual conversation, political

and ceremonial events, theatrical representations, singing, mourning) and among particular groups of people (women, men, children, chiefs, commoners, priests, orators, doctors, etc.). Through the selection and classification of social activities on the basis of language use, linguistic anthropologists are able to produce more accurate accounts of language structure and use than those provided by cultural anthropologists with only limited training in linguistic methods and models.

The danger of a too restricted understanding of the social life of a community – a danger seen mostly through the lenses of verbal codes and verbal performances – must be compensated for by relying on direct or indirect collaboration with other researchers, who may be studying the same group with different research foci. Such collaboration has produced some of the best linguistic anthropological studies of the last few decades. For instance, Bambi B. Schieffelin's (1990) ethnographically informed study of language socialization among the Kaluli people of Mount Bosavi in Papua New Guinea and Steven Feld's (1982) portrait of the interpenetration of sounds, feelings, and social relations among the same people clearly benefited from each other. Furthermore, they both crucially relied on Edward Schieffelin's (1976) earlier work on the cultural organization of sentiments (anger and appeal in particular) in the same community. Genevieve Calame-Griaule's (1965) much celebrated study of the linguistic ideology of the Dogon (in Mali) was made possible by the massive number of previous ethnographic studies, including the seventy or so publications by her father, French anthropologist Marcel Griaule. His work provided a solid foundation on which she could present a complex series of hypotheses about how language works both as a metaphor and a connecting element in the Dogon cosmology and philosophy of everyday life.

These projects, among others, have shown us that the image of the lone fieldworker traveling to a foreign land never visited by an anthropologist before and then writing single-authored papers and monographs is an anachronism, perhaps nothing more than a mixture of romantic humanistic ideals with methodological solipsism.

The criticism of isolated projects or the praising of collaboration should not be interpreted as an imperative to write only co-authored papers and open up all one's notebooks and files for anyone to see; there are still many issues, including those of privacy and the protection of the people who allowed us to witness their daily lives, that need to be reckoned with. But an increased awareness of the dialogic nature of any epistemological search is certainly in the air, accompanied by a renewed sense of the importance of the connection between knowledge and power, access and responsibility. As a new generation of students from a wide range of ethnic, racial, and national backgrounds enters the western academic arena, our descriptions are bound to be affected; our discourse of the Other will

never be the same. The grandchildren of the "primitives" described by the founding fathers (Boas, Malinowski, Radcliffe-Brown) and mothers (Benedict, Mead, E. C. Parsons) of anthropology are not just reading our books, they are also sitting in our classes, assessing our descriptions, and, hopefully, getting trained to ask new questions and propose new answers. Authorship and cooperation are bound to have a new meaning in future ethnographic works. These issues have been to a great extent brought to light by the contributions of feminist anthropologists who have forced anthropologists and other social scientists to deal with the gendered nature of so-called objective accounts and with the situatedness of any ethnographic description (Haraway 1991; Harding 1986; Spivak 1985).

4.2   Two kinds of field linguistics

Linguistic anthropologists are not the only ones who travel long distances to go and live within a community of speakers with the goal of describing their language. Linguists have been doing it for a long time as well and field methods courses are an important part of any linguist's training, at least in the United States. There are however, some important differences between the ways in which linguistic anthropologists and most linguists work in the field. The practice of ethnography I just outlined is one such difference. For linguists exclusively or mostly interested in grammar, the reason to travel to a distant location and live within a community of speakers is usually to have the luxury of virtually unlimited access to speakers of different ages, gender, and social status, who can provide a much more reliable and varied data base than the one produced by meeting with one or two native speakers in a research office inside the walls of a western academic institution. Although they may occasionally participate in the life of the community, being on site is not seen by most field linguists as an occasion to capture speakers' use of the language with one another. Instead, the field experience is an occasion to train a number of native speakers to become **language consultants**, who learn to use their **intuitions** to provide judgments of acceptability of different grammatical forms. "Can you say – ?" the linguist says; the native speaker's reaction to the proposed expression is noted down and the next structure is presented, "How about – ?", followed by a series of other questions: Which one is better? What's wrong with this one? How would you say it instead? And so on. These techniques are important for uncovering regularities in the linguistic system and for getting access to forms that might not be very common in everyday usage. At the same time the exclusive use of such methods systematically avoids getting into what makes language a social institution and a cultural practice. Linguistic anthropologists, on the other hand, make extensive audio and video recording of everyday encounters. These forms of documentation are comple-

mented by participant-observation and a number of related field techniques for the study of verbal performance, including ethnographic notes, drawings, maps, interviews, and still photography. Such techniques are used with the aim of revealing local verbal practices as well as local conceptualizations of such practices and their place in the social organization of the community (see table 4.2).

Table 4.2 *What linguistic anthropologists are interested in*

- The basic organization of the relationship between sounds and meanings as revealed by actual language use in a variety of social activities and (if grammatical descriptions are already available) the extent to which previous grammatical descriptions reflect actual language use or only special, e.g. literacy-bound, uses
- local conceptualizations of what constitutes "language," including characterizations of newborns' and outsiders' speech
- the spatial distribution of language uses (e.g. is there a central place for public verbal performance, like the *marae* in Ancient Polynesian societies or the "gathering house" among the Kuna? Are there differences in the ways language is used in different parts of a house?)
- the features and cultural significance of what is understood as ritual or ceremonial language vis-à-vis everyday speech
- the social distribution of different styles, genres, and speech events (e.g. what are the ways in which different social groups mark themselves off through special linguistic registers or verbal performances?)
- the extent to which local theories of language structure and language use relate to local cosmologies
- the role of language socialization in the shaping of notions of person, mind, and social relations
- the interpenetration of different codes (e.g. speech, gestures, clothes) in the constitution of messages and their interpretation.

The general theme behind these questions is the different ways in which language as an abstract system of classification (of the natural and cultural worlds) and as a mode of social interaction provides the material out of which a group of people recognize themselves as a community.

4.3   Participant-observation

There are different modes of participant-observation, from *passive participation*, in which the ethnographer tries to be as unintrusive as possible to *complete participation*, in which researchers intensively interact with other participants and might even get to participate in and perform the very activity they are studying (Gold 1969; Spradley 1980: 58–62; Williamson et al. 1982: ch. 8). In the case of

linguistic fieldwork, complete participation means being able to interact competently in the native language and even perform the verbal genres one is studying. This might not necessarily be a voluntary choice by the researcher. In Samoa, for instance, when I was sitting on the side of the house where orators sit, I would be expected to perform if the occasion arose. Local experts acted as teachers, advisers, and sympathetic supporters. The expectation that I should speak competently in public was not due to my declared interest in language and verbal art but to my acquired social identity as "chief" and spokesperson (Duranti 1994a: 23). Being the only adult male in our research team,[4] I was the most suitable candidate for verbally representing what was considered my "extended family." Any time someone would address our group with a ceremonial speech, the other participants would turn toward me, their faces conveying the expectation that I would speak next. In these situations, it was much more difficult for me to keep track of what was going on around me, run a tape recorder, or have time to scribble down notes. At the same time, these experiences gave me insights into the pathos of performance that I could have never gained from observation or interviews.

