Reader

The following references are a selection of texts dealing with the main issues we will focus on during our training sessions. They are articulated in two sections.

The first section concerns mainly methodological issues about fieldwork practices and data collection. The texts are rather general (OLIVIER DE SARDAN, WERNER & RUSSELL), focusing on specific tools we will use, as biography/oral history (PORTELLI), participatory mapping (SLETTO) or participatory research with “natural” or “more-than-human” (NOORANI & BINGSTOCKE). This section is the same of the Reader for the 1st Training Session.

The second section concerns the sociological or thematic context we will focus on during fieldwork. The texts are about customary law and land tenure (BABIKER), agricultural “modernization” in Jezira area (BERNAL), ethnicity and labour division (O'BRIEN), Sudanese pastoralists (CASCIAIRRI & AHMED), history of the Ashraf and tariqa Al-Hindiya (BARCLAY) that we found both in Sharafa and Burri (for this read again the excerpts by H. Barclay 1964 in the 1st Session’s Reader).

Methodology and fieldwork


Fieldwork context and topics


CHAPTER 2

THE POLICY OF FIELDWORK: DATA PRODUCTION IN ANTHROPOLOGY AND QUALITATIVE APPROACHES

INTRODUCTION

As mentioned in the introduction, sociology, anthropology, and history share a common epistemology. Interpretative methods, hypotheses, heuristic choices, paradigms, and processes of object construction are usually similar or transversal. However, these disciplines do not necessarily produce data in the same way. Though close to one another, each seems nonetheless to have its own favorite form of empirical inquiry. Archives for the historian, inquiry through questionnaires for the sociologist, “fieldwork” for the anthropologist: at first glance, these three modes of data production appear to be linked, respectively, to each of these related social sciences.

Granted, this is merely a question of the dominant characteristics of each discipline. And borrowing from the neighbor is quite common. Fieldwork, in particular, has sometimes acquired a prominent role in sociology.¹

A bit of clarification may be helpful at this point. The term “qualitative methods,” while reasonably common, comes with a number of disadvantages, especially that of implying that qualitative methods are not concerned with scales or figures. This is incorrect (see below what I call the “procedures of intensive survey”). Conversely, defining sociology through questionnaires as “quantitative sociology” incurs the risk of wrongly implying that it is clear of value judgments and nonquantitative interpretations and is based solely on statistical data.
Moreover, and above all, quantitative and qualitative methods both employ a series of shared, fundamental intellectual operations, such as the elaboration of research problematics, the use of scholarly references, and theoretical and interpretative arguments. Nevertheless, quantitative and qualitative methods follow different paths, produce different types of data, and offer distinct pictures of the reference reality. The obvious difference between inquiry through questionnaires and inquiry through fieldwork needs to be taken into consideration. These are two poles or two ideal types (fortunately, intermediary or combined forms do exist, as do fanatics attached to one method or the other). They differ as much in their respective modes of data production and in the characteristics of the data they produce as in their approach to the problem of representativeness. Surveys conducted through questionnaires gather limited and codable information based on statistically representative samples. They operate within the artificial context of questioning in which answers are recorded through the mediation of paid inquirers. Anthropological fieldwork, on the other hand, aims at getting as close as possible to the natural context of the subjects involved—everyday life, conversation—in a situation of prolonged interrelations between the researcher in person and the local populations. The objective is to produce in situ, contextualized, transversal knowledge, accounting for the “actors’ point of view,” everyday representations and practices, and their indigenous significance. Statistical inquiry is extensive while fieldwork is intensive, and each possesses inverse advantages and disadvantages.

Table 2.1 Field inquiry and inquiry through questionnaires

<table>
<thead>
<tr>
<th>Field inquiry</th>
<th>Inquiry through questionnaires</th>
</tr>
</thead>
<tbody>
<tr>
<td>broad, multidimensional, noncodable information</td>
<td>circumscribed, univocal, codable information</td>
</tr>
<tr>
<td>artisanal processing</td>
<td>statistical processing</td>
</tr>
<tr>
<td>natural situations or situations close to natural situations (conversation)</td>
<td>artificial situations (interrogation)</td>
</tr>
<tr>
<td>researcher in person</td>
<td>paid investigators</td>
</tr>
<tr>
<td>immersion in the milieu</td>
<td>sporadic interaction</td>
</tr>
<tr>
<td>information on contextualized processes and logics (without statistical representativeness)</td>
<td>information on decontextualized variables (statistical representativeness)</td>
</tr>
<tr>
<td>intensive, duration, returns</td>
<td>extensive, brevity, linearity</td>
</tr>
<tr>
<td>research trails, iteration, improvisation</td>
<td>verifiable, precise hypotheses</td>
</tr>
<tr>
<td>many intertwined variables</td>
<td>dependent/independent variables</td>
</tr>
</tbody>
</table>
Indeed, the ideal inquiry should combine these two approaches, and many voices are now raised in support of mixed method research.\textsuperscript{5} But the prerequisite of various complex skills, as well as the dynamics of university careers, makes it particularly difficult for a researcher to master these two types of savoir faire.\textsuperscript{6}

Moreover, the multidisciplinary teams rightly advocated over the last 30 years at least by research policy rhetoric collide with the tremendous issue of leadership (who holds the reins of scientific and institutional power?) and of the collaborative elaboration of research problematics (how to effectively work together)\textsuperscript{7}. Thus, most collaborative experiments of this type are condo-type arrangements (each one manages his own apartment) rather than co-ownership operations. Be that as it may, this part of our discussion will focus on the qualitative aspect—and on its emblematic form, namely, anthropological (often called “ethnographic”) fieldwork.

There exist numerous courses and manuals on quantitative methods, but for nonpractitioners fieldwork methodology seems unclear. Owing to this opaque or mysterious nature of data production in the field, anthropology, seen from outside, remains the most misunderstood, the most fascinating, and the most disputed of all social sciences. Anthropology is often credited with empathy and the anthropologist lauded for his experience. On the other hand, both are often accused of impressionism and subjectivity. Certain irritating and even grotesque aspects of the myth surrounding fieldwork make matters worse. One example is that of the anthropologist posing as a hero and dramatizing the difficulties encountered in the field.\textsuperscript{7} Yet field inquiry is only one of several modes of data production employed in the social sciences. Like all the others, fieldwork has its advantages and disadvantages. It possesses specific modes of methodological vigilance and stands to gain from an explanation of the “principles” based on which it works. Hence, dissipating the “vagueness” surrounding fieldwork is an issue that must be addressed.

Each method has a particular type of rigor, a specific means of making data valid or plausible. But the seriousness of field data cannot be converted into statistics, as opposed to the seriousness of data obtained through inquiry via questionnaires. Of course, quantitative data are not devoid of bias—quite the contrary, and we may remember “Coase’s statement that the [statistical] data will confess if you torture them long enough” (White, 2009: 8). But this could also be said about qualitative data. Both types of data imply rigor on the part of the researcher and scrutiny on that of the reader. The practice of fieldwork in anthropology requires training and competence. The problem
is that this competence is a matter of savoir faire, and the training required is a process of hands-on learning in the field. In other words, field inquiry cannot be learned in a textbook. There are no set or formal procedures to be observed, as is partly the case with so-called “quantitative” inquiry. Fieldwork is above all a matter of know-how; it uses intuition, improvisation, and tinkering (bricolage). The “initiatory” character of fieldwork, often mentioned in sarcastic comments on the anthropological tradition, is not simply a myth or merely a ritual. It is also, above all, a matter of apprenticeship. And the apprentice learns mainly through practice. The researcher who has conducted inquiries based on a ready-made questionnaire readily notices the extent to which inquirers are inhibited by an excessively narrow or directive framework. Confrontation with the countless misunderstandings between the inquirer and the inquirer highlights the *quid pro quos* that pepper this type of research situation. Only after he has mastered the local codes of civility and decorum can the field researcher finally relax during an impromptu conversation, which is often the most productive method of gathering information in the field. The researcher in the field begins by improvising awkwardly and gradually learns to improvise skillfully. It is only after wasting a tremendous amount of time in the field that one finally understands that these downtimes were indispensable. “Learning to be a field researcher poses the same problems as learning to live in society” (Hughes, 1996: 279).

The paradox in what follows results from the attempt to give a written account of a compendium of “tricks of the trade” picked up in the field while addressing a type of scientific rigor that can only be acquired through practice, without the assistance of a standard reference situation. Should we take the risk of venturing into this intermediary zone between epistemology (whose enunciations, however apt, do not help to acquire the required competence: of what practical use are Sperber’s interesting texts?) and methodology (against which ethnographic practice seems to rebel: efforts at proposing “a methodological kit” soon become ridiculous)? Abstract theoretical thought and a cookbook are worlds apart. My aim is not to fill this median vacuum but simply to propose a few points of reference. This requires an analytic review of the primary modes of data production specific to fieldwork. The perspective will then be widened in view of sketching a “policy of fieldwork,” captured along the road of its bumpy quest for plausibility. In other words, what will be highlighted are some of the practical conditions of this anthropological validity, i.e., the efforts to reveal—despite the multiple “biases” that permeate inquiry, and through their management—this methodological requirement, this targeted “qualitative rigor.”
The Six Types of Data Production in Fieldwork

Field inquiry (i.e., anthropological inquiry, or qualitative inquiry) rely, roughly speaking, on the combination of five and sometimes six major forms of data production:11

1. The relatively prolonged immersion of the inquirer in the social milieu of the inquirees (often called “participant observation”), forming the general framework of the inquiry.

In this context of participant observation, researchers usually produce four sets of specific data:

2. Interviews (discursive interactions deliberately provoked by the researcher)

3. Observations (seeing and describing clearly determined social sequences)

4. Intensive microsurvey procedures (recourse to devices constructed with the aid of systematic or semiquantitative inquiries)

Figure 2.1 Data production
5. Written sources—these raise certain extremely specific problems and are rarely used.

6. A final type of data must be mentioned: audiovisual data

**Immersion in the Milieu (Participant Observation)**

Whether the often contested term “participant observation” is suitable or not is of little importance. Its connotation is quite clear. Thanks to a prolonged stay among the people he intends to interview (and by learning the local or professional language if he does not understand it), the anthropologist enters into physical contact with the reality he wants to study. He is thus able to observe, if not from the “inside” in the strict sense of the word, at least at close quarters, those who live this reality and to engage in sustained interaction with them.

The forms of immersion in the milieu are varied (they will be discussed in Chapter 5). Following Gold (1958) and Junker (1960), it is therefore possible to differentiate between “the complete observer,” “the observer as participant,” “the participant as observer,” and “the complete participant.” But, regardless of the role he adopts, more or less deliberately, the researcher maintains direct albeit highly variable contact with the group and/or the processes he is studying.

This sociological or ethnographic “immersion” is, at once, the context of data production and a very particular mode of data production. The information and knowledge acquired can, on the one hand, be systematically recorded by the researcher, who takes advantage of his presence on the spot to interview, observe, note, count. On the other hand, knowledge and information may also remain latent and be, as it were, “assimilated” into the researcher’s presence, through his memory, perceptions, emotions, and unconscious mind. When produced and recorded more or less deliberately and systematically, observations and interactions are transformed into data, compiled, and organized within a corpus. Otherwise, they still play an important role, related to impregnation.

**Data and Corpus**

Much of the field inquirer’s time is devoted to interviews, which remain the researcher’s favorite mode of producing information. These interviews are turned into data through note taking or transcription, in the extremely material shape of notebooks (and, nowadays, as texts saved on a computer). A corpus of discursive data is thus elaborated.

It is often fairly challenging to pin down data derived from observation. Observation may be transformed in written descriptions of
real-life situations, sometimes written on the spot, sometimes later. Sometimes data from observation focus on particular social sequences, whereas in other contexts they are diffuse, sporadic, and indeterminate. But this type of data is also, in one way or another, ultimately transformed into written corpuses (notebooks or computer files).

Counting, measurements, diagrams, plans, intensive surveys, inventories, and various other connected procedures offer another type of corpus, which we may term intensive microsurvey procedures.

Written documents derived from fieldwork and audiovisual materials produced or collected by the researcher form the last two types of corpuses.

These five types of corpuses (discursive, observatory, intensive microsurvey, documentary, and audiovisual) will be examined in greater detail.

The common denominator of all data thus produced is that they arise from interactions between the researcher and the social milieus he studies. They give rise to handwritten or electronic traces, which may be treated and analyzed at a later date. Each corpus is created based on procedures combining improvisation, coherence, and systematic methodology. Improvisation is inherent to fieldwork, but so is rational selection.

Obviously, data, as defined herein, do not refer to illusory “fragments of reality” gathered and conserved in their original state by the researcher (this is a positivist illusion). Being neither figments of the researcher’s imagination nor products of his sensibility (a subjectivist illusion), data are the transformation into objectified traces of “fragments of reality,” as perceived and selected by the researcher.16

But their intelligibility implies that a conceptual language of description is already “available.” This is what Passeron underlines, with reference to Bachelard: the “epistemological vector” goes from the rational to the real, and not the other way around.17 In the absence of preliminary questions, intellectually preprogrammed curiosity, and initial problematics, the researcher cannot produce significant data. Pure, naïve observation is a fallacy, and listening is always purposeful. It is a well-established fact that a researcher’s observations are structured by his research questions, language, problematic, training, and personality. However, the “empirical aims” of the anthropologist should not be underestimated. The researcher’s yearning for knowledge and his research training may, in part, override his prejudices and feelings (empirical social sciences would be totally impossible otherwise).18 Owing to observation, preliminary problematics may be modified, discarded, or expanded. Fieldwork is not simply the coloring in
of a ready-made drawing. It implies subjecting preconceived curiosity to the test of reality. The competence of the field researcher consists in his ability to observe the unexpected (although the prevailing tendency is usually the discovery of the expected) and in his ability to produce data that impose an alteration of his own hypotheses. Field inquiry must attempt to contradict the Bambara proverb from Mali, according to which “a stranger sees only what he already knows.”

Impregnation

But the daily life (whether social, professional, familial, associative, or religious) in which the researcher participates, in one way or another, is not simply a fertile site supplying the sequences needed to produce corporuses and fill up notebooks.

The researcher is in fact continuously involved in a range of interactions. Far from being a mere eyewitness, he continuously engages in simple or complex, verbal or nonverbal social relations: discussions, chitchatting, games, etiquette, solicitations, etc. The anthropologist operates within the realm of ordinary communication; “he adopts the forms of ordinary dialogue” (Althabe, 1990: 126); he encounters local actors in day-to-day situations, in the world of their “natural attitude,” as noted by Schutz (1987). But many of the everyday conversations and activities in which the anthropologist participates are directly or indirectly related to his professional curiosity, i.e., to his research topic.

Hence, many local discursive interactions that occur in his presence are ones in which the researcher is involved minimally or not at all. The researcher is a voyeur as well as an eavesdropper. The dialogues that people have among themselves are as valuable as those that he has with them, and the everyday behaviors of local actors can be as relevant as the blatantly visible rituals and spectacles that occur.

These forms of impregnation are, in a sense, “close to the corpus,” insofar as the researcher therefore tries, whenever useful, to turn relevant interactions and observations into data, i.e., to organize their transcription, description, and record as fieldnotes, whether or not these interactions hinge significantly on the role assigned to the anthropologist in the local arena.

So, these fieldnotes, to which the researcher might become obsessively attached, and which are sometimes invested with a mythical character they hardly deserve, have nothing to do with the personal diary, or the explorer’s notebook. They are simply a basic tool of the trade, wherein participant observation is transformed into data to be treated in the future. As reflected in the title of Sanjek’s work, “the
making of anthropology,” field notes are the stuff of which anthropology is made.20

The field researcher also observes and interacts without paying special attention, without having the impression of working, and therefore without taking notes, during or after his stay in the field. Happily, he does not always feel like someone on an official assignment. He eats, chats, jokes, flirts, looks around, listens, likes, dislikes. In the process of living, he observes, despite himself, as it were, and these observations are there, present, unwittingly “engraved” in his unconscious mind, in his subjectivity, in his “I,” or whatever we wish to call it. These observations are not transformed into a corpus and are not recorded in the field notebook. They nevertheless play an indirect but important role in “familiarizing” the anthropologist with the local culture, in enabling him to effortlessly decode other people’s actions and reactions. In other words, with their help, his interpretation of a given situation becomes almost a reflex. Many of the daily interactions in which the researcher engages are unrelated to the inquiry, are not consigned to the field notebook, and are therefore not transformed into data. This does not make them insignificant, however. Good neighborly relationships, the good spirits of nightly chats, jokes exchanged with the pretty neighbor, a round of drinks at the bar, or the festivities surrounding the baptism of the lodger’s child are activities that take place outside of working hours. But this is how one learns the codes of polite behavior (and this understanding of proper etiquette will impact indirectly and unconsciously but nonetheless effectively the way in which his interviews are conducted); this is how one gets acquainted with the runnings of daily life, how one learns what the spontaneous topics of conversation in the village or at the hospital are (and this will come to bear indirectly and unconsciously but nonetheless effectively on the way in which data relating to the inquiry are interpreted). This is what Erving Goffman underlines in his own terms: “It’s deep familiarity that is the rationale—that, plus getting material on a tissue of events—that provides the justification of such an apparently ‘loose’ thing as fieldwork” (Goffman, 1989: 130).

It is possible to see the researcher’s “brain” as a “black box” and forget the way in which it actually functions. Yet, all that he observes, hears, and sees in the process of fieldwork and in the course of interpersonal relationships will “go into” this black box, will impact the conceptualizing, analyzing, intuiting, interpreting machine, and will therefore, in part, later “come out” of the said black box and partially structure interpretations throughout the research process, either during fieldwork or during the exploitation of the corpus, at
the writing-up stage. This is the main difference—one that is particularly visible in descriptive works—between a field-worker who calls on experiences lived (through immersion and impregnation) and an armchair researcher working on the basis of data collected by others. This mastery of the sense system of the group under inquiry is unconsciously acquired, for the most part, in exactly the same way in which a person picks up a language through practice.  

**Interviews**

Data produced by the researcher, based on the indigenous discourses he has himself solicited, remain a central element of field research. First, this is because participant observation is an insufficient means of gaining access to a wide range of necessary information. It then becomes necessary to call on the knowledge or memory of local actors. Second, the representations of local actors are indispensable for social understanding. In a sense, the main ambition of anthropology is to supply the actors’ point of view. The interview remains the preferred and in general the most economical mode of data production, one that is capable of producing discursive data providing access to indigenous, local representations (sometimes described as “emic”; see Chapter 3). Notes obtained from the transcription of interviews are the mainstay of anthropological data.

In contradiction to what is often stated, I do not believe that interview “techniques” actually exist. This is not to deny the reality of a certain “savoir faire.” But I think that it would be more precise to refer to such skills as an “interview policy,” whose main lines will be described below.

**Consultancy and Experience**

Interviews generally fluctuate between two poles. Let’s call them consultancy and personal experience. The so-called informant is therefore consecutively a consultant and a narrator, usually both at once.

1. The interview usually involves social or cultural references regarding which the interviewee is “consulted.” Called upon to give his opinion on one subject or another, the interviewee is therefore deemed to reflect, to some extent, a common knowledge that is shared, if only in part, with other local actors, or possibly with all members of the given social group. What does he know about such and such a topic? What does he think about it? His “competence” on
the local society or his knowledge of one of its segments is solicited. Reference to the “competence” of the informant does not imply that the local society necessarily considers him to be an “expert” on society or even within his professional group. Nor does it suggest that we should accept the principle of a “key informant” construed as a learned or celebrated initiated person on whom the researcher may depend for what may later be presented as a “collective” narrative. The notion of “consultant” refers, in this case, to a specific discursive register concerning interview situations rather than to the specific status of the interviewee. In the same vein, the notion of “competence,” as used here, simply refers to the interviewee’s capacity to pronounce relevant statements about a referent partly outside of his own direct experience. It does not imply a value judgment of his level of knowledge. Every interlocutor should be regarded as possessing interesting knowledge about his group, profession, or society.

2. Nevertheless, the inquiree may also be questioned regarding his personal experience. He might be asked to talk about a slice of his life or to give an account of events in which he was an actor. In this case, the first-person narrative is preferred. One particular and systematic form of this is the life history or “guided” autobiography of the interviewee, who becomes the theme of the interview or even of the inquiry itself. However, the “slice of life,” i.e., the limited, episodic biographical narrative chosen in keeping with its relevance to the inquiry, is easier to access and to manage than an actual biography. Hence, depending on the research theme, the personal narrative fragment might relate a migratory experience, various therapeutic remedies used during illness, conversion to a new religion, the story of a divorce, the triggering events of a strike, or the stages of an apprenticeship. Such slices of life represent priceless data to the extent that they may be used at various levels: as a main corpus subjected to comparative analysis, as elements of a case study, or as examples illustrating or demonstrating arguments in the final text.

**Interview as Interaction**

An interview should not be considered as a mining-type operation aimed at extracting information. Regardless of the situation, the research interview remains an interaction: its proceedings obviously depend on the strategies of both (or all) of the partners in the interaction, on their cognitive resources, and on the context in which it is situated. This interaction may be analyzed from various angles.

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**The Policy of Fieldwork**

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The work of Briggs, for example, is based entirely on the observation of the interactive reality of the interview. He offers constructive criticism of the widespread omission of this interactive reality, denounces the myth surrounding interviews, and highlights the “illusion of reality” and the “false sense of objectivity” that this omission occasions. The cultural and linguistic characteristics peculiar to the context and situation of the interview give rise to various “biases” concerning the referential contents, which sociologists and anthropologists tend to interpret at face value. Conversely, Briggs insists on the fact that the interview is an intercultural encounter, more or less decided on by the inquirer, one that provides for the confrontation of dissimilar and sometimes incompatible metacommunicational norms. But he falls into the opposite excess by amalgamating various types of interviews in a bid to prove his point. By systematically emphasizing the analysis of metacommunicational norms and indexical significance, he converts all types of interviews into a corpus of sociolinguistic analysis. As a result (and this is also, incidentally, the trend in ethnomethodology), the referential functions of the interview, i.e., the relevant information on the reference reality derived through these “biases,” when all is said and done, are particularly neglected or played down. Yet the quest for relevant information lies at the heart of “field policy.” Taking the metacommunicational situation into account might be connected to two different strategies: (a) either it allows a better interpretation of elements of information; or (b) it constitutes an end in itself and disregards the contents of the elements of information. Only the first strategy is of use to the field researcher. Schwartz rightly underscores the risk of watering down the reference reality: “The fact that the ‘things that are said’ are not immediately true information about the world . . . is no reason to underestimate their informative or cognitive value” (Schwartz, 1993: 283–84).

Any interview is liable to call on three interconnected levels of interpretation, none of which is to be neglected despite the complexity of their constant embeddedness: (a) information on the world (on the reference reality); (b) information on the point of view of the interlocutor concerning the reality in question; (c) information on the communicational structure of the interview. In an ordinary inquiry, the focus is on the first two levels, and the third is relevant only to the extent that it influences the other two levels.

Moreover, by overemphasizing the hegemonic communicational effects induced by the interview, Briggs concomitantly underestimates the reactive capacities of interviewees (their ability to resist, sidetrack, or countermanipulate). The emphasis that Bourdieu and his disciples
place on the effects of domination inherent in the situation of the interview, given the difference in social status between researchers and their interlocutors who originate in more modest social milieus, goes in the same direction (see Chapter 5).

*Interview as Conversation*

Getting the guided interview closer to an ordinary interaction like a conversation is a common strategy of the ethnographic interview. The aim is to reduce, as far as possible, the artificial aspect of the interview and to prevent the inquirer from imposing annoying meta-communicational norms.

“Dialogue,” an ingredient of all conversations, is not seen here as an ideological obligation, contrary to moralistic postmodern discourses. Dialogue is a methodological constraint. It aims at creating, if necessary, a situation in which the anthropologist’s informant may enjoy real freedom of speech instead of the impression of being questioned by a judge or a policeman. In other terms, it is a question of getting the interview as close as possible to a mode of communication that is recognizable in the local culture. The field interview thus tends to be the opposite of the administration of a questionnaire—an extremely artificial and directive procedure, akin to the “mining” perspective mentioned above.

This has practical implications regarding the manner in which interviews are conducted. Some interviews do in fact maintain the questionnaire structure, even though the questions are said to be “open.” Owing to this, the interview guide runs the risk of limiting the inquirer to a list of standard predetermined questions, thus impeding the spontaneous improvisation of a real discussion that is marked by digressions, backtracking, beating around the bush, hesitations, pauses, anecdotes. The field interview must, as far as possible, adopt the conversational mode and avoid the model of the questionnaire. It is therefore useful to propose a distinction between an interview guide and an interview framework. An interview guide organizes beforehand the questions one asks and is not far from a questionnaire or an interrogation. An interview framework, on the other hand, is a personal “memory jogger”; all it does is help the inquirer to remember important topics, without disrupting the internal dynamics of an ordinary discussion.

Furthermore, while the interview guide forces the interviewer to go through a set of questions, one after the other, in a given order, the interview framework leaves the researcher free to adapt to the person he is interviewing: depending on his centers of interest, his
competences, or the drift of the conversation, certain points will be
discarded whereas others will be given in-depth attention. With each
new interview, the researcher goes shopping, choosing certain items
on his list.

The framework, in other words, is limited to “the questions on
your mind,” leaving to improvisation and craft the task of transform-
ing them, in the course of the interview, into “questions that are actu-
ally asked.”

In fact, the questions on the researcher’s mind are specific to his
problematic, topic, and language. They are relevant only within his
sense-world. They have no immediate meaning for his interlocu-
tor. It is therefore necessary to transform them into questions that
make sense to the interlocutor. This is where the informal know-how,
acquired through participant observation (and through the difficul-
ties and misunderstandings of initial interviews), is transformed, often
unconsciously, into the capacity to dialogue with the interlocutor, on
his own ground and in his own codes.

**Recursivity in the Interview**

The field interview has a number of particularities. One predominant
characteristic is that, far from being a means of getting the “right
answers,” an interview must also make room for the formulation of
new questions (or the reformulation of old ones). Herein resides
another major difference between the interview, carried out by a
researcher, and the questionnaire, subcontracted to inquirers. This is
another case in which informal know-how pays off. Accepting the
interlocutor’s detours and digressions, his hesitations and contradic-
tions, is not simply a matter of “getting him to relax.” In fact, this
implies adopting an epistemological attitude. When an interlocutor
goes “off the topic” or gives confusing answers, it is time for the
researcher to prick up his ears. And, far from dismissing the anecdote,
he would do well to go for it, as it is “meaningful” and full of new
insights. This brings us to what we may call the recursive nature of
the field interview. In fact, the interviewer should rely on what has
already been said by the interviewee in order to produce new ques-
tions. These questions correspond, simultaneously, to the “questions
on the researcher’s mind” (strategic level of the research questions)
and the “questions that are actually asked” (tactical level of the inter-
view interaction).

