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This Book is Dedicated

To the Memory of Jorge Buenful Acuña

[10] Due to Michael's methodology, in particular his brief visits and his role as guest, he can only assume that the performances he watches, feed back into life. There is no way for him to estimate or experience their impact. Elsewhere, I have presented my own approach to fieldwork and to this ritual in particular by using Michael's presence as a heuristic device (Hervik 1995).

[11] Later, I was able to discuss this incident with Roberto. He explained that upon arriving he, the folklorist and his fiancé, had thought that I had declined to greet them and that I did not want to be associated with them. When they arrived I was engaged in informal talk with the Campechanos when one of my sons told me that the Americans had come. I later found them engaged in the front room, in testing out their video equipment and hesitated to interrupt and returned to my previous conversation.

[12] He did not suggest how, if I deleted him from the manuscript, I could maintain quotes and paraphrases from his public works.

CHAPTER SEVEN

Shared Social Experience and Co-developing Reflexivities

The towering effort in the social sciences to unfix fixed representations could leave the impression that we can never know anything about the reality of others if we cannot get past ourselves (Hastrup 1995a). In the post-modernist critique, reflexivity emerged out of the assessment of other scholars' use of literary strategies in their production of scientific authority, and complying with this critique anthropologists began exploring various forms of selfreflection in their writings. The bulk of the so-called experimental, reflexive ethnographic writings revolved strictly around the anthropologist as author of texts and how he or she was incorporated into the text (Marcus and Cushman 1982, Clifford and Marcus 1986, Marcus and Fisher 1986).

The reflexive approach and the attention devoted to representation have been a crucial contribution to our critical reading of scholarly texts. Postmodernists have taught us much about the inherent literary pre-suppositions that may inform and restrain ethnographic writing and have made us aware of literary manipulations used to establish ethnographic authority. However, the conclusion drawn from the critique is that since all descriptions are partial truths anyway, then evocations of daily life and pre-existing schemas are best suited to avoid the problem of the inherent inadequacy of representation (cf. Tyler 1987). What I object to is the manipulation of anthropology into a therapeutic enterprise of textual evocations in the reader of already existing understandings. In Tyler's approach the best anthropologist becomes the "genius" of writing and speaking, since discourse is the object and means of post-modern ethnography. In my view, this kind of anthropology creates a new form of authority based not on

understandings of people and their relations but on creativity in evoking in texts the shocks, the fragmentations, and the multiplicity of the lived world which often draw heavily on the anthropologist's own "exotic" explorations. If the impulse is to maintain a methodological focus on textual representation then I disagree strongly. The "production of text by means of texts, rather than by means of fieldwork" (Fardon 1990: 5) is a development that sees fieldwork practice as unproblematic and postmodernists repeat a fallacy of modernists. (Okely 1992: 3).

This chapter is based on the assumption that since our empirical material in anthropology is made up of real lives then there is only one way to understand people; through shared social experiences (Vendler 1984: 201, Hastrup 1995a). Although language is a powerful tool in the analysis of social relations, no text will give us access to cultural models and habitus—the out-of-awareness dispositions derived from experience (Holland and Skinner 1994). Since not everything is immediately sayable in our kind of written prose (Hastrup 1993a: 732) and since language is a tool that inevitably objectifies the world (Hanks 1993: 139) it seems reasonable to argue that much cultural experience lies beyond language (Hastrup 1995a, Hastrup and Hervik 1994b),

Fieldwork practice is always concerned with relationships (Okely 1992: 2), but our social positions in the field are nevertheless not fully theorized. As fieldworkers we do not experience or represent an unmediated world of others, but the mediated world *between* ourselves and the others (Hastrup 1992: 117). These others, I argued in the preceding chapter, include other scholars. Epistemologically the betweenness is an unsurmountable barrier. Our relations with local native actors and attending researchers are the primary resources of participation and reflection and will at some point of condensation and comparison end up as anthropological knowledge in publications. Therefore it is imperative that we continuously scrutinize the implications of the relations of power, the importance of our class origin, ethnicity, religion, sex, age, marital status, and lived experience. Postmodernism has made an important contribution to this, but it has been unbalanced in its focus on representation and the production of text.

The notion of shared social experience delimits a locus of study and directs attention to the conscious identity of the ethnographer, the position of the ethnographer in the field, the nature of the relationship with local informants, his or her willingness to be engaged in the lived experience of the native actors, and allows people to be salient in the text. This approach and attitude allows for the legitimizing of the presence of multiple subjectivities in the text including those of the anthropologists and the informants. It does not follow that the anthropologist should engage in a process of *self*-exploration as the ultimate goal of ethnographic writing. In Harraway's words "To insist on value and story-ladenness at the heart of the production of scientific knowledge is not equivalent to standing nowhere and talking about nothing but one's biases" (1989: 13). The integration of the ethnographer's cultural and personal identity and past is only germane to the extent that it is relevant to the enterprise including choice of area and topic, the experience of fieldwork (Okely 1992), and local language abilities.

Shared social experience is inseparably bound up with the notion of reflexivity.[1] In prior work, I have argued that a reflexivity in the "world" is different from textual reflexivity and that it can be seen as a linkage between social experience and general anthropological knowledge. Reflexivity does not bend back exclusively to the ethnographer but also to a social interaction in processes of shared reasoning, which is a kind of reflexivity that cannot be reduced to the individual. In addition, I argue that shared social experience is our only means of access to the reflexivity of other people (Hervik 1994).