Complete participation, when possible and ethically appropriate, gives researchers a great opportunity to directly experience the very processes they are trying to document. Though it is by no means equivalent to entering the mind and body of a native speaker, performing gives a researcher important insights into what it means to be a participant in a given situation and suggests hypotheses and further questions. The epiphany produced by entering the activity one is studying is well captured by Feld's recounting of his experience of among the Kaluli:

> While there were many things I was able to understand about
> Kaluli ideals of sound expression as a result of traditional
> participant observation, I don't think I really began to feel many of
> the most important issues, like … the construction of a song climax,
> until the day I composed a song about [E. Schieffelin] and Bambi
> [Schieffelin]'s leaving Bosavi that brought tears to the eyes of
> Gigio, one of their oldest and closest friends. I wept, too, and in
> that intense, momentary, witnessing experience, I felt the first
> emotional sensation of what it might be like to inhabit that aesthetic
> reality where such feelings are at the very core of being human.
> (Feld 1982: 236–7).

[4] This does not mean that women in Samoa never deliver ceremonial speeches or engage in complex negotiations; I have encountered and heard very gifted female orators. There is however a strong preference for men, especially titled ones (*matai*), to be the spokespersons on most occasions. This preference does not apply to activities that are organized and run by women.

At the same time, the preoccupation with one's own performance implies an attention to one's role and one's perception by others that can be very absorbing and, from the point of view of documenting what is going on, extremely distracting. For this reason, ethnographers must often restrain themselves from being complete participants. They learn to assume the strange status of accepted **bystanders** or professional **overhearers** (see also section 9.3.2). This sometimes implies finding what amounts to a **blind spot** in the scene, that is, the least intrusive place where to sit or stand. For Ochs studying Samoan children's language, this meant sitting in what is considered the "back" region of the house, where she would *not* be treated with all the honors of high status guests (see section 9.5). For anyone studying the order of servings in a ceremony, the blind spot is the place where one would not be served. For someone recording a conversation, the blind spot is the place where the participants would not feel obliged to include him. For an ethnographer studying a classroom, the blind spot might be a seat where one would not be in the continuous visual gaze of the majority of the students; one would want to stay away from the boards on which teachers write or the place where children stand to give oral presentations. In general, it is much harder to find the right place to be in more informal and intimate settings than in public, formal ones. Participant-observation inside a house occupied by a large family might be one of the biggest challenges an ethnographer might encounter. Leichter provides a striking portrait of the problems encountered in such situations in her description of an observer's dilemmas in trying to learn about a family's literacy practices:

> On entering a home with the intention of learning how the family
> handles literacy, the observer is immediately faced with such
> practical problems as where to sit or stand, what areas of the home
> to attempt to observe, and which family members to watch and talk
> with. Even with a focus as definite as television-viewing, the
> observer is faced with numerous decisions about how to focus
> observations. Sitting beside family members while they watch
> television, for example, makes it impossible to observe their eye-
> gaze direction. Since more than one activity is generally going on
> simultaneously in most households, the observer must continually
> face the question of where to focus his or her attention. These
> decisions are made more difficult by the realization that watching
> one activity frequently means missing another. (Leichter 1984: 43)

In addition to finding the appropriate place, researchers must also find the right demeanor for a given place. Sometimes this means that they must be immobile so as not to draw attention; other times, it means that they have to keep busy.

For instance, one might be writing notes on a book or attending to some object or tool (the tape recorder, the camera) that requires one's unconditional attention.

The underlying rationale for finding the blind spot and trying to be as unintrusive as possible is not to pretend that one is not there, but to get as close as possible to what it is like to be a marginal participant. While it is not ethically appropriate and practically feasible to completely hide one's presence, at the same time it is very limiting to collect data solely on participants' response to *our* presence on the scene. Although such data have been shown to be instructive (Duranti 1990; Haviland 1986, 1991; Howe and Sherzer 1986), they should not constitute the bulk of our corpus.

There are also times when the most appropriate behavior is to accept being treated as a guest or being the center of attention (this is especially true during the first days in a community or the first few visits to a particular site). For this reason, there are no absolute rules about how one should conduct oneself while engaged in participant-observation. Questions of social sensibility must determine in each case what is the most appropriate response to our hosts' expectations. This is an area where mistakes are common, often unavoidable, but usually not fatal, although there have been cases in which the disrespectful behavior of earlier ethnographers has caused a ban on any future research. A guiding general principle here is that respect for our hosts' sensitivity should always override our desire for "good" data and the thrill of documenting something exemplary for our research goals.

Overall, it is safe to say that a variety of modes of participation is necessary for a rich description of any event or social situation. This means that ethnographers must routinely alternate between moments of high involvement and moments of low involvement in the activities that surround them.

4.4 Interviews

Interviews, in the loose sense of the term, are a common form of interaction during fieldwork. Ethnographers are continuously asking questions and many of the questions they ask are about topics and issues they are trying to make sense of. In this sense, ethnographers' questions are never as naive or as useless as they might sound, given that any answer, even what might appear the most guarded or the least informative, might be quite informative for the researcher – if not at the time, sometimes later. There are however specific times when the researcher sits down (often with a note pad in his hands or the tape recorder running) and presents a series of more or less structured, partly preplanned questions to a member of the community who is believed to be particularly knowledgeable about a specific area of expertise. For linguistic anthropologists, the interview might be a time to obtain background cultural information that is crucial for

understanding particular speech exchanges they are studying. For some researchers who follow sociolinguistic methods (Labov 1972a, 1972b), the interview might be an occasion for getting a linguistic corpus for studying grammatical forms, stylistic variations, and attitudes toward the language (Hill and Hill 1986). In these cases, the linguist is not looking for "experts" but simply for "speakers" and one of the main concerns is how the speech produced during the interview is representative of the speaker's usage. Such a concern is part of a more general issue regarding the appropriateness of the interview situation for gaining access to local knowledge and local communicative practices. For William Labov (1984: 29), for instance,

> *[f]ace-to-face interviews are the only means of obtaining the volume and quality of recorded speech that is needed for quantitative analysis.* (italics in the original)

Most linguistic anthropologists do not agree with this general principle and believe that, although at times useful, interviews can rarely provide the richness of information needed for a culturally informed linguistic analysis. There is no substitute for the observation and recording of actual interactions among native speakers involved in everyday tasks, whether private and mundane or public and institutionally oriented. Presently available audio and video technologies allow for a high level of accuracy even when speakers are not speaking directly into a microphone while sitting in a quiet environment in front of the researcher. When interviews are considered necessary or unavoidable, a number of caveats must be kept in mind in order to know what to expect and how to handle an interview situation.

4.4.1 The cultural ecology of interviews

Reactions to the researcher's questions will vary, depending on a number of factors, including the extent to which the interview format fits into local practices of obtaining information (see below) or the nature of the topics discussed. Questions might be directed to a domain of knowledge that is recognized as valuable in the culture, as it is typically the case for public speaking and certain kinds of specialized (sometimes esoteric) knowledge (medicine, magic, genealogies), or an area that may not be seen as a worthy domain of expertise such as, for instance, activities involving children (e.g. verbal games, children's songs, socialization routines, speech errors made by children).