This ability for “immediate decoding,” in the very course of the
interview, is at the heart of the field researcher’s know-how. The
researcher, far from being a simple “recorder” or a simple “inquirer,”
remains as interpretatively alert as possible. He mentally sorts out the relevance of what interlocutors tell him (regarding his research) and the meanings that arise from this, and he is thus able to identify, in the course of the interview itself, the statements or information that might illustrate a given conclusion, reformulate a particular problem, or reorganize one set of facts or another. In this respect, the interview, like participant observation, is a privileged space for producing “grounded theories,” which are tested as they emerge.

The Interview as an Invisible Negotiation
The inquirer does not have the same interests as the inquirer, nor does he have the same representations of the interview. In a certain sense, each one tries to manipulate the other. The informer is in no wise the researcher’s pawn, nor is he a victim caught in the trap of his unbridled curiosity. The informant does not shun the use of active strategies enabling him to take advantage of the interview (to gain prestige, social recognition, or monetary returns, in the hope of future assistance, in view of legitimizing his own point of view) or defensive strategies aimed at minimizing the risk of what he says (giving little or false information, getting rid of the nagger as soon as possible, being obliging by giving the answers he thinks the inquirer expects). The researcher’s problem is a double-bind dilemma, that is to say, he must remain in control of the interview (since his inquiry must progress) while allowing his interlocutor to express himself freely, in his own style (since this is a condition of the success of the interview).

Empathy and Interview
There is another double bind inherent in the researcher’s management of the interview. The researcher is professionally bound to give credit to what his interlocutor has to say (however strange or dubious the former’s statements may appear from the vantage point of the researcher’s sense-world) and to listen to him with empathy, approval, or complicity. Of course, conflict or contradiction may arise during interviews, but empathy is still the rule. This is not simply an inquirer’s trick (nor is it a form of duplicity on his part). In fact, a primary objective of the researcher in the field is to understand, with as much finesse as possible, the logics underpinning his interlocutor’s representations, perceptions, or actions. Consequently, he is required to “take seriously” all that he is told and to “be on the same wavelength” as the person speaking to him. Rather than a trick, then, empathy is the precondition for gaining access to the logics and sense-world of persons
being studied by the anthropologist. It is by taking them seriously that he is able to fight against his own preconceptions. This is what Bellah terms “symbolic realism” and what is also termed the “principle of charity” by others. The researcher should take serious account of the “reality” described in the informant’s discourse, in keeping with the significance that the informant ascribes to it. It is this reality that the researcher must attempt to approach as closely as possible. At the same time, we would hope, the researcher is not naive! The need to be critical alerts him against an unquestioning acceptance of what he is told. The discourse on reality must not be confused with reality itself.

This is a real dilemma. How can we combine empathy and distance, respect and suspicion? As is usually the case in a dilemma, there is no clear-cut solution. However, the attempt to introduce a time gap between these two operations appears to be a sound research policy. Giving total credit must precede systematic doubt: it is even a condition of the latter. During the interview, credit is given to the interlocutor’s discourse as making sense: in fact, one can only have access to this sense by taking all that is said seriously. The interview is therefore managed on the benefit of the doubt. Later on, critical or even suspicious decrypting will comment on the sense of this meaning, as well as the relation between the speaker and the statement, between the referent and the context. However, the experienced researcher is able to combine these two levels, or to navigate between them in the course of the interview, empathizing with his interlocutor while remaining mentally vigilant.

Interview and Duration

The insertion of the interview in a diachronic dimension stands in contrast to the “mining” perspective. Any interview is potentially the first of a series. It might, moreover, mark the beginning of a relationship (however short lived). An interview is not a closed case, over and done with once and for all, but rather an open, perfectible file. After several interviews with the same interlocutor, one gets nearer and nearer to the mode of a real conversation; a subsequent interview often allows development of and comments on questions arising from a previous interview. Besides, with each new interview, the now familiar interlocutor sees the researcher in a more favorable light, as a person with greater competence. This enhanced status is highly advantageous for the researcher. In fact, the more one has the impression of dealing with an incompetent stranger, the more one tends to take him for a ride.
The Group Interview

The interview is typically considered as an interaction between two persons. It is, indeed, thanks to face-to-face exchanges that a relationship based on trust is developed, that the exchange may be at least partly delivered from the impact of social control, that discursive strategies can take place peacefully. But rightfully viewing this dyadic form as primordial should not lead us to conclude that it is the only possible form. Interviews are sometimes collective.

It is important to note that the group interview is usually impromptu and unsolicited by the researcher. It is related to the fact that the anthropologist works in “natural settings.” Having a normal everyday chat with someone means assuming the risk of the family, close relations, neighbors, or colleagues—depending on the situation—getting involved in the conversation. The appropriate strategy in such a situation is neither to forgo the possible advantages of this multiple interaction (the opportunism and know-how of the researcher allow him to make the most of the remarks—or silences—of each participant) nor to relinquish the benefits of a private interview (which can always be rescheduled, in a more discreet setting).

But the group interview may also be programmed and solicited. It might involve a group that already exists (the executive council of an association, a family, a class) or an ad hoc group put together through the researcher’s initiative. The most familiar form of the ad hoc group is the focus group.\(^3\) The problem with the focus group is not so much that it is an artificial situation (why not, as long as one is aware of this and controls the consequences?); the problem is the naive or lackadaisical end to which it is often employed. It is naive when it sees the remarks made, especially when they are consensual, as expressing a collective point of view or, worse, the point of view of the “community” that the participants are supposed to represent. It is lazy when it becomes the only, all-purpose strategy of inquiry, i.e., a kind of cut-rate qualitative inquiry. We are forced to recognize that focus group inquiries, which are on the rise in Africa, in the context of expertise financed by development institutions or in the framework of so-called rapid and participative inquiry,\(^3\) generally combine naivety and laziness.

Observation

It seems that over the last few years, observational data have been neglected or treated like the ugly duckling of anthropological corpuses. Underaddressed in works devoted to field inquiry,\(^3\) underused by
numerous researchers who tend to focus only on interviews, observation has, moreover, been excessively criticized by postmodern scholars in the name of the original sin of naturalism or positivism. Observation is, nonetheless, a “natural” cognitive attitude, one that we all apply successfully in the course of our everyday activities. Consequently, it is surprising that the field researcher is much too often satisfied with a lackadaisical practice of routine forms of observation, without making any attempt to systematize, discipline, and organize observation in view of its utilization in the production of the corpus itself.

Social activities are equally accessible through what is seen and what is heard. They produce sense in both registers. Besides, certain social activities, and not the least relevant among them, chiefly entail prediscursive or infradiscursive information, or even silence or latent materials.37

Chapter 4 will focus specifically on various epistemological aspects of the question of observation and description, which are closely related.

**Intensive Microsurvey Procedures**

Intensive microsurvey procedures, sometimes involving specific interviews or observations, are not extensive surveys in the ordinary sense of the word but they target the systematic production of a finite quantity of intensive data: counting, inventories, nomenclatures, plans, lists, genealogies, etc.38 It is impossible to draw up a list of such techniques, since for ten thousand different problems there are ten thousand different techniques to be invented on one’s own; in one case, the issue is the space occupied by association’s members during a general assembly; in another, it is the daily work of a woman and her husband; in yet another, it is the chart of family relationships within a municipal council, the list of therapists consulted by each member of a domestic group over three months, the length of speech in a palaver, the number of adjournments in a court case, the percentage of nurses on a ward compared to doctors. The importance of this type of data production must not be underestimated: this is how one learns the “trade”; it is through a reasonably systematically organized quest for empirical data that the researcher arrives at the necessary distance in relation to discourses (other people’s) and impressions (one’s own). This is where the collection of “emic” data (discursive data aimed at making indigenous representations accessible) is combined with “etic” data (data constructed by means of observation or measuring); see Chapter 3.
Intensive microsurvey procedures offer various advantages. They sometimes supply figures, though not necessarily in the form of percentages or samples. It is therefore no longer a “qualitative” issue but rather a matter of “intensive quantitativeness” involving a limited set of elements. Intensive inquiry procedures produce indicators that, when correctly elaborated, belong to the category of “unobtrusive measures.”

Intensive microsurvey procedures are in fact observation frameworks or measures that the anthropologist constructs in the field, when necessary, and in his own style, i.e., in keeping with his research problematic (in constant evolution), his interrogations (constantly renewed), and his (relatively cumulative) knowledge of the field. Certain techniques (such as kinship charts and lists of land holdings) have now become standard owing to their connection with several classic research topics and with certain currently orthodox problematics.

These techniques might intervene during different phases of the inquiry process and might therefore assume a variety of meanings. At the beginning of fieldwork it is mostly a question of charting “backgrounds,” in the concrete and the metaphorical sense. This makes it possible to situate main actors, relevant spaces, and everyday life. Such elements provide the newcomer with markers and points of entry, enabling the acquisition of organized knowledge. The last stage of the inquiry is mostly used to verify intuitions, to provide more “objective” elements, and to gather proofs and confirmation. During this phase, field research mechanisms are less polyvalent and more focused.

Written/Documentary Sources

Although conventional and not specific to field inquiry, documentary sources should be neither forgotten nor minimized.

We may thus evoke the following in passing:

- scholarly sources on the domain considered (anthropology, history, economy, etc.)
- “gray literature” (reports, evaluations, master’s theses)
the press
archives
local written sources (pupils’ exercise books, letters, account books, diaries, tracts, etc.)

A part of such data is gathered before the fieldwork phase (scholarly works on the domain in question and “gray literature”). These allow familiarization with the field, or, better yet, elaboration of exploratory hypotheses, hunches, and personal interrogations. Others (writing produced by actors, local archives, and the local press) are an inextricable part of the field inquiry into which they are integrated. Still others are a part of independent corpuses, distinct from and complementary to those produced in the field (press, archives).

The fact that the greater part of written data involves preexistent documentary sources does not mean that we ought to abandon the option of getting local actors to write about matters related to the inquiry: a teacher might agree to make pupils write an essay on a subject suggested by the researcher, a trainee midwife might agree to keep a journal of her night duties, or a police officer might agree to write down, in the evening, an account of his missions accomplished during the day.

The frequent—and abusive—association of anthropology with the study of “oral societies” and the fact that the data transcribed from fieldwork are essentially oral in origin often overshadow the fact that there is currently no society that is not referenced in a written document. Nor is there any society in which writing does not play a role. For the anthropologist, written sources are therefore simultaneously a means of introducing a diachronic perspective, indispensable tools for widening the context and scale of the inquiry, and pathways into the contemporary experiences of the group under study.

Audiovisual Sources

They have been with us since the early days of ethnographic inquiry, bringing to mind the pioneering attempts made by Mead and Bateson. But, paradoxically, their use still remains largely marginal in written production (a few scattered illustrations or an annexed CD). In fact, as a corpus in the strict sense, audiovisual data pose the daunting problem of treatment and analysis and that of the integration of the iconic analyses into a descriptive and interpretative argumentation, which is necessarily presented in writing. Except for certain specialized fields (body techniques, ethnomusicology, ethnmethodology,
etc.), it is clear that few really conclusive experiments exist in this field, which, consequently, is still largely experimental, despite the fact that the advantages of such delayed and reproducible observation allowed by film viewing, photographs, or video cassettes were analyzed some time ago.42

In fact, audiovisual data often form a parallel and distinct final product (such as a documentary film or a photographic exhibition) and are not subjected to the same epistemological narrative and communicational constraints as a book or an article. This takes us beyond the scope of our present subject.43

However, audiovisual data may also comprise preexisting images and sound recordings (such as wedding videos, family photographs, local radio programs), produced not by the researcher but by the local actors, or even by external actors (as with radio or television broadcasts). In this case, the corpus is created in the same way as one comprising written documents.

**DATA COMBINATION**

The almost permanent combination of the various types of data mentioned earlier is one of the particularities of fieldwork inquiry. It will suffice to mention two aspects, among others.

**Data Eclecticism**

The field inquiry makes use of all available materials. Its resolutely eclectic empiricism employs all possible modes of data collection and source combination.44 It is obvious that the types distinguished above frequently intersect and often exist in synergy. Eclectic sources have a great advantage over inquiries based on a single type of data in that they allow the researcher to take into account the various registers and stratifications of the social reality being studied. It is therefore difficult to understand peremptory statements about the intrinsic superiority of one type of data over another. In contradiction with Harris, who places observational procedures at the top of the pyramid (etic: see Chapter 3) in the name of a strongly positivist cultural ecology, Fabian favors verbal interactions in the name of a dialogic ethnology that reminds us of a certain type of postmodernist extravagance.45 Against all this, there is every reason to take into account data entailing a variety of references, relevance, and reliability, providing access to different fragments of reality and their peculiar characteristics, and whose overlapping, convergence, and intersection guarantee enhanced plausibility.46
However, the interview is often used almost exclusively as a mode of data production, separated from participant observation, in particular. There is sometimes, in this case, a tendency to standardize methods of data collection (sometimes labeled guided interview, free interview, semidirective interview, or semistructured interview) or methods of treatment (content analysis, discourse analysis software). Interview sociology thus becomes a particular methodological configuration, thanks to the autonomization of the interview procedure, seen as the primary mode of data production. This incurs the tendency to stray away from what I refer to herein as the field inquiry, which happens to be fundamentally polymorphic.

Furthermore, each research topic, each field inquiry has its own characteristics, demanding the use of certain methods rather than others. Each research context has its specificities: the researcher must adapt to the context instead of trying to force the contextual reality into prefabricated methodological routines. Eclecticism in data production means inventing new combinations, in sync with the problem considered; it implies a skillful dosage or modulation of proven modes of putting together a corpus.

The Case Study

The “case study” is an exceptionally fertile mode of data combination—one among others. It brings together, in a single sequence, delineated in space and time, the types of data referenced above. This sequence could be a community event or a set of specific interactions, and it might be structured, from the actors’ point of view, as a ritual, as a routine, as an objective, or as a “problem” (social and/or individual). The anthropologist combines sources revolving around a particular social situation: observation, interviews, intensive microsurvey procedures, and written sources. This might take the form of a land tenure conflict, a political nomination, a religious ceremony, a divorce, a strike, an accusation of witchcraft in a village, a therapeutic episode, or a local election. There are countless cases whose description and decoding offer possible inroads into broader research objectives.

The Manchester School is undeniably the first to have conceptualized this method in anthropology, which had long since existed in practice, at least since the inception of field anthropology: Malinowski or Evans-Pritchard, among others, made ample reference to such cases. The Chicago School, for its part, based its work primarily on case studies. The Italian “microstoria” has recently imported and
systematized a peculiar brand of this approach into the field of history, although history has always made relatively frequent use of this method.

There exist numerous interpretative and theoretical uses of the case study, addressed by an abundant literature proposing various typologies of cases and of their usage. Some limit their attention to illustration, others describe and analyze the intrinsic significance of local situations, while yet others extrapolate from a reference case in order to produce middle-range analyses, which is the main level of theorization in anthropology and qualitative sociology.

Glaser and Strauss (1973: 152) remark that “case studies can serve as examples for pre-existing general theories or as a means of generating new theories.” Stake (1994: 238), on the other hand, underlines the dangers of what we may call the “pretext case”: “Damage occurs when the commitment to generalize or create theory runs so strong that the researcher’s attention is drawn away from features important to understand the case itself” (a phenomenon that will be addressed later using the term “overinterpretation”; see Chapter 7). Certain case study approaches have become more systematic and attempt to avoid the pretext case by including the borderline case, the marginal case, or the negative case: the “analytic induction” method thus proceeds from case to case in order to generate and readjust its interpretations.

However, it is important to differentiate between the field case, in the specific sense assigned to it in this work, and the abstract case or the aggregate case, understood as any unit of a comparative series, regardless of the level of generalization or abstraction. In the broad sense of the word, even vast multivariate statistical analyses compare cases. In a different register, comparative political studies consider towns, countries, types of constitutions, or political regimes as cases that can be included into a limited set in an attempt to discover explanatory variables: these are greatly aggregated macrosocial units, with no concrete meaning when seen in the perspective of a policy of fieldwork. Conversely, the field case is directly accessible to the researcher, via his usual modes of empirical data production. To take an example from another discipline, the “clinical case” examined by a practitioner (the equivalent of a field case) has nothing to do with the “case of the American health-care system,” which is strongly aggregated and analyzed by the public health specialist.

However, the field case in anthropology is neither an imprisonment in the case nor an imprisonment in the field. A field case, even in its “micro” version, even in the event of in-depth study for its own sake,
even when it is not included in a corpus of cases aimed at comparison, even if it does not generate fresh theorization, always alludes to something larger than itself, beyond itself: a larger set, a broader social context, other cases, a research theme, a problematic. It works as a revealer, descriptor, indicator, symptom, or example. Note has been taken of the importance of the diagnostic event—a social event the researcher chooses in view of its symptomatic properties, because it is “revealing,” because, in the interactions it entails, one may observe the interplay of wider social logics, the implementation of more extensive practical norms, the deployment of recurrent strategies, the weight of exogenous constraints, the effect of external forces or mechanisms.

This metonymic research posture in the face of the case study differs from the posture of ethnomethodology, which, inversely, tends to limit and even to imprison analysis within the strict bounds of selected cases (in this instance, sequences of interaction).

**Fieldwork Policy**

So far, I have outlined the primary forms of data production in fieldwork. The process of field research can also be apprehended, briefly, in relation to a few methodological transversal principles, which “nevertheless” identify anthropology as an empirical social science, hence different from erudite journalism, chronicle, or exotic autobiography. Fieldwork, which brings together the various forms of data production mentioned above, is indeed the outcome of a guiding “scientific strategy” behind the work of the researcher. This strategy might be relatively explicit or essentially implied. Implicit strategies may conceal methodological shortcomings. Consequently, the intention of this work is to be as explicit as possible, in order to highlight some of the “principles” that seem to regulate or to optimize “fieldwork policy.”

**Breaking Free from Hypothesis**

Sociology, marked by a long-standing predominance of quantitative approaches, has often conflated—incorrectly in my opinion—the elaboration of a problematic and the development of preconceived hypotheses that empirical research is then supposed to validate or invalidate, with the indiscriminate aid of either quantitative or qualitative research strategies. The claim has therefore been made that “the organization of a research around working hypotheses constitutes the best means of conducting research with order and rigour” (Quivy and van Campenhoudt, 2006: 113). Our objective, in this work, is
to seriously nuance statements of this type (insofar as they confuse the formulation of hypotheses with determining measurable indicators and with the search for causalities) and to show that there are many other ways of being rigorous. The use of hypotheses is indeed fundamental when it comes to preparing questionnaires for a statistical analysis. But this approach is not really adapted to anthropological fieldwork, where the understanding of social logics is gradually assimilated, where questions evolve and problematics “shift” as the inquiry advances, and where the researcher pursues “trails” and is not managed by the search for confirmations, falsifications, or verifications.

This does not mean that fieldwork is able to bypass the putting together of a research proposal or the elaboration of a problematic. Nothing could be further from the truth. This preliminary phase obviously remains fundamental. It allows the researcher to establish a review of the literature on a topic, to organize and hierarchize a series of research questions, and to outline approaches so as to not arrive naive and ignorant in the field. It is a provisional construction, one that is destined to evolve step by step, one that is expected to change in keeping with largely unpredictable dynamics.

Reasoning in terms of hypotheses, in the strong sense of the term—in other words, becoming the prisoner of a “mental structure of hypotheses”—might, on the contrary, harden the inquiry based on a rigid, preconceived interpretative model, thus preventing “discovery,” “surprise,” and fresh theorizations based on data. However, the term “hypothesis” may also be used in a “soft” or “weak” sense as an equivalent of “pathway” or “provisional interpretation,” in order to avoid imprisonment in a “hypotheses mind-set.”

The hypothesis mind-set incurs another risk: that of reasoning in terms of causalities, whereas very often the register of explanation, strictly speaking, is not a part of the anthropological approach, which is fundamentally descriptive and in which emic meanings are crucial (see Chapter 3). In fieldwork, the question of “how” is more to the point than that of “why.” Fieldwork highlights processes, logics, official or practical norms, strategies, representations, but it seldom takes the risk of proposing explanations, much less of formulating laws like “if A then B.” Yet, asking ourselves “why” is not without interest, nor is it irrelevant; on the contrary. Economics and quantitative sociology do this quite frequently. Occasionally, anthropology may address this issue, which is liable to produce the positive outcome of intellectual stimulation. But its qualitative methods are generally incapable of dealing with the description of causalities, especially when it comes to determining independent and dependent variables. Sociology
and comparative politics do possess methods aimed at searching for explanatory variables based on qualitative corpuses (small-N studies), through the use of Boolean algebra. But I would argue that this runs the risk of aggregating and dichotomizing data and therefore comes at a certain heuristic cost.  

The search for causalities in the social sciences is indeed legitimate, but the explanatory project cannot be allowed to predominate. The equal legitimacy of the descriptive project must also be recognized as it is in fact a continuum, and also owing to the fact that this ideal-type opposition of explanation and description should not be exaggerated. The explanatory project, when combined with the current frenetic emphasis on quantitative approaches, tends to relegate—somewhat condescendingly—comprehensive qualitative research to the status of nice handymen in the service of “real science,” i.e., science based on figures.  

Yet qualitative approaches through fieldwork are as “scientific” as quantitative ones and operate in another register, beyond the language of strong hypotheses or the logics of explanatory variables.

### Triangulation

Triangulation is the underlying principle of all types of inquiry, whether criminal or ethnographic: various elements of information must be combined! Information obtained from an individual source must be verified: this is as true for an alibi as it is for a religious ritual. This may seem to be a matter of plain common sense, and long ago historians established the principle. But there is a certain ethnological tradition that goes counter to common sense by seeing a given individual as the fount of the knowledge of an entire society. We are referring to the famous “key informant” used by numerous “classic” researchers.

It goes without saying that the researcher is liable to get along with certain members of a social group and to dislike others, and one can hardly blame him for preferring to talk to Marcel rather than Jacques, or Usman rather than Ibraheem. Similarly, when it comes to clarifying certain questions, all interlocutors are not equal, and common sense dictates a preference for John’s competence and clarity over Michael’s confused rambling. But going beyond these obvious commonsense choices and focusing on a single resource person is at once a lazy research posture and an epistemologically indefensible position.

Simple triangulation helps the researcher to cross-check informants in order to avoid limitation to a single source. Complex triangulation,
on the other hand, involves rationalizing a choice of multiple informants. Complex triangulation aims at gathering a variety of information in the face of a problem to be addressed. The objective is to cross viewpoints that seem to bear a meaningful discrepancy. Consequently, the issue here is not one of “blending” information or “checking” information in view of arriving at the “real version.” Instead, what is at stake is the search for conflicting discourses, the study of discursive heterogeneity, a reliance on variations rather than erasing or attenuating them. In short, the objective is to construct a research strategy based on the quest for meaningful discrepancies.

This brings us to the notion of the “strategic group.” It may be understood as an aggregate of individuals who, faced with a particular “problem” or issue have the same attitude, this being determined in general on the basis of the similarity of their social response to the problem (“social relation” being understood in the broad sense of the term to define a cultural, symbolic, political, or economic connection). In contradiction with classic sociological definitions of social groups (for example, the Marxist definition of a social class), I do not think that “strategic groups” are formed once and for all and are relevant in every case. They vary according to the problem considered. They may refer to statutory or socioprofessional characteristics (sex, caste, profession, etc.), kinship affiliation, solidarity or patron and client networks, biographical itineraries, or sectarian affiliations. Hence, the notion of strategic groups is fundamentally empirical. This simply implies that in a given community all actors involved do not share identical interests or representations; depending on the “problems” in question, their interests and representations undergo varying combinations, which owe little to chance. It is therefore possible to formulate presumptions regarding the composition of strategic groups in the face of a given “problem.” Fieldwork will indicate, later, whether these presumptions were founded or unfounded and whether the strategic groups at the finish are those that were expected at the start. Another empirical task is to find out if a given strategic group is simply the result of a sum of comparable, unconnected individual behaviors, due to similar “positions” in regard to a particular “problem,” or if there is in fact a distinct morphology, i.e., whether the social configuration in question is really a corporate group, a network, etc.

The existence of “invisible” or “external” strategic groups, indispensable for triangulation, must also be considered. Interviewing (in relation to a particular “problem”) marginal individuals who are not acting in harmony with others, who are on the sidelines, is often one of the best ways of gaining access to different points of view. In the
same way, underdogs should not be ignored in favor of leaders, more or less charismatic front liners, or self-proclaimed spokesmen.

Such an approach is obviously in contradiction with a certain “culturalist” point of view, postulating the homogeneity and coherence of a “culture.” A “nonhomogeneous” perspective is more productive, from the point of view of interpretation. To find diversity, one has to look for it! Approaching a society through the conflicts in its midst is also more productive than presuming consensus, although conflicts do not exclude some level of cooperation among actors.

Iteration

Field inquiry uses iteration: it functions in a back and forth manner. Iteration may be concrete (the inquiry meanders between informants and information) or abstract (data production modifies the problematic, which modifies data production, which modifies the problematic).

In its simplest and most concrete form, iteration refers to the comings and goings of a field researcher. Indeed, as opposed to a “questionnaire” inquirer, who starts at one end of the street or of the telephone book and ends at the other, the field researcher goes to see X, who sends him to see Y at the other side of the village or of the town, then he goes back to see Z who lives near X. This is because his interlocutors are not chosen in advance through a method of classification (statistical, random) but rather fall into place in keeping with an ongoing compromise between the researcher’s agenda and the availability of the persons he wants to interview, the opportunities that arise, family ties or prior friendships, and a few other variables. The choice of persons to be interviewed usually functions through “ramification,” “arborescence,” or “snowballing”: each interview triggers new orientations and new potential interviewees through direct or indirect suggestions arising from the interview. The inquiry thus takes on its own momentum and plots its own course, one that is largely unpredictable at the outset, one that the statistician or the epidemiologist would see as illegitimate, but which nonetheless effectively unveils the “real” networks operating in the study site. The individuals in a field inquiry (as opposed to individuals in a sample inquiry who are per se and of necessity representative of abstract standardized variables) are not considered outside their life contexts, are not abstracted from their personal or family networks, patron and client networks, or modes of sociability. Fieldwork is thus adapted to local social circuits, to their complexity, embeddedness, and distortions. It is by no means
linear. The researcher obviously draws up lists of persons to interview, but such lists are continuously updated and modified. He identifies “strategic groups,” but these are adjusted as the work progresses.

Iteration is also, in a more abstract sense, a back and forth movement between hunches and data, interpretations and results. Each interview, observation, or interaction offers the occasion for discovering research perspectives and provides opportunities for modifying the initial research problematic or for elaborating new ones. Throughout the fieldwork process, the researcher is continuously engaged in interpretations: in the course of encounters, observations, or interviews. But this process is often latent and imprecise. The phase of data production may also be seen as a continuous restructuring of the problematic, due to contact with data and through an incessant rearrangement of the interpretative framework as the accumulation of empirical elements progresses. Baldamus thus mentions “reciprocal double fitting” by using the analogy of a carpenter adjusting a door in its frame by alternately planning the door and the frame.62

Making Interpretations Explicit

This point is linked to the previous one. The fact that interpretations and reformulations of the research object occur during data production often leads to a contradiction or paradox. Prolonged fieldwork entails continuous back and forth movements between data production and interpretation, between questions and answers. It implies permanent verbalization, conceptualization, and self-evaluation: it is an ongoing intellectual dialogue. But a lengthy field inquiry is an essentially solitary exercise, and this solitude is not particularly conducive to conceptual dialogue. The researcher is forced to dialogue with himself, in a manner that is largely virtual, unfinished, and implicit.