The way the notion of reflexivity has been used in social science literature has severely limited its utility. Researchers have focused on consciousness and language. On the one hand, studies dealt with the author's degree self-awareness and the degree to which the discipline has examined itself critically. On the other hand, reflexivity has focused on how authors used self-reference in the production of text and finally reflexivity has been studied as an inherent capacity of language (for general analyses of reflexivity see Ashmore 1989, Babcock 1980, Bloor 1976, Myerhoff and Ruby 1982, and Watson 1991). Bruno Latour's definition of reflexivity, for instance, concerns texts (the ultimate product of science) as the object of reflexivity. In his

scheme reflexive denotes: "any text that takes into account its own production and which, by doing so, claims to undo the deleterious effects upon its readers of being believed too little or too much" (Latour 1988). On a higher level of generalization, reflexivity has been referred to as "the capacity of any system of signification to turn back upon itself, to make itself its own object by referring to itself. Subject and object fuse" (Myerhoff and Ruby 1982: 2). My point here is not to try to organize or explain all of the possible levels of textual reflexivity or consciousness of oneself in and about writing, but simply to argue that reflexivity need not be confined to the researcher's external objectivist point of view. There is a reflexivity beyond the author (Hervik 1994). Reflexivity of the "world," unlike the Latour's, Myerhoff's and Ruby's reflexivity of the text, is a condition of anthropological practice. Our relations to people in the field are more than a verbal relation but a personal relationship through which we affect our subjects. Reflexive practice is the continuous, conscious, cognitive effort to scrutinize the social and cultural encounters for inherent relations of power and systematically question the position and dispositions of the ethnographer in this relationship which might misguide the social enquiry. This delineation of reflexivity has similarities to Bourdieu's sense of reflexivity, but whereas he restricts the use of reflexivity to the construction of the research object within the academic field (Wacquant 1992: 39), I take reflexivity to be the necessary part of practice in the concrete day to day encounter between the anthropologist and the people with whom he or she is involved, including other commentators.

In this chapter, I will seek to elaborate some important attributes of the notions of shared social experience and nontextual practical reflexivity using brief vignettes as illustrations. My arguments concern the actual experiences that grow out of interaction and exchange between ethnographer and the people of the field closest to him or her.

The first section deals with the experience and subjectivities of the anthropologist. I argue that the experience of the ethnographer is the starting point in a long process leading to general anthropological knowledge. The first experience of relativism occurs before dialogue is initiated. This is made particularly clear by Bachnik's reflections on her relationship with her Japanese hosts. Because of her limited linguistic skills, she worried about her host family's reactions and feelings toward her, but the family guessed her concerns before she could express them in any verbal form. They were constantly assessing this newcomer's slightest change of feelings (Bachnik 1994). From that point we can only move outward and towards objectivity. In the second section called "social experience," I argue *against* focusing on the individual experience of the ethnographer exclusively and in favour of speaking for the collective character of the cultural interpretive frameworks that the anthropologist brings into the field and of the social encounters that frame most of the enduring experiences in the field. In other words, I argue for an anthropology predicated upon the centrality of the social and the cultural in human life. I argue for an anthropology that recognizes that it must account for the collective nature of any individual (Holland and Valsiner 1988: 247) including the anthropologist. This is the next theoretical step away from the unanalyzed experience of relativism and towards an increased knowledge of the mismatching social categories. To this I add "shared social experience" in the third section where I deal with interactions between the anthropologist and native actors at a time during fieldwork when some common ground has been reached; the ethnographer has internalized or embodied some local cultural proficiency and insight which provide common points of reference in interaction. Access to native reflexivity, to the extent this is ever possible, is greatest at this point. Shared social experience takes us a step further towards a narrowing of the mismatch between the categories of the observer and the other. This process was earlier coined "relativism," but the anthropologist's increased knowledge and shared understanding of different categories shows that relativization is not a form of anti-objectivity, but is our only mode of objectivization (Ardener 1989: 212–3). Objectivity is about achieving agreement and agreement implies the display of a degree of coherence with experience of both anthropologist and the other (Hastrup 1993a: 734–735). Finally, in the fourth section on shared social experience and reflexivity, I explore the production of knowledge as general literature is brought in to scrutinize and understand what went on during the shared social experience.

EXPERIENCE AS INDICATOR OF GOOD ETHNOGRAPHY

There is no doubt which of the following two pieces of ethnographic writing is considered the better choice: One bearing a relationship to the author, or the people written about, the other not does not bear any such relationships. Experience *is* the touchstone of good ethnography (cf. Davis 1994).[2] In anthropology, experience primarily refers to the anthropologist's experience as it becomes part of analysis and of the lived experiences of the local individuals. The most immediate and personal experience is that which place the anthropologist at the center and as both subject and object (Callaway 1992: 44). I will restrict the use of experience without the modifier "social" to this meaning of experience.[3] Most of our experience is of course social, since "most of what any human ever thinks has been thought before, and most of what any human ever thinks has been learned from other humans. Or, to put it another way, most of what anyone knows is *cultural* knowledge" (D' Andrade 1995: xiv).

Experience is a holistic, ongoing process that draws on all senses. Writing, in comparison, is a disembodied, linear practice. It follows that experience cannot be spelt out as a genre, it can only be lived. If speaking itself is "to situate oneself in the world, to take up a position, to engage with others in a process of production and exchange, to occupy a social space" (cf. Hastrup and Hervik 1994a), then how do we express experience without decontextualizing it and dehumanizing it? The traditional ethnographic response is to deconstruct it as experience and transform it into totalizing professional models and knowledge. To strive for another solution, we need to attend carefully to the lived interactions in which the ethnographer takes part, forgetting or discarding the positivistic ideal of social science, which seeks to filter out the "impurities of experience" (Callaway 1992: 38). The notion of experience can be evoked to dismantle positivism, since it emphasizes the engagement of all the senses in fieldwork and the always situatedness of the anthropologist (Okely 1992: 3).

Arrival stories are a genre of ethnographic writing that in post-modernist critique have been deconstructed as the author's hidden strategy to claim authority. By portraying his or her arrival to a remote area under some extreme, unexpected circumstances, the anthropologist invites the reader's credulity by indirectly saying "I was there, so you have to believe me" (Clifford and Marcus 1986, Marcus and Cushman 1982). The presentation of everyday life situations in the field can be endowed with the same strategic intentions on behalf of the author.[4] Arrival scenes can definitely be constructed in ways that reveal the contingent nature of the following text, but the near exclusive focus on arrival scenes as an index and device of ethnographic authority building obscures at least two important facts. First, it deflects the point that arriving at the fieldsite is an important experience in the life of the ethnographer. Arrivals take place in a situation of extreme personal estrangement since class, cultural, religious, and ethnic boundaries are visible and questioned. Moreover, the ethnographer arrives with an agenda of different themes and hypotheses which we impose on our "hosts," which are immediately thrown into doubt in the first experiences of relativism. I use arrival in the plural, since I am uncertain when the arrival begins. In my own case, I made a short visit to what was later to become my fieldsite. But I was not sure at that time if my research would be funded. Another "arrival" could be the first visit I made with my wife and our three children to the broker family with whom we discussed living arrangements. That was before we moved to Oxkutzcab. Or, arriving could be participating in the celebration of the 25 years of the Maya bilingual and bicultural education program in a village which was most likely to be the site for my fieldwork. My point is that the experience of arrival is ongoing and covers important incidents throughout the fieldstay; arriving scenes cannot be explained solely in terms of a true literary genre that has been the object of post-modernist critique.