In some communities, access to certain topics and events might simply be forbidden to an outsider. This is known to be the case with Australian aborigines' rituals pertaining to the Dreaming and with some Native American religious ceremonies. When fieldworkers are allowed to participate in or witness what is

considered a sacred ceremony with limited access (e.g. only for adults or only for initiated males), they must be very careful not to violate the trust that has been placed in them. Any reporting about such events must be weighed carefully and negotiated with members of the community.

✔ Fieldworkers must be aware of the fact that each community has its own ways of conceptualizing what an "interview" is. When, as is often the case, a culture does not have such a speech event in its repertoire, local notions of giving out information or learning must be taken into consideration for understanding members' reactions to the researcher's attempt to conduct an interview. In Madagascar, for instance, as reported by Elinor Ochs Keenan (1974; 1976), information is considered a *scarce good* and people are reluctant to provide both insiders and outsiders with what might be considered "news." Like in many other societies in the world, genealogies are often jealously protected and the fieldworkers who are interested in them might have to wait months or years before finding anyone willing to discuss the subject in some detail. In Samoa, it is not appropriate to ask questions about people's personal motivations. Questions like "why did he do it?" for instance often produce either a standard generic refusal to commit oneself (*ta'ilo* "[how would poor] me know?") or in cases of deviant behavior, "(he was) drunk" (*ōnā*) – an answer that does not presuppose factual knowledge about the alcohol intake of the person spoken of. Any further inquiry is not likely to produce many more details or insights. Not only do Samoans not like to venture into psychological explanations or speculations about individuals' inner states of mind, but the request to engage in such interpretive practices by the researcher can be seen as inappropriate and even dangerous. For instance, the reconstruction of past events to be presented as causally linked to a present crisis can reopen old wounds and get people emotionally drained. This is made clear in formal occasions such as the village council (*fono*) where participants are urged to look forward rather than to reintroduce into the discussion conflicts that happened in the past and were considered resolved (Duranti 1994a: 97).

One should also never forget that getting information out of people might leave them with the feeling that something precious is being taken away. Paying someone an informant's fee might not be sufficient for compensating the sense of loss an individual might experience when something he might have mentioned in a moment of intimacy or as a gesture of friendship toward the fieldworker is turned into a piece of data to be potentially shared by thousands of people around the world.

Researchers also need to study the local **ecology of questioning**. In other words, fieldworkers need to find out who is allowed to question who, when, and how. In western societies questioning is expected and permitted during the early stages of the learning process (especially in the context of school activities) but in many places in the world asking questions is not seen as an appropriate activity for a novice. In many societies, novices are expected to observe and imitate what experts do rather than bothering them with questions (Lave 1990; Rogoff 1988). Thus, when Charles Briggs tried to learn about carving in a Mexicano community in Northern New Mexico by using interviews, he was faced with all kinds of "procedural problems" (1986: 43). People either did not answer his questions directly or provided very limited or apparently contradictory information. Fortunately Briggs recorded his attempts over time and from a careful study of his own questions and his consultants' answers, he gained a new understanding of the process of interviewing that can help other researchers who might find themselves in similar situations.

> This material provides insight into some of the communicative blunders I committed in research with Mexicanos ... I simply assumed that a knowledge of Standard Spanish, a research project that proved acceptable to the couple and their community, and the development of a friendship would enable me to begin interviewing. I similarly believed that interviews would provide the best means of gaining social-cultural and sociolinguistic competence ... Because I was ignorant of the community's oral traditions and lacked command of any of the requisite pragmatic skills, the elders had no choice but to regain control of the interaction by breaking the interview frame. (Briggs 1986: 64)

Briggs discovered that in order for him to learn about carving and tradition, he would have to enter the role of a traditional apprentice. His hosts' preferred mode of instruction was to hand him a piece of wood and a penknife and help him learn how to carve. Only in that context was Briggs able to obtain more detailed information on the carvings and their socio-cultural meaning.

> I then found myself in the position of being able to gain additional information by repeating one of their statements, followed by a tag question: "So your father used to be a great joker, did he?" Thus, once I had grasped the appropriate means of learning and had gained a minimal level of competence, the Lópezes were quite willing to provide me with information on the carving art. Fortunately, the couple allowed me to turn on my tape recorder at such times. This not only provided a wealth of background noise for my initial recordings, but it provided me with data on the way the Lópezes were teaching me to learn. (Briggs 1986: 65)

As this passages indicates, one needs a considerable level of analytical sophistication to detect from the transcripts of the interviews both where miscommunication occurs and which specific linguistic mechanisms are being used by the interviewer and interviewees to convey the respective understanding of the event.

4.4.2 Different kinds of interviews

Although linguistic anthropologists tend to rely on spoken interviews rather than on interviews based on written questionnaires, they do prepare written material to plan and guide their oral interviews with a member of the community. In such contexts it is important to gain an understanding of the local implications associated with using and producing written records. Depending on the history of the community, members may have distrustful attitudes toward interactions and documents that may have socioeconomic or legal implications (e.g. filling out a form). The same considerations apply to taking notes and audio or video taping while talking to people (see below).

Different considerations apply depending on whether one is conducting a few occasional interviews or numerous interviews that are expected to produce comparable data. Urban sociolinguists have developed several methods for collecting dozens or even hundreds of structured interviews. One of these methods is a **standardized questionnaire**. It is designed for use by different fieldworkers and can be adapted to a variety of situations, including subjects' different class or ethnic background. Shuy, Wolfram, and Riley (1968) used standardized questionnaires in their Detroit Dialect Study, which was developed to guide educational policies by surveying the various English speaking subcultures of the city. Fieldworkers dealt with approximately 700 speakers, of four age groups from a wide variety of social and ethnic backgrounds. Despite the researchers' commitment to the notion that "[t]he informality of the interview was a crucial factor in obtaining data on casual speech" (p. 40), the requirement of high quality sound needed for phonetic analysis produced what for most linguistic anthropologists would be considered a very formal context:

> The framework of the interview was simple and standardized. The fieldworker would hook up the microphone around the informant's neck, start the tape, which had already been threaded onto the machine, and ask the informant to give his name and count to ten. This gave a recited list, one of the more formal styles we wished to obtain, and served as a further identification on the tape in case it should be mixed up with others. The fieldworker would then proceed with Parts I through IV of the questionnaire ... (Shuy, Wolfram, and Riley 1968: 41)

In Parts I through IV, the fieldworker was instructed to ask questions such as "What kinds of games do you play around here?", "What are your favorite TV programs?", "Do you have a pet? Tell me about it."

Although these techniques were very effective in eliciting a large data set of linguistic forms that could be compared with one another and submitted to statistical analysis, their goal was limited to eliciting speech forms in various styles rather than elucidating the relationship between each speech style and the context of its use. Furthermore, the fact that most of the questions were pre-planned guaranteed a certain uniformity and continuity from one interview to another, but limited the development of topics that were of interest to the informants and might have suggested new questions for the interviewer (see also Wolfson 1976).