The field diary plays an important role in this regard by allowing regular “recapitulations” that attenuate the lack of scientific dialogue in the course of the inquiry. Of course, the field diary has other more frequently underlined functions. Occasionally, it gives rise to specific end products (see Chapter 5). It is, additionally, a framework for thinking through the interpretative processes linked to data production, during the fieldwork phase, as well as a method of clarifying things in the solitude of the field. This function is generally overlooked despite its strategic importance in the entire inquiry. It may be replaced by systematically noting interpretations on index cards. Strauss calls this “memoing,” an operation he considers to be crucial during the field stage, alongside data collection and coding.63
Making interpretations explicit through verbalization can also be done through dialogue with a “research assistant.” This is usually a member of the local society who has a long-standing relationship with the researcher, by whom he is progressively initiated into methods of research and inquiry. Obviously, the research assistant is also a source of bias, but he may be of great assistance in “semiologic translation” (making the sense systems of the local milieu intelligible to the researcher), over and beyond his more common role as interpreter (doing “linguistic translation”).

Lastly, there is the solution of teamwork, one that is much too uncommon. In this case, verbalization and objectification are ensured by the ongoing debate that occurs throughout the empirical research. We are all aware of the central role that debate plays, or should play, in the social sciences (this is probably the best guarantee of epistemological plausibility). But debate is usually—at the most—ex post and “rigid.” Introducing debate into the inquiry process itself—through collective work, at the levels of data collection and the interpretative strategies it involves—is a procedure that should not be ignored. I have practiced it on numerous occasions using the “ECRIS canvas.”

Constructing “Describers”

This is one method of making interpretations explicit, but through the search for ad hoc data that change interpretations into “observable” elements derived from field cases and examples. These organize the production of a “subcorpus” around a specific topic. The search for coherent and meaningful data (discursive or otherwise) aims at verifying, supporting, modifying, or rejecting a hunch; it accommodates a mix of methodology and improvisation and introduces order and organization into fieldwork, which is otherwise highly susceptible to moods and impressions.

Each investigation produces its own describers: determining which topics or “slices of life” to collect; conducting systematic surveys on popular semiology; organizing a precise series of targeted observations; focusing on certain key actors, whether eminent or obscure; making a panorama of existing associations or significant conflicts; and so forth.

In the case of multisite comparative studies, which are increasingly popular, the construction of common describers is, moreover, essential in view of a certain formal homogeneity of the qualitative data produced in each site, thus ensuring their comparability.
The term “indicator” might also be used, although it is commonly understood as having a strong quantitativist connotation. It is in fact a matter of constructing relevant sets of qualitative data that might be used to corroborate or infirm, and especially to modify, specific interpretative hunches. What are the specific observables items provided for testing such and such a supposition or for producing new ones? Indicators may also be used as markers for possible research trajectories or as landmarks identifying research trails.

The word “clue” can also be used instead of “indicator.” Each field inquiry thus elaborates its own clues, which may be multiple or varied. These are never standardized but are instead clear-cut and specific. The advantage of the anthropologist is that he is able to “construct” clues, whereas historians must be content to identify signs or to discover traces. Ginzburg uses these words—clues, signs, and traces—in support of the “clue paradigm,” which brings to mind Sherlock Holmes, and which, in his opinion, characterizes microstoria. Indeed, the term “inquiry” links the detective, the journalist, and the anthropologist. Admittedly, the objectives, professional constraints, deontology, analytic interpretative procedures, and, in part, methods (or usage of clues) differentiate these professions, but this does not prevent them from sharing certain methods of empirical knowledge construction: the words “flair” and “rigor” characterize excellence in all three, in regard to finding traces or sources and getting them to tell a story—i.e., interpreting them.

**Saturation**

How do we know when it is time to put the fieldwork phase of research to bed? The framework of qualitative field inquiry, as opposed to that of a sample survey, does not include a fixed stop sign. In fact, at some point the researcher is forced to notice a decline in fruitful observation and a decrease in the productivity of interviews regarding the “problem” being investigated. With every new sequence, with each new interview, less and less new information is obtained. This means that one has more or less “exhausted” the representations in a given field or the range of strategies linked to a particular arena. How long this process takes obviously depends on the empirical properties of the field or arena in question, i.e., on the characteristics of the research theme defined by the researcher with respect to a specific social group. But it depends also on the competence of the researcher and his familiarity with the topic and site investigated.
Glaser and Strauss are the first to have developed this notion of saturation. But they gave it a more theoretical meaning by associating it with the progressive construction of “categories” (sorts of ideal types) allowing for the comparison of groups and societies: “Saturation means that no additional data are being found whereby the sociologist can develop properties of the category. As he sees similar instances over and over again, the researcher becomes empirically confident that a category is saturated” (Glaser and Strauss, 1973: 61).

Of course, the principle of saturation is not just a stop sign: it is a methodological guarantee of prime importance, one that is complementary to triangulation. Postponing the end of field research on a theme or subtheme until no new data are collected on that theme or subtheme prevents satisfaction with insufficient or sporadic data. It enables a process of relative data validation and makes room for confrontation with divergent or contradictory data. “We go for constraints that impose the postponement of induction” (Schwartz, 1993: 286).

In this regard, the concept of a strategic group complements the principle of saturation: by choosing a variety of interlocutors in order to take their differing perspectives into account, the researcher rationally pushes back the limits of saturation. When this point is finally attained, saturation is all the more relevant since the entire range of postures has been explored. The same applies to the quest for counterexamples.

In fact, working with counterexamples seems to be one of the requirements and advantages of field inquiry (see Chapter 7). Anthropological fieldwork accords the same attention to the exception, the “negative case” or the modal case. Departure from expected behavior, noncompliance with official or social norms, and discrepancy in conversation thus serve as eye-openers regarding the array of existing formal and informal norms and indicators of the difference between formal norms and actual practices.

The Control Group

It is usually useful and even necessary to choose a site of intensive research on the grounds of a social network or a face-to-face setting that might later serve as the base for more extensive inquiries beyond this site. The “control group” varies according to the theme of the inquiry. While the scale may vary, the context is always restricted: a family, a village, a group of youth, a workshop, a neighborhood, an office, a police station, an urban block. Within a given social space,
participant observation, in-depth interviews, intensive microsurvey techniques, and the search for written data may be combined. At any rate, a basic requirement of participant observation is a relatively prolonged stay within a group, network, or setting. Intensity also provides for a constant intersecting of various sources of information. The restricted and in-depth context allows the researcher to establish relationships among different types of knowledge and various registers; it also permits a transversal, “holistic” approach in which social actors are understood in keeping with their diverse roles. In this way religion, family ties, politics, sociability, patron and client networks, production, and other such social categories, which cannot be understood simultaneously in an empirical light on a large-scale level, may be connected thanks to the researcher’s proximity to social actors and their everyday interactions. Actors continuously “switch” from one social configuration to another. The researcher may thus note personal and “multiplex” intermingling. The linking of “spheres” and “levels” of social practice, usually broken up by analysis, remains a major advantage of field inquiry, even when, and indeed especially when, the researcher is confronted with a highly specialized topic.

The trap, into which many have fallen, is to focus exclusively on this “control group.” The result is the exclusive production of monographs on microgroups in microsocieties. In fact, moving on to other larger sites, where the stay is counted in days rather than months, seems indispensable. Previous work in the “control group” allows the researcher to take full advantage of extensive work by providing an initial in-depth context of references. How can one compare without having a base for comparison? In other words, inasmuch as a prolonged stay with a “control group” appears to be a good research strategy, leaving the group and “stepping aside” appears to be equally important. Getting on the sidelines by doing inquiries elsewhere opens up new perspectives and enables changes that often validate and complete, invalidate, or relativize the initial picture. Thus, the notion of control group attempts to recycle one of the resources of yesterday’s ethnographic monograph (prolonged stay among a small group) while avoiding its drawback (imprisonment in the microlevel) and adapting it to new contexts, notably comparative multisite inquiries into nonlocalized or professional groups.

Finally, it must be noted that a “control group” sometimes comprises a single strategic group that is considered to be central. But it usually includes persons from several strategic groups interacting in the same arena.
Key Informants

The key informant may obviously be viewed as an extreme case in which the control group is restricted to a single individual. In many instances, the strategy of turning to a single individual stems from a culturalist approach that takes a single individual as an expert, as the depository of an entire culture, i.e., as an “omniscient informant.” The culturalist point of view is moreover combined with a lackadaisical research strategy. However, the problem of the key informant goes far beyond the uses to which it is sometimes put.

Let’s be clear about this: there is no researcher who does not have few favorite informants. But choosing to call on a specific interlocutor can and ought to be combined with the principle of triangulation. It is of course impossible to do without key informants, owing to various reasons. One is that interpersonal affinities play an important role in field research. Then, there is the fact that, from one research theme to another, from one “problem” to another, local competences are varied and unequal. Another is that communicational skills vary widely from one researcher to another, for all consultants and all narrators are not equal in terms of volume and quality of information.

It might also be productive to distinguish between various types of key informants. Some are generalists and provide clear and comprehensive access to everyday representations. Others are “brokers,” “mediators,” or “porters,” who open access to other key actors or quasi-inaccessible social scenes. Lastly, there are the experts who play the role of consultants and narrators. Though the criteria of expertise vary from one field to another, each domain or theme still has its experts, seen from the researcher’s point of view.

The cases of recourse to a key informant and the type of key informant selected vary in keeping with the phase of the inquiry. Finding a mediator, a “broker,” or a “resource person” to lean on is certainly necessary at the beginning of an inquiry. Breaking free from this person usually happens at a later stage.

Managing “Biases”

Field inquiry, like inquiry through questionnaires, obviously has its own biases. Fieldwork policy picks its way through these biases. But this is unavoidable. The problem is how to control biases. Four examples of this, directly linked to data production, may be noted. Biases of a more interpretative type, related to the intellectual posture of the researcher, will be addressed elsewhere (see Chapters 5, 6, and 7).
The Modification of Behavior
This section addresses the inevitable question of the influence of the researcher’s presence on the behavior of persons being studied.

The degree to which observation modifies the phenomena observed is (from Heisenberg to Gadamer) a long-standing and ongoing debate. Although there is no consensus on theoretical solutions, some practical solutions do exist. 71

1. An appreciable amount of behavioral habits are slightly or not all modified by the anthropologist’s presence, and one of the dimensions of the researcher’s know-how is the ability to identify which ones. Becker has pointed out that the researcher impact often constitutes a negligible constraint or stake compared to those that come to bear on the group from day to day. 72 The prolonged presence of the ethnologist is of course the primary means of reducing any disturbance caused by his presence: people get used to him. 73

2. Regarding behaviors significantly modified by the researcher’s presence, there are two radical solutions:

- The first is to attempt to cancel this modification through various means, all aimed at eliminating the foreignness aspects of the status of observer, through the researcher’s assimilation as a native indistinguishable from others involved in the local game. This gives rise, on the one hand, to an endo-ethnology, i.e., the training of “indigenous” inquirers, 74 or, on the other, to the “conversion,” “disguise,” or “indigenization” of the exogenous researcher (see Chapter 5). 75
- The second is to try, to the contrary, to take advantage of the situation. The process of modification thus becomes an object of research in its own right. The inquiry takes itself into consideration, as it were, and becomes its own evaluator. As Devereux (1980) suggested, it can be useful to “exploit the disturbances caused by observation.” Reflective analysis therefore becomes inevitable.

In reality, the attitude adopted is usually situated midway between these two extremes. The anthropologist gradually becomes a “friendly stranger” or “a companion along the road,” or rather is placed in such a position by the “host group.” His “integration” is relative but real. This, however, does not prevent him from observing the effects produced by his presence, including the type of “integration” conferred.
Getting Caught Up in Cliques and Networks

The researcher’s integration into a social arena never concerns the entire arena but rather is limited to certain groups. The researcher gets integrated into some networks but not into others. The resulting bias is both daunting and inevitable. The researcher always runs the risk of being assimilated, often unintentionally but sometimes knowingly, into a certain local “clique” or “faction.” This is doubly inconvenient. On the one hand, there is the danger of echoing his adoptive clique and repeating its opinions. On the other, there is the risk of rejection by other local cliques. Getting involved in a clique, deliberately, accidentally, or owing to a strategy of the clique in question, is undoubtedly one of the main problems of fieldwork. The very fact that local actors are bound together in networks makes the anthropologist dependent on networks in data production. He easily becomes the hostage of one network or another. Using an interpreter, who is always a “key informant,” introduces particular forms of “involvement”: the researcher thus becomes caught up in the likes and dislikes of his interpreter, in the affiliations or ostracism that go with the informer’s status.

For this problem, as for others, there is no miracle solution. However, the savoir faire, tinkering, and other indirect strategies that the researcher uses to prevent getting caught up in cliques are all based on an awareness of the problem. Consequently, methodological lucidity is, once again, the first step in every solution.

Source Monopoly

The monopoly a researcher often exercises over the data he has produced, and even over the population among whom he has worked, is undoubtedly a methodological problem peculiar to fieldwork. Historians have access to the sources of their colleagues and may consult primary sources over and over again. This is totally different from the protective and often deliberate solitude of the ethnologist. How can anything approaching the critique of sources take place in such a situation?

There are only two solutions to this problem. The first is to have several anthropologists working consecutively or simultaneously in an identical or similar field. This brings to mind well-known quarrels, like the Redfield/Lewis or Mead/Freeman disputes that arose from such situations, not to mention the myriad problems involved in the decoding disagreements of this type. However, confrontation, whether direct or time-differed, between researchers in the same field does not always have to be so hostile. It may also be complementary and at times even convergent.
The second solution is to provide at least relative access to one’s sources, originating in the corpuses that one has produced, or to samples of these corpuses, in order to allow others to conduct reinterpretations in the future. A minimal form of this is to allow the reader to recognize as far as possible who is speaking at each stage of the ethnographic text, by giving to each his due, so as not to be accused of imposing meaning. It is important to make sure that the anthropologist’s interpretations remain clearly distinguishable from the statements of his informants, that sources of description are identified, and that indirect style is not allowed to conceal the fact that the statements of real speakers have been amalgamated and linked together. Giving examples and identifying the owners of statements thus becomes the imperative seal of scientific prudence. Let’s remember Malinowski’s famous sentence (1922): “I consider that only such ethnographic sources are of unquestionable scientific value, in which we can clearly draw the line between, on the one hand, the results of direct observation and of native statements and interpretations, and on the other, the inferences of the author, based on his common sense and psychological insight”.

Clearly, applying such a constraint is easier said than done, and there is no anthropologist or sociologist who does not break this rule. Besides, it has become an epistemological exercise to reread and analyze anthropological classics in order to underline ambiguities in their techniques of narration, and in particular the use of indirect speech that makes it impossible to identify the author of each statement. But no one, not even the most vigilant critics of other people’s approximations, entirely escapes the mist of ambiguity. Being clear, “as far as possible,” about who says what and specifying conditions of data production (interviews as well as observation) are relative rather than absolute guarantees. This makes it even more important to set down a few rules for yourself. There is no way of avoiding Spradley’s two “principles”: the language identification principle and the verbatim principle.

Representations and Representativeness
Undue indication of representativeness constitutes another bias. This occurs when the narratives or statements of a number of individuals are presented as a reflection of “a culture,” be it the culture of a social class (popular culture, working-class culture), that of a people, or that of an “ethnic group.” Fieldwork usually references typical representations and practices rather than the representativeness of representations and practices. It makes room for the description of the space of
contemporary or outstanding representations and practices in a given social group, without possibility of stating their statistical distribution, although the use of intensive microsurvey procedures sometimes allows for the production of numerical data. We should not expect fieldwork to do the impossible. It is able to propose a description of the main conceptions or behaviors that key groups of local actors have vis-à-vis a given problem, but that is all. It will have nothing to say about the quantified representativeness of these representations or strategies, barring recourse to another methodological configuration. This is of course all the more reason to promote “mixed methods” studies, combining qualitative and quantitative approaches.

The Subjectivity of the Researcher
The researcher’s personal role is a resource. As mentioned above, it derives from immersion, which gradually opens access to local codes and norms. But this role also entails a bias. Most data are produced during the researcher’s interactions with others. These data therefore contain a certain “personal element.” Such a bias is inevitable and should neither be denied (positivist attitude) nor overestimated (subjectivist attitude). It can only be controlled, and at times utilized or minimized. This work will attempt (see Chapter 5) to define a few dimensions of the researcher’s implication as well as the explicitations he works out in this regard for the benefit of his readers.

This is another function of the field diary mentioned above: it helps the researcher to manage his subjective impressions. It allows him to evaluate his personal feelings and to give an account of the modalities of his personal involvement. Teamwork, already mentioned, may be used to advantage in this case as well. Collaboration and complementarity in teamwork also serve as reciprocal checks countering subjectivity. These checks are obviously relative but nonetheless important.

Many other problems may be mentioned. The “question of subjectivity” is too complex for us to systematically address at present. It will suffice to call attention to two related problems.

The first concerns the constant pressures that stereotypes and ideologies exert on the anthropologist’s outlook (see Chapters 6 and 7). But the anthropologist is certainly not the only one to be subjected to these pressures. The same is true of all social sciences, even the most quantitative ones: they are constantly faced with the risk of misinterpretation and overinterpretation at various levels, from the construction of the research topic to the manifold ensuing levels of interpretation.
The second has no definitive solution but can be negotiated in practice: everyone with whom the anthropologist interacts, from the chance interviewee to the key informant, is more or less playing a game or putting on a show, for the benefit of the anthropologist or of someone else. Fieldwork is therefore a realm of “impression management.”

**CONCLUSION**

**Plausibility and Validity**

Various contemporary efforts have been made to define the conditions of **validity** in ethnography. They are, by and large, independent of formerly predominant positivist conceptions. The three criteria proposed by Sanjek offer good illustrations: they combine, in their own way, many of the questions mentioned above: (1) To what extent is the anthropologist’s theorization grounded in the field data presented as “evidence”? (2) Is information provided with respect to the “field itinerary,” i.e., are we told who informants are and what methods of collecting information were used? (3) Are the interpretative decisions made in the course of fieldwork explicitly mentioned?

I am not sure that “criteria” is an apt term to use in a case like this. What is certain, however, is that the preoccupation with data validity (another way of defining the objectives of “qualitative” rigor and empirical adequacy) must be placed at the heart of fieldwork. To my mind, it is only on this condition that anthropology can claim to be plausible. It is a matter of validating, as far as possible, the anthropologist’s assertions, based on the data produced through inquiry, and of guaranteeing the reliability and validity of these data.

The restitution of the inquiry to actors investigated is sometimes presented as a decisive test of validation. Although restitution, when possible and relevant, seems appropriate and even necessary, this procedure cannot be taken as the ultimate criterion of the evaluation of empirical adequacy. This would be tantamount to disregarding the fact that all social groups are traversed by cleavages (and this usually invalidates the consensus of interviewed subjects concerning the results of an inquiry as soon as social stakes are involved). It would also imply disregard for the fact that the researcher’s perspective is necessarily different from that of the persons he interviews. “Few people like . . . to be objects of anthropological research. In addition, most communities are traversed by conflicts. This makes it
impossible to achieve a restitution that satisfies everyone” (Burawoy, 2003: 441). We cannot but agree with Bloor’s conclusion that “while the previous analysis shows that members’ pronouncements on findings cannot be treated as a test of validity, it should also be clear that a member validation exercise can generate material that is highly performant to the researcher’s analysis . . . In particular, negative reactions from members should be a stimulus for a reanalysis” (Bloor, 1983: 172).

In fact, the reactions of interested parties are always useful. Sometimes, they make it possible to rectify badly established facts or excessively one-sided interpretations, and they may serve to eliminate a reasonable amount of misunderstanding about facts, words, or meanings. They may also provide confirmations (including through their disagreements with the author), produce new research materials, or open new paths.

But it is on the level of external readers (pairs, students, scholars) that the plausibility of anthropological research generally comes into question.

Plausibility, then, is primarily ensured by what one may call “the final presence of data” in the researcher’s written production, over and beyond the use of data as grounds for interpretation, in order to give the reader direct access to a few samples of sources. A part of the data will in fact be used as “prime” materials, while the rest is likely to be reframed and used as arguments, examples, or validating statements in the researcher’s final narrative and analytic framework. The sociologist working with questionnaires “places” his tables and factorial analyses where the anthropologist “places” his quotes from interviews, descriptions, intensive microsurveys, and case studies. These data, resulting from corpuses, derived from field notebooks, are then “edited” (as in the case of a film); they are cut, reframed, and produced, in keeping with the researcher’s narrative and demonstrations. Descriptions are rewritten, sometimes in a style rather different from the shorthand notes in the field notebook (and they are often saturated with interpretative annotations, in the style of Geertz’s “dense description”). Interviews are presented through relatively brief quotations, in translation, and abstracted from the conditions in which they were conducted. Case studies are contracted, impoverished, and occasionally concentrated into a standard form. Their multiple sources are reduced and leveled out, their complexity simplified. Yet, in spite of all these constraints, the simultaneous presence of descriptions, quotes, and cases in the final anthropological product (report, article, book) speaks to the presence
of empirical fieldwork, guarantees validity, and makes room for critique. The “touch of reality” derived from the selective use of fieldwork data is not merely a rhetorical stunt. It also bears witness to the empirical aim of anthropology. It distinguishes empirically founded ethnological interpretation from free hermeneutics, philosophical speculation, or essayism. During the writing-up phase, traces and evidence of data from fieldwork come to the surface. The reader is not simply gratified with abstract models; he is also provided with samples, excerpts, examples, or quotations that bring him into closer “contact” with the social world described. This world becomes more tangible, and this enables easier access to the meaning of the words employed and the scenes experienced.

Consequently, this is not about seeking preinterpretative purity in data, nor is it a question of collecting data abstracted from a surrounding social reality. Field inquiry is subjected to the same constraints as the construction of the research object or any other social science practice. In northern countries and southern countries, in Western societies and non-Western societies, in the country and in town, in factories or stadiums, fieldwork is regulated by the scientific project of
describing, understanding, and comparing logics of action and representations—alongside their systems of constraints. And these logics, which the researcher must learn to navigate, do not necessarily correspond to the norms at work in his own universe. Such differences incur innumerable misunderstandings. The researcher’s know-how ultimately consists in resisting these misunderstandings and in transforming, for his readers, the unknown, the exotic, or the picturesque into ordinary and familiar experience. The use of what Geertz calls “concepts close to experience” or what Glaser and Strauss refer to as “sensitizing concepts” goes in the same direction.

This familiarization plays a central role, and it has thus been said that at the end of his fieldwork the researcher should be able to act like those he has studied, if he were in the same situation. “Comprehension can be displayed in a variety of ways. One classic test that some ethnographers try to pass is: if you think you understand the X then you should be able to act like X. This goal is represented for example in Goodenough’s (1957) definition of ‘culture’ as the knowledge necessary to behave appropriately” (Agar, 1986: 54). This criterion of “accomplishment” in the ethnographic field is generally accepted: “I feel deep empathy for the truth of Evans-Pritchard when he claims, in substance, that he is capable of reasoning in the logic of those he studies” (Augé, 1975: 315). Evans-Pritchard provided a very cautious formulation: “To get to know the people one studies really well, to see and hear what they do and say.”

The validity of data collected in the field is largely linked to a “criterion” of this type, as long as exotic fantasies are ruthlessly extirpated from it. “Modern” research themes, whether in Africa or in Europe, when stripped of the irritating and recurrent opposition between “them” and “us” (people of the South/intellectuals of the North), are better equipped than colonial ethnology and its pet objects when it comes to understanding the logics of other people, while steering clear of the picturesque or fascinated glance: an inquiry into corruption requires an understanding of the logics of the corrupted customs officer; an inquiry into the leaders of Trotskyist organizations entails the ability to penetrate the logics of a revolutionary activist; an inquiry into the World Bank implies understanding of the logics of a development expert.

At any rate, it is clear that the “criterion” of familiarization or of personal understanding of the other people’s logics is no more susceptible to formalization, objectification, and quantification than the data whose evaluation it is meant to ensure. Nevertheless, all inquiries are not equal, all data are not equally valid, all descriptive statements
do not have the same veracity, all social logics are not understood with the same finesse, and the plausibility of interpretative assertions likewise varies depending on the quality of the empirical references on which they repose. It is precisely for such reasons that a policy of fieldwork is required.
At first glance, the term overinterpretation, as used in the social sciences, irresistibly conjures up the idea of an excess of meaning imposed on the “reference reality” on which the researcher claims to report. By way of contempt, incompetence, or negligence, the researcher guilty of such an offense is thought to have mistreated data. He may be suspected of ignoring or masking data. We may say that he “adds on” or that he overdoes things. He goes beyond the bounds of what may be imputed to the reality described and proposes an image of it that is excessively nonaccurate, enhanced, biased, and, in a word, “false.” He twists the available elements or makes statements that ignore or even contradict them.

This spontaneous idea of what overinterpretation is amalgamates a value judgment (overinterpretation is bad) and a judgment of fact (overinterpretation can be refuted on the basis of data). This places us in the heart of a latent epistemology that is both practical and normative. We all practice it “naturally” and spontaneously with respect to the writings of others (when it is a question of expressing our concerns and criticisms) but somewhat less frequently where our own work is concerned (although scruples of an empirical nature sometimes quell our flights of fancy). It is all well and good to suspect or to denounce the moralism behind a value judgment and the positivism behind a judgment of fact, but that changes nothing: it is impossible to do without this type of judgment in the ordinary practice of our profession.

Obviously, this notion of overinterpretation concerns scholarly common sense. But is it possible to arrive at a more formal characterization?
Can overinterpretation be used as a basis for better-argued methodological or epistemological thinking applicable to the daily exercise of our disciplines? This seems possible. First, because the question of overinterpretation has the enormous advantage of leading to a fundamental social science debate regarding an awkward issue that we all try to avoid, more or less. We readily admit, on the one hand, that there is absolutely no clear line separating acceptable interpretations from unacceptable overinterpretations; there is no epistemological police officer standing ready to issue a fine to those guilty of violation. Yet we must admit, on the other hand, that each of us has witnessed the interpretative excesses of one colleague or another, has found them upsetting, and has responded with arguments derived from a better knowledge of the reality in question that has been shamefully distorted or ignored by the colleague! These are two completely different discursive registers. In the normal functioning of social science disciplines, they are almost never brought face-to-face. Arguing that interpretation and overinterpretation are indistinguishable in the final analysis places us in the abstract register of epistemology. Opposing the scandalous ignorance or barefaced distortions of X or Y regarding the “facts” places us in the concrete register of criticism.