The other point ignored by the critics who see arriving stories as purely literary strategies can be drawn from the British novelist George Eliot:

> Men can do nothing without the make-believe of a beginning. Even Science, the strict measurer, is obliged to start with a make-believe unit, and must fix on a point in the stars' unceasing journey when his sidereal clock shall pretend that time is at Nought (...) and with his clock-finger at Nought

> (Science) really sets off *in medias res*. No retrospect will take us to the true beginning; and either our prologue be in heaven or on earth, it is but a fraction of that all-presupposing fact with which our story sets out (Eliot 1984: 3).

Narratives have a beginning while experiences are not ever entirely new. Physically, of course, one is present for the first time (in this village, in the house of a certain shaman, in your own new house) but the encounters are framed by and draw from interpretive schemas, both idiosyncratic individual ones and the cultural constructed interpretive frameworks brought from home. Arriving in a remote area levels a serious challenge at the interpretive framework brought into the field and the challenge continues during the fieldwork and long after.

Upon arriving, I knew more about the pre-Columbian Mayas than of the contemporary living Maya speakers of Oxkutzcab, my fieldsite. My knowledge of the Maya people was like that of many people, heavily biased towards the past. Contemporary people do not play an essential part in this cognitive interpretive framework. Despite my studies, I was much affected by these biases. When I encountered a milpa farmer rushing home to his traditional thatched-roof-house to watch major league baseball, or a traditionally dressed Maya women cooking nourishing meals on electric stoves in a similar house, and occasional American cars with Oregon license plates outside traditional houses, I was stunned.

The arrival experience is, as are all experiences, interpreted. The ethnographer's experience is framed by the whole history of the person; by the social position of the subject, the conscious identity, and the cultural models which a person use consciously or unconsciously to make sense of experience. The ongoing reflections by the ethnographer on the author's position and dispositions is a form of self-analysis and political awareness that take place long before the computer is turned on and continue through all phases of writing.

My experiences of the paradoxical co-existences of familiar and unfamiliar cultural elements accumulated. I went in and out of thatched-roof-houses without electricity but with electric food-processors, irons, and vacuum cleaners, and houses with impressive tv-antennas but no television sets. We did not catch on to what was going on until we found that the young photographer, Gonzalo, (chapter Five), could not produce a single photo illustrating "paradoxes" which I had gone through much pain to explain, but he had hundreds of wonderful shots of everyday life. Finally, one night when he left his traditional house and came to visit us dressed in a fancy black leather jacket, I realized that he did not perceive the co-existing cultural elements as paradoxes and that my own cultural model of the Maya people was formed in a external context that had filled it with images of pyramids, ruins, steles, hieroglyphics, caves, ancient mythology and so on. Months of adaptation to life in Oxkutzcab and the increasing proficiency in the daily rhythm of the town had not been enough to understand what Gonzalo saw or did not see in portrayals of "ordinary life." It turned out that no one in Oxkutzcab saw it as a contrast to make milpa (corn garden) south of the town and then watch international sports at night. Our cultural model produced in a different part of the world and by different means provided the clue for understanding the difference.

In general, there is no congruence between the externally constructed "Maya" and the locally constructed "mestizo" model. They are not even variations on an analogue scale. At the outset of fieldwork, these external constructions gave a particular character and depth to "arrival" experiences, but then as lived interaction and participatory experience accumulated these external constructions were constantly questioned and revised. The externally generated cognitive interpretive framework becomes an object of reflection. Deconstructing this framework is the first step in moving beyond our selves and engaging in shared social experience with others.

From the anthropologist's first subjective experience he (or she) can only work outward *through* this experience in an attempt to transcend it. We have to start with experience and recognize that it is interpreted. The ethnographer's initial personal experience with particular people in particular places and a particular time is an experience of relativism which is the only the basis for a general understanding. This process is far from a mechanical relationship that can be taken for granted or downplayed as is the case in much of the post-modern literature (Okely 1992).

SOCIAL EXPERIENCE: SITUATING THE ETHNOGRAPHER

Even if the subjective experience of the ethnographer is the starting point, ethnography is not about the author. If knowledge gained by the ethnographer is only personal, it is not necessarily valid knowledge for others (Carrithers 1992: 148). We must get past ourselves and account for the *collective* nature of the individual experience. We have to address, reflect upon, and transcend our own individual subjectivities in order to concentrate on the mediated space in which we, as ethnographers take part. Thus, I add the modifier "social" to experience to indicate a further step of comprehension on the road from individual experience (of the ethnographer) to generalized anthropological knowledge.

There is also a cognitively based reason for using the modifier "social" in describing this stage of research. The interpretive schemas we draw upon in speech and action are resources used, consciously or unconsciously, in understanding. They are widely shared (but not universal), culturally formed, cognitive schemas. A number of different analytic categories in the literature refer to roughly the same phenomenon but emphasize different aspects of cognitive schemas. "Cultural models" (Holland and Quinn 1987, D'Andrade and Strauss 1992), "semantic density" (Ardener 1989), "cultural understandings" (Quinn and Strauss 1994), "culturally constructed worlds" (Holland & Skinner 1994), and "habitus" (Bourdieu 1977, 1990) are to name but a few. Common to these analytic categories is that all of these concepts are seen to be socially rather than individually derived. Cultural models are constructed out of the patterning of accumulated experiences, or regularities in our understanding which we use to reason with in interaction. Experiences that "stick" are those that are repeated over and over, connected to emotional involvement, or accompanied by intense problem-solving-thought. Some cultural models contain strong directive forces conveyed by the specific circumstances of internalization, social evaluation, and identification of self within the culturally devised system (Holland and Quinn 1987, Holland 1992 and Lutz 1992).