Linguistic anthropologists' interviews tend to be less structured than the ones organized around a standardized questionnaire, but they can be equally focused on some specific topics, including linguistic forms. The main difference between sociolinguistic methods and linguistic anthropological methods is that most linguistic anthropologists do not use interviews as their main technique for collecting speech samples, but as occasions for eliciting native interpretations of speech already collected in other situations, mostly in spontaneous interactions. In some cases, linguistic anthropologists might ask native speakers to produce certain linguistic forms and even engage in lengthy performances – which might produce stories, myths, magical formulae, oratorical speeches, polite expressions, and a number of grammatical forms –, but such occasions are usually designed to complement or clarify information collected in non-interview situations.

A typical question-answer type of focused interaction between the fieldworker and the native speaker is centered around the transcription of tapes previously recorded (see section 5.7). Another common type of interview is one that centers around the compilation of native taxonomies of speech genres. Such taxonomies are useful because they give researchers a way of getting a sense of the range of linguistic phenomena – or **repertoire** (Gumperz 1972) – that are possible/available in the community (see section 3.4). The knowledge of such a repertoire helps researchers decide how representative a certain style of speaking is, how it is related to other styles, and how it is seen by the people who perform it and their audience. One of the most extensive and complex taxonomies of speech genres ever described was collected by Gary Gossen (1974) in his study of Chamula oral tradition (see figure 4.1).

Gossen (1974: 52–55) offers an informative description of the methods he followed in collecting the taxonomy; from such a discussion we learn not only how he collected his data, but also the rationale for the choices he made in selecting his informants and pursuing certain themes revealed in their answers:
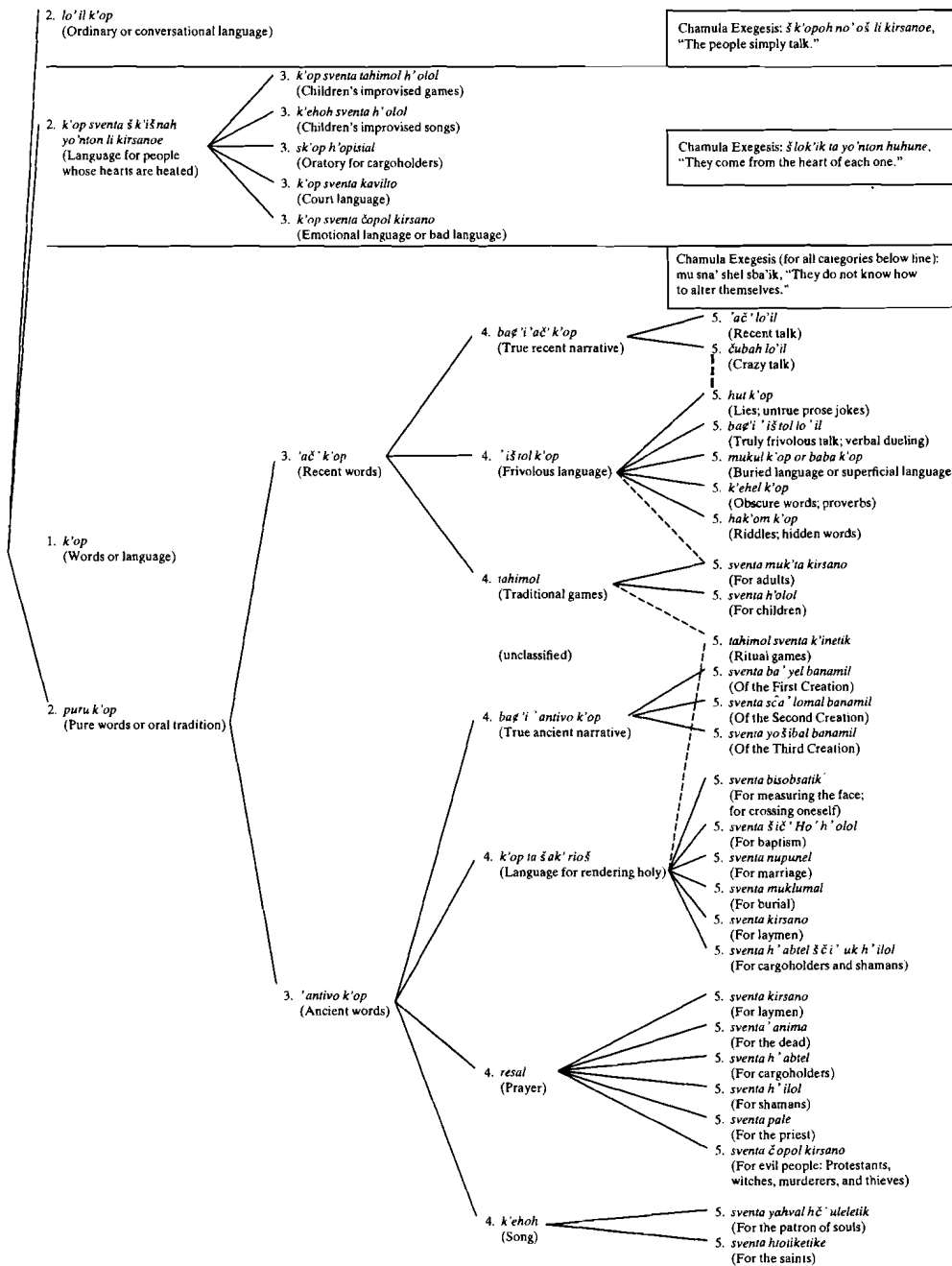
Figure 4.1 (folk taxonomy tree):

2. *lo'il k'op* (Ordinary or conversational language)

Chamula Exegesis: *š k'opoh no' oš li kirsanoe,* "The people simply talk."

2. *k'op sventa š k'išnah yo'nton li kirsanoe* (Language for people whose hearts are healed)
- 3. *k'op sventa tahimol h'olol* (Children's improvised games)
- 3. *k'ehoh sventa h'olol* (Children's improvised songs)
- 3. *sk'op h'opisial* (Oratory for cargoholders)
- 3. *k'op sventa kavilto* (Court language)
- 3. *k'op sventa čopol kirsano* (Emotional language or bad language)

Chamula Exegesis: *š lok'ik ta yo'nton huhune,* "They come from the heart of each one."

Chamula Exegesis (for all categories below line): *mu sna' shel sba'ik,* "They do not know how to alter themselves."

1. *k'op* (Words or language)

2. *puru k'op* (Pure words or oral tradition)

3. *'ač' k'op* (Recent words)
- 4. *baṭ 'i 'ač' k'op* (True recent narrative)
  - 5. *'ač' lo'il* (Recent talk)
  - 5. *čubah lo'il* (Crazy talk)
- 4. *'iš tol k'op* (Frivolous language)
  - 5. *hut k'op* (Lies; untrue prose jokes)
  - 5. *baṭ'i 'iš tol lo'il* (Truly frivolous talk; verbal dueling)
  - 5. *mukul k'op or baba k'op* (Buried language or superficial language)
  - 5. *k'ehel k'op* (Obscure words; proverbs)
  - 5. *hak'om k'op* (Riddles; hidden words)
- 4. *tahimol* (Traditional games)
  - 5. *sventa muk'ta kirsano* (For adults)
  - 5. *sventa h'olol* (For children)
- (unclassified)