How can we explain this type of schizophrenia separating a general discourse that some may regard as overly relativistic (with a hint of “epistemological correctness”) from a specific discourse that others may consider to be basely positivist? Using overinterpretation as a point of entry obliges us to confront this divide. It brings us to another more fundamental problem at the center of this work: the relation between interpretative risks and empirical adequacy in the social sciences in general and in anthropology in particular. The position of this work is simple: it remains impossible to draw the line between interpretation and overinterpretation; the contours of overinterpretation cannot be precisely delimited in a formal manner. Nevertheless, it is possible to locate—and to caution against—some of its manifestations. They constitute various forms of violence done to the empirical data based on which the social sciences propose their interpretations.

**Overinterpretation, Underinterpretation, Misinterpretation**

*Overinterpretation* obviously mobilizes terms that are both contrasted and related. *Interpretation*, *overinterpretation*, *underinterpretation*, and *misinterpretation* thus seem to form a semiologic system comprising affinities, differences, and nuances that require clarification.
On first reading, *overinterpretation* appears to exceed the acceptable limits of *interpretation*. It seems to be the opposite of the symmetrical deficiency of *underinterpretation*. Finally, it seems distinct from *misinterpretation*, which is merely a benign blunder or a venial sin.

In fact, this typology, which goes from the greatest to the least, is not as satisfactory as it may appear. The real epistemological challenge is primarily the divide between *interpretation* and *overinterpretation*. This is the case if our concern is to arrive at an operational rather than an essentialist definition of overinterpretation and if our aim is to break free from parasitic meanings attached to misinterpretation and underinterpretation. This is what the present chapter will attempt to develop.

This chapter will define as overinterpretation all cases in which there is a significant contradiction between empirical references and interpretative propositions. The issue here is not to ascertain whether or not “overinterpretation” is in fact the right word (a better word, if offered, would be readily accepted) or whether this word has other meanings (it does). It is instead a matter of assembling under a single concept the range of situations (that are not uncommon in our disciplines) in which the excessive projection of preconceptions is combined with a certain degree of methodological sloth, thereby generating the abuse or even the refusal of data. It is quite possible to challenge or to erode the plausibility of overinterpretative anthropological statements (in the sense of the term we have suggested) not only on the basis of logical, theoretical, or alternative interpretative grounds but even more so on empirical grounds, by demonstrating that data have been maltreated or discounted.

“Overinterpretation” used in this sense is different from the way in which this term is understood in the fields of literary criticism, psychoanalytical thought, or philosophical hermeneutics. These disciplines function on the basis of the accumulation and confrontation of interpretations. They are not subjected to the artisanal constraints of the empirical adequacy proper to the social sciences. In psychoanalysis or philosophy, which are based on exegesis, the concept of overinterpretation is necessarily different.

Umberto Eco argues in *The Limits of Interpretation* and in *Interpretation and Overinterpretation* that we cannot force a text to say just about anything. This statement, despite its apparent proximity to my own approach, is a good illustration of the difference in methodology. Indeed, Eco is obliged, for lack of an underlying reference reality, to resort to the somewhat enigmatic concept of “the intention of the work” (*intentio operis*). This may be understood as the right of the
text to be respected in its coherence, as a protective measure against too much overinterpretation. In the social sciences, it is the production of an organized and elaborated set of empirical data that plays this cautionary role, by creating specific constraints for interpretation. These are not of the same order as those involved in the writing of a novel or of an essay. We could of course attempt to reduce anthropology to anthropological writing and consequently include anthropological writing among the common category of other forms of writing. But this would be at the exorbitant expense of forgetting or refusing empirical constraints. As for the Geertzian vision of “culture as a text,” this is little more than an expression. It is quite debatable and hardly productive as a research program.

If we accept the foregoing definition of overinterpretation, in the specific framework of the social sciences, what do we then make of the neighboring notions of underinterpretation and misinterpretation?

Let us begin with underinterpretation. This term could certainly be viewed as the opposite pole of overinterpretation. Between the opposite pitfalls of overinterpretation and underinterpretation we would then find the relevant middle ground of interpretation—a space characterized by “neither too much nor too little.” However, this linear vision—situated between one excess and another, between lack and excess, underinterpretation and overinterpretation, with interpretation as a middle ground legitimated by its central position—is in many respects misleading.

On a first level, underinterpretation does not make sense. Supposing that data can speak unassisted and that the less they are interpreted the more they are respected is an untenable position, as is evidenced by innumerable demonstrations that have led the overwhelming majority of today’s social science researchers to admit that it is not possible to obtain results without interpretation, that there is no research without interpretation, that there is no data production without interpretation, that there is no fact without interpretation. Underinterpretation as an antitheoretical posture, a refusal of the interpretative risk, is not a matter of making “the lowest bid”; it is a discredited positivist posture, although it has not completely disappeared.

But on another level, underinterpretation may take the form of a deliberate and provisional methodological moment. It is liable to emerge as a normal stage in the process of research or (sometimes) of research restitution. It is a moment in which the anthropologist tries to “remain as close as possible to data,” to adopt a “low profile strategy” or a “raw document policy” (maybe as a deliberate or constructed effacement). These phases are necessary and productive. In
other words, even a research process that is clearly and completely interpretative still has relatively less-interpretative stages. These are methodologically indispensable. Transient, artificial, and controlled, they have nothing in common with underinterpretation as a positivist posture.

As for misinterpretation, my position here is that it does not have a specific space that is proper to its operations alone. In the case of competing, empirically legitimate interpretations (i.e., interpretations that are all equipped with a minimum of veracity) that are all acceptable in terms of empirical adequacy, each person uses the term “misinterpretations” to label interpretations that he is challenging (i.e., regarding as erroneous and less convincing than his own). In the case of empirically illegitimate interpretations, i.e., interpretations that are quasi-refutable in terms of veracity, misinterpretation represents a form of overinterpretation.

When an anthropological presumed misinterpretation abuses accessible data, refuses to take them into account, or mutilates them, it enters the space of overinterpretation (as defined above). When, on the other hand, the term misinterpretation is used to stigmatize an interpretation that is relatively compatible with data but that appears to be unfounded, that simply amounts to not sharing someone else’s interpretation and is located in the space of competing, more or less plausible interpretations.

The skeptic reader will invariably object that the crux of the matter is how to decide between these two spaces. He will argue that it is not enough to declare that they exist. Doesn’t the battle of interpretations, within what we have called the space of empirically legitimate interpretations, also involve empirical objections? Isn’t it a part of the game to try to oust the empirical plausibility of those with whom we disagree, to eject it, at least partially, from the space of empirically legitimate interpretations?

This question calls for a slight detour.

**Interpretativity and Empiricity**

We will take as an entry point two postulates with which readers are already familiar: (a) the social sciences are fundamentally interpretative (corollary: scientism, positivism and naturalism are untenable); (b) the social sciences are empirical sciences (corollary: epistemological anarchism and postmodernism are untenable).

This obviously defines the watershed at which the greater part of social science research operates. As mentioned (see Chapter 1), this is
a space of plausibility (as opposed to falsifiability) in which interpretative processes, omnipresent as they may be, acknowledge empirical constraints and equip themselves with procedures of methodological vigilance (not only of logical vigilance). These procedures set out to preserve as far as possible a certain fit between empirical referents and interpretative statements. This empirical adequacy may be represented in the form of a necessary double connection: (a) between the “reference reality” and the data that research procedures produce in its regard; (b) between these data and the interpretative statements that are proposed (see Figure 1.1).

The first connection attempts to guarantee that the data produced give some idea of the “reference reality” and strive to “represent it” in a certain way, imperfect though this representation may be. This connection is primarily a methodological one. The second connection does its utmost to substantiate the statements that the researcher makes in his staging or narration of these data (in the form of attestations, examples, quotations, tables, descriptions, charts, cases, etc.): this connection is of a predominantly argumentative nature (see Figure 2.2). The two are inextricably related. In both cases, it is undeniably a question of legitimating interpretative statements in the name of a certain index of veracity. This veracity may be defined as the “particular form taken by truth in the empirical exemplification of a sociological proposal” (Passeron, 1994: 76). The interpretative imagination accepts in this case the constraints or the particular types of vigilance that the search for veracity, guaranteed by the empiricity of the social sciences, imposes. It is in this manner that social sciences distinguish themselves from the “free exercises” (in this respect) of literary hermeneutics or speculative philosophy. This, then, is the entangled ensemble (“reference reality” + “data produced on this reality” + “argumentative use of these data”) that defines the empirical substrate of the social sciences. The title of this chapter is consequently a somewhat abusive shortcut; it is not to “data”—as such—that overinterpretation does violence but to the empirical substrate of which data represent the artifacts and the traces.

The epistemological space specific to the social sciences is therefore at the same time completely interpretative and strongly empirically constrained. While this may appear to be a contradiction in terms, it is really more of a difficulty. The social sciences are obviously interpretative, but this does not mean that all interpretations are equal, that only their respective rhetorical talents and faculties of intellectual persuasion set them apart. All interpretations are not equal. All interpretations should be accountable to data and to the reference reality
to the extent that they are subjected to the requirements of empirical plausibility and veracity (relative as it may be). The interpretative sovereignty that the first postulate claims must relinquish some of its prerogatives to the second postulate of empirical adequacy.

The relationship between interpretation and empirical adequacy is further complicated by the fact that interpretations are not all the same and that reference realities likewise differ. It is impossible to make relevant use of the term “interpretation” in a general sense (as for the term “sense”) given that “interpretation” covers an extremely wide range of cognitive procedures that may be applied to a variety of referents (and types of data). Interpretative statements do not only depend on the posture of the researcher, on his research strategies, or on the scope of the research that he has determined. They also involve the social objects to which they relate, the type of data that are produced (“emic” and/or “etic,” specific and/or comparative, “qualitative” and/or “quantitative”), the characteristics of the local social context, and so on.

It is nevertheless possible, through extreme simplification, to distinguish from among this immense variety of interpretative situations and their empirical referents two poles in the relation between interpretativity and empiricity. Some statements are quasi-refutable empirically. It is more or less possible to “prove” that X made a wrong interpretation in his translation of the Wallachian term *zdrychol* or that Y did in fact attend the divination ritual on February 30 and accurately described its spatial setting. But statements that are almost nonrefutable empirically are in the majority. No one can ever “prove” that W was wrong in affirming that the history of humanity is the history of exchange or that Z was right to compare a possession ritual to commedia dell’arte.

Quasi refutability in this case is not about the invalidation/confirmation of theoretical hypotheses. It involves, instead, the empirically grounded interrogation of the empirical adequacy of an anthropological statement. In order to respect the full sense of the notion of refutability, close to Popperian falsification, it seems preferable to employ the more cautious term of “occasional quasi refutability” with regard to the social sciences. This distinction seems to present the advantage of demonstrating that there are perceptible variations of degrees in the empirical plausibility of anthropological or sociological statements.

In fact, at the extreme of quasi refutability, the empirical statements embedded in interpretative argumentation are descriptive or observational and their referents are quasi-verifiable to the extent that they
are quite bounded and of a factual nature. They remain close to fieldwork data.

At the other extreme is the pole of nonrefutability: such interpretative statements incorporate few empirical data and these are so excessively diluted, aggregated, or unrecognizable that they cannot be quasi-refuted on an empirical basis. The empirical arguments used are far from the data; they employ extremely comprehensive comparative, hypothetical, or explanatory argumentations (even in the weak sense), referring to broad and heterogeneous space-times: they are always complex aggregates. This is definitely a more speculative level.

These two extremes, quasi refutability and nonrefutability, are opposites with respect to interpretative modes and to empirical referents. They form the poles of a continuum along which social science plausibility circulates. At the quasi refutability end, the demand for veracity increases and the taking of interpretative risk decreases. At the quasi nonrefutability extreme, the reverse is true. The statements that social scientists make are distributed between these two poles and incorporate both interpretative risk taking and empirical legitimation. It has often been demonstrated that all statements that have an explicit empirical status—that claim to give an account of a precise state of the world in a given space/time—are in fact interpretative (if only because of the very choice of this statement instead of another), even when not presented as such. In other words, any statement, even one that is deliberately situated in a descriptive or observational register, is nevertheless filled with interpretations. But conversely, and this often goes unmentioned, any openly interpretative statement (in the social sciences, of course), i.e., any hypothetical, comparative, or explanatory statement, implies some legitimation of an empirical order and affirms, more or less explicitly, that certain states of the world are regarded as “truthful” and as documented. Researchers necessarily make use of “effects of reality” in their writings. Hence, empiricity and interpretativity are always combined in social science arguments, but to considerably varying degrees.

Based on the overall context thus rapidly summarized, the notion of overinterpretation implies that something is going wrong in the empiricity/interpretation duo. As underlined above, data are made to say too much, no account is taken of established facts, a reality of a different nature is wrongly invoked, an excess of sense or an erroneous sense is imposed on the studied phenomena, no solid element is put forward in support of the statements proffered. These are the reproaches that we all make whenever we criticize, from an empirical point of view, the remarks made by a researcher who seems, in this respect and regardless of his rhetorical talent, to be imprudent, under
par, or even incompetent. We have all done this. In the debates and commentaries that constantly agitate the very small world of researchers, all is not confrontations of paradigms, antagonisms of schools, and quarrels among individuals, although there is undoubtedly quite a lot of that around. In this world, we also quarrel about the veracity of arguments produced and frequently question the empirical (explicit or latent) content and value of the statements made by a colleague. Although everybody recognizes that facts are constructed, who among us has never opposed the “facts” to abusive interpretations and decided that the “facts” were right?

Some Figures of Overinterpretation

As we know, a researcher is supposed to do the utmost to break free from his preconceptions. For some this is an “epistemological break,” while simpler exercises of distrust of ideologies will suffice for others. But it is impossible to completely elude preconceptions. Still, a little bit of preconception is not the same as too much. And the fact that there is no scale to indicate excesses of preconceptions does not mean that they do not exist. There is no greater temptation for the researcher than that of depicting reality in convenient colors or taking scientific desires for realities. The incentives for excess are numerous. Heuristic paradigms (hard or soft), postures, local or middle-range theories, ideologies of all kinds, idiosyncratic fantasies: these unavoidable features abound in the research landscape. They are relatively fertile where producing preconceptions is concerned.

On the other hand, excesses of preconception are obviously easier to commit when one’s methodological guard is down. Methodological vigilance allows us to guard against the tendency toward interpretative excesses: the intersecting of sources, the search for counterexamples, the identification of statements, linguistic competence, and many other “tricks of the trade” are tools that help us to observe a suitable empirical caution in the very midst of compulsory interpretative risk taking. Overinterpretation nevertheless exists. We have all encountered it and, moreover, practiced it. Let us now take the risk of describing some of its major and recurrent figures.

**Figure 1. Reduction to a Single Factor**

Conflict is raging in the heart of A., a charming Bielo-Italian village. This conflict involves opposition between the mayor and his principal assistant and embitters the life of the entire village. A distinguished
anthropologist is interested in this village and in this conflict. Without hesitation he analyzes the situation as the expression of “ethnic” antagonisms. In fact, the mayor belongs to “ethnic group” E1 and the principal assistant to “ethnic group” E2. The past has seen tensions, insults, and rumors, bearing on the “ethnic” identity of one person or another. The anthropologist uses this as evidence in support of his interpretation. He also quotes an abundant ethnological-type literature on the enduring conflicts between E1 and E2, sometimes involving E3.

But it so happens that the mayor is also the father-in-law of his assistant (interethnic marriages are as frequent in A. as they were in ex-Yugoslavia and in Rwanda). And there are of course also numerous examples of conflicts between fathers-in-law and sons-in-law in the vicinity of A. (as in the town of Washington).

In addition, the mayor resides in the high district while the assistant lives in the low district. The history of the village is rife with conflicts between the two districts, each of which is multiethnic.

To this we must add that the mayor is Presbyterian and his assistant has recently converted to a new evangelical church. There again, we could note that, in the country, the coexistence of these two confessions is not always peaceful, to say the least.

Lastly, the mayor has an impossible character and the principal assistant is not easy. Both have strong personalities and have formed their own network of friends, clients, and relations. Besides, these two networks each support a different football club. The two networks are not really on friendly terms.

As trivial or caricatured as this made-up example may be, it is still related to a process that we have all observed time and again: a change of context and of variables is enough to change our reading of a given situation. It is a relatively common social science practice to reduce factors, all empirically observable and susceptible to playing a role in the interpretation of a local social event, to a single factor. The other factors all disappear as if by magic. This practice is not unrelated to some ideological predispositions of the researcher, who has a tendency to prioritize such and such factor, regardless of the situation. Hence, we have specialists in interpretation through the ethnic group, specialists of interpretation through the social class, through “gender,” religious affiliation, etc. Of course, all these factors, or others, may come into play. But the complexity of social life and its interactions is such that it is rare to encounter contexts in which attention can be limited to a single factor. Nonetheless, “monofactoriality”—in other words, proposing a single “cause” or “origin” for a given social process, whereas many research experiences and the history of the social sciences plead in favor of multifactoriality—is still alive and kicking in
the social sciences. Reducing complexity to one variable (as in many large-N studies) or even to three variables at the most (as in small-N studies) is a common practice in social science. And anthropology, despite its greater interest in the complexity of daily life, does not always escape this caricature of reality.

Let us also recall that the monofactorial obsession is not necessarily something that we decide on beforehand. Likewise, research that resolutely aims at placing the priority on empiricism is also liable to progressively succumb to the temptation of focusing on a single factor—the one that is the most apparent locally, or the most elegant scientifically, or the most tempting intellectually—while “forgetting” all the others. Let’s be clear: attempting to ponder many factors and to highlight a final or a dominant cause is by no means an illegitimate social science ambition when one is confronted with a multiplicity of variables liable to lead to interpretative abdication. Simple enumeration is risk free: but is that good anthropology (sociology, history . . .)? It is a normal procedure to seek an explanation that is more central or more convincing than others. Of course, we could invoke the descriptive character of the social sciences in a bid to challenge the quest for explanation or causality. But this would be a semantic game. A descriptive approach remains interpretative, and interpretation frequently embodies certain forms of explanations, “soft” or latent though they be. But this does not entail blindness to the other factors that are always a part of the reference reality. To say “this conflict is ethnic” (and providing only “evidence” that goes in this direction and keeping quiet on all the rest) is not the same as saying “among all the factors that might explain this conflict”—and detailing these factors by providing the corresponding “evidence”—“I think that the most influential may be the ‘ethnic’ one, and this is why . . .” But this is of course a point at which it is possible to move swiftly beyond a legitimate interpretative risk in the direction of an overinterpretation that has freed itself from empirical constraints.

Figure 2. The Obsession with Coherence
The famous ethnologist V. has painted a portrait—acclaimed as masterful—representing the complexity of the cosmogonic, mythological, ritual, and symbolic system of the P. people. It is, moreover, his best work. He shows that just like the Greeks, the Hebrews, or the Egyptians, and even better than these peoples, the P., an unjustly disregarded tribe of continental central Oceania, had developed a particularly complex cosmogony with the elaboration of a system of correspondence that never ceases to astonish. To each spiritual principle,
revealed over the centuries by the Ancestors, according to the Great Myths, corresponds an element of ritual, which may be found in the great diversity of lineage and village ceremonies distributed throughout the year. Each principle in turn corresponds to a plant that is itself situated in a system of botanical classification wherein one will recognize, at the price of some routine operations of inversion, transformation and substitution, a transfer of the cosmogonic system previously identified. What is more, the habitat, artisanal work, and culinary preparations, to mention just three activities, preserve the traces of this matrix of an omnipresent symbolism.

While reading ethnological writings, we have sometimes had the feeling that this is too good to be true. To those of us who are constantly faced with heterogeneous, fluctuating, and even “fragmented” oral knowledges that are unequally distributed, diversely structured, relatively unsystematized, strategically uttered, and politically manipulated, grand symbolic frescoes or well-ordered “ethno-scientific” portraits often appear surprising or dubious. The ordinary life that anthropologists experience all over the world entails ambiguity, a multiplicity of value systems, contradictory statements, frequent discordance between norms and practices, as well as a diversity of social logics and unclear structures of systems of representations.

It is of course our job to order this disorder, to organize this mess and this confusion as well as we can. By definition, the social sciences, by creating meaning, produce a minimum of coherence: meaning does not like too much incoherence. But this obligation of coherence inherent in the mental activity of the social sciences does not subsequently imply that we are free to be “coherent” or to “systematize” however we please. The imagination geared toward unbridled coherence and systematization sometimes noticed in anthropology is nothing to be proud of. The comparative exercise, which as we all know is the basic principle of the social sciences, is more productive when based on divergences than when it is focused on similarities alone. The monographic exercise itself, a part of any comparative strategy, has more to gain from investigating heterogeneities, contradictions, and differences than from flattening them out.

Figure 3. Sense Inadequacy

Imagine a Poldavian anthropologist who speaks almost no English disembarking in Washington. He is extremely eager to rapidly confirm his flattering reputation as a brilliant and daring researcher that he enjoyed at the University of Q. (the intellectual capital of his country).
After the first few days, he had already noticed that the natives were all addressed as *bastad*. The term was visibly associated with clan membership. Similarly, a sublineage was sometimes distinguished by the expression *mothafuka*. A month later, he had made considerable progress in deciphering the local religious system based on totems and amulets piously deposited before the altar of a pagan divinity (undoubtedly the maternal ancestor of the natives). The divinity was venerated in the form of various avatars (*goodma, holymaid, ourlade*), and the faithful were required to maintain, by night and day, fires of wax burning before her shrine. Six months later, having become, in his opinion, an expert in English, he felt capable of finer semiologic analyses: he had thus highlighted, through the patient recording of daily conversations, that the natives all believed in three distinct mystical forces, linked by extremely complex bonds, forces they called *luk*, *chans*, and *forchun*. He was determined to figure them out later with the aid of a Fulbright grant. These forces were responsible for the eternal fortunes and misfortunes of life: *gotnoluk*, *lackoforchun*, and *chanshasgon* were some of the numerous ritual complaints addressed to these forces when they abandoned the party concerned. The complaints generally led to propitiatory formulas such as *sorrytwilbeokay*.

Using a currently classic rhetorical process in anthropology, I have attempted to show how hard it is for the foreign observer who does not master the local codes to remain “faithful” to the conceptions of the “native” sense-world of the local actors. This is clearly a pitfall that is more of a threat to the ethnologist working in a distant country or even to the historian than it is to the sociologist. Unfortunately, errors of this type, not necessarily so grotesque but of the same order, are not infrequent in anthropological literature. We are all capable of associating some names of colleagues to famous or obscure mistakes that have been made regarding “indigenous” meaning. How could interpretations based on seriously mistaken descriptions, or translations of the emic meanings of the reference reality, be credited with any indication of veracity? Yet there is nothing to prevent these interpretations, “empirically distorted” though they may be, from being perfectly alluring for readers who have no knowledge of the local reality.

However, “emicity,” or the respect of the “sense adequacy” derived from Weber (see Chapter 3)—in other words, the capacity of the social sciences to provide as much as possible an accurate picture of the sense systems of target groups, to give an account of local values, codes, and norms, to collect the representations specific to the social actors—seems to be the necessary condition of empirical research and interpretative
work. Admittedly, these conditions are insufficient. The recourse to other
types of data (such as observation; see Chapter 4), for example, remains
essential. But interpretative proposals would be “inadequate from the
standpoint of meaning” if they reposed on false meanings or misinterpre-
tations at the emic level and would consequently lose all validity.

In fact, sense inadequacy is so vast that its manifestations may be
broken down into several subfigures.

Subfigure 3.1. Linguistic Incompetence
Can this incompetence prevail even when working in other dialects
of one’s own language (social dialects such as “verlan,” the popular
reverse youth language of Marseille or Paris, or intellectual or profes-
sional dialects)? This is certainly possible, but the risk is considerably
greater when working in a foreign language, especially when it is cul-
turally and linguistically distant from the researcher’s mother tongue
(oral languages of Africa or Oceania, for example). Nonmastery of
everyday usage of the local languages is a deplorable failing that is
contrary to the principle of good ethnography and obviously paves
the way for far-fetched etymologies, unfounded homophonic associa-
tions, partial and incomplete definitions of local terms, and interpreta-
tive hallucination based on semiologic mistakes.

Subfigure 3.2. Oriented Translation
This involves the serious difficulties that arise when attempting to
translate into English (French, Spanish . . .) a local term whose seman-
tic field has no possible equivalent in the language of readers.11 Let us
take a simple example from among countless possibilities. The local
leaders of possession cults, i.e., persons in charge of medium groups
or brotherhoods, are referred to in Niger as serkin bori in Hausa and
as zimma in Songhai-Zarma. How do we designate them in English,
a language that knows no function of this type, or anything approach-
ing? By far, the two most frequent translations are “priests” and “heal-
ers.” The consequences are a “religious” bias, on the one hand, and a
“therapeutic” bias, on the other.12 Clearly, we cannot escape this kind
of dilemma, except by peppering our work with nontranslated local
terms, thus rendering them quasi-illegible. But we may at least avoid
walking blindly up the semantic alley created by the bias in question:
we must warn the reader about the risks involved when we are obliged
to employ a relatively forced translation.

Subfigure 3.3. “Hardening”
We are still in the problematic of translation. But this time the issue is
the process that takes an indigenous expression “out of the context”
in which it was proffered—which is often mechanical, absentminded, and ambiguous—and “overloads” it with exegesis (more often than not in a symbolic register). To use another analysis from Roger Keesing (who himself drew on George Lakoff),13 “conventional metaphors” belong to a loose and routine apprehension of the world, based on images evoking perceptions and feelings with no explanatory claim whatsoever—e.g., “following hot on the heels of” or “the moon is full.” To transform a conventional metaphor into a metaphysical statement or a speculative assertion is to produce a huge misinterpretation.

This risk, if one is not careful—and examples unfortunately abound in Western ethnology with respect to the “exotic” object—is to detect the presence of a fragment of a worldview, of the conception of personhood, or even of cosmogony, in the most ordinary of everyday expressions. This would be the equivalent of the Poldavian anthropologist in the United States taking literally expressions such as “the sun rises at five a.m.” (supposedly testifying to a massive resistance to scientific modernity, in the name of the pre-Galileo Christian heritage) or “his heart is broken” (construed as an indigenous conception regarding the body, with the heart as the seat of feelings, which may even lead to its physical destruction).

**Subfigure 3.4. The Abusive Emic Imputation**

This subfigure of sense inadequacy is particularly vast, since it covers all situations wherein the researcher imputes to social actors motivations, behaviors, or logics that appear in reality to a better informed or more attentive observer as a contradiction of what is known about the motivations, behaviors, and logics of the aforementioned actors. History and literature are very familiar with psychological or cultural anachronisms of this type, but anthropology also has its lot of similar ethnocentrisms.

This is certainly not easy to demonstrate, as it is usually necessary to employ indirect and complex indicators in an attempt to refute abusive imputations. These, nevertheless, do exist. To take a well-used example, numerous statements still attribute to actors the sole rationality of maximizing profits and minimizing costs, whereas cognitive psychology testifies that the real processes of decision making are different, and anthropology shows that logics of action are both diverse and culturally constrained.