In D'Andrade's terminology cultural models can be divided into three levels depending upon the generality of the interpretations they produce and their directive force; the master motives, such as love and work are at the top level; middle-level motives depend on the top level to gain directive force and instigate action, for instance marriage depends on love, job depend on work; while the bottom level contains schemas for things such as memos and birthdays which do not instigate action except when higher-level motives interact with them. D'Andrade's hypothesis is that a person's most general understandings will serve as important goals for that person (D'Andrade 1992: 30). What is important for my focus on social experience is that these goals come from experience of the social world. The cognitive interpretive frameworks we take into experience are socially constructed rather than individual.[5]

Bourdieu also stresses the social nature of habitus and fields.

> Since the history of the individual is never anything other than a certain specification of the collective history of his group or class, *each individual system of dispositions may* be seen as *a structural variant* of all the other group or class, habitus, expressing the difference between trajectories and positions inside or outside the class [emphasis original] (Bourdieu 1977: 86).

Although each individual trajectory of the shared culturally constructed worlds may not be reducible to any other individual trajectory, but it is still a reflection, a variant of the shared culturally constructed framework (ibid.).

The stereotypes and expectations of the Mayas that I brought with me as experiential luggage to the field are a widely shared cultural construction in the Western world produced not only in the academic world and the world of mass tourism, but also in the context of the production of Mexican national identity. According to this cultural model which we described earlier, residents of the Yucatán peninsula, the "Maya people," are associated with the magnificent cultural achievements of more than 800 years ago. As with all ethnographers, I had to transcend my experiential baggage. Since cultural models are a product of our primary world of reference, engaging in practical chores and social interactions in the fieldsite provides a means to contest preexisting models.

Obviously, the ethnographer in the field also becomes an object of reflection. People relate to the ethnographer as an individual, a cultural category, and as a potential resource for work, borrowing money, and practical favors.

A vignette from my fieldwork is illustrative. Upon entering the field we hired two local Maya women to help with the daily chores and with the children. Within a week, one of these Maya women had asked for a two month advance on her salary. This money would enable her to cover the down payment on a piece of land that she wished to buy for a future home and for herself and her parents in their old age. We gave her the advance and another week went by, and she asked optimistically for a new loan. This time, she needed the money for the remaining seven months she was to work for us. It happened that this amount would cover precisely the remaining sum for the land. In rural Yucatán, her requests were not uncommon.

Her action shows clearly how I was perceived and related to as a potential resource. My subject position, and that of the two Maya women, did not fully determine how we related to each other, but served as a resource which could be exhausted for certain actions and which could be used to realize the two Maya women's strategies. On a day-to-day basis we interacted informally, working together in a friendly way. The relationship that grew out of the daily encounters is what enabled her to ask for the advance. We also perceived the two Maya women as resources for practical help and direct access to Maya culture. One day the entire household made a trip to some local ruins. I thought that such a trip would be pleasant and I anticipated that the Maya women not only knew about the ruins, but would also feel excited about showing me part of their cultural heritage and could tell me details without my having to ask questions. As it turned out, they were indifferent to ruins and history, exposing my expectations as inappropriate and the product of intellectualist bias about Maya cultural continuity.

It would be incorrect to assume that native actors and speaker are engaged in the same quest for explaining what goes on in the community as is the anthropologist. Bourdieu's critique of those who focus on textual reflexivity—those who limit reflexivity to the author and his or her production of texts—is precisely, that they have "fallen

into the scholastic trap of seeing the anthropologist and the native as jointly involved in interpretation" (Wacquant 1992: 42). In my experience, "natives" viewed the ethnographer as a resource in their own practical strategies and did not share the ethnographer's project.

Sometimes local people relate to the ethnographer (and his family) in ways that only later prove to be much more than practical strategies for their own ends. The other Maya woman who helped us spoke of *our* children as *her* children (hijos). She had become particularly close to one of the twins. Coming to the close of our fieldstay, she admitted that the first day she saw Simon in the town square she was determined to quit her job at a nursery and work for us and help take care of the children. Leaving the field strained her emotionally as she was disengaged from her children.

Such personal relationships that grew out of our fieldwork situation were (and always are) a prerequisite for acquiring intimate knowledge of individual experiences and histories and for studying individual appropriations of traditional celebrations such as the Okosta Pol. Moreover, we can now argue that different appropriations and different methods of study afford different "subject positions" to the natives.

In the cross-cultural meeting space between the ethnographer and local people an experience of mismatching categories occurs. As our relationship unfolded, the categorization of the two women we hired was first that of Maya women of Oxkutzcab and later these categories dissolved into the identities of Elena and Sonia. Their different temperaments surfaced, their worries, they got upset, made innuendos, and quarrelled with each other. In this process of moving from "categories" to concrete individuals, those who were once seen as "different" become more like variants of "us" (Rorty 1992: 471). We realize the fact that we like other people are just as "uncategorizable" as we consider ourselves (Cohen 1992). As dialogue continues and deepens common misunderstandings become more subtle, we realize that the first experiences of relativism are departures from objectivity, but paradoxically, our only means of objectification (Ardener 1989: 212).

One main goal of reflexive practice in ethnographic fieldwork is to contextualize the situatedness of the anthropologist and the

pre-existing culturally informed experiences taken into the field. Reflexivity does not belong solely to the historical world of the ethnographer nor is it confined to the anthropologist's culture, but exists in a cross-cultural relationship. This relationship should be deconstructed for its positions and dispositions with the same rigorous attention as in other areas of the discipline. This must occur before we can take the next analytic step and speak of shared social experience.