3. *'antivo k'op* (Ancient words)
- 4. *baṭ 'i 'antivo k'op* (True ancient narrative)
  - 5. *tahimol sventa k'inetik* (Ritual games)
  - 5. *sventa ba' yel banamil* (Of the First Creation)
  - 5. *sventa sča' lomal banamil* (Of the Second Creation)
  - 5. *sventa yošibal banamil* (Of the Third Creation)
- 4. *k'op ta šak' rioš* (Language for rendering holy)
  - 5. *sventa bisobsatik* (For measuring the face; for crossing oneself)
  - 5. *sventa šič 'Ho'h'olol* (For baptism)
  - 5. *sventa nupunel* (For marriage)
  - 5. *sventa muklumal* (For burial)
  - 5. *sventa kirsano* (For laymen)
  - 5. *sventa h'abtel š či' uk h'ilol* (For cargoholders and shamans)
- 4. *resal* (Prayer)
  - 5. *sventa kirsano* (For laymen)
  - 5. *sventa 'anima* (For the dead)
  - 5. *sventa h'abtel* (For cargoholders)
  - 5. *sventa h'ilol* (For shamans)
  - 5. *sventa pale* (For the priest)
  - 5. *sventa čopol kirsano* (For evil people: Protestants, witches, murderers, and thieves)
- 4. *k'ehoh* (Song)
  - 5. *sventa yahval hč'uleletik* (For the patron of souls)
  - 5. *sventa htotiketike* (For the saints)

Figure 4.1 A folk taxonomy of Chamula verbal behavior (Gossen 1974)

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A complete taxonomy of Chamula folk genre ... was elicited at intervals over a period of one year essentially from six male informants ranging in age from eighteen to sixty years. The same informants provided the majority of the texts that are included as genre examples and as an appendix to this book. Five informants came from two contiguous hamlets; the sixth lived in the nearby ceremonial center. They were selected from this limited territory so that it would be possible to control for spatial data in their texts. Both formal question frames and informal discussion were used to elicit the categories of the taxonomy. The two methods were complementary in that formal interviewing (for example, "How many kinds of – would you say there are?") produced a taxonomy and genre labels that could be used informally to identify and talk about types of texts after they had been recorded or transcribed. A typical question asked countless times was, "Is this a – ?"

The taxonomy was useful in that it provided explicit native genre labels for organizing the collection of texts and also helped to assure a more comprehensive coverage of the forms of verbal behavior recognized by the Chamulas ... the design of the field work depended in large part on the initial information that was obtained from the taxonomy. (Gossen 1974: 53)

Gossen also mentions the places in the taxonomic scheme which represented "fairly general agreement" and those that "were not given as consistently by all informants" (1974: 54). Such statements are important not only factually – they let other researchers know how to weigh the information displayed in the chart –, but also methodologically, because they alert readers not to overemphasize the psychological as well as phenomenological reality of the classification. This means that a taxonomy like the one reproduced in figure 4.1 above is *one* of the possible ways of organizing the information provided by several native speakers. One should also be reminded that classifications of this sort are of little use without a documentation of actual performances of the speech genres. In my work on Samoan oratory, for instance, I found that orators differed in some of their statements about different parts of a traditional ceremonial speech. Some of those differences, however, could be explained in terms of variations and constraints on the speeches during real-life situations. The recreation of those genres in separate contexts (e.g. exclusively for the researcher's tape recorder) failed to produce the modifications necessary to accommodate to a knowledgeable, demanding, and interactive audience (Duranti 1994a). Similarly, Tedlock (1983: 292) discovered that the version of a story told in the presence of a tape recorder

might be not as explicit as the one told among family members and in front of the fieldworker without his tape recorder.

These events tell us that researchers must counteract the likely **variation in performance** of any speech form with a **variation in types of participation**, including alternating between passive and complete participation and between the presence and the absence of an electronic recording device. Although asking questions is a professional habit for researchers, sometimes, as Myers reminded us (see above), simply listening to what is going on around us is the best strategy for learning. This of course implies that the fieldworker is able to understand what people are saying.[5]

4.5 Identifying and using the local language(s)

In isolating a language to be used for an ethnographic study, it is also important not to create a "gap" in what Gumperz called the **"communication matrix,"** namely, the totality of communication roles within a society (Gumperz 1968: 464). This means that we should not exclude English from a study of an urban community in India, just as it would not be methodologically sound to exclude Spanish in the study of the English of the Hispanic population in Southern California or Texas. The relevance of a code at a particular moment in an interaction is of course an empirical matter that must be decided on the basis of investigation. But the method for collecting data is a theoretical choice. This is why it is important not only to conduct interviews with native speakers about speech genres and speech styles, but also to get a more direct sense of the range of events in which members of the community participate (see section 9.2).

There is no question that fieldworkers should try their best to become familiar with the language(s) used by the people they study. This is important not only for the ability to conduct interviews without interpreters, but also, and most importantly, for understanding what is going on. As eloquently stated by Witherspoon,

> The greatest value of learning the language of another people
> does not come from being able to interview informants without
> interpreters or from providing native terms in ethnographic
> writings; it comes from being able to understand what the natives
> say and how they say it when they are conversing with each other.
> (Witherspoon 1977: 7)

[5] See Mead (1939) and Lowie (1940) for a debate about the use of native languages as ethnographic tools. See Owusu (1978) and section 4.5 for a discussion of the use of translators in fieldwork and the problems generated by ethnographers' lack of familiarity or fluency in the languages spoken by the population they want to study.

However clumsy and inadequate ethnographers' attempts to speak the local language might sound, they symbolize a commitment, and show respect and appreciation for the cultural heritage of the people they study. When, for sociohistorical reasons, the people themselves have a low opinion of their own language or dialect, the use of their language or dialect by fieldworkers might be resisted. In this as in other circumstances, the use of a particular language or dialect becomes a political statement that can have long-term consequences for personal as well as public relations between people.

Unfortunately, many early classic anthropological studies were done by researchers who had only a very limited knowledge of the native languages. Writing about work done in the African continent from the point of view of a scholar and a "native," Maxwell Owusu (1978: 327) remarked:

> ... one may very well ask how many Euro-Americans know our
> language beyond the usual literal dictionary translations that
> inevitably make a caricature of native terms and idioms and
> confuse local meanings and expressions? I have not met one yet,
> certainly not among our esteemed ethnographic "experts" and
> critics. And what is even more disturbing about their general
> attitude is that they continue to produce "authoritative"
> monographs and essays on African cultures without seriously
> worrying about the degrading effects of their language deficiencies
> on the quality of the data. Publishing editors often cannot ensure or
> do not care whether the native terms are even spelled correctly.

Realistically speaking, it is often difficult for a researcher to be already fluent in the local language before arriving at the field site. This means that the most common situation (for those who work outside of their own community) is one in which the ethnographer knows *something* about the language (for linguistic anthropologists this is likely to be, minimally, information about the typological and structural characteristics of the language – or languages – spoken in the area), but is not a fluent (or even a minimally functional) speaker. The most typical situation is then that of trying to rely at first, as much as possible, on bilingual speakers who are able to speak either our native language or a language we already speak with some fluency. Jane and Kenneth Hill (1986), for instance, in their study of **linguistic syncretism** (a term replacing the more judgmental "language mixing")[6]

[6] "The term 'syncretic' [in speaking about Mexicano] is a more appropriate choice than the word 'mixed,' partly because the people of the Malinche take a negative view of mixing in language, and partly because by its very technicality the term 'syncretic' suggests something of the work and creativity of the Mexicano speakers of the Malinche" (Hill and Hill 1986: 1).

among Mexicano speakers in Central Mexico, relied on a literate sixteen-year-old native speaker of Mexicano for all of their interview data, which were based on a standard questionnaire (see also section 4.4.2). The same person was also responsible for the first transcription of all interviews. Hill and Hill (1986: 67–89) discuss at length the contexts of the interviews and the role played by the interviewer, giving readers a good sense of both the advantages and the limitations of such a method.