Focusing exclusively on resistances (see Chapter 6) is also related to abusive emic imputation. Consequently, a peasant (or, as required, a young person, woman, or worker) may be transformed into a “resister,” active or passive, direct or indirect, manifest or latent,
thanks to a systematically oriented reading of the least behavior. Through their rituals, they express their resistance. By their rumors, they express their resistance. Through their games and bouts of drinking, they express their resistance. By their migration, they express their resistance. By their refusal of migration, they express their resistance.

“Hypersymbolism,” transforming the “natives” into full-time theologians, mythologists, mystics, or philosophers is of the same order. Examples may be provided ad infinitum. At all events, the issue is the projection of preconceptions, consisting in dressing actors in costumes that suit our fancy.

Figure 4. Abusive Generalization

W. was undeniably a great scholar. Although illiterate, from his youth he had listened to the sermons of the imams and of the priests of the small town in which he was born. He had followed with great attention and for many years the geomancy séances of his old uncle who was a diviner and had carefully memorized the words of the griots who recited the glorious feats of the past. He never failed to listen, on his transistor radio, to the historical programs of Radio France International. Oh, I almost forgot: W. had a respectable career as a tirailleur sénégalais, which had led him from Frejus to Saigon, from Bordeaux to Constantine. A second career as traveling tradesman familiarized him with Dakar, Bamako, and Douala. One day, the ethnologist, who was of course a European, came along. They got acquainted. The ethnologist was dazzled by W.’s knowledge. The ethnologist was working on “the conception of the world and of the person among the H.” Of course, W. himself was an H. (otherwise, they would not have become acquainted). Aided by age and experience, he had become one of the oldest among the H., and the most respected person in the district. The ethnologist and H. became inseparable.

Six years later (the normal timeline in the profession), the dissertation of the ethnologist was defended. As one might well imagine, W.’s statements formed its framework. Three years later (here, the ethnologist had definitely made better time than the average scholar), a book appeared, tackling the arduous problem of the cultural unity of Africa: Is there an African conception of personhood? (The ethnologist had abandoned the stories about the conception of the world, which he now found boring.) In keeping with W.’s opinion, but supported by many erudite readings, the ethnologist said yes.

Abusive generalization occurs at all levels and in all fields. We may distinguish, for the sake of convenience, internal generalization from comparative generalization.
First, abusive generalization may be internal to a field. This is precisely the problem of “shared representations” or “shared meanings.” To what extent may the statements of an “informant” be regarded as representative of a group or of a culture? Owing to insufficient methodological vigilance (overlapping, triangulation, saturation; see Chapter 2), the anthropologist’s favorite collaborator quickly becomes his “grand witness,” his almost single source of data, his inspiration. The same applies to the researcher’s analytic coherence: In what way does a host family express the broader situation of other local families? In what way is a village site of inquiry typical of all the villages of an area? In what way is a city housing program the prototype of housing programs in the entire country? What degree of generalization must one accord to statements based on qualitative data bearing in mind that they are invariably highly contextualized and circumscribed in their production, representing merely a few scattered points in the social space the researcher aims to relate? Anthropological work on “the young people of the Parisian suburbs” or on “love among the Fulanis of Oudalen” is generally founded mainly on discussions and interactions with a few young people from one specific suburb or on a more or less participating observation in a few Fulani camp sites.

But isn’t this really what the day-to-day exercise of anthropology is about? Isn’t it abusive to talk about overinterpretation when dealing with a phenomenon of inevitable extrapolation, which the researcher can try to keep within bounds? The answer is, once again, “yes, but nevertheless! . . .” Since extrapolation is inevitable and necessary, it must be done with all the more caution, prudence, and vigilance. The sharedness of perceptions and practices must be verified as much as possible by as much crosschecking as possible (see Chapter 2). However, caution, prudence, and vigilance are too often absent. But it is true that the failure to respect the requirements of empirical veracity is, in the case of abusive internal generalization, particularly difficult to denounce on the basis of evidence.

This is even less possible in the case of external abusive generalization, i.e., the undue extension of comparisons, a case in which carelessness is even more apparent. That which I have said about group X (supposing that the generalizations internal to this group X are founded) and which I apply without hesitation to set Y, whose group X may be regarded as a subset, can be faulted if I do not make the effort to argue this extension empirically.

The work of Clastres, like others already mentioned, provides a good example of abusive generalization through the extension of comparison. What had putatively been “demonstrated” in connection
with the Guayaki or Tupi-Guarani Indians (in the event that the demonstration was valid and empirically founded, which is another matter, one to which we will give the benefit of the doubt) is extended by the author to all Amazonian societies, or even to all Indians, and then, while we are at it, to all so-called “primitive” societies!

All expressions of the grand partage (great divide)—an ethnocentric theory splitting current human societies into two mutually exclusive blocks—fall moreover in the realm of abusive generalization. Hasn’t anthropology made a specialty out of “interpretative leaping” without a safety net, i.e., out of sometimes taking vertiginous shortcuts between the local and the general, the anecdotic and the globalized? This brings us once again to the touchy question of the “passage” into overinterpretation. For there can be no question of dissuading the anthropologist from indulging in the theoretical pleasures of constructing patterns and models. It is clearly one of the joys of the trade to take the risk of this type of exercise. But this joy should respect the constraints of empiricity. The breaking of empirical moorings, concerning either the model’s empirical reference situation or its undue extension to other empirical situations, provides legitimate grounds on which to suspect overinterpretation.

Figure 5. The Ploy of Hidden Sense

Once upon a time there was a provincial football club that lived a quiet little life in the local league. A team of researchers in sports anthropology happened to pass by and decided to take an interest in the peaceful contentment of this team without problems. An annual subsidy was paid by the local municipality, the team maintained itself in the middle of the standings, after a match all were in high spirits and drink flowed freely, the president was unaware of the liaison between the team captain and his wife. All was going well, when bang! . . . The inquiry was already quite advanced, when one fine day things fell apart. The team had its first of a series of 12 defeats leading to relegation, an innocent bout of drinking ended in collective food poisoning due to a problematic pizza brought in by the mayor, the president of the club had the misfortune of catching the lovers in the act, etc.

The team of researchers had two opposing interpretations (which ultimately led to the demise of the social science unit). For some, an excessive duration of peace could only lead to such a state of disorder, through an inversion typical of the collective unconscious of small groups: the death drive repressed for too long a time had caused a regrettable collapse of ego and superego to the benefit of the id. For the others, who were specialists in ethno-occultism, a spell had been
unconsciously cast by the wife of the mayor, who was also in love with the captain of the team.

Of course, grotesque interpretations of this kind will never be found in the work of a serious researcher (at least not in this register and not about such a team). But the principle of a “hidden sense,” which remains unverifiable however you take it, which is pulled out of a hat and put forward as the explanation of a social condition, has many amateurs.

By definition, this is a way of releasing oneself from any empirically founded plausibility, be it emic or etic, since the sense suggested is opaque to the actors (there is no adequacy in terms of meaning) and seeing that there is no observational procedure to either confirm or infirm the hypotheses. This type of overinterpretation is self-sustaining. It moves in loops and keeps on moving since calling on a “hidden reality” is enough to found the argumentation, with no fear of refutation resulting from data of any kind. Even if you multiply counterexamples, you cannot destabilize the argumentation. The crime is almost perfect. It is not completely perfect, however, as the absence of any mobilizable empirical reference or evidence indicates the culprit to anyone who might be interested. A theory that claims to be empirical (if only by claiming to be anthropology, sociology, or history) but which operates only outside empiricity, by calling on “principles” or “logics” that are in themselves unverifiable or unexplainable, is almost by definition something that should make us prick up our ears.

But this does not seem to be the case since the “ploy of hidden sense” continues to enjoy success in its various forms and transformations. The unconscious, for example, is still attractive while other theories have become obsolete. The “sense of history” is no longer fashionable, and “functionalism,” which belongs to the same ilk, is also quite depreciated. Another variant, still very much in use, consists in attributing to abstract sets (social aggregates or institutions, concepts, ideological constructs, or scientific artifacts) the specifically human qualities of will, decision, and intentionality. “The system decides that . . . ,” “to reproduce itself, the culture needs . . . ,” “monotheism aims at . . . ,” “in keeping with the expectations of the working class . . . ,” “it is in the nature of habitus to allow . . . ,” etc. This is the problem with the “macroanthropos,” a contraband fiction whose ill effects have been underlined by Mannheim. This highlights one of the most common overinterpretative drifts of “ideological holism,” in the same way that the figure of abusive imputation shown
above is liable to pave the way for “ideological individualism” (see Chapter 6).

COMBINING FIGURES

All these figures (and a few others that we could unearth) are readily encountered in combinations of two, three, or more.

Let us take the example of the famous book *The Gift*. The enduring international success of the concepts of *mana* and *hau* obviously owe much to the talent of Mauss. But this success is not unrelated to the ideologies, scientific or otherwise, which, from Bataille to Deleuze, have accredited (and still do) the famous *grand partage*, which read Mauss’s Oceanian version of gift giving as the demonstration that the gift is the antithesis of capitalist venality. These “preconceptions” have “overacted”; they have deviated into overinterpretation, but the work of Mauss himself, *as we see it today*, is by no means free of this shortcoming. What formerly appeared to be empirically grounded now not only seems discredited or implausible to specialists on Oceania but is also, on certain points, quasi-refuted. It is now a known fact that in Polynesia or Melanesia *mana* was not a substance. It was not the soul of the giver locked up in the gift, nor was it a metaphysical concept; rather, it was a popular concept evoking “effectiveness,” “power,” and “capacity” related to the ancestors and the spirits. The same applies to *hau*, even if from Firth to Sahlins scholars continue to discuss the best translation of the Maori text based on which Mauss developed his argumentation. It is all the more interesting to note the distance that separates contemporary critics on the exact emic meaning of *mana* and *hau* from the earlier criticism of Lévi-Strauss. The latter in fact showed little interest in the 1950s in questioning the validity of the materials used by Mauss. *He did not question their empirical adequacy.* Instead, he accused Mauss (very respectfully, of course) of having allowed himself to be mystified by indigenous conceptions by taking them too seriously! His criticism was thus purely “interpretative,” or rather “reinterpretative,” since it was a question of replacing Mauss’s somewhat “substantivist” analysis with a “relational,” not to mention structuralist, one. Nowadays, whereas purely reinterpretative comments have not disappeared, they currently rub elbows with criticisms based on a questioning of the sources of Mauss and the use to which they were put (from mistranslations to nonaccountability for the conditions of enunciations of reported narratives), and on recourse to new data unknown at the time.
Hence, regardless of the homage due to Mauss for his professed concern with basing his work on “indigenous” representations and for grounding his comparative approach on them, we are nonetheless obliged to observe that it is the very opposite that transpired, since a combination of most of the figures of overinterpretation can be found in *The Gift*.

The presence of mistranslation combined with the hardening of metaphors (the figure of *sense inadequacy*) is obvious. This is blithely intermingled with *abusive generalization*, extending the erroneous meaning given to *mana* or *hau* to all “primitive societies.” The *obsession with coherence* also enters the picture, transforming *mana* into a general principle of reciprocity or even into a general “worldview.” Finally, the question is whether projecting a complex set of transactions onto *mana* alone is not tantamount to reducing these to a single factor.

Of course, this issue of overinterpretation can be evoked only by way of anachronism, since none of this was perceptible in the days of Mauss. Besides, the nonspecialist of this field remains oblivious to all of this. The difference, however, is that there are now specialists in this field capable of contradicting the second-hand data on which Mauss based his work. The standards of empirical rigor are also potentially considerably higher, at least in areas where competence in veracity might appear. The advantage of a debate on overinterpretation is precisely drawing attention to the problem of empirical adequacy, by calling to greater vigilance all those who make mechanical use of references external to their fields of competence (*i.e.*, all of us!).

But it is also true that in the social science framework of scholarly referencing, certain works that have become “classic” function purely as “texts” to the extent that they are disconnected from their empirical referents. Anthropology (like sociology or history) therefore has a component related to the “history of ideas” in the discipline, whose famous works are valued not because of their empirical relevance (often outclassed by better documented if more modest subsequent productions) but because of the novelty, appeal, or originality, at the time, of their arguments, their paradigms, their postures, or even the phrases that they contain, and which led to their notoriety. Of course, like all other works that claim to belong to the social sciences, these scholarly works originally included empirical legitimation. But it would appear that this legitimacy has been progressively dematerialized and blurred, leaving room only for brilliant arguments or emblematic theoretic demonstrations. It is currently possible to prove...
that *The Gift* is a work that is empirically debatable or that Durkheim’s book *Suicide* uses an outdated methodology, but this quasi refutability no longer threatens the place of these illustrious references in the history of anthropology or sociology. Owing to the empirical detachment from which they have benefited, they are incorporated into an erudite (or metainterpretative) hermeneutic circuit in which realm they are no longer really accountable as far as veracity is concerned.

Clearly, such exceptions may exist. And it is hardly appropriate to blame totemic ancestors of anthropology for not having respected by anticipation the empirical vigilance that we now have the right to expect from our colleagues and ourselves. But without tarnishing their legacy, it is still possible to deplore the fact that the massive references to their writings are not more frequently combined with cautionary notes concerning the obsolete character of their procedures and the unreliability of their data. For we must not close our eyes: these famous predecessors are often cited by our contemporaries in a bid to justify their own overinterpretative excesses.

**The Necessary Interpretative Risk**

The various figures of overinterpretation reviewed above have obviously been used as a “foil.” Overinterpretation is one of the ills of our profession. But this does not mean that the solution to the problem of overinterpretation is an overcautious attitude, glued to data and minimizing interpretation. *Taking empirically constrained interpretative risks is central to anthropological research.* In this sense, the snare of overinterpretation is always present, because social science work is impossible otherwise. The comfortable posture that entails taking refuge behind the phrase “this is a lot more complex than you have said” without proposing an alternative model is clearly unproductive. Restricting ourselves to a purely descriptive and legalistic ethnography is sterile. There is no safe haven to shelter us from interpretative risks.

However, the procedures of indispensable interpretative risk taking are liable to shift continuously in the direction of overinterpretation. This has been pinpointed in our rapid exploration of the figures of overinterpretation. Each of these figures is related to a normal, necessary, healthy exercise of the anthropological (sociological, historical) imagination. *There is no socioanthropology without the quest for more or less dominant factors, no socioanthropology without the quest for coherence, no socioanthropology without translation, no socioanthropology without generalization, no socioanthropology without the quest for some not directly “visible” sense.*
But that does not mean that we must restrict ourselves to a single factor, to obsessing about coherence, to meaning inadequacy, to abusive generalization, or to the “ploy of the hidden sense”!

Risk taking and reasoned interpretative leaps, insofar as they are empirically argued and do not enter into contradiction with the data available at the time, and do not claim to be more empirically grounded than they really are, cannot be qualified as overinterpretations. The issue is the absence of methodological or argumentative vigilance in relation to “data.” This is the “hallmark” of overinterpretation. Overinterpretation in the sense used here is, paradoxically and first, a deficit in meaning, since it disregards all other possible meanings entailed in the data it ignores or deforms. In other words, overinterpretation provides neither more meaning nor more audacity; for better or worse, it is, rather, less meaning and less audacity with respect to the empirical work. Empirically, it is the lowest bidder. It is deficient in terms of scientific exigency, in its theoretical and hypothetical, descriptive and factual, dimensions. To give in to the seduction of overinterpretation is to force data to fit our needs, to take the easy way out, to refuse the theoretical challenge of taking into account the complexity that data present to the rigorously interpretative imagination.

**Criticism, Counterexample, and the Collective Investigation**

This brings us to a redoubtable question: How are we to proceed, seeing that there is no vaccine for, antidote to, or prophylactic against overinterpretation?

The sole response seems to reside in an indefinable mixture of professional experience and empirical vigilance, which may preserve us from the most visible traps of overinterpretation. Indeed, there are no formal rules or methodological procedures capable of making interpretation totally safe. Since veracity is integrated into the argumentative and interpretative procedures from which it is inextricable, it cannot be guaranteed outside these procedures by establishing a kind of extra-interpretative and extra-argumentative methodological guardrail. Yet, attempting to secure some degree of interpretative safety through empirical adequacy is necessary. Hence the call for know-how and vigilance, despite the fact that such a mix is not easy to define in epistemologically acceptable terms. It is nevertheless possible to suggest two intellectual operations whose repeated exercise remains the best remedy, in terms of both prevention and cure.
1. The first is debate and criticism, and sometimes controversy. In our disciplines, the more we favor the exercise of critical thought (institutionally, scientifically, and intellectually), the easier it will be to detect the pitfalls of overinterpretation. However, debate must also address the empirical adequacy of interpretations; it must not be limited, as it too often is, to theoretic confrontation, the coherence of statements, postulates and postures, and intellectual or political argumentation. As underlined above (see Chapter 1), criticism in anthropology must also be a criticism of sources. Criticism must also relate to “evidence,” to the “data produced,” to empirical plausibility, and to the veracity of the assertions; it should not be limited to the confrontation of paradigms. In this way, the tension between empiricity and interpretativity may be regulated.

In this respect, great progress has made been in anthropology over the last decades, with the multiplication of revisited fieldwork sites, the relative departure from exoticism, and the globalization of the profession. The relocation of anthropological pastures and the phenomenon of anthropological overgrazing have become realities. This should be a source of satisfaction rather than reason for disapproval. There is almost no region in the world that has not arrived at its second or even third generation of anthropological studies. In the world today, villages or professions that have been ignored by inquiry have become increasingly rare. Moreover, anthropology is happily no longer the prerogative of European or North American intellectuals. The results arising from anthropological inquiry are increasingly discussed directly in the countries concerned. For all these reasons, it is now possible to go on to counterevaluations. In the past they have been cruelly lacking owing to the appalling system of Western prerogative in classic (Western) ethnology. Thanks to the multiplication of works of various origins, the relation between reference reality and interpretation can at long last become the object of documented criticism. The presumption of overinterpretation can now be argued. This is virtually a revolution in anthropology, one whose importance has rarely been highlighted, and whose consequences have not yet been taken into account.

2. The second productive intellectual procedure is the countereexample. It is also a strategy of debate. But debate, with or without the use of countereamples, relates almost exclusively to finished products. However, when points of view are already structured and crystallized in the form of writing, and when the ego gets involved, it is doubtful whether the author appreciates the full impact
of opposing counterexamples that question a ready-to-use interpretative model. The positive repercussions of ex post criticism are therefore indirect and delayed. No researcher ever Rewrites a paper or a book following the expression of criticism about it! At the best, he hopes to do better next time!

Counterexample is all the more beneficial as it is located prior to the end product. It is in the process of research that it proves its effectiveness, especially during the phase of data production when the first interpretative hypotheses “are not yet hardened” and still leave room for others— when new, more fertile, and more empirically founded interpretations may still emerge. Therein lies the interest of what has been called analytic induction, an approach aimed at constructing the process of theorization regarding the search for and the analysis of negative cases.

Counterexample is in fact quite advantageous at the level of fieldwork epistemology that interests us here. It is an empirical argument. On the one hand, it obliges us to go back to data, to the double relation—[reference reality/data] and [data/interpretations]—referred to above as empirical adequacy. It gives flesh, form, and contents to the exigency of methodological control. On the other hand, it is a latent counter-interpretation, an invitation to an alternative, better-grounded empirical interpretation.

Far from taking us backward—out of the frying pan and into the fire, from overinterpretation into renouncing interpretation—counterinterpretation obliges us to rebound by summoning the interpretative imagination to produce a “model” that is more demanding in terms of empirical plausibility and veracity by dint of integrating one or more counterexamples.

This obviously leaves us with a practical and trivial question: In the field, who is to say to the anthropologist, “Your current hypothesis is unsatisfactory, because there are case D and case G which you forgot, and which contradict it”? The solitary exercise of anthropology, which remains predominant, makes this difficult. Of course, there is a certain form of schizophrenia present in the profession that may be of help. But it is usually the combination of savoir faire and vigilance forged by professional experience (if not in all cases, at least in some) that allows the anthropologist engaged in fieldwork to have a critical dialogue with himself.

There is another solution, however: team fieldwork. This approach is as old as anthropology itself but has remained relatively marginal in a discipline strongly marked by an individualistic tradition
(going as far as treating the field as a private domain). In American qualitative sociology, in particular, team fieldwork has been commonly practiced. “Team field research offers the only alternative to the Lone Ranger approach in field research.”

The forms and objectives of team fieldwork have changed over the last few decades, as the image of fieldwork itself. In the perspective defended here, teamwork offers many advantages, insular as the coexistence of researchers in common or similar fieldwork sites, on the basis of affinities relating to problematics, methodology, and common research questions, allows work to function in a constant to and fro movement: between interpretative hunches and the search for data that either validate or contest them, and between unexpected and irrelevant data and the interpretations that organize them in a meaningful way. Such a process is all the more effective when conducted collectively and when each team member may at any time indicate counterexamples to another.

Consequently, we have attempted, Thomas Bierschenk and myself, to elaborate a framework of multisite team fieldwork, implemented in many recent research programs in Africa. Team fieldwork allows, in certain conditions, the confrontation of interpretations of field data, an enhanced explicitation of problems, better triangulation, greater attention to counterexamples, and greater vigilance with respect to empirical rigor. But it is in no wise a panacea. In fact, doing research as a team implies alternating phases of teamwork and personal work and always entails, following the teamwork phase, a period of in-depth individual fieldwork and participant observation. It necessarily mobilizes the professional know-how of which this work has attempted to define certain conditions. It is certainly no substitute for the indispensable vigilance of the field researcher. But it enhances this vigilance. Finally, it allows for the tackling of comparative multisite research and, consequently, the issue of qualitative comparison.

Admittedly a mechanism of interpretative exaggeration, a follow-the-leader attitude and cliquishness may also work their way into teamwork. Like any other methodological procedure, team fieldwork is not always a foolproof guarantee against overinterpretation. But a certain experience of it guides my opinion that, at least in certain conditions—and especially within a common acceptance of rules such as free mutual criticism, the search for counterexamples, the refusal of dogmatism, and the absence of obsequiousness—teamwork enables the stimulation of the empirically grounded
interpretative imagination and significantly diminishes the pitfalls of overinterpretation.

If we agree that there is no miracle solution lurking in the hat of an epistemological magician, we have no choice but to make do with the type of tinkering set out in this chapter. But this is certainly better than nothing.
Chapter 2


2. Oppositions between the qualitative and the quantitative have sometimes led to epistemological confusions: “The division between qualitative and quantitative methods became associated—unfortunately and inappropriately—with the rival positions of positivism and interpretivism” (Gerring and Thomas, 2005: 1). For example, Burawoy (2003: 435–38) opposes the model of positive science (represented by inquiry through questionnaire) and the model of reflexive science (represented by the case study). If, on the contrary, one accepts the epistemological unity of the social sciences, regardless of their methods (the position defended by Passeron and by Berthelot, which is also adopted here), positivism and hermeneutism, these warring brothers, can be regarded as scientific ideologies (or biases) functioning independently of the methods of data production, even if positivism is more of a threat to quantitativists, and hermeneutism to qualitativists. But this is only one tendency; hence, we have the famous examples of anthropologists with a strong positivist inclination, such as Lévi-Strauss, Murdock, or Radcliffe Brown.

3. It should be recalled that qualitative methods are not at all limited to anthropology, although most of them stem from anthropological field inquiry, which is the focus of the present work. Besides, the term “qualitative methods” is used mainly by practitioners of other disciplines. They may be found in a variety of forms (some of which are more “applied”) in political and administrative sciences, criminology, social psychology, the sciences of education and communication, development studies, public health or social services studies. Some qualitative methods may be quite different from anthropological fieldwork: they are sometimes experimental, are sometimes formalized; they range from group psychology to the comparative political analysis, from content analysis to the Boolean truth tables.

4. Strauss (1987: 2) therefore notes that the force of the qualitative inquiry resides in the taking into account of contexts, whereas the force of the quantitative inquiry derives from the fact that it is multivariate and cross-comparative on a large scale. For Katz (1983: 137), “Statistical evidence of representativeness depends on restricting a depiction of qualitative richness in the experience of people studied. A similar practical trade-off confronts those who do inductive research but it forces the opposite choice.”

5. See Tashakkori and Teddlie, 2003; Ridde, 2008; and the Journal Mixed Methods Research.

6. Schatz (2007) underlines these difficulties.

7. Fieldwork thus becomes a “myth” (see Schwartz, 1993: 270–71), “an entitlement to glory” (see Schwartz and Jacobs, 1979: 125). “The subjects of ethnographies, it should not be forgotten, are always more interesting than their authors” (Smith, quoted in Sanjek, 1991: 610). See Chapter 5.
8. The term “bricolage” (tinkering) is often used to describe the methodology of anthropology and qualitative methodologies in general (Denzin and Lincoln, 1994: 2; see also Weinstein and Weinstein, 1991, who consider Simmel to be the prototype of the tinkering sociologist). Terms like “craft” relating to artisans are also used (see Epstein, 1978).

9. One might consider field inquiry as a “natural analysis” (Schwartzman, cited by Strauss, 1987: 3), in the same way that one speaks of a “natural language” or again in the way in which social sciences are said to operate in the register of “natural reasoning” (Passeron, 1991). In all of these expressions, “natural” is by no means opposed to “cultural” (these are obviously social constructs); its inverse is “artificial” (created by the researcher), and what it refers to are everyday interactions, ordinary practice, and common sense.


11. Outlines of a “history” of anthropological fieldwork (and of the evolution of methodological and epistemological thought on this subject) may be found in Jongmans and Gutkind, 1967; Stocking, 1983; van Maanen, 1988; Sanjek, 1990; Cefaï, 2003.

12. It appears, quite significantly, that this term, bearing a strong anthropological connotation, was coined in 1924 by a sociologist, Lindeman, associated with the Chicago School (Kirk and Miller, 1986: 76). Other more or less equivalent terms, including “immersion” which we have resorted to here, or “involvement” (which we use in Chapter 5 to discuss the forms of participation/adhesion linking the researcher to the social group being studied), are also used.


14. In fact, these roles are often the result of circumstances, of the constraints of the inquiry, or of the habits of the researcher rather than that of a deliberate choice, contrary to frequent affirmations.

15. “The role of participant observer may be either formal or informal, concealed or revealed; the observer may spend a great deal or very little time in the research situation; the participant observer role may be an integral part of the social structure or largely peripheral to it” (Schwartz and Schwartz, 1955: 343).

16. Goffman uses the term “strip” to designate the “fragments of reality” that interest the analyst (Goffman, 1991).


18. “If there are indeed problems in ethnographic description, they will not be solved by less detailed fieldwork and writing” (Parkin, 1990: 182).

19. Audrey Richards addressed this subject as “speech-in-action” in 1939 (Sanjek, 1990: 212). The increasingly massive recourse to interviews as well as certain exhortations in favor of “dialogic anthropology,” which prioritizes verbal interaction between the researcher and the populations (Fabian, 1983; Clifford and Marcus, 1986), overshadow the nonetheless fundamental dimension of participant observation.