SHARED SOCIAL EXPERIENCE AND ITS LIMITATION

In his experimental ethnographic writing "Tuhami—Portrait of a Moroccan," Crapanzano seeks to write the life story of Tuhami, his Moroccan friend. While Crapanzano is sympathetic to Tuhami, he only allows himself to be reflexive in the text, while Tuhami speaks only directly (1980: 23). His reflexivity leads him to discuss his social encounters with Tuhami. However, he still keeps Tuhami at a distance by taking the theoretical position that we can only know the experience of another by what they say. Within this approach encounters are primarily made up by three to four hour weekly interviews with an interpreter.

The scheme of shared social experience put forward here calls for a further deconstruction of the social encounter and although Crapanzano has invested much space in the book on this theme, many critical questions remain unanswered. First of all, why did Tuhami's practical concerns not interest Crapanzano? Why did he wait 10 years to write the monograph, having then to rely on notes for Tuhami's story and memory for his reflexivity? Why was Tuhami not allowed to ask questions of Crapanzano? It is therefore not surprising that Crapanzano stresses that his writing is evocative rather than informative. In this enterprise his own reflections take up much space at the expense of practical concerns.

The willingness to engage with another world, life, or idea (Wikan 1992: 463) allows us to give in to the drama of the field, to suffer the emotional costs that arise from having to leave the field but without resorting to "supply befriending" for the sake of information. One central axiom of shared social experience can then be formulated as a rule-of-thumb in the words of Fredrik Barth: "Any existential human problem will have found diverse solutions, which must be worth knowing about, thinking about, and comparing" (Barth 1994: 351).

Mutual engagement and openness to the concerns of others was a necessary condition of our fieldwork situation because we brought our three small children to the field. We ran hundreds of errands in the community and had tasks to do that were not directly guided by ethnographic interest as such, but rather involved providing the basic necessities for the "survival" of the family. The families of the two Maya women in our household became our first important acquaintances; we become particularly engaged in the daily concerns of the poorer of the two families, mainly because they were constantly struck by accidents and illness and came to us for assistance. This family consisted of parents, two sons and eight daughters; three of the children lived in Oxkutzcab.

One day, don Iz complained to me about his severe stomach ache. What should he do? He could not work and the family was ruined financially from paying other medical bills. He did not know whether to go to the modern clinic or a traditional Maya *jmen* (shaman). A few weeks earlier don Iz had been to a *jmen*, but the herbal medicine had not removed the pain. In a conversation of what I have called "shared reasoning"—which I take to be a specific example of shared social experience—we drew on our various cultural models about illness and healing searching for guidance from our different experiences (Hervik 1994). During our talk, I remembered that my wife had suffered similar pains a long time ago, and which had been diagnosed first symptoms of a stomach ulcer. Therefore, I asked about his son, who was his co-worker in both farming and odd-job pursuits —a potential area of stress. My question triggered both despair and anger about the son's lackadaisical attitude towards the grave situation of his parents. Furthermore, a daughter and her son lived at home without contributing in any way to the household economy or chores. This daughter had previously run away for several months with her fiance. After marrying this man and having a child with him, he decided to abandon her. When she returned to live with her parents she felt that she could not cook or wash clothes due to pains in her

chest. Don Iz's complaints became more intimate and revealed anxiety that according to my cultural understanding might have caused stomach ulcer. In the end, we decided that my wife should join us for a new visit to the *jmen*. The *jmen* could then pose questions directly to my wife about her pain and then from hearing about her experience hopefully arrive at a more accurate diagnosis for don Iz. As it turned out, the *jmen's* wife was sick keeping him away from any consultations for several months, and we had to leave. However, on a later visit, don Iz told me that he did finally see a *jmen*. With the help of the additional information on my wife's experience with her stomach ulcer the *jmen* was in fact able to compose a more accurate medicine and soon after don Iz's pain subsided.

In this case of shared reasoning it does not make sense to speak only of individual reflexivity. Analytically the experience of the two participants can be separated, but empirically they cannot. Reflexivity in the form of shared reasoning was integral to the interaction. Don Iz and I were both aware of his suffering and we tried to formulate a plan for proper care and relief in a creative process. This process of developing an understanding involved semiotic mediation which is dependent on partly overlapping social experiences and utilizes available cultural models. Our collective work was built on mutual trust established over months of practical engagement in the welfare of each other and each other's families. This type of close relationship is of course not unique to my fieldwork, but it has been ignored and downplayed in previous works on reflexivity. Crapanzano, for instance, was not interested in Tuhami's practical problems and concerns.

Reflexivity is not solely a matter of speculating about the mode of production of a particular text or other kind of scientific account; it is part of the human social condition in which scholars must abide as well. Mutual reflexivity is the fundamental mode of shared reasoning; and reasoning about the world must always be shared (cf. Hastrup and Hervik 1994a).

We can now speak of two different types of practical reflexivity. On one hand, there is shared reasoning which is a reflexivity born out of interaction and exchange. Native reflexivity constitutes a second type. That is the reflexivity of people in the field, people who do not write ethnographies themselves, but whose reflexivity and agency that forms the bread and butter of ethnographic knowledge. Elsewhere, I have described an example of the reflexivity of Elena who told people that we kept a shotgun to protect ourselves and her. She knew we did not keep any weapon, but she felt it prudent that people was led to believe we had a gun and could defend ourselves in this way. I argued that her reflexivity did not revolve strictly around herself, but embraced a situation of which she was a part but which also included the our entire household (Hervik 1994). Both sorts of reflexivity are important in anthropological knowledge.

To further explicate the difference between shared social experience and social experience, we may say that shared, in the sense proposed here, implies more than mutual presence, interaction, and common interpretation. It requires that we "attend to people's multiple, simultaneous, compelling concerns and to follow them, as they move, bridging scenes and encounters, if we are to grasp what is at stake and how they, people in various positions, feel-think and act" (Wikan 1987: 291). We can not separate emotional and mental involvement in our relations in the field; it would be bad faith to try. Moreover, in many situations, friendship is a necessary condition for having access to personal knowledge. This condition will always carry with it the dilemma of whether you should use personal and intimate knowledge gained through friendship in scientific publications and at what cost. This dilemma is not unique to anthropology but is common to the whole field of fiction, the humanities, social sciences and sciences.