In situations in which a pidgin is common in the area – as is the case for instance in East Africa or in Papua New Guinea and other parts of Melanesia –, researchers can begin their work by using the pidgin and then slowly move into the use of the local language. The experience of several researchers I have spoken to over the years suggests that this is an efficient research strategy for the first few weeks or months, but it should be only a momentary or complementary part of the interpretive process in the field. The researcher's goal should be to move quickly to interacting as much as possible with monolingual speakers (when they constitute the majority of the population) or in the language that is the most commonly used, typically, the language that children are expected to speak – things get more complicated when a community has more than one native language or when children are not learning the same language their parents learned as children (see Kulick 1992). One should also be wary of relying too much on bilingual speakers. Except for communities in which almost everyone is bilingual, there are usually important reasons for certain individuals to know a second language; that is, they are often people who have lived and worked outside of the community for a certain period of time or have relatives from another area or country. This means that they are more capable of taking the point of view of the researcher and understanding his needs, but at the same time, that they are probably not the most typical individuals in the community. This is one of the paradoxes that field researchers must live with, namely, that the people who understand us the best and are most easily understood by us are usually the ones who are the closest to the way we are (Duranti 1996). One of the difficulties of fieldwork is to be able to take advantage of the insights that such people can provide without exclusively indulging in their accessibility at the expense of our attempts to communicate with other members of the community.

As we shall see in the next section, linguistic anthropologists try to overcome some of these problems by relying on direct recording of spontaneous interaction not only between them and their subjects but also, and mainly, between the subjects themselves. Electronic recording and play-back allow the researcher to employ members of the local community to transcribe and help translate linguistic interaction at its normal speed and are an invaluable means for training the researcher's ear to the subtleties of the local ways of speaking.

4.6 Writing interaction

> Meaningful action is an object for science only under the condition of a kind of objectification which is equivalent to the fixation of a discourse by writing. (Ricoeur 1981: 203)

Although writing is not the only thing that an ethnographer does, there is no ethnography without writing (Geertz 1973). This is true from before fieldworkers arrive at their site (they must first convince advisers, colleagues, funding agencies, and local authorities of the worth of their project) to the moment when they hand over the final draft of a manuscript with the result of their work. What interests us in particular here is the parts in between these two moments.

One of the distinguishing features of linguistic anthropologists is their reliance on recording machines, especially tape recorders and camcorders (video cameras that can also play back the recorded tape) – technologies that can be conveniently used to capture and analyze spontaneous interactions. Contemporary research complements – without claiming to completely replace it – the use of ethnographic notes with additional, especially electronic, recording devices. This section introduces the readers to the specific properties of some of these devices and to the ways in which the information stored in them is transformed for analytical purposes.

The concept of "writing interaction" presents problems from the very start. Regardless of how good we are as writers, we know that if our goal is to have the most accurate record of a given interaction, writing is a very poor technology for describing the richness of the experience of either *being* in an event or *witnessing* it as an observer. There is no question, for instance, that a good quality video recording or a film with a sound track of an event is going to have a lot more information than a written description of it. At the same time, it is also true that (i) we cannot make visual and sound records of everything – for a variety of reasons that include ethical as well as economic, practical, and even theoretical considerations –, (ii) even if we could approximate such a total audio-visual documentation, it would still never be the same as the experience of "being there," and (iii) as I will discuss below, there might be situations for which a written record might be more revealing than a visual one.[7]

In order to deal with the inherently problematic nature of the use of writing in describing interaction in general and verbal interaction in particular, we must

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[7] I am quite aware of the fact that the dichotomy between "written" and "visual" is potentially misleading given that writing, after all, is a visual medium. What changes in the two recording modalities discussed here is the relative degree of arbitrariness, and, more precisely, the relative iconicity of the medium.

start from the following assumption: *any process of documentation is, by definition, partial, that is, it assumes a point of view and it is selective* – this implies that we will never have a "perfect" recording device that would reproduce the exact context of the recorded event. Such a recording device would have to be a time-machine that would be able to bring us (and everyone else involved) back to the time of the event. Since in order to leave everything exactly the way it was we would need to be there without a memory of having been there, this research strategy would create an infinite loop; we would keep going back to the same interaction and we would never come up with an analysis of it.

Once we accept such a partiality, however, we also realize that it is part and parcel of our goal, namely, analysis. In other words the selective nature of any kind of description gives it its analytical properties. An **analysis** is, after all, *a selective process of representation of a given phenomenon with the aim of highlighting some of its properties*. An analysis that tried to reproduce a perfect copy of its object would not be an analysis, it would give it back to us the way it was. *Analysis implies transformation*, for some purpose. This applies to using a thermometer to check our body temperature as well as to writing down on a piece of paper a word we heard for the first time. In both cases we are using a tool (a thermometer, pencil and paper) to mediate our interaction with a certain object or phenomenon (our body, people interacting in front of us). In both cases, we are doing this with an interest in seeing certain properties – and certain properties only – of the phenomenon. Only in a different moment of the documentary process will we be concerned with integrating the information gathered with supplementary information. The strength of the thermometer as a tool is precisely that it can ignore everything except the temperature. The strength of the scribbled note is that it allows us to focus on that one word and ask someone about it at another time or look for it in a dictionary. Of course the word is not everything that went on at that particular time, but it is something; it can point us in other directions; it can help us learn about other words, other meanings, other interactions.

The advantage of seeing things this way is that we don't have to engage in the hopeless search for the perfect recording tool or the perfect description. At the same time we don't have to spend our energies complaining only about the deficiencies of the tools we have at our disposal. What we need to do instead is to understand the specific properties of such tools. Once we know the specific limitations and advantages of each tool, we are in a better position to know how to integrate technology to provide richer descriptions and more comprehensive analyses of complex sociocultural phenomena. We know now that, when used properly, tape recorders, video recorders, and computers *can* be used to our advantage, including producing more accurate analyses of people's interactions. A tape recorder, for instance, is certainly a more suitable tool to store a

complete conversation than our memory, no matter how good we think we are at listening and remembering. A photograph can allow us to see details of a scene that we might have missed with our naked eyes. It might also work at refreshing our memory about who was present and where that person was located. The same can be said of films and video recordings, which have – like tape recorders – the additional property of having a temporal dimension and hence of storing information about movement. With these tools we have the tremendous advantage of being able to see, over and over again, how auditory and visual access are exploited by members in constructing meaningful interactions. In fact, a video tape has a richness of information that is well beyond our present analytic abilities. Although at this point a video tape is, albeit limited, the best type of record we can have if we are interested in the integration of speech with body movements and, more generally, with visual communication, we are still trying to learn how to take advantage of such a tool. New directions in computer technology can offer new solutions to this problem. More generally, the invention of new tools that can be used for storing, replaying, manipulating, and reproducing information about human interaction not only offers new solutions to old problems, it also opens up the possibility of new analytical questions (see the Appendix on practical tips for recording human interaction).