21. On this subject, Sluka and Robben (2007: 8) make the following comment on the work by Keesing and Strathern (1998): “Keesing and Strathern draw attention to the gap between the data described in field notes and the lived experiences, sounds, smells, and scenes that cannot be captured in writing but are sedimented in the unconscious. Their use of the term ‘unconscious’ is somewhat misleading because it suggests that the fieldworker is unaware of these impressions, but Keesing and Strathern are right in arguing that the anthropologist draws upon an unpronounced understanding that is larger than field notes and gives the ethnography a richness which could not have been obtained any other way than through an extended immersion in the field.”

22. Anthropology is often characterized as “actor oriented” (see Long and Long, 1992: 9). It is supposed to put into practice the comprehensive sociology invoked by Weber, who, paradoxically, provides no empirical tools to this end (see Chapter 3). One might call to mind Malinowski’s sentence at the end of the famous introduction to *Argonauts of the Western Pacific*: “[The final goal of ethnography] is, briefly, to grasp the native’s point of view, his relation to life, to realise his vision of his world.” (Malinowski, 1922).


24. This has not always been the case. We know that Griaule, for example, and other colonial ethnologists and anthropologists used, and sometimes abused, directivity; see van Beek, 1991: 139–58. Conversely, for Hughes, “Interviews lean towards the form of the society’s conversation to the extent that, once the interviewer catches on to the level of the interviewee’s discourse and adapts himself accordingly, communication becomes supposedly close to communication between equals, and the information collected is considered as coming from a man talking freely with a friend” (Hughes, 1996: 287).

25. This corresponds to what Cicourel calls “ecological validity” (Cicourel, 1982: 11–20), in other words, the degree to which the circumstances created by the researcher’s procedures match those of the everyday worlds of the subjects (Briggs, 1986: 24). This is why it is often advisable to start interviews with an informal chat, or with so-called “descriptive” questions that solicit the interlocutor on a familiar or convenient register of discourse. Spradley insists particularly on these types of descriptive questions (Spradley, 1979: 81–83). He also establishes a parallel between conversations and the ethnographic interview as being two close types of “speech events” whose similitudes and differences he analyzes.

26. “Appropriate or relevant questions are seen to emerge from the process of interaction that occurs between the interviewer and the interviewees . . . ; the success of this undertaking is ultimately contingent about the skill and sensitivity of the interviewer” (Schwartz and Jacobs, 1979: 40). “The best question is not one in the grid, it is the one that is spurred by what the ‘informer’ just said” (Kaufmann, 1996: 48).

27. Schwartz and Jacobs, 1979: 45.

29. Look back at van Beek’s analysis of Dogon’s manner of answering to Griaule (van Beek, 1991).


32. For example, Passeron reminds us of the propensity interviewed actors have for putting a “patina of logic,” according to Pareto’s expression, on what they say (Passeron, 1995: 62, 80).

33. In the same vein, some authors have suggested that a distinction should be made between “empathy”—an attitude that is a methodological requirement—and “sympathy,” comprising personal feelings or even identification and entailing the risk of bias (see, for example, Paillé and Mucchielli, 2003: 71). “These personal sentiments would appear to become a potential biasing feature of the research when the observer’s intentions to empathize with the individuals in the setting, that is, to truthfully understand the situation from the actors’ perspective, become transformed into a sympathetic stance. A sympathetic stance exists when the observer takes the side of, or promotes the perspective of the group he studies” (Johnson, 1983: 215). This is the type of bias that will be analyzed in relation to the subject of populism (see Chapter 6).

34. See, inter alia, Agar and MacDonald, 1995; Morgan, 1988. “Saying that a focus group is a workable option for a project is not the same as saying that they are the best way to gather the data for that project” (Morgan, 1988: 24).

35. Cf. RRA (rapid rural appraisal) and PRA (participative rural appraisal) (see Chambers, 1994).

36. “Qualitative observation has remained under-addressed in the methodological literature . . . Most of the major research treatments of qualitative methods focus on participant observation to the virtual exclusion of observation as a method in its own right” (Adler and Adler, 1994: 378).

37. The ethnographic observation has been described as “the ‘silent’ dimension of the social” (Hirschauer, 2006: 413).

38. “Researchers almost systematically use additional techniques of a quantitative type, such as intensive survey, statistics, genealogies and interviews structured around a carefully selected sample” (Ghasarian, 2002: 10). Maget’s manual (1953) had already set out “all the artifacts which stabilize data: statistics, technical drawings, morphological schemas, graphs, maps and photographs” (Cefaï, 2003: 29).

39. “Qualitative research implies a commitment to field activities. It does not imply a commitment to innumeracy” (Kirk and Miller, 1986: 10). “Examples of methods of survey and inventory abound in works based on observation. The arsenal of methods used to make inventories, surveys, and frequency calculations, are almost unlimited” (Peneff, 1995: 123). Becker evokes the usefulness of what he calls “quasi-statistics,” “imprecisely sampled and enumerated figures” (Becker, 1970: 81).
40. Schwartz and Jacobs, 1979: 75.
41. See the various examples quoted in Becker, 1970.
42. See Hockings, 1975; de France, 1994; Devereaux and Hillman, 1995.
43. See Olivier de Sardan, 1999.
44. “Ethnography consists of a cluster of methods rather than a single one” (Schatz, 2007: 6).
47. Briand and Chapoulié (1991) consider this to be peculiar to French sociology, less inclined to the practice of observation than American sociology. However, Sanjek, for his part, distinguishes this tendency in urban Anglo-Saxon anthropology, of which he deplores the “interview-based” character (Sanjek, 1990: 247).
48. “All forms and tactics of social research must be continually related and successfully embedded in the social setting being researched. Each method has its values and problems and these must always be weighted against each other” (Douglas, 1976: 34).
54. This may be seen as advocacy for what Stake terms the intrinsic case study, in the framework of a typology in which he differentiates between the instrumental case study (the case studied based on broader objects that it serves to elucidate and illustrate); the intrinsic case study (the case studied for itself, as a specific object); and the collective case study (several similar or distinct cases are collected, analyzed, and compared). He adds other varieties appraised as marginal: the case for pedagogical use, the biography, and the legal case (Stake, 1994: 237–38).
55. See Lund, 2014.
56. “Anthropological analysis commences through the discovery of ‘problems.’ These are not a priori hypotheses” (Wright, 1994: 11). Or, “The research commences with one series of questions and finishes with a very set of interrogations” (Burawoy, 2003: 434).
58. See Quivy and van Campenhoudt (2006: 84): “A research in social sciences tends to exceed a simple description of the social phenomena (even if a good description is no easy matter and can be invaluable); it aims at explaining these phenomena.”
The term “triangulation,” which is used here in reference to the choice of varied interlocutors with the aim of confronting points of view, has been the object of broader definitions, intersecting with various aspects here addressed under other names. Denzin (1978) thus distinguishes triangulation through data (which I prefer to call source combination; see above), triangulation through researchers (this, in fact, refers to collective inquiries or revisits to the field; see below), triangulation through theories (I evoke instead the refusal of theoretical orthodoxies, or the combination of heuristic points of view; see Chapter 6), and triangulation through methods (which aims at associating qualitative methods and quantitative methods, but which can also intersect with the triangulation of sources).

For a more detailed discussion of this concept initially elaborated by Bierschenk, based on a preliminary approach by Evers and Schiel, 1988, and of its application in collective field inquiry, see Bierschenk and Olivier de Sardan, 1997.

63. “Writing in which the researcher puts down theoretical questions, hypotheses, summary of codes, etc.” (Strauss, 1987: 22).
64. See Rabinow, 1988.
65. See Bierschenk and Olivier de Sardan, 1997.
66. Ginzburg, 1980 (also cited in Ginzburg, 1989). Since then, this metaphor has been extensively used to describe the relationship between qualitative data and interpretations; for example, in political science, see Gerring and Thomas, 2005: 13.
67. “I shall argue in this book that the investigative methods of journalists and some of the investigative occupations, such as detective work, are of great importance in developing all forms of field research” (Douglas, 1976: 14).
69. See the notion of gatekeeper (Schwartz and Jacobs, 1979: 55).
70. I will not take “statutory” biases into account here, not because they do not exist, but because their impact is very difficult to evaluate concretely and varies considerably according to the various facets of the researcher’s savoir faire (see Chapter 5). These statutory biases can refer to the social condition of the researchers (largely underlined by Bourdieu and his disciples), to their national or ethnic origin (evoked in postcolonial studies), or to gender (highlighted by feminist authors denouncing “a male bias”; see Ghasarian, 2002: 17).
71. This can be compared to the bias of the cinematographic eye (see de France, 1979) in documentary or ethnographic films: the presence of the camera clearly modifies the attitudes of the persons being filmed, but to varying degrees depending on the theme, duration, and immersion of the filmmaker, among other factors.
Notes

74. See, for example, Labov, 1976.
75. See, for example, Favret-Saada, 1985.
76. “Most scholars who have thought about the methodology of fieldwork warn the ethnographer about identification with only a faction of the community he is studying” (Barnes, 2003: 171).
77. See Berreman’s (1962) very clearly analyzed example.
78. For example, Goffman, in the face of what he calls the “the affiliation issue,” gives the following advice: “You can’t move down a social system. You can only move up a social system. So, if you’ve got to be with a range of people, be with the lowest people first” (Goffman, 1989: 130).
79. An assessment of controversies of this kind can be found in Shipman, 1988.
80. See, for example, Borel, 1990; Geertz, 1988.
82. See the pioneering work of Berreman, 1962.
83. Olivier de Sardan, 2014.
84. This corresponds approximately to the term “validity of meaning” (Pourtois and Desmet, 1988).
85. It is true that, increasingly, the members of a group that has been studied are readers of the report that concerns them (see Brettel, 1993). In this case, it is possible to make remarks similar to those made on the topic of restitution.
86. Marcus (1995a) uses the film-editing metaphor, but in aid of a “postmodern” line of argumentation (which is not mine, as one might well imagine), underlining the artificial character of narrative procedures and the total dissolution of realism.
88. Quoted in Fassin, 1990: 100.
89. Quoted in Barnes, 2003: 169.

Chapter 3

1. In French, it is the phonetic/phonology opposition that was mobilized, the phonological revolution (corresponding to phonemic) stressing the way in which the speaking subject uses and understands sounds (see Prieto, 1975).
3. Harris, 1976.
4. Headland (1990: 21), for his part, adds to this the following oppositions: description/theory, private/public, and ethnography/ethnology.
54. For a theoretical presentation of “practical norms” and how they relate to actors’ agency, cf. Olivier de Sardan, 2015b.

55. This is true whatever the meaning of the term institution—the classic anthropological sense (Douglas, 1989) or the neo-institutional economy sense (North, 1990; Ostrom, 2005).

56. Valade pleads also for a “methodological pluralism which takes its source in the various ways of questioning” social phenomena (Valade, 2001: 400).


CHAPTER 7

1. It is certainly easier to pinpoint excesses in the work of others, but it is clear that each of us has already been guilty of overinterpretation.

2. In the sense developed by Boltanski (1982) and mentioned above (Chapter 6).


4. Some of these operations were described in Sperber, 1982, in particular interpretations of indigenous interpretations and interpretative generalizations.


6. We could also evoke the complementarity between “structural constraints” and “contextual constraints” in Strauss (1993).

7. An enlightening example, among others, of the fact that “traditional knowledges” are not necessarily organized in “systems” is provided by Last (1981: 387–92), concerning, in this case, Hausa therapeutic knowledges in northern Nigeria.

8. Of course, critique of this obsession with coherence was already made a long time (the same applies to the other figures presented here). Within contemporary anthropology, in addition to Keesing (1987), we may mention Boyer, who attacks the “presupposition of cohesion” (Boyer, 1990: 3–4). Evoking Marx, Mannheim, Lukacs, and Goldmann, Passeron denounces, for his part, this trend that has “always tended to overestimate the coherence of systems of ideas to which the thought of an individual or a group makes reference” (Passeron, 1995: 111).


10. “Some of our ethnographic accounts and interpretations are simply wrong, constructed out of misunderstandings and mistranslations—failure to grasp meanings that are, for native participants in a community, the stuff of everyday life and talk” (Keesing, 1989: 459).

11. This question of translation in anthropology has given rise to a vast literature; see, inter alia, Needham, 1972; Sperber, 1982. Obviously, many of
the debates concern the space of competing empirically legitimate interpretations. But there are also translations that can be “quasi-proved” to violate emic data for the needs for their argumentation.

12. I have developed this demonstration elsewhere (Olivier de Sardan, 1984) and have proposed (Olivier de Sardan, 1993), also in connection with possession “cults,” an analysis of another type of overinterpretation, which related to Subfigure 3.4 below (the abusive emic charge).


15. The grand partage, opposing surviving “traditional” societies and “modern” societies, traverses the entire history of the social sciences and its various disciplines, from Tonnies to Deleuze and Guattari, via Parsons. This recurring ideology is related to culturalism (cf. Olivier de Sardan, 2015a). For critiques of the grand partage, see Latour, 1983. To the extent that some authors tried to justify the grand partage in the name of Weber, by opposing traditional legitimacy in the South and rational-bureaucratic legitimacy in the North, it is necessary to quote the following clarification: “Nothing is more foreign to Weber than the notion that global societies, epochs, and configurations (be they technical, economic, or symbolic) may be entirely defined by ‘tradition,’ or ‘rationality,’ or by a specific form of rationality. It is clear that the schema of the Grand Partage . . . is first and foremost the manifestation of mental sloth” (Passeron, 1995: 114).

16. Abusive generalization in this regard reminds us of what Boudon (1984) calls the “upgrading of enunciations” (an enunciation of possibility becomes an enunciation of conjecture, and an enunciation of conjecture becomes a nomologic enunciation).

17. See Passeron, 1999: 11.

18. See Mauss, 1983 (Essai sur le Don was first published in French in 1923).


22. See Godelier, 1996.


24. “The theory is generated by a constant confrontation with ‘negative cases,’ those that do not confirm the formulation in progress. The researcher is thus engaged in a process of reformulation of hypotheses and redefinition of phenomena” (Strauss, 1993: 286). “To the extent that the goal of interpretation predominates, contrary evidence represents nothing more than the fully anticipated deviation of empirical events from ideal-type images . . . The departure of empirical events from theoretical models is not confounding; it is raw material for historical explanation . . . Contrary evidence is typically used as a basis for refining, not rejecting theory” (Ragin, 1987: 167).

25. Douglas, 1976: 193. Douglas was one of the first to argue about the advantages of team research.
26. Passeron poses the same problem in connection with the four-handed *writing* of a text: “It takes at least two to make reciprocal objections to something that is liable to change the course of a reasoning; the objections raised to oneself are too readily neutralized or transformed into a rhetorical monologue; and in the case of objections made when the text is already written the author is inclined to be defensive in his responses” (Passeron, 2003: 120). The particularity of fieldwork is that it is *during the production of data* that interpretations are worked out and that the counterexemplary dialogue usefully occurs.

27. Bierschenk and Olivier de Sardan, 1997; Olivier de Sardan, 2011.

Chapter 8

2. See, for example, Stoller and Olkes, 1987; Gibbal, 1988. For a more developed critique of the exoticizing of magic and “ethno-ego-centrism,” cf. Olivier de Sardan, 1992a.
4. Phenomenological sociology, with which the name of Schutz is associated, should not be confused with the phenomenological invocations that deconstructionist or postmodern postures sometimes accumulate. Egon Bittner, who defends the realistic anchoring of phenomenological sociology (“the claim to realism—of faithfulness to reality,” Bittner, 1983: 149), thus warns against what he calls an “abortive phenomenology,” taking the form of a “pallid ideology of cultural relativism” (Bittner, 1983: 155).
12. For a critique of the opposition between science and ideology, see Ricœur, 1974. It should also be noted that the terms ideology and common sense, as used here, are not synonymous concepts, even if they do communicate. Moreover, our focus is on ideologies influencing the scholarly sense. These terms cover various levels of discourse and representations. They are therefore used here merely as convenient approximations.
13. For example, Becker proposes the *successive* use in the field of various metaphors as “tricks” to avoid entrapment in a single perspective: to see the social situations as purely random, then to see them as machines, then to see them as organizations (Becker, 2002: 51–103).
15. We may note, besides, that the *folk classifications* that served as a basis for the ethno-science of the 1960s did not keep their promises as a research program (see Keesing, 1972).
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Short Take 13: 
Ethnographic Sampling

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For many cultural anthropologists, the term “ethnographic sampling” is an oxymoron. When we talk about sampling, we usually mean a probability sample that represents a population. Can an ethnography establish what is typical? Is it even reasonable to ask this question, given the constraints under which ethnographers must work? In this column we try to answer these questions.

Werner was reminded of the problem of ethnographic sampling on rereading Clyde Kluckhohn’s (1944) book Navajo Witchcraft. What a remarkable ethnographer Clyde Kluckhohn was! He collected his first Navajo witchcraft stories in English in 1923 when he was 19 years old.1 Almost every year thereafter, until the publication of his book in 1944, he spent some time in the field collecting stories. From 1932 on, he collected stories systematically (Kluckhohn 1944:15).

Kluckhohn described his sample of stories in great detail. Unfortunately, his description was entirely in prose, with even large numbers (>10) written out in words (Kluckhohn 1944:16). We summarize his prose in the following tables. Table 1 shows what Kluckhohn told us about 132 formal interviews he had with 93 different consultants.

Kluckhohn adds that he took notes in the presence of his informants. The notes ranged from two pages to 99. The asterisked numbers are questionable or are our conjectures. In what are labeled “passive” interviews, witchcraft was not the main topic but it came up in the course of the interview. The 28 interviews in this column (“25 plus two or three others,” Kluckhohn 1944:15-16) are based on consistent or comprehensive questioning. He conducted these to arrive at “statistically sound generalizations” (Kluckhohn 1944:16). We have no explanation for Kluckhohn’s report of 113 informants in the age column.

Table 2 summarizes 81 informal interviews. Since the total number of interviews is not given explicitly, we presume that Kluckhohn conducted exactly 81 informal interviews to a full day after casual interviews, and range from a few lines to 10 pages. The 73 interviews in Navajo were “direct,” presumably without interpreters (Kluckhohn 1944:16). The remainder were in English. Whether these consists of 29 individuals or 42 interviews is not clear.

Reading Kluckhohn’s introduction to Navajo Witchcraft, one is struck by the apparent comprehensiveness of his sampling and by his effort to arrive at “statistically sound generalizations.” Once his prose is tabulated, however, his sampling looks incomplete. For example, we don’t know the gender of the consultants who participated in the informal interviews, or whether Kluckhohn used an interpreter for some of the informal interviews conducted in Navajo. And we know nothing about the consultants who participated in Kluckhohn’s casual interviews.

We can further check Kluckhohn’s sample by looking at the geographic distribution of his interviews. He again describes the facts in prose and in great detail listing locations in which he conducted interviews. He presents no maps to show the geographical

Continued on page 8
distribution of his interviews; the following maps are interpretations of his prose.

The Hopi Reservation is completely contained within Navajo Country. The Jicarilla Apache Reservation is to the east of the northeastern end of the reservation. The small black circles indicate at least one interview in that location (Kluckhohn gives no figures). The larger black circles in the Ramah, NM and Two Wells, NM areas (the latter not named on the map) represent 38 interviews, plus 8 from the "check" group (the 28 or so formal interviews based on consistent and comprehensive questioning). In addition, 23 of the 81 informal interviews came from these two areas. The geographical distribution of the casual interviews cannot be determined from Kluckhohn's text.

Thus 32%, or 69 interviews came from the Ramah/Two Wells area—Kluckhohn's home turf. His statement about the geographical distribution is confusing: "The western Navajo territory is much less well represented than the eastern. The north is scarcely represented at all" (Kluckhohn 1944:17). The map tells a different story. The north is reasonably well represented while the west is scarcely represented at all. But "west" may refer to the locations of Tuba City, Kayenta, and Navajo Mountain where he may have conducted more interviews than in the north. Exact figures would help.\(^5\)

The major lesson we learn from Kluckhohn's sampling is that tabulation of data about interviews—the age, gender, occupation, and economic status of the consultant, the location of the interview, and so on—helps make ethnography more systematic and more useful. Malinowski had recognized this before Kluckhohn was a student: "The method of reducing information, if possible, into charts or synoptic tables ought to be extended to the study of practically all aspects of native life" (Malinowski 1961:14; orig. 1922). Kluckhohn would have done better had he followed Malinowski's advice.

Can we make "statistically sound generalizations" from Kluckhohn's sample of interviews? The answer is obvious: we cannot. Only a random sample can capture (with a known probability of error) a distribution of traits similar to the distribution of the traits in the population from which the sample was taken, and there is certainly nothing random about Kluckhohn's sample: he interviewed those he could. Do we reject Kluckhohn's work as unscientific? Not at all. For while Kluckhohn's sample of consultants was not statistically representative, Kluckhohn covered the waterfront. He established the range of witchcraft phenomena among the Navajo.

That's what good ethnographic sampling is about — establishing the range of phenomena, not establishing the proportions of traits in a population at large. Figure 2 represents the difference between the goals of a probability sample and an ethnographic sample.

A random sample (Figure 2a) establishes what is typical in a social system (population). In an ethnographic sample (Figure 2b) we usually have no idea what is typical. We work with key consultants, often experts, on topics like witchcraft, hunting, manioc planting, etc. Not knowing where to start, we start anywhere, often close to our entry point. We develop our network of contacts from there and the resulting contact tree (see Short Take 1 in CAM 1[1] and Short Take 6 in CAM 3[1]) is anything but random. We cover as much variation as we can of the phenomena in which we're interested and, while we then cannot say what is typical, we can say with some authority what range of traits does occur.

We can never claim that we've discovered all the variation within a social system, but a contact tree can show, for example, that we have concentrated in our interviewing too heavily on one type of consultant at the expense of the other. We may have too many rural consultants or too few urban ones, too many women or too few, too many weavers, or too few. The goal is to maximize variation.
To sum up, here are some pointers on how to maximize coverage of phenomena in an ethnographic sample of interviews:

1. Use tables and maps to see how well you’ve covered demographic and geographic parameters of interest.

2. Use contact trees to show how well you’ve networked through a population and to what extent the same types of people, with similar views, may be overrepresented. Starting a contact tree early allows you to exert some control over your contact network.

3. When three or more consultants agree on a fact within a homogeneous social system it is time to move on to another group that views things somewhat differently (D’Andrade 1989). Had Kluckhohn checked (with tables) to see if his consultants were converging on consensus about certain items, he could have maximized his sampling effort and covered much more ground.

4. Look for divergences of opinion and practices as well as for convergence. This will help you cover, as much as possible, the entire range of phenomena. Kluckhohn does not tell us about the controversies concerning witchcraft among the Navajo. Instead he gives us a homogenized picture of Navajo witchcraft as if it were an internally consistent system. In all likelihood, this consistency is his only. In any human society there coexist multiple views. In any controversial field (like witchcraft) within a culture there will be strong disagreements concerning facts and conceptions.

Because we can never interview everyone, sampling is inevitable. It is also inevitable that ethnographic samples can only under the rarest of circumstances, and following great effort, be probabilistic. The strength of ethnography lies in examining in depth the range of variation of traits. Ethnography and a sample survey, based on a probabilistic sample, are thus complementary.

In order to do ethnographic sampling well, however, an ethnographer must learn another culture systematically and not casually. Naroll (1962) found that, when it came to the quality control of ethnographic data, he could not tell amateurs from professional ethnographers. We think this is due to unsystematic, uncontrolled casual learning of cultures by amateurs and professional ethnographers alike.

Proper control of the ethnographic sample, and focusing on the range of variation of cultural traits is a safeguard against superficial understanding of another culture. To understand a foreign culture we need to know not only what they believe or do, but also what they argue about.

Notes

1. We have greatly benefited from comments on an earlier draft by Thomas Schweitzer.

2. We consider this fact of Kluckhohn’s life a sound argument in support of ethnographic field schools for undergraduates — if such schools need any defense at all.

3. If readers know of similarly thorough descriptions of an ethnographic sample, we would appreciate if they would let us know.

4. In addition, Kluckhohn mentions 16 interviews with non-Navao traders and 87 pages of field notes from other ethnographers.

5. Collecting data over so wide an area as Kluckhohn did in the 1920s, 1930s and 1940s was a very difficult undertaking. There were virtually no paved roads on the reservation until after World War II. Just the trip from Ramah to Kayenta by car may have taken a couple of days with sand and mud impeding progress. Even the famous Route 66 between Albuquerque, NM and Grants, NM was not fully paved until after the war (William Y. Adams, personal communication).

6. Similar ideas about consultant consensus that may help restricting sample size are also expressed by Romney et al. (1986) and Weller and Romney (1988:77). "The important point to observe is that small samples can yield extremely reliable data." For details consult the literature on consensus theory listed, for example, in Weller and Romney (1988).

References


6 What makes oral history different

Alessandro Portelli


‘Yes,’ said Mrs. Oliver, ‘and then when they come to talk about it a long time afterwards, they’ve got the solution for it which they’ve made up themselves. That isn’t awfully helpful, is it?’ ‘It is helpful,’ said Poirot… ‘It’s important to know certain facts which have lingered in people’s memories although they may not know exactly what the fact was, why it happened or what led to it. But they might easily know something that we do not know and that we have no means of learning. So there have been memories leading to theories…

Agatha Christie, Elephants Can Remember

His historical researches, however, did not lie so much among books as among men; for the former are lamentably scanty on his favorite topics; whereas he found the old burghers, and still more their wives, rich in that legendary lore, so invaluable to true history. Whenever, therefore, he happened upon a genuine Dutch family, snugly shut up in its low-roofed farmhouse, under a spreading sycamore, he looked upon it as a little clasped volume of black-letter and studied it with the zeal of a book-worm.

Washington Irving, ‘Rip Van Winkle’

MEMORIES LEADING TO THEORIES

A specter is haunting the halls of the academy: the specter of oral history. The Italian intellectual community, always suspicious of news from outside—and yet so subservient to ‘foreign discoveries’—hastened to cut oral history down to size before even trying to understand what it is and how to use it. The method used has been that of charging oral history with pretensions it does not have, in order to set everybody’s mind at ease by refuting them. For instance, La Repubblica, the most intellectually and internationally oriented of Italian dailies rushed to dismiss ‘descriptions
“from below” and the artificial packages of “oral history” where things are supposed to move and talk by themselves, without even stopping to notice that it is not things, but people (albeit people often considered no more than ‘things’) that oral history expects to ‘move and talk by themselves’.¹

There seems to be a fear that once the floodgates of orality are opened, writing (and rationality along with it) will be swept out as if by a spontaneous uncontrollable mass of fluid, amorphous material. But this attitude blinds us to the fact that our awe of writing has distorted our perception of language and communication to the point where we no longer understand either orality or the nature of writing itself. As a matter of fact, written and oral sources are not mutually exclusive. They have common as well as autonomous characteristics, and specific functions which only either one can fill (or which one set of sources fills better than the other). Therefore, they require different specific interpretative instruments. But the undervaluing and the overvaluing of oral sources end up by cancelling out specific qualities, turning these sources either into mere supports for traditional written sources, or into an illusory cure for all ills. This chapter will attempt to suggest some of the ways in which oral history is intrinsically different, and therefore specifically useful.