Social experience implies, as noted in the previous section, the co-existence of two or more people in a specific social space in which the individuals each occupy their own unique variant of cultural models. The latter are social in nature rather than individual, but they come from different social worlds. Shared social experience, on the other hand, implies more than co-existence. It implies mutual engagement and a willingness to give in to the situation and allowing oneself the potential to change in the process. This is precisely what I believe Crapanzano failed to do as he plainly admits himself (1980: 139). The postmodern moment caught up with him years after his meetings with Tuhami making him unable to distinguish between the actual encounter and the literary reencounter.

The term "shared" then means something different in American popular usage where sharing may be social in the sense of simply telling or making avaliable for an audience irrespectable of the kind of relationship that exist between the storyteller and the listeners. In shared social experience, sharing means that some basic sense of identification and prior relationship to the person with whom one is engaged is required. It does not require that people identify the situation in the same way, or share the same interpretation. In the case of a person speaking to a group as one of its members, it is likely that participation will generate a shared social experience with some members of the congregation although this is not necessarily the case. Sharing as in shared social experience implies that we are part of the social experience ourselves, and it is this process of sharing that provides us with the unique entry into an understanding of how shared moral spaces are constituted and transformed (cf. Hastrup 1995a).

According to the notion of shared social experience, one inevitably begins any social interaction with one's own subjective experience. As the cultural contrasts become apparent to the anthropologist, his or her experience becomes identified as a social experience which is framed by culturally informed background knowledge acquired in a different social world. When we become aware of the predispositions that we take into the encounter and as we gain skills in local ways of enacting, we can gradually begin to abandon our ethnocentric view of the world as the yardstick for viewing others. From here on, the road is assessable for shared social experience with others.

The discussion of shared social experience raises a question as to the degree of sharing that can come about between people. Maya people from across the entire Mesoamerican area repeat a relevant maxim "Each person is a different world, thus you can never really know the thoughts, reasoning and motivation of another person." It seems clear that we cannot ever be sure what other people's "experiences" and understandings are. However this is hardly an argument for giving up the anthropological project. The process of trying to understand other people's experience will itself reveal important solutions to the practical and existential problems that arise in communication. Still, we need continually to discuss the discipline's professional limits as to when we can say we have reached an acceptable depiction of other people's experiences.

The questions of the limits to how close we can get to other people's understandings is discussed critically in several articles in Hastrup and Hervik (1994b). I shall draw from Tamara Kohn's discussions on newcomers.

The young incoming brides among the Yakha of East Nepal do not speak the language of their husbands' families when they move into their households. Through this practice of linguistic exogamy the brides share daily tasks and traditions without being able to speak the language of the household. The other newcomer, anthropologist Tamara Kohn, also lacked competence in the Yakha language at the start of her fieldwork. Without linguistic competence she felt unable to begin her fieldwork, because she was taught by her university professor that learning culture is a process of learning emic categories and their meanings. Nevertheless, Kohn found that she did acquire knowledge from the start and her incoming bride friends claimed that it took them several years before they could speak Yakha properly. Much, if not all, of the initial knowledge of both the brides and Kohn was embodied rather than verbal. Taking part in local daily chores along with incoming brides Kohn learned to like the tastes of certain foods and even to judge whether or not a dish was prepared well. However, in tasting from the same bowl of rice, it was impossible for Kohn to know to what extent her experience of the taste was shared by her companions. Kohn could learn to respond to smells in the kitchen in a culturally appropriate manner and to embody the sense of that space, without ever knowing through linguistic or any other means the tastes and the sensual experience of the others. The experience of others can, she concluded, never be completely shared; it can only be imagined. Nonetheless Kohn goes on to argue that: "This is not to say that we cannot learn deeply about other cultures and other selves" (Kohn 1994: 21). To bridge the differences between two different cultural worlds, or two or more parts involved in shared social experience, we rely on imagination (Hastrup 1993a). According to Carrithers, the best way to "imagine" people is to engage personally with them:

> as an accountable member of the social setting. This is often called "participant observation," but I would rather call it 'engaged learning' in order to capture the inescapably sticky and involving nature of the process. Anthropologists learn how people judge each other by being judged themselves, or by being so closely a part of the scene that they react directly, intimately and inwardly, often with discomfort and perplexity, to people's judgments of each other (1992: 148).

Imagination in return is guided by signs, available cultural models, and the degree of affect (Holland and Valsiner 1988). Signs (or mediating devices) are here understood in the Vygotskian sense as the cultural historical products that individuals encounter in social interaction, and which become tools for construction and for reflection as they are internalized to organize knowledge about one's self and various aspects of the world (cf. Holland and Skinner 1994). Adopting this analytic scheme within the realm of practice theory allows us to avoid monolithic descriptions through the focus on the level of collectivity and how it is produced and reproduced through mediating devices.

The notion of shared social experience makes a point out of stressing the personal and emotional investment involved in the ongoing attempt to identify with and understand other people.

REFLEXIVITY AS AN AFTERMATH OF SHARED SOCIAL EXPERIENCE

If experiences are what we live by, we need to express them as part of ethnography (Bruner 1986: 29). But to express experience in language is always to objectify it. Experience is difficult to render in words—words seem to betray endlessly what they seek to represent; as the Russian saying goes "An idea once expressed is a lie" (cit. in Friedrich 1992: 214). Verbal and written expressions are a part of the category of mediating devices or signs that turn back and influence experience; the use of expressions enables (and restrain) humans "to affect their own mental states and gain some degree of self-direction. Without this ability we would be subject to whatever stimuli appeared in the environment; we would lack 'agency' " (Holland and Skinner 1994).

Shared social experiences, like all experiences in fieldwork, draw on the whole being including all the senses. Much knowledge is embodied and out-of-awareness, only to become the object of reflection when visual imagery, smells, tastes, sounds, and fieldnotes trigger the embodied knowledge and make it available for further reflection. For embodied knowledge to become a part of the anthropological product, shared social experiences must be related to a general field of knowledge.