4.6.1   Taking notes while recording

The discussion of new tools and especially electronic devices should not be interpreted as the end of the traditional ethnographic notes. Ethnographic notes can add dimensions of description that cannot be captured on tape, not even on video tape. First, there is an experiential, subjective dimension of "having been there" that is not quite visible or audible on a tape – although tapes can reveal important aspects of how our being there was enacted, perceived, and negotiated. Second, the notes can be used to document information about the participants in an interaction, including their cultural background, their profession or social status, age, previous knowledge of one another, their relationship with us. All of these and many others bits of information, which can be collected by simply talking to people, add a depth of knowledge about events and people that cannot be seen by simply watching a tape. We never know what kinds of questions we will be asking later on. For this reason, it is important to collect as much information as possible about whatever seems potentially relevant. The fact that we will not be able to know *everything* is not a reason to know *nothing*. Our curiosity is always triggered by our interests and we develop a sense of what we like to know about people and situations. At the same time, it is important to follow intuitions and the directions indicated to us by others. Third, we do want to be able to be more than just the "cameraperson" in each interaction we participate in. It is important for

a researcher interested in how people communicate with one another to assume different roles (from passive to active participant, for instance) and along different degrees of visibility in the scene. Having a little notebook with us allows us to scribble down a few notes, sometimes just a word, or to make a sketch of a situation, indicating in it where people are seated or who is moving in which direction. It also allows us to note down what is happening that is not being recorded (people moving behind the camera or leaving to go somewhere else). We might be suddenly struck by an idea, a connection we never made before and feel the urgency of writing it down (that's the way most of us have been trained to deal with new ideas!) rather than waiting until later when we are alone. When we go back home, at the end of the day, those short sentences and sketchy drawings will prove very useful in our attempt to put together a descriptive narrative of what we experienced during the day. It is not uncommon that even a few hours later our memory will have already started to act so selectively (and so analytically) that the notes can be very useful in correcting our shaky recollections. It is thus imperative for researchers to look over their notes as soon as possible after the recording session and write down extensive fieldnotes based on those notes. I have found that fieldnotes contain crucial information which helps me contextualize what I recorded on tape.[8]

4.7　Electronic recording

> Looking ahead, it appears that a future science of language and
> communication, both visible and acoustic, will be made possible, in all
> probability, not by refinements in notational systems but by increasing
> sophistication in methods of recording, analyzing, and manipulating
> visible and auditory events electronically.
>
> 　　　　　　　　　　　　(Armstrong, Stokoe, and Wilcox 1994: 354)

The introduction of recording machines such as the tape recorder and the video camera (or camcorder) among the field researcher's tools has a number of advantages over the traditional method of participant-observation based on the researcher's skills at listening, seeing, and (most importantly) remembering – whether or not aided by written notes. The ability to stop the flow of discourse or the flow of body movement, go back to a particular spot and replay it allows us to concentrate on what is sometimes a very small detail at the time, including a particular sound or a person's small gesture. Recent work based on audio and visual

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[8] A very basic form of note taking which turns out to be very helpful is the writing of the date of the recording and the names of the participants on the tape label. For audio tapes, the researcher can give information about the situation into the microphone before starting to record and for video tapes, date and time can be displayed either throughout the recording or at the beginning and after any "cut" or interruption.

recordings has shown that participants are in fact sensitive to the most minute details of an interaction, including the quality of a single sound and the direction of a very brief glance. Since such sensitivity is usually not at the conscious level, it cannot be investigated by simply asking informants about it. Once a "phenomenon" is identified and selected by the researcher, however, members – as well as other "experts," including the researcher's colleagues – will have a chance to assess it in their own terms,[9] in some cases confirming in other cases throwing doubts on the researcher's hypothesis. Through such an experience, others can add their reactions and evaluations to the researcher's. As a larger number of people enter the interpretive process and the researcher's interpretation becomes more vulnerable, the quality of the hypotheses made increases.

4.7.1　Does the presence of the camera affect the interaction?

Invariably, every time I discuss an interaction with the aid of a video tape, there is someone in the audience who asks: "Didn't the presence of the camera affect the interaction?" Video images seem to trigger this question more than, say, verbal descriptions of a given situation in the field or transcriptions of stories told by informants into a tape recorder. One could make an argument that the presence of the tape recorder and of the researcher's notebook also affect the situation. Carried to its logical consequence, the "impact" question could be used to argue that it would be better *not to be there* at all. This could be realized in two ways: (i) by *not* studying people or (ii) by not letting the participants know that their interaction is being recorded. The first option is self destructive and hopefully unacceptable to anyone who has made it so far in this book. It implies that we should not try to improve our understanding of what it means to be human and have a culture (including a language) simply because we cannot find the ideal situation for naturalistic-objective observation. The second proposal is first of all unethical and, second, impractical under most circumstances outside of laboratories with two-way mirrors. Some researchers try to circumvent some of these problems by giving the camera to a member of the community. This method has the advantage that it offers a different perspective from the ethnographer's – the categories whereby something is selected for recording might be different[10] – but it does not really solve the ethical problems given that members might feel entitled to intrude much more than outsiders in the lives of their family and neighbors and this might create even more ethical dilemmas.

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[9] Despite the fact that the framing of a phenomenon already directs future listeners and viewers to hear and see in a selective way, there is room for a certain level of independent judgment that is not possible when researchers simply state what they observed.

[10] This was what Sol Worth was interested in when he gave cameras to the Navajos so that they would make their own movies (Worth and Adair 1972).

In fact, the camera-effect is only one special case of what is usually called the **participant-observer paradox**: to collect information we need to observe inter-action, but to observe interaction (in ethically acceptable ways) we need to be in the scene; therefore, any time we observe we affect what we see because others monitor our presence and act accordingly. If we think a moment about this logi-cal loop we realize that it is not only part of doing research. It is part of being a social being, a member of a society and a producer/consumer of cultural inter-pretations. Being a social actor, a participant in *any* situation and in *any* role, means to be part of the situation and hence affect it (see section 4.1.2). Is there a solution to this paradox? Life itself is an attempt to resolve the participant-observer paradox. So-called neutral observation, where the observer is com-pletely separated from the observed is an illusion, a cultural construction. This does not mean that we should ignore the paradox, but that we should deal with it with the awareness of its unavoidability. In the social sciences, dealing with the paradox means to *understand the different ways in which the presence of certain types of social actors (e.g. ethnographers) or artifacts (e.g. cameras, tape recorders, notebooks, questionnaires) play a role in the activity that is being studied, and the different kinds of transformations that each medium and technique produces*. For example, there is no question that our presence as observers is more intrusive in some situations than in others. There is a difference between walking with a camera in our hands into a room where two people are having a conversation and bringing a camera to a public event that involves dozens of people. At the same time, the way we present ourselves, what we do as well as what our hosts are occupied with have a lot to do with the impact of our presence and the camera on the observed. Video recording (or filming) raises some of the same questions raised by other documentary techniques such as interviewing (see section 4.4.1 above). We must develop ways of evaluating how what we see around us changes when we bring into a situation a video camera or any other type of recording device. At the same time, it should be kept in mind that, perhaps with the excep-tion of obvious **camera behaviors** (e.g. certain types of camera-recognitions or salutations like staring into the camera and smiling), people usually do not *invent* social behavior, language included, out of the blue. Rather, their actions are part of a repertoire that is available to them independently of the presence of the camcorder. One might even argue that the presence of the camera may be used as an excuse for certain types of social actions that might have been done anyhow, like when people point to the camera to provide a reason to be polite or be gen-erous. I believe that most of the time people are too busy running their own lives to change them in substantial ways because of the presence of a new gadget or a new person. As shown by many researchers over and over again, even with a lens aiming at them, participants still manage to argue with one another, be overrun

by emotions, reveal intimate aspects of their private lives, or engage in lengthy evaluations of the private lives of other people (including the fieldworker!).