THE ORALITY OF ORAL SOURCES

Oral sources are oral sources. Scholars are willing to admit that the actual document is the recorded tape; but almost all go on to work on the transcripts, and it is only transcripts that are published.² Occasionally, tapes are actually destroyed: a symbolic case of the destruction of the spoken word.

The transcript turns aural objects into visual ones, which inevitably implies changes and interpretation. The different efficacy of recordings, as compared to transcripts—for classroom purposes, for instance—can only be appreciated by direct experience. This is one reason why I believe it is unnecessary to give excessive attention to the quest for new and closer methods of transcription. Expecting the transcript to replace the tape for scientific purposes is equivalent to doing art criticism on reproductions, or literary criticism on translations. The most literal translation is hardly ever the best, and a truly faithful translation always implies a certain amount of invention. The same may be true for transcription of oral sources.

The disregard of the orality of oral sources has a direct bearing on interpretative theory. The first aspect which is usually stressed is origin: oral sources give us information about illiterate people or social groups whose written history is either missing or distorted. Another aspect concerns content: the daily life and material culture of these people and groups. However, these are not specific to oral sources. Emigrants’ letters, for instance, have the same origin and content, but are written. On the other hand, many oral history projects have collected interviews with members
What makes oral history different of social groups who use writing, and have been concerned with topics usually covered by the standard written archival material. Therefore, origin and content are not sufficient to distinguish oral sources from the range of sources used by social history in general; thus, many theories of oral history are, in fact, theories of social history as a whole.  

In the search for a distinguishing factor, we must therefore turn in the first place to form. We hardly need repeat here that writing represents language almost exclusively by means of segmentary traits (graphemes, syllables, words, and sentences). But language is also composed of another set of traits, which cannot be contained within a single segment but which are also bearers of meaning. The tone and volume range and the rhythm of popular speech carry implicit meaning and social connotations which are not reproducible in writing—unless, and then in inadequate and hardly accessible form, as musical notation. The same statement may have quite contradictory meanings, according to the speaker’s intonation, which cannot be represented objectively in the transcript, but only approximately described in the transcriber’s own words.

In order to make the transcript readable, it is usually necessary to insert punctuation marks, which are always the more-or-less arbitrary addition of the transcriber. Punctuation indicates pauses distributed according to grammatical rules: each mark has a conventional place, meaning, and length. These hardly ever coincide with the rhythms and pauses of the speaking subject, and therefore end up by confining speech within grammatical and logical rules which it does not necessarily follow. The exact length and position of the pause has an important function in the understanding of the meaning of speech. Regular grammatical pauses tend to organize what is said around a basically expository and referential pattern, whereas pauses of irregular length and position accentuate the emotional content, and very heavy rhythmic pauses recall the style of epic narratives. Many narrators switch from one type of rhythm to another within the same interview, as their attitude toward the subjects under discussion changes. Of course, this can only be perceived by listening, not by reading.

A similar point can be made concerning the velocity of speech and its changes during the interview. There are no fixed interpretative rules: slowing down may mean greater emphasis as well as greater difficulty, and acceleration may show a wish to glide over certain points, as well as a greater familiarity or ease. In all cases, the analysis of changes in velocity must be combined with rhythm analysis. Changes are, however, the norm in speech, while regularity is the norm in writing (printing most of all) and the presumed norm of reading: variations are introduced by the reader, not by the text itself.

This is not a question of philological purity. Traits which cannot be contained within segments are the site (not exclusive, but very important) of essential narrative functions: they reveal the narrators’ emotions, their
participation in the story, and the way the story affected them. This often involves attitudes which speakers may not be able (or willing) to express otherwise, or elements which are not fully within their control. By abolishing these traits, we flatten the emotional content of speech down to the supposed equanimity and objectivity of the written document. This is even more true when folk informants are involved: they may be poor in vocabulary but are often richer in range of tone, volume and intonation than middle-class speakers who have learned to imitate in speech the monotone of writing.\(^5\)

**ORAL HISTORY AS NARRATIVE**

Oral historical sources are narrative sources. Therefore the analysis of oral history materials must avail itself of some of the general categories developed by narrative theory in literature and folklore. This is as true of testimony given in free interviews as of the more formally organized materials of folklore.

For example, some narratives contain substantial shifts in the ‘velocity’ of narration, that is, in the ratio between the duration of the events described and the duration of the narration. An informant may recount in a few words experiences which lasted a long time, or dwell at length on brief episodes. These oscillations are significant, although we cannot establish a general norm of interpretation: dwelling on an episode may be a way of stressing its importance, but also a strategy to distract attentions from other more delicate points. In all cases, there is a relationship between the velocity of the narrative and the meaning of the narrator. The same can be said of other categories among those elaborated by Gérard Genette, such as ‘distance’ or ‘perspective’, which define the position of the narrator toward the story.\(^6\)

Oral sources from nonhegemonic classes are linked to the tradition of the folk narrative. In this tradition distinctions between narrative genres are perceived differently than in the written tradition of the educated classes. This is true of the generic distinction between ‘factual’ and ‘artistic’ narratives, between ‘events’ and feeling or imagination. While the perception of an account as ‘true’ is relevant as much to legend as to personal experience and historical memory, there are no formal oral genres specifically destined to transmit historical information; historical, poetical, and legendary narratives often become inextricably mixed up.\(^7\) The result is narratives in which the boundary between what takes place outside the narrator and what happens inside, between what concerns the individual and what concerns the group, may become more elusive than in established written genres, so that personal ‘truth’ may coincide with shared ‘imagination’.

Each of these factors can be revealed by formal and stylistic factors. The greater or lesser presence of formalized materials (proverbs, songs,
formulas, and stereotypes) may measure the degree in which a collective viewpoint exists within an individual’s narrative. These shifts between standard language and dialect are often a sign of the kind of control which speakers have over the narrative.

A typical recurring structure is that in which standard language is used overall, while dialect crops up in digressions or single anecdotes, coinciding with a more personal involvement of the narrator or (as when the occurrences of dialect coincide with formalized language) the intrusion of collective memory. On the other hand, standard language may emerge in a dialect narrative when it deals with themes more closely connected with the public sphere, such as politics. Again, this may mean both a more or less conscious degree of estrangement, or a process of ‘conquest’ of a more ‘educated’ form of expression beginning with participation in politics. Conversely, the dialectization of technical terms may be a sign of the vitality of traditional speech and of the way in which speakers endeavor to broaden the expressive range of their culture.

EVENTS AND MEANING

The first thing that makes oral history different, therefore, is that it tells us less about events than about their meaning. This does not imply that oral history has no factual validity. Interviews often reveal unknown events or unknown aspects of known events; they always cast new light on unexplored areas of the daily life of the nonhegemonic classes. From this point of view, the only problem posed by oral sources is that of verification (to which I will return in the next section).

But the unique and precious element which oral sources force upon the historian and which no other sources possess in equal measure is the speaker’s subjectivity. If the approach to research is broad and articulated enough, a cross section of the subjectivity of a group or class may emerge. Oral sources tell us not just what people did, but what they wanted to do, what they believed they were doing, and what they now think they did. Oral sources may not add much to what we know, for instance, of the material cost of a strike to the workers involved; but they tell us a good deal about its psychological costs. Borrowing a literary category from the Russian formalists, we might say that oral sources, especially from nonhegemonic groups, are a very useful integration of other sources as far as the fabula—the logical, causal sequence of the story—goes; but they become unique and necessary because of their plot—the way in which the story materials are arranged by narrators in order to tell the story. The organization of the narrative reveals a great deal of the speakers’ relationships to their history.

Subjectivity is as much the business of history as are the more visible ‘facts’. What informants believe is indeed a historical fact (that is, the fact that they believe it), as much as what really happened. When workers in
Terni misplace a crucial event of their history (the killing of Luigi Trastulli) from one date and context to another, this does not cast doubts on the actual chronology, but it does force us to arrange our interpretation of an entire phase of the town’s history. When an old rank-and-file leader, also in Terni, dreams up a story about how he almost got the Communist Party to reverse its strategy after World War II, we do not revise our reconstructions of political debates within the Left, but learn the extent of the actual cost of certain decisions to those rank-and-file activists who had to bury into their subconscious their needs and desires for revolution. When we discover that similar stories are told in other parts of the country, we recognize the half-formed legendary complex in which the ‘senile ramblings’ of a disappointed old man reveal much about his party’s history that is untold in the lengthy and lucid memoirs of its official leaders.

**SHOULD WE BELIEVE ORAL SOURCES?**

Oral sources are credible but with a different credibility. The importance of oral testimony may lie not in its adherence to fact, but rather in its departure from it, as imagination, symbolism, and desire emerge. Therefore, there are no ‘false’ oral sources. Once we have checked their factual credibility with all the established criteria of philological criticism and factual verification which are required by all types of sources anyway, the diversity of oral history consists in the fact that ‘wrong’ statements are still psychologically ‘true’ and that this truth may be equally as important as factually reliable accounts.

Of course, this does not mean that we accept the dominant prejudice which sees factual credibility as a monopoly of written documents. Very often, written documents are only the uncontrolled transmission of unidentified oral sources (as in the case of the report on Trastulli’s death, which begins: ‘According to verbal information taken…’). The passage from these oral ‘ur-sources’ to the written document is often the result of processes which have no scientific credibility and are frequently heavy with class bias. In trial records (at least in Italy, where no legal value is accorded to the tape recorder or shorthand transcripts), what goes on record is not the words actually spoken by the witnesses, but a summary dictated by the judge to the clerk. The distortion inherent in such procedure is beyond assessment, especially when the speakers originally expressed themselves in dialect. Yet, many historians who turn up their noses at oral sources accept these legal transcripts with no questions asked. In a lesser measure (thanks to the frequent use of shorthand) this applies to parliamentary records, minutes of meetings and conventions, and interviews reported in newspapers: all sources which are legitimately and widely used in standard historical research.

A by-product of this prejudice is the insistence that oral sources are distant from events, and therefore undergo the distortion of faulty memory. Indeed,
What makes oral history different

This problem exists for many written documents, which are usually written some time after the event to which they refer, and often by nonparticipants. Oral sources might compensate chronological distance with a much closer personal involvement. While written memoirs of politicians or labor leaders are usually credited until proven to be in error, they are as distant from some aspects of the event which they relate as are many oral history interviews, and only hide their dependence on time by assuming the immutable form of a ‘text’. On the other hand, oral narrators have within their culture certain aids to memory. Many stories are told over and over, or discussed with members of the community; formalized narrative, even meter, may help preserve a textual version of an event.

In fact, one should not forget that oral informants may also be literate. Tiberio Ducci, a former leader of the farm workers’ league in Genzano, in the Roman hills, may be atypical: in addition to remembering his own experience, he had also researched the local archives. But many informants read books and newspapers, listen to the radio and TV, hear sermons and political speeches, and keep diaries, letters, clippings, and photograph albums. Orality and writing, for many centuries now, have not existed separately: if many written sources are based on orality, modern orality itself is saturated with writing.

But what is really important is that memory is not a passive depository of facts, but an active process of creation of meanings. Thus, the specific utility of oral sources for the historian lies, not so much in their ability to preserve the past, as in the very changes wrought by memory. These changes reveal the narrators’ effort to make sense of the past and to give a form to their lives, and set the interview and the narrative in their historical context.

Changes which may have subsequently taken place in the narrators’ personal subjective consciousness or in their socio-economic standing, may affect, if not the actual recounting of prior events, at least the valuation and the ‘coloring’ of the story. Several people are reticent, for instance, when it comes to describing illegal forms of struggle, such as sabotage. This does not mean that they do not remember them clearly, but that there has been a change in their political opinions, personal circumstances, or in their party’s line. Acts considered legitimate and even normal or necessary in the past may be therefore now viewed as unacceptable and literally cast out of the tradition. In these cases, the most precious information may lie in what the informants hide, and in the fact that they do hide it, rather than in what they tell.

Often, however, narrators are capable of reconstructing their past attitudes even when they no longer coincide with present ones. This is the case with the Terni factory workers who admit that violent reprisals against the executives responsible for mass layoffs in 1953 may have been counterproductive, but yet reconstruct with great lucidity why they seemed useful and sensible at the time. In one of the most important oral testimonies of our time, Autobiography of Malcolm X, the narrator describes very vividly
Critical developments

how his mind worked before he reached his present awareness, and then judges his own past self by the standards of his present political and religious consciousness. If the interview is conducted skillfully and its purposes are clear to the narrators, it is not impossible for them to make a distinction between present and past self, and to objectify the past self as other than the present one. In these cases—Malcolm X again is typical—irony is the major narrative mode: two different ethical (or political, or religious) and narrative standards interfere and overlap, and their tension shapes the telling of the story.

On the other hand, we may also come across narrators whose consciousness seems to have been arrested at climactic moments of their personal experience: certain Resistance fighters, or war veterans; and perhaps certain student militants of the 1960s. Often, these individuals are wholly absorbed by the totality of the historical event of which they were part, and their account assumes the cadences and wording of epic. The distinction between an ironic or an epic style implies a distinction between historical perspectives, which ought to be taken into consideration in our interpretation of the testimony.

OBJECTIVITY

Oral sources are not objective. This of course applies to every source, though the holiness of writing often leads us to forget it. But the inherent nonobjectivity of oral sources lies in specific intrinsic characteristics, the most important being that they are artificial, variable, and partial.

Alex Haley’s introduction to Autobiography of Malcolm X describes how Malcolm shifted his narrative approach not spontaneously, but because the interviewer’s questioning led him away from the exclusively public and official image of himself and of the Nation of Islam which he was trying to project. This illustrates the fact that the documents of oral history are always the result of a relationship, of a shared project in which both the interviewer and the interviewee are involved together, if not necessarily in harmony. Written documents are fixed; they exist whether we are aware of them or not, and do not change once we have found them. Oral testimony is only a potential resource until the researcher calls it into existence. The condition for the existence of the written source is emission; for oral sources, transmission: a difference similar to that described by Roman Jakobson and Piotr Bogatyrev between the creative processes of folklore and those of literature.

The content of the written source is independent of the researcher’s need and hypotheses; it is a stable text, which we can only interpret. The content of oral sources, on the other hand, depends largely on what the interviewer puts into it in terms of questions, dialogue, and personal relationship.

It is the researcher who decides that there will be an interview in the first place. Researchers often introduce specific distortions: informants tell
What makes oral history different

What makes oral history different from written history is that in oral history, the researcher has to accept the informant rather than trying to impose their own frame of reference. The informant has their own agenda, and the researcher has to respect their desire to tell what they believe they want to be told, rather than what the researcher wants to hear. Rigidly structured interviews may exclude elements whose existence or relevance were previously unknown to the interviewer and not contemplated in the question schedule. Such interviews tend to confirm the historian’s previous frame of reference.

The first requirement, therefore, is that the researcher ‘accept’ the informant, and give priority to what she or he wishes to tell, rather than what the researcher wants to hear, saving any unanswered questions for later or for another interview. Communications always work both ways. The interviewees are always, though perhaps unobtrusively, studying the interviewers who ‘study’ them. Historians might as well recognize this fact and make the best of its advantages, rather than try to eliminate it for the sake of an impossible (and perhaps undesirable) neutrality.

The final result of the interview is the product of both the narrator and the researcher. When interviews, as is often the case, are arranged for publication omitting entirely the interviewer’s voice, a subtle distortion takes place: the text gives the answers without the questions, giving the impression that a given narrator will always say the same things, no matter what the circumstances—in other words, the impression that a speaking person is as stable and repetitive as a written document. When the researcher’s voice is cut out, the narrator’s voice is distorted.

Oral testimony, in fact, is never the same twice. This is a characteristic of all oral communication, but is especially true of relatively unstructured forms, such as autobiographical or historical statements given in an interview. Even the same interviewer gets different versions from the same narrator at different times. As the two subjects come to know each other better, the narrator’s ‘vigilance’ may be attenuated. Class subordination—trying to identify with what the narrator thinks is the interviewer’s interest—may be replaced by more independence or by a better understanding of the purposes of the interview. Or a previous interview may have simply awakened memories which are then told in later meetings.

The fact that interviews with the same person may be continued indefinitely leads us to the question of the inherent incompleteness of oral sources. It is impossible to exhaust the entire memory of a single informant; the data extracted with each interview are always the result of a selection produced by the mutual relationship. Historical research with oral sources therefore always has the unfinished nature of a work in progress. In order to go through all the possible oral sources for the Terni strikes of 1949 to 1953, one ought to interview in depth several thousand people: any sample would only be as reliable as the sampling methods used, and could never guarantee against leaving out ‘quality’ narrators whose testimony alone might be worth ten statistically selected ones.

The unfinishedness of oral sources affects all other sources. Given that no research (concerning a historical time for which living memories are
available) is complete unless it has exhausted oral as well as written sources, and that oral sources are inexhaustible, the ideal goal of going through ‘all’ possible sources becomes impossible. Historical work using oral sources is unfinished because of the nature of the sources; historical work excluding oral sources (where available) is incomplete by definition.

WHO SPEAKS IN ORAL HISTORY?

Oral history is not where the working classes speak for themselves. The contrary statement, of course, would not be entirely unfounded: the recounting of a strike through the words and memories of workers rather than those of the police and the (often unfriendly) press obviously helps (though not automatically) to balance a distortion implicit in those sources. Oral sources are a necessary (not a sufficient) condition for a history of the nonhegemonic classes; they are less necessary (though by no means useless) for the history of the ruling classes, who have had control over writing and leave behind a much more abundant written record.

Nevertheless, the control of historical discourse remains firmly in the hands of the historian. It is the historian who selects the people who will be interviewed; who contributes to the shaping of the testimony by asking the questions and reacting to the answers; and who gives the testimony its final published shape and context (if only in terms of montage and transcription). Even accepting that the working class speaks through oral history, it is clear that the class does not speak in the abstract, but speaks to the historian, with the historian and, inasmuch as the material is published, through the historian.

Indeed, things may also be the other way around. The historian may validate his or her discourse by ‘ventriloquizing’ it through the narrator’s testimony. So far from disappearing in the objectivity of the sources, the historian remains important at least as a partner in dialogue, often as a ‘stage director’ of the interview, or as an ‘organizer’ of the testimony. Instead of discovering sources, oral historians partly create them. Far from becoming mere mouthpieces for the working class, oral historians may be using other people’s words, but are still responsible for the overall discourse.

Much more than written documents, which frequently carry the impersonal aura of the institutions by which they are issued—even though, of course, they are composed by individuals, of whom we often know little or nothing—oral sources involve the entire account in their own subjectivity. Alongside the first person narrative of the interviewee stands the first person of the historian, without whom there would be no interview. Both the informant’s and the historian’s discourse are in narrative form, which is much less frequently the case with archival documents. Informants are historians, after a fashion; and the historian is, in certain ways, a part of the source.

Traditional writers of history present themselves usually in the role of
What makes oral history different

What literary theory would describe as an ‘omniscient narrator’. They give a third-person account of events of which they were not a part, and which they dominate entirely and from above (above the consciousness of the participants themselves). They appear to be impartial and detached, never entering the narrative except to give comments aside, after the manner of some nineteenth-century novelists. Oral history changes the writing of history much as the modern novel transformed the writing of literary fiction: the most important change is that the narrator is now pulled into the narrative and becomes a party of the story.

This is not just a grammatical shift from the third to the first person, but a whole new narrative attitude. The narrator is now one of the characters, and the telling of the story is part of the story being told. This implicitly indicates a much deeper political and personal involvement than that of the external narrator. Writing radical oral history, then, is not a matter of ideology, of subjective sides-taking, or of choosing one set of sources instead of another. It is, rather, inherent in the historian’s presence in the story, in the assumption of responsibility which inscribes her or him in the account and reveals historiography as an autonomous act of narration. Political choices become less visible and vocal, but more basic.

The myth that the historian as a subject might disappear in the objective truth of working-class sources was part of a view of political militancy as the annihilation of all subjective roles into that of the full-time activist, and as absorption into an abstract working class. This resulted in an ironical similarity to the traditional attitude which saw historians as not subjectively involved in the history which they were writing. Oral historians appear to yield to other subjects of discourse, but, in fact, the historian becomes less and less of a ‘go-between’ from the working class to the reader, and more and more of a protagonist.

In the writing of history, as in literature, the act of focusing on the function of the narrator causes this function to be fragmented. In a novel such as Joseph Conrad’s *Lord Jim*, the character/narrator Marlow can recount only what he himself has seen and heard; in order to tell the ‘whole story’, he is forced to take several other ‘informants’ into his tale. The same thing happens to historians working with oral sources. On explicitly entering the story, historians must allow the sources to enter the tale with their autonomous discourse.

Oral history has no unified subject; it is told from a multitude of points of view, and the impartiality traditionally claimed by historians is replaced by the partiality of the narrator. ‘Partiality’ here stands for both ‘unfinishedness’ and for ‘taking sides’: oral history can never be told without taking sides, since the ‘sides’ exist inside the telling. And, no matter what their personal histories and beliefs may be, historians and ‘sources’ are hardly ever on the same ‘side’. The confrontation of their different partialities—confrontation as ‘conflict’, and confrontation as ‘search for unity’—is one of the things which make oral history interesting.
NOTES

1 B. Placido in *La Repubblica*, 3 October 1978.
2 One Italian exception is the Istituto Ernesto De Martino, an independent radical research organization based in Milan, which has published ‘sound archives’ on long-playing records since the mid-1960s—without anyone in the cultural establishment noticing: see F. Coggiola, ‘L’attività dell’Istituto Ernesto de Martino’, in D. Carpitella (ed.), *L’etnomusicologia in Italia*, Palermo, Flaccovio, 1975, pp. 265–270.
6 In this article, I use these terms as defined and used by G. Gennete, *Figures III*, Paris, Seuil, 1972.
8 For instance, G. Bordoni, Communist activist from Rome, talked about family and community mainly in dialect, but shifted briefly to a more standardized form of Italian whenever he wanted to reaffirm his allegiance to the party. The shift showed that, although he accepted the party’s decisions, they remained other than his direct experience. His recurring idiom was ‘There’s nothing you can do about it.’ See Circolo Gianni Bosio, *I giorni cantati*, pp. 58–66.
“We Drew What We Imagined”: Participatory Mapping, Performance, and the Arts of Landscape Making

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Participatory mapping has emerged as a dominant paradigm in participatory approaches to international planning, conservation management, and community development in the Global South and is considered a technology with emancipatory potentials for subordinate or marginalized groups. However, the literature on community-based mapping has been criticized for its dualistic approach to power, culture, and the local and for reifying material and discursive forms of domination operating through Western projects such as development and global environmentalism. An ethnographic engagement with mapping projects conducted in Trinidad in the fall of 1998 and in Venezuela from 2001–2004 provides a deeper understanding of participatory-mapping workshops as theaters for the performance and negotiation of identities, reflecting the complex articulations between global, political-economic processes and desires for place and belonging. Ultimately, this critical reading indicates an urgent need for greater reflexivity in the application of participatory-mapping approaches.

What we drew on our map, was all we knew, to the limit of our knowledge. We have drawn places we have heard about but that I have never been to. I have never seen Rio Arapopo. What we have put down is imaginary. But much is missing still.

—Rosa Emilia Fernández

Rosa Emilia Fernández had a complex, unusual, and intimidating audience when she spoke these words in 2002. She and two other women had spent the afternoon sketching a map of what they understood (and desired) to be the territory of Kumarakapay, an indigenous Pemon village in the Gran Sabana, in far southeastern Venezuela. Rosa Emilia, Leticia Fernández (no close relation), and Cristina Rossi (Leticia’s daughter) had never before drawn such a map without the direction of elder men. They were now participating in a workshop to map land uses and places with historic significance, in other words, doing the work typically considered the right and responsibility of elder men. Leticia, Rosa Emilia, and Cristina were alternatively bemused, insecure, and pleased to take their place among the men to draw what Leticia at one point called the women’s “own map.”

They were drawing their own map in part because I had made an intrusive request a few days earlier. I was in Kumarakapay to assist with a project to map the lands of the indigenous Pemon in sector 5, one of eight self-designated sectors of the Pemon homeland. A few days before the workshop in Kumarakapay, I had suggested to the local coordinator for the mapping project, Claudio Gómez, that the entire community be invited, that the participants be divided into groups by age and gender, and that each group draw a map and present it to all participants at the end of the day. I had made the point to Gómez and his brother Juvencio, the village chief (capitan) at the time, that if the mapping process incorporated the vision and knowledge of both women and men, the final territorial map would be more complete and rhetorically powerful, and it would thus better serve the goal of achieving land rights for the Pemon. As would happen again on many occasions, I had made a conscious, albeit uneasy, intervention in the social life of the village. As a social scientist, I was concerned about reproducing the essentializing dichotomy between indigenous tradition and modernity; as an activist scholar, I was aware that indigenous movements often obtain more influence when they employ primitivist imagery (Haley and Wilcoxon 1997, 764; Milton 1996; Ulloa 2005).

When Rosa Emilia presented the women’s map to her audience—the dozen or so teenagers, a handful of other women, a half-dozen adult and elder men, and the white, male cartographer from abroad—her words were measured, almost apologetic: the women had drawn places they had heard about but never seen. Because vision and walking are privileged ways of gaining knowledge among their people, the women felt that their map was “incomplete.” As she explained to me later, two of the village elders present, César Duran and Antonio Pérez, were abuelos (grandfathers) known for their ex-
exceptional knowledge of the farthest reaches of village territory. Rosa Emilia believed that the women's map failed to live up to the standards of detail and accuracy she imagined were reflected in the elders' map. At the same time, she relished the opportunity to present the women's perspective on the landscape in which she, Leticia, and Cristina had lived their entire lives.

As I became friends with Rosa Emilia, Leticia, Cristina, and their extended families over the course of the next four years—playing my sometimes contradictory roles as researcher, cartographer, colleague, friend, teacher, rival, and intruder—I kept returning to her words. I began to appreciate the complexities of the "culture-making" taking place in the mapping project: how the performances of project participants were structured by the presence of complex audiences and complicated by a lack of clearly established rules of production and reception (Myers 1994). I began to realize that "the map," as it simply came to be known among Pemon in the Gran Sabana, was contingent not only on knowledge of the materiality of the landscape but also on the temporalities and imaginaries that infuse it and give it meaning. The map was the result of contested performances of people who in different ways embody this landscape and (re)produce its meanings; in so doing, they negotiate and reproduce what it means to be Pemon and to rightfully belong. I came to understand that places existing only in someone's imagination also are constitutive of landscapes and contribute to forming personal biographies (Tilley 1996). The map was not simply a technical product of local and scientific knowledge but also a work of art, embodying, reflecting, and acting on the social as well as the material. Ultimately, I realized that the process of making the map had been as important as the end product for what it had revealed about the entanglements of identities, social relations, landscape, and power in places on the margins.