The scientific luggage, carried in memory and in the form of written material, is part of the asymmetry between the ethnographer and the people in the field. The ethnographer delineates a geographic locality as a social space; the general object of study. For him or her it is a "remote area:" "a specification, and a perception, from elsewhere, from an outside standpoint; but from the inside the people have their own perceptions" (Ardener 1989: 221). The native actors, always embedded in this social space, only intermittently perceive it as an object.

> For the non-native social anthropologist the act of interacting with an alien social space, even relatively successfully, forms the basis of that 'daily experience of *mis*understanding' (and not only the ethnographic level but the theoretical level) which is the undoubted source of our greater readiness to see the space as object (of study), and thus, like Durkheim, to see 'social facts as things' (Ardener 1989: 212).

The material and mental luggage we carry to the field further marks this space as an object. Especially, when a social interaction is 'over' it can be turned into an object of reflection. An important part of fieldwork is the short periods of detachment from the stream of events and interactions (see Kondo 1986). This process of turning past experience into an object for the powers of imagination to work on reveals an important part of the ethnographer's reflexivity as an aftermath of experience.[6] Put more generally; "Social science is reflexive in the sense that the knowledge it generates is "injected" back into the reality it describes" (Wacquant 1992: 36).

The aftermath of experience continues after the anthropologist leaves the field. The encounter with the folklorists studying the

"Dance of the Pig's Head" is an example that can clarify this point. It was only much later that I grasped the complex and unique nature of observing Michael, the folklorist, recording on video an interview with three ritual specialists in the evening of the "Dance of the Pig's Head." At the time I was completely absorbed by the episode and the setting. A tall blond researcher swayed his camera back and forth while giving instructions to the interviewer, a broker friend of his.

The episode came under intense scrutiny at a time when I had been encouraged by my dissertation supervisor to explain my approach to the "Dance of the Pig's Head" in writing. The participation of another scholar became a heuristic device to elaborate what I at the time coined "participant observation as a goal in itself" (Hervik 1992b, 1995) whilst being reflexive about my own participation in the celebration of the Pig's Head, town life forced me to think thoroughly through my relations with other participants (Okely 1992: 24), including the folklorist, the broker, and other important participants and the imprint of these relations on the production of anthropological knowledge.

Reflexivity is a cognitive reasoning process that influences the internalization or enculturation of cultural knowledge embedded in social practice. The substance of reflexivities comes from shared social experience and from embodied knowledge absorbed through social interaction. Anthropological reflexivity is different from native reflexivity in that it is partly informed by a "scientific habitus" consisting of analytic categories learned within an academic field.

I find much resonance with Bourdieu's recent work on reflexivity (Bourdieu and Wacquant 1992). He targets reflexivity at a point beyond the individual particularly aiming at the social and intellectual unconscious embedded in analytic tools and operations (Wacquant 1992: 36). The reflexivity advocated by Bourdieu includes "the systematic exploration of the 'unthought categories of thoughts which delimit the thinkable and predetermine the thought' . . . as well as guide the practical carrying out of social inquiry" (Wacquant 1992: 40).

I find support for my position in his critique of textual reflexivity. He writes that, infatuated by the hermeneutic process of cultural interpretation in the field and the remaking of reality through ethnographic inscription, textual reflexivists spin their reflexivity around the author and think the natives are engaged in the same game of interpretation (Wacquant 1992: 41–42). He criticizes textual reflexivity as containing an intellectualist bias "which entices us to construe the world as a spectacle, as a set of significations to be interpreted rather than as a concrete problems to be solved practically..." (ibid.: 39).

However, I am uncomfortable with the interpreters of Bourdieu who write "The habitus is self-reflexive in that, each time it is animated in practice, it encounters itself both as embodied and as objectified history" (Postone, LiPuma, and Calhoun 1993: 6). This view treats reflexivity without referring to the reflexive practitioner. The collective has no mind and habitus does not exist independent of persons, therefore it is not the habitus which is self-reflexive, but the actor who uses the cognitive scheme, habitus, to act and reflect. At times the reflections may target the habitus, but most often habitus is embodied knowledge that lies out-of-awareness unable of being self-reflexive. Another commentator claims that the scientific habitus differs from other kinds of habitus in its reflexivity, because it contains a disposition to monitor its own productions and to grasp and make explicit its own principles of production (Brubaker 1993: 225). In spite of the greater readiness to see the fieldsite as an object, which in general characterizes a "remote area" (Ardener 1989), I am far from convinced about the workings of such an inbuild disposition to evoke a self-reflexive scientific habitus which is unique to the ethnographer. Perhaps, this is a misreading of Bourdieu. According to Bourdieu and Wacquant, even the self-reflexive individual can hardly escape or control the first inclination of habitus (1992: 136). They go on to state that

> . . . reflexive analysis, which teaches that we are the ones who endow the situation with part of the potency it has over us, allows us to alter our perception of the situation and thereby our reaction to it. It enables us to monitor, up to a certain point, some of the determinisms that operate through the relation of immediate complicity between position and dispositions (ibid.).

Generally, the only concrete reflexive agent in Bourdieu's work is himself in his capacity as scientist. The reflexivity that Bourdieu

discusses deals almost exclusively with the social and intellectual unconscious in the practice of anthropology. Unfortunately, Bourdieu does not direct his reflexivity to the social and political relations with friends and informants in the non-academic fieldsite. Moreover, to my knowledge, he does not refer to particular people who reflect and are reflexive in his publications (cf. Quinn and Strauss 1998).

Scientific habitus with its inherent reflexive disposition is different from the reflexivity of native actors. Gusdorf argues that autobiography, which is another kind of reflexivity, is tied to Western individualism and since individualism in the Western sense is not found outside our culture area there is no reflexivity outside our cultural area (cited in Okely 1992: 6). Okely argues against this by claiming that autobiography has been the privilege of power; The privilege to be "individual." Individualism can therefore not be isolated to the Western world. Women and minorities have not the luxury to be "individual" and publish their own reflexivities (ibid.).

We are now in the position to argue that the current practice of reflexivity—that spins strictly around the author, his or her production of texts, and the inclusion of self in the text—is a Western privilege and by no means the only form of reflexivity. Furthermore, we argue that the idea of a self-reflexive habitus can only be maintained and practiced from a privileged position, where it will not be contested.