An understanding of the impact of the camera on a given context also implies an understanding of the kind of information represented by it. A tape contains a filtered version of whatever happened while the tape was running. It has, how-ever, the power to capture social actions in unique ways. Thus, as I discussed ear-lier (section 4.6), cameras have the power to keep a record of an interaction that maintains some of its temporal and kinesic characteristics.[11] Such a record can be viewed by different people and subjected to analysis in ways that are quite differ-ent from the ways in which a narrative by an observer of the same event would allow. As with any other recording device, rather than blindly rejecting the use of a camera because it might influence people or embrace its use as a technology that can produce the ultimate objective accounts, we must work at understanding what a camera can offer for our theoretical and methodological goals.

4.8   Goals and ethics of fieldwork

> Qu'est-on est venu faire ici? Dans quel espoir? A quelle fin?
> Lévi-Strauss, *Tristes Tropiques*

The *Anthropology Newsletter* published monthly by the American Anthro-pological Association is full of ethical dilemmas. More and more writing within and outside of anthropology has been focusing on ethical and political issues implicit in the practice of studying human beings. Within linguistic anthro-pology, Penelope Harvey (1992) and Niko Besnier (1994) have recently addressed ethical problems in tape recording interaction. In a frank and intriguing discus-sion of a very difficult subject, Harvey risked taking an unpopular position defending clandestine tape recording while recognizing its ethical implications. She argued that without tape recording drunken speech, she would have not been able to understand some important aspects of the relationship between lan-guage and power in the Peruvian Andean community she studied. The ethical problem about not sharing our goals with our informants, she argues, comes from the nature of representation and authorship in anthropology. We cannot "be entirely open about exactly what data are being collected, since it is only at the stage of writing that the collection of memories, impressions, notes and recordings become 'data' by going on record" (Harvey 1992: 82).

Besnier (1994) wrote about the unforeseen consequences of exposing recorded interaction to members of the community who were not present at the time of

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[11] There are many aspects of a situation that not even a camera can capture, including smell, a dimension of context that has been vastly underestimated in the study of human conduct despite its most obvious effects such as the activation of memories.

the recording – like when, for instance, we ask a third party to help us transcribe a recorded tape. Like Harvey, he argues that the ethics of fieldwork are more complex than the principle of informing participants that one is recording their actions or not allowing someone to listen to what other members of their community said when they were not present. Besnier elaborates on some of Harvey's points and turns the discussion of the ethical problem he faced into an occasion for a criticism of the implicit wisdom of participant-observation without audio or video recorders:

> I would like to take Harvey's point further, suggesting that anthropological methods that base ethnographic analyses on *impressionistic re-creations* of what is said during a drunken episode or a gossipy moment are more abusive of scientific authority than methods based on the microscopic analysis of a transcript of what is said, without ignoring, of course, the ethnographic authority embedded in the transcribing process (see Tedlock 1983).
>
> (Besnier 1994: 27)

Poststructuralist and postmodernist critiques of the role of the researcher in visiting foreign places and making claims of authority have certainly made these discussions more frequent in recent years, but such issues have been in the minds of anthropologists for quite some time, as shown by the above quote from Lévi-Strauss's autobiographical *Tristes tropiques*. His questions "What have we come to do here? With which hope? For which goal?" succinctly capture one of the main issues in ethnographic work. What is behind the ethnographer's quest for knowledge of the Other? Are there hidden, unwritten motivations, sometimes within, sometimes without the researcher's conscious motivation for fieldwork experience? What are we looking for? What do we want to find? Who sent us?

There is no question that travels of discovery, in the name of science, have often been travels of conquest (Reill and Miller 1996). For these reasons, the age of naiveté in anthropology is over. What replaces that age must be negotiated through theoretical and empirical attempts to deal with the conflicts that accompany any search for other ways of being, doing, and saying. There are many different solutions, none of which is the ultimate one. The Italian anthropologist Ernesto De Martino, who worked half a century ago on what he saw as oppressed subcultures in the south of Italy, suggested that ethnographic research should start from "a commitment to tie our traveling to the explicit recognition of an actual passion, connected to a vital problem in our own society" (1961: 20, translation mine). It is the goal of the researcher to explain how such a passion is translated into an ethnographic account, with an awareness of the complexities I have hinted at. There is however no way of escaping the responsibility we have as researchers towards the people we study. This does not mean that we should always and only write what we think they will like, but that whatever we decide to say publicly and publish should be informed by our awareness of the potential consequences of our research (the American Anthropological Association offers some guidelines on the ethics of fieldwork, but they by no means exhaust the possible issues and situations encountered during fieldwork). We need to develop a theoretical understanding of our position and positioning in engaging in ethnographic methods. The concept of ethnographers as cultural mediators discussed above is one way to come to terms with the complex reality of anthropological fieldwork. Ignoring the problems or deciding to stay home are not viable solutions.

4.9 Conclusions

In this chapter I have shown how, by drawing from different fields dedicated to the study of human interaction and communication, linguistic anthropology provides a unique blend of recording techniques and analytical dimensions for our understanding of human cultures. In the next chapter, I will explore how the information recorded through the methods discussed in this chapter is transformed into texts and other forms of visual representations that can help us improve our analytical understanding of language as a cultural practice.

An important aspect of the linguistic anthropological methods discussed in this chapter is the integration of traditional participant-observation methods with new recording techniques that allow for a different kind of access to the ethnographer's experience. In the next chapters I will refer to a number of other disciplines and approaches (in linguistics and sociology in particular) that make use of similar recording devices and ultimately produce what appear to be similar types of documents (texts, transcripts). Since these other disciplines have something to offer to our understanding of the ways in which language enters the constitution of social action, it is important to maintain an open and informed attitude toward them. There is also no copyright on methods in the social sciences. One should feel free to use what seems to work for one's goals. Experimenting with new techniques (e.g. video, computers) can provide insights and reveal phenomena that had been previously ignored or left unanalyzed. At the same time, new technologies also bring new ethical and political problems. A discipline that is concerned with the issue of representation must grow by maintaining a vigilant eye on the pros and cons of new methods of documentation while developing a critical understanding of the pros and cons of the old ones.