Forms of reflexivity are also to be found outside written autobiographies, but they are widely different for women, minorities, and many non-Western peoples (S. Friedman cit. in Okely 1992: 7). The native reflexivities I have touched upon here that emerged in shared social experience involve personal agency and express conscious reflections of the individual about his or her situation including relations to others as well as the ethnographer.

CONCLUDING REMARKS

Reflexivity can focus on the production of the text, but it might focus on other processes as well. In the above I have explored reflexivities that not only take place before texts are written but are also always a condition of the practice of anthropology. These reflexivities can no longer responsibly be taken for granted or ignored. The extent to which the ethnographer is part of the text *and* the object of the ethnography must be addressed critically and with the same rigor that applies elsewhere in the discipline. Native reflexivity, reflexivity stemming from a common-being-in-the-world, and the ethnographer's reflexivity depend on shared social experience. Shared social experience, I argue, is the only locus of study and methodological principle for studying co-existing reflexivities. Furthermore, it depends on a willingness to personal engagement. Shared social experience is not: a blueprint to use others to trigger one's fantasies, or to be confused with; the conventional autobiographical focus on public success or failure of the lone hero; or, mistaken for diary—the personal articulation of dailiness; it is not about the author in the field; strictly speaking it is not the anthropologist's biography of others; or to be confused with dialogues as such; the words of other's (see Okely 1992); or, *post festum* reflections on fieldwork. These potential misunderstandings of shared social experience are rooted in an narrow concern with individuality and the individual production of knowledge. The concept of shared social experience is based in a commitment to sociality and the collective production of knowledge. It is a notion that embraces emotional, political, and intellectual properties of interaction—which are accumulative over time in any relationship—and built from a direct access to personal knowledge. Personal knowledge is a prerequisite rather than a tool of research. Shared social experience is our only means of access to different types of reflexivity which will enrich our understanding of local practices. Moreover, reflexivity is the process through which we transform experience into knowledge.

On one hand there are native reflexivities that may not be written down but are nonetheless objectifications of the origin of their own knowledge and that of the anthropologist. On the other hand, there are reflexivities that occur within the interactions, as shown in the case of "shared reasoning" with don Iz. Still further, there is the reflexivity of the anthropologist as scientist which is the "problem-solving-kind of thought" where the anthropologist systematically reflects upon a certain experience drawing upon all bodily senses, the available cultural models, as well as the anthropological theories learned prior to fieldwork. This latter kind of reflexivity is a type of

cognitive skill of reasoning that ties social experience to anthropological knowledge.

If our only route to the reflexivity of others is shared social experience, we may ask new questions not only as to what extent we can really share the social experience but also the extent to which we can be reflexive about habitus—the out-of-awareness dispositions.

In Brubaker's recent formulation: "how can scientists do what other agents can't; consciously master their habitus without interfering with their workings? In what sense can we speak of an unconscious disposition towards conscious self-scrutiny, an unreflective disposition to reflect?" (1993: 225). I believe shared social experience can contribute to answering these questions in its insistence on allowing ourselves to give in to the drama of the field and become changed in the process. Then, only by working through our subjectivities and deconstructing the cognitive cultural framework in which we usually posit ourselves as the prototypes of humanity—they are more like us, than we are like them—can we embody the habitus, reflect upon it and the cultural constructions of other people, and abandon Western society as the yardstick for the rest of the world.

NOTES

[1] The reflexivity continuum—from automatic inborn cognitive reflex to self-reference over self-awareness to reflections of the self reflecting—can also be expressed in terms of "reflex" movement, which in classic non-metaphoric meaning would mean simply "turning" or "bending back," whereas the metaphor of the mirror allows a lingering on the meaning of "think more thoroughly," "think through," "look at," or "examine more closely." To "reflex" would be the automatic bending back, while "reflect" would imply the application of consciousness. Such a division is contained in German within the word "reflexion." The term reflexivity would not translate readily into current use of "reflexivity" in anthropology with its bias towards the media of "thought" and "language." If "thought" is articulated in the language medium, then reflexivity would remove us one step further away from consciousness. Consciousness of being self-conscious (Babcock 1980:2). Or, the activity indicated by the term: thinking about thinking, consciousness about being conscious, etc. (Ruby and Myerhoff 1983: 17).

[2] The English word "experience" has several inherent submeanings such as incidents (event experienced), something lived through, accumulated practical skills and knowledge, or knowledge gained through participation or observation (Webster's Collegiate 1989). In German and Danish (and other Scandinavian languages) experience translates roughly into *"Erlebnis"* (or *"oplevelse"*) and *"Erfahbrung"* (or *"erfaring"*). The English term "experience" is thus broader than the German and Danish.

[3] This is contrary to Bruner and Turner (1986). They rely heavily on Dilthey's notion of inner experience: "Reality exists for us in the facts of consciousness given by inner experience." (Dilthey cit. in Bruner 1986: 4). In anthropology, they take experience to deal "with how individuals actually experience their culture, that is, how events are received by consciousness" (ibid.).

[4] Personally, I am not taken in by this trick. In fact, I think few anthropologists are really deceived by these literary devices. Upon reading the arrival story that is part of a monograph—or about the everyday life situations with the ethnographer in an embarrassing encounter—the reader will already be somewhat familiar with the writer's sex, age, status in the profession, and alleged membership of a religious, ethnic, sexual, or other group. The arrival story then adds to the reader's biographic knowledge. This knowledge forms part of a general pool of knowledge used to determine whether a given book is convincing. Even if literary device makes the reader believe you more or less, such a belief is beyond the control of the author. Hence, I argue against overstating and overdoing the consequences drawn from the analysis of arrival stories and everyday situations.

[5] If we focus on "inner experience" exclusively we will not be able to account for how (social) experience frames social relations and social practices and vice versa (cf. Widlok 1994).

[6] This is not to be confused with a strict textual reflexivity. Upon returning to our house, I would engage in speculations about participation and understandings with my wife, who is also an anthropologist. Insight gained from these conversations in the detached space of the home could then be tested almost immediately in new social encounters.