Participatory Mapping, Performativity, and Talking Landscapes

In this article, I wish to consider the roles of memory, performance, and embodiment in participatory mapping, in part to draw attention to the negotiations of power, identities, and authenticities implicated in community-based productions of spatial representations. I draw primarily on my experiences with the mapping project in Venezuela—and also with a briefer project in Trinidad—to suggest that an ethnographic consideration allows us to consider critically the notion of participatory mapping as a counterhegemonic activity. First, I reflect on the ways in which the processes of imagining, talking about, and drawing landscapes shape and refract relations of power in multiple, complex, and sometimes contradictory ways, and second, I consider how these processes ultimately reveal the ways in which neocolonial structures shape places on the margins.

Drawing in part on insights from the anthropology of art, performativity, and landscape (e.g., Geismar and Tilley 2003; Gell 1998; Ingold 1993; Myers 1994; Okafor 1994; Rose 2000; Stoller 1994a, 1994b, 1996, 2004; Taylor 2003; Tilley 1996; Trigger and Mulcock 2005), I suggest that participatory map production is intimately implicated in performances of the past, the present, and often divergent, imagined futures. Focusing explicitly on process instead of form thus allows me to foreground the embodied performances taking place in mapping projects (see Ingold 1993 and Rundstrom 1990 for early cultural interpretations of indigenous maps). I am speaking here of performances similar to those of Stoller's West Sahelian griots, who negotiate social life by "talking it" (Stoller 1994b, 357). In the case of drawing maps—and even talking about and preparing to draw maps—these performances articulate desires and imaginations of what the map should look like, should do, and will do, for maps are always made for a purpose: they are the abstracted material representation of landscapes as they (are seen to) have been, are, and could/should be.

Unlike griots, however, performers in community mapping workshops challenge each other for the right to "talk" the past, the present, and the future, their authority derived in part from the ways in which they embody space and time. Performers become the rightful "owners" of the landscape (Rose 2000, 289) based in part on their embodiment of the landscape and on their relationship with the histories that are embodied within it and thus define it. From the outset, then, I assume that mapping is inextricably linked with questions of rights and authority, not simply in binary terms of local or indigenous versus nonlocal claims to land rights, resources, and so on but also in terms of the negotiations of what constitute authentic readings of histories and futures. Through these storytellings, authenticities and meanings of landscapes are (re)defined, relations of power are negotiated, and ultimately, structures and processes of neocolonial control are made visible. The stories informing and deriving from mapmaking thus provide frameworks for social and political assessment and agency (Taylor 2003); that is, if we view the performances associated with participatory mapping as art, then participatory mapping is implicitly and unavoidably action "intended to change the world rather than encode symbolic propositions about it" (Gell 1998, 6).

I should clarify what I mean by "performance" and "storytelling" in the context of mapmaking. I am not speaking here of the ritualized telling of stories, such as elder Pemons' carefully staged recitals of ancestral pantons, but rather conscious, competitive talking about landscape, time, and people akin to Trinidadian "liming." As Eriksen (1990) explains, liming is best understood as a semiformal activity constituted by relationships between the performers, who are consciously reflecting on the social and political-economic situation of the storytellers and their community. However, although it could be argued that members of all societies "lime" (Eriksen 1990), I do not propose to use the term as a metaphor for the linguistic performances in mapping workshops. Instead,
the concept of liming allows me to arrive at a positive categorization of what I mean by performance and storytelling in this context: by "stories," I mean speech acts informed by an awareness of social and political-economic relationships, that is, linguistic performances that are simultaneously rhetorical and embodied. By coupling this interpretation of storytelling with a modified version of Stoller’s (1994a) concept of "performance," I attempt to draw attention to the links between the speech act and the embodiment of the storyteller, arguing (with Ingold 1993) that landscapes are imbued with the histories and lived experiences of people. Thus, claims to and perceptions of such embodiments of landscapes inform what stories "count" and hence how landscapes are produced.

Although I propose to expand Stoller’s (1994a) definition of the term “performance” beyond commemorative ceremonies to speech acts akin to liming, I draw on his notion that “social memories are constructed and conveyed” (p. 638) through embodied performances of speech acts. These performances “constitute rather than reflect action. They are not limited to verbal utterances, they are also ‘encoded in set postures, gestures and movements’” (Stoller 1994a, 638–639, quoting Connerton 1989, 59). This is to say that words spoken about places, landscapes, and people are weighed and to varying degrees repeated by the various audiences (not only the ethnographer or cartographer, but also local listeners) based not simply on their rhetorical quality but also on the embodiment of the speaker and his or her relationships with the entanglements of space and histories.

From this perspective on performance as social action, participatory mapping can be understood as a performative process of place making (I borrow the notion of performance as social action from Myers 1994, 680). Through the performative, iterative productions of identities and spatialities that take place during the mapmaking process, the workings of power are made visible. Storytelling as liming sheds light on—and shows awareness (or lack of awareness) of—the “turbulent relations among perception, state power and lived experience” (Stoller 2004, 821; see also Taylor 2003). As a semistructured theater of embodied performances, map production is thus always informed by and implicated in relations of power; that is, the simultaneous negotiation of identities and production of spatialities occurring when participants think and talk about place and landscape unveil relations of power and the shifting and fluctuating relationships between place and people.

This brings me to another conceptual leap, which I hope will be justified by the case studies presented below. I suggest that by making explicit and visible such productions of spatialities and their embeddedness in structures of inequality, ethnographers and cartographers engaged with participatory mapping can move beyond cultural relativism and facilitate an emancipatory politics—in terms of both “abuses by other societies . . . and the protection of individuals within a society” (Turner 1997, 276–277; see also Bourgois 1997). Perhaps, by revealing the tensions, negotiations, and contestations that characterize participatory-mapping projects, we can heed the “first rule of practice” of griots: “to create a dynamic tension between the poetic and the political, the past and the present,” and thus foreground the power relations in the world (Stoller 1994b, 358).

Participatory Mapping, Knowledge, and What It Means to Be Local

Before I turn to my case studies of participatory mapping in Venezuela and Trinidad, it is necessary to explore what maps are and what maps do as well as what violence is inflicted when cartographers draw maps of (and even with) indigenous and local communities. Maps are representational objects intimately implicated in projects of place making, and therefore they are tools of power. They have unwritten indigenous occupations of places, shaped public opinion in times of crisis and war, and created expectations for the proper ordering of the social and the natural. Maps put things and people in their place. Not only do they order the material world and make us visualize the here, but through their rhetorical power they also simultaneously obscure the why. Most maps—especially “scientific” maps produced by regional, state, and global institutions and their agents—are mute about the social context and consequences of their own existence.

The late historical geographer J. B. Harley (1988, 1989, 1990, 1992) postulated that maps are inherently ideological representations that reflect the social contexts and interests of their creators, that cartography is a social practice used to reproduce dominant world views, and that claims to mimesis provide the map with its rhetorical power. Subsequent authors have explored the role of cartography in early state building, the implications of cartography in the colonial project, the ways in which cartography has been used in the orientalization of indigenous and native peoples, and the authoritative role of geographic information systems (GIS) and remote sensing in excluding alternative spatialities in development, conservation, and urban planning.

Exactly because of the power of maps to underwrite hegemonic, symbolic, and material practices, indigenous peoples, residents of marginalized urban neighborhoods, and other subordinate groups now draw on the rhetorical power of maps to present alternative world views and futures. Such "ethnocartography," "indigenous mapping," "cultural mapping," or "community-based mapping" is said to represent local or indigenous interests and conceptions of landscapes and so lead to more democratic decision-making processes. However, beyond the potential role of participatory mapping in development and conservation planning, such "countermapping" (Peluso 1995) also represents an alternative and more just way of thinking about and producing landscapes (see Chapin, 1.

The conception of participatory mapping as a means to justice and self-determination has been the subject of rigorous and thoughtful critiques by both mapping practitioners and activists and theorists in anthropology and related fields. These analyses often focus on the potential Western uses and abuses of native knowledge involved in the reinscribing of gendered, temporal, and dynamic conceptions of space into synchronic Cartesian cartographies (see, e.g., Rocheleau 2005) and the incorporation of indigenous information into the knowledge management systems of development institutions (Samoff and Stromquist 2001). Also of concern are the limits imposed on participation in mapping projects, the social and cultural implications of technology transfers, the role of such projects in local conflicts, and the potential reproduction of uneven gender relations or other inequalities of power and access to resources in local communities (see, e.g., Sundberg 2004 on the role of mapping in the gendering and labeling of indigenous peoples in Guatemala). “Mapping” has thus become central to the discussion surrounding culture, conservation, and territory, both as a research and planning tool and as a metaphor for exploring the complex links between place and people at multiple levels of analysis (see especially Brosius, Tsing, and Zerner 2005).

The literature on participatory mapping has been criticized for reproducing the binaries that define the raison d’être of development: between local and global, developed and underdeveloped, First and Third World, modern and nonmodern, indigenous and nonindigenous, and most important, rational science and un-“disciplined” local knowledge (see, e.g., Brosius 1997, 1999, 2001; Kirsch 2007; Milton 1996; Nygren 1999; Uloa 2005). Instead, according to such critiques, local and scientific knowledge must be seen as contested, “heterogeneous” or “hybrid” knowledges incorporating local and global dimensions (Agrawal 1995; Appadurai 1995; Gupta 1998; see also Moore 1996; Myer 1998; Nader 1996; Nygren 1999, 282; Sillitoe 1998). Similarly, cultures are no longer comfortably viewed as discrete phenomena occupying neatly demarcated “fields” but instead are seen as fluid and contingent results of the increasingly rapid contraction of space and time in a globalizing world (Stoller 1996; see also Appadurai 1992; Escobar 2001; Gupta and Ferguson 1997).

These understandings of the mutual production of the local and the global are useful for problematizing claims of authority in mapping projects and speak directly to the debate regarding anthropologists’ role in reproducing what some authors refer to as “invented traditions,” “imagined communities,” and the like (Anderson 1991; Barth 1969, 1995; Hobbsan 1983). Conversely, anthropologists concerned with deconstructing essentializing claims have been chided for taking an “uncharitable” stance toward research subjects (Erikson 1993, 71) and for ignoring the genuine enchantment with nature commonly found among indigenous people (Brosius 1999). This debate has been amply reviewed elsewhere (see, e.g., Haley and Wilcoxon 1997; Kirsch 2007); here, I emphasize the importance of teasing out the material relations of power that also shape these productions of identities and spatialities. More specifically, desires, fantasies, and fears shape and are shaped by both the material and the symbolic production of landscapes. In turn, these subjectivities lead to the production of specific spatialities, for example, spatial imaginaries such as Rosa Emilia’s map of places she had never seen but which she nevertheless intensely desired to see.

I turn now to an analysis of two participatory-mapping projects, one a short-term project I initiated and conducted in an Indo-Trinidadian squatter settlement on the social and geographical margins of the Caribbean island of Trinidad and the other a longer-term, more extensive project in indigenous Pemon territory in Venezuela. Despite their different histories and social geographies and despite my shorter engagement with the Trinidadian project, I include both in this article, to illustrate the ways in which participatory mapping in local places brings contests surrounding rights, identities, and authenticities to the fore and simultaneously facilitates the unveiling of the multiple, complex relations of power that shape the production of local-global landscapes in the Global South, whether these landscapes are popularly considered “indigenous” or not.

Kernahan, Trinidad: Performance, Contestation, and Not-So-Local Knowledge

Located on the edge of the 24,000-hectare Nariva Swamp on the east coast of Trinidad, Kernahan is an informal settlement populated by about 300 descendants of indentured workers from India. The majority of the settler families arrived in the 1970s from Indo-Trinidadian communities on the west coast of the island, especially the communities fringing the Caroni Swamp. Like the Caroni, the Nariva is a seasonally dry palustine wetland characterized by a patchwork of swamps, mangroves, marshes, and uplands supporting a complex mosaic of scattered plant communities (Bacon 1990, 223). Most of the swamp is passable on foot only in the dry season, or “crop time,” from January through May. In the rainy season, only the sandy upland areas (remnants of sea ridges created through successive sea level decreases) are accessible to local hunters and fishermen.

These extreme conditions of alternating drought and flood shape the principal, intimately connected land-use systems in...
Kernahan: rice farming and freshwater fishing. Small-scale rice planting is often viewed as a tradition that defines Indo-Trinidadian village identity (Vertovec 1992, 213), but it is also an adaptive strategy linked in intriguing ways to the freshwater cascadu fishery. The cascadu (Hoplosternum littorale) is found in seasonally inundated wetlands in Trinidad and lowland South America and is characterized by a highly vascularized gut that enables it to breathe air (Carter and Beadle 1932; cited in Ramnarine 1994, 291). Its life cycle begins at the onset of the wet season in June or July, when the first flood induces the fish to scatter throughout the swamp, build nests, and breed. During the dry season, the fish retreat to the deepest parts of the rivers, which are permanently inundated.

To more easily catch cascadu, fishermen "clean" rivers to keep them open and thus provide habitat for the fish. Similarly, rice paddies provide a suitable environment for cascadu and make them easy to catch in close proximity to the village. My primary research interest was to study the links between fluctuating subsidies for rice and attendant changes in the spatial extent of the freshwater fishery. Specifically, with increases in subsidies, the acreage of rice paddies expanded, incomes from rice production increased, and more cascadu became available to catch near the village. As a result, less fishing was conducted in far-flung areas of the villagers’ fishing zone (Sletto 1998, 2002, 2005).

To elucidate the specific changes in the spatial extent of fishing zones during the past two decades, I used a mixed, participatory research methodology including surveys, interviews, and participatory mapping. My principal informants were a group of about a dozen fishermen, about half of whom were older than 50 and retired from fishing and the other half young and still actively fishing. I recruited these fishermen through word of mouth, and with the help of the elder fisherman Michael Cecil, I organized a series of participatory-mapping workshops to draw natural features, toponyms, and the location of fishing grounds over the course of the previous 15 years. The mapping project, then, was not intended to serve any political purpose, such as land titling, and would not bring financial or material rewards to participants. The fishermen’s work would merely serve to illuminate local land use and document local toponyms for the benefit of an eager graduate student.

Why, then, apart from a sense of goodwill, and perhaps to take advantage of the opportunity to lime about a landscape produced and embodied by their experiences and their memories, did these fishermen participate in these time-consuming mapping workshops? I came to realize that their participation—their multiple performances of place and people—made sense in the social and political context of the time, which was characterized by both a contentious national debate about the meaning and future of the Nariva and a localized conflict about alternative methods of catching cascadu. It appeared that, in the words of Taylor (2003, 245), through “the arts of memory and narrative performance,” the fishermen were (re)producing village identity to “make sense of their own presents.”

The fishing conflict revolved around the use of cascadu traps, and the main protagonists were a group of about a half-dozen young, unmarried men who had settled in the village in the early 1990s and the dozen fishermen born and raised in Kernahan who participated in the mapping project. The fishermen native to Kernahan did not use cascadu traps, claiming that they led to overfishing and financial losses for those who use cast nets or hook and line. The trappers, meanwhile, maintained that they were not harming the fish stock or threatening the livelihoods of other fishermen. I soon found myself positioned on the side of the original fishermen in this contest over resource access, in part by my personal association with the oldest settler families, that is, those opposed to trapping. These families were Hindu traditionalists, and in a community sharply divided over religion—about half the village had joined two Evangelical churches—I was identified as a supporter of Hindu perspectives on ecology and resource use. Members of Hindu families often suggested to me that their spirituality prohibited them from doing the landscape any harm; in other words, their “spiritual belonging” supported their claim for “deep belonging” to the Nariva Swamp (Trigger and Mulcock 2005, 307). To further exacerbate the division between the two groups, the more recently arrived fishermen belonged to a kin group different from that of the original settlers. Men who had been identified to me as “trappers” therefore avoided the mapping workshops and spoke with me infrequently and with caution.

From the half-dozen interviews I held with trappers, I learned that cascadu traps are set when water levels in the swamp are rapidly changing and the fish are on the move. This occurs at the onset of the dry season in December, when the fish leave the marshes to find refuge in deeper parts of perennial rivers, and during the first torrential rains in June, when the fish disperse into the marshes and swamps. One well-made trap can catch hundreds or even thousands of fish. Nevertheless, trappers argued, the fish stock was not threatened by their use of the cascadu traps. Also, because of the depressed domestic rice market at the time, it was necessary to sell more cascadu to make ends meet, hence the traps. Such arguments held little sway with older fishermen. They needed to adjust to lower rice prices, too, but they still cared about the environment and did not want to overfish the swamp.

These positionings are more illuminating when interpreted within the political-economic context of the late 1990s. At the time, a national park was being planned for this area, and fishermen often discussed with me the possibility of obtaining jobs as game wardens or tour guides. As Michael Cecil, the elder cascadu fisherman who led the mapping workshops once told me,
If these traps continue this way, my children will wonder what a cascadu is. I could make a trap, I could set a trap, but I don't want to. Because I want the cascadu to be here tomorrow and the next day and the next year... If I was a game warden, I would check every day, and make sure there were no traps.

Thus, by voicing their opposition to the practice of trapping, villagers not only contended for access to resources but also presented an image as the swamp's protectors. The mapping workshop became an opportunity for some fishermen to represent their local knowledge as most appropriate for the proper management of fish resources. In fact, as some of the fishermen told me on various occasions, they participated in the workshops from a sense of responsibility to report "accurately" what they knew about fishing in the Nariva Swamp.

At this stage, then, it became necessary to draw a distinction between intention and use of these maps—not just from the perspective of the participants, but also from my perspective as a geographer (Morphy 1994). My presence and initiative had provoked not simply a collaborative process to document local knowledge but the telling of stories to effect relations of power (Taylor 2003, 256). The mapping project had become a theater for the public performance of prototypical traditional fishermen. Those who associated themselves with this identity formation—that is, the "original" settlers who adhered to what they saw as environmentally sound practices—claimed to be the "natural" stewards of the Nariva Swamp and therefore claimed the right to determine the rules of the cascadu fishery. The mapping project thus provided a stage for the "authentication of their experience" in front of a sympathetic audience of friends and an activist researcher (Myers 1994, 694). This is partly why the toponyms included on the final maps reflect the histories of the original settler families and even the personal histories of some of the older fishermen (figs. 1, 2). Gold Teeth Corner, for example, is named in memory of one of the fishermen's former girlfriends; Breakass Hill marks a particularly steep and slippery hill. Two rivers are named in honor of retired fishermen—Pedro Stream and Malik River—because these fishermen were the first (in the memory of Kernahan's elders) to fish and "clean" these rivers.

However, these performances of authenticity were not informed only by the contest for local fish resources. In the late 1990s, the Nariva Swamp was threatened by an increase in commercial rice production, prompted in part by greater state support for the rice industry. Trinidadian environmentalists—mostly well-connected, wealthy residents of the capital, Port of Spain—mounted a campaign against the commercial rice producers, representing their large-scale clearing of swamp forests as the antithesis of the time-honored, sustainable practices of the "local" fishermen. They constructed Kerhanan as a "typical" Indo-Trinidadian village where people had developed intimate ties with their characteristic environment. The fact that the village owed its existence to economic and political marginalization and that residents were still economically beholden to descendants of European plantation owners was left out of these narratives (Bretton 1974, 1981, 1993; Clarke 1993; Henry 1993; Singh 1974; Vertovec 1990, 1992; Weller 1968). Instead, this construction of Kernahan as a bounded, "indigenous" locality provided the impetus for conservation interventions and greater state control over village life. Land tenancy is being regularized, leading to concerns that properties will fall under the control of wealthier villagers, and the state wildlife section is increasing its monitoring of the swamp, potentially limiting villagers' access to resources (Sletto 2002, 2005).

Thus, the performances in the participatory-mapping project not only were informed by a local contest surrounding knowledge and authenticities but also reflected regional and state political economies and contested constructions of nature, place, and people. Even though the "Battle of Nariva" was largely fought in the remote spaces of national media and academia, it was becoming apparent to villagers that the Nariva Swamp was being "discovered" and reconfigured into a conservation space. Claiming to be a traditional, authentic fisherman would be strategically useful in encounters with external agents, who more often than not would be concerned with environmental conservation rather than economic development (see Baptiste 2008 for more on villagers' environmental concerns). The participatory-mapping project, then, became a space for articulating the past in the context of the present, for seizing "hold of a memory as it flashes up in a moment of danger" (Benjamin 1968, 255). Talking about the landscape meant talking about history and rights and belonging but also about the threat of commercial rice growers, "intruders" who threatened the cascadu stock, and the increasing presence of state agents. Thus, the mapping project became a means to imagine futures desired and futures feared.

Making Maps: Positionality, Method, and Conflict

While the Indo-Trinidadians in Kernahan live in a marginalized rural community with little resort to claims of indigeneity, the Pemon, one of the Carib group of indigenous peoples, have inhabited the Gran Sabana since before the arrival of Europeans in the New World (Huber 1995, 54–55) and have been vocal in expressing their indigenous identity. They number today about 20,000 and live throughout the southeastern corner of Venezuela, in southern Guyana, and in western Brazil (see Cousins 1991; Mansutti 1981; Thomas 1982; and Urbina 1979; see also de Armellada 1943 and Koch-Grunberg 1981). The Pemon are undergoing complex, uneven processes of social change, depending to a large degree on their location within the savanna landscape. In communities located along the Pan-American Highway between Puerto Ordaz and Boa Vista, the capital of the Brazilian state of Roraima, residents are increasingly dependent on a cash economy based almost exclusively on tourism, while in more re-
Figure 1. Mental map with place names, drawn by fishermen of Kernahan, Trinidad, fall 1998.
Figure 2. Mental map of streams and waterways, drawn by fishermen of Kernahan, Trinidad, fall 1998.
The political potential of the new constitution, and I saw the procedural conflicts, and the actual demarcation of indigenous lands has been implemented in its wake. Pemon leaders are also increasingly frustrated by the tight\_gob.ve/minpi/content/view/61/1/)

significance for the sustenance of traditional culture) and thus constituted "habitat" (places of cultural, historic, and economic management within the boundaries of Canaima National Park, which covers most of the Pemon ancestral lands in the Gran Sabana. In part, I sought to assess dominant narratives about indigenous burning practices, specifically, claims by EDELCA fire managers that indigenous fire use was causing loss of gallery forests. As I eventually came to realize during my four years of living in the two communities of Kumarakapay and Monte Bello, these claims were too simplistic. Instead, forest loss through indigenous burning is contingent on geographical realities and social change. Fire suppression appears to be leading to increased fuel loads and more extensive fires. Indigenous burning appears to reduce fuel loads in forest-savanna boundaries, preventing fire encroachment in forest patches (Sletto 2006, 2008; see also Rodriguez 2004, 2007).

Shortly before my arrival in the Gran Sabana for the first time in summer 2001, a new Venezuelan constitution appeared to create an increasing role for civil society in national governance and to provide indigenous people with limited territorial rights to so-called habitats. A subsequent demarcation law provided a more explicit definition of what constituted "habitat" (places of cultural, historic, and economic significance for the sustenance of traditional culture) and thus provided a legal framework for the participatory-mapping project. Although progress has been slow in the actual granting of indigenous habitats under the demarcation law, the constitutional guarantee was a milestone for indigenous rights in Venezuela, and a number of new mapping projects have been implemented in its wake.

Before arriving in the Gran Sabana, then, I was aware of the political potential of the new constitution, and I saw the opportunity to conduct a research project that incorporated an activist dimension drawing on my academic training in GIS and cartography. From the outset, however, I was concerned about the many technical, logistical, and social limitations and implications of such participatory-mapping projects as well as the unpredictable political consequences of my engagement. As I became more visible in the Pemon communities through my work, teenagers, women, and elder Pemon became increasingly enthusiastic and supportive: young people and women in part because of the opportunities afforded to participate and elders because of the privileging of their knowledge.

However, some Pemon, in particular a small group of elite young political leaders, began to increasingly distance themselves from me. Toward the end of my four years in the Gran Sabana, I was indirectly informed of their complaints that I had assumed too much control of the project. Through subsequent conversations with these political leaders, who held elected offices or were appointed to regional or national institutions, I learned that although the project was successful in one aspect (my collaborators and I had produced a map of Pemon sector 5, as promised), they felt that I had failed to sufficiently train Pemon in GIS or to leave behind any computer equipment for them to continue mapping other sectors of Pemon territory. The project had been part of a trade from the outset: community leaders had given me permission to conduct my research of fire management in return for leading the mapping project, and no mention had been made about furnishing computers or GIS training. But despite my self-righteous frustration with what I felt were unreasonable complaints, I realized that these national and regional leaders, who rarely had time to visit remote villages and to whom I had little access because of their location in large urban centers far from the Gran Sabana, had felt marginalized. Although the map was completed, they were not pleased with how I had unwittingly sidelined them during the project.

These were not the only community factions leery of my work. A few community members in Kumarakapay—especially members of families who had little or no engagement with outsiders, had been marginalized in the increasingly divisive competition for access to the tourism industry, and had no rela-
important reminders of the need to reflect on the unknowable consequences of such global dissemination (Myers 2004). The ethnocartographers, project coordinators, and I were also surprised during two mapping workshops when a few community members adamantly refused to work with us because of fears that I and my collaborators had arrived in the village to "take land away from them," either to "sell" or to incorporate in the greater land-use area of Kumarakapay, which is the largest and most acculturated community in the Gran Sabana, the home of my collaborators, and also where I spent most of my time. We came to realize that in both cases the complaints were informed by animosities between some community members and the caciques of Kumarakapay. These animosities stemmed in part from what was seen as Kumarakapay's betrayal during a conflict in the late 1990s concerning the construction of a high-voltage power line through Pemon lands. While I was waiting for a resolution to these conflicts, which in both cases lasted for an entire day, my collaborators repeatedly explained the purpose of the mapping project—to produce a map of the entire sector 5 to be used in subsequent negotiations for land rights—and we were eventually welcomed in all 12 communities in sector 5.

The collaborators I have been referring to were 12 graduates of two one-month training programs I held in the spring of 2002 and 2003 in Kumarakapay. Developed and taught with the collaboration and assistance of the cacique at the time, Juvenicio Gómez, and the coordinator of the mapping project, Claudio Gómez, the two courses were intended to instruct students in basic cartographic techniques and to share experiences with participatory mapping in other indigenous landscapes. The 12 graduates from these courses self-identified as "ethnocartographers" and organized and led all the subsequent mapping workshops, working directly with elders to draw maps based on landmarks they had previously traced from remote-sensing images. Although I was present in most of these workshops, my input was limited to a personal introduction, occasional consultations with ethnocartographers about technical questions, and day-long hikes to record geographic coordinates with global positioning system (GPS) receivers. The ethnocartographers also participated in digitizing the sketch maps, editing the GIS, and designing the final map, which I completed in its entirety with my laptop while I was living in Kumarakapay. My relationship with the ethnocartographers (10 males and two women aged 17–28) and the dynamics within the group deserve a much deeper exposition than is possible here. The group was far from homogenous, and they initially became embroiled in conflict related to gender relations and hierarchical family relationships. Although Cristina Rossi and Brenda Fernández eventually emerged as popular leaders of the group, in part through my encouragement, they initially met resistance from male ethnocartographers. Also, graduates from the first course in spring 2002 were members of wealthier families more closely connected with the tourism industry. The second cohort, who graduated in spring 2003, were members of less well-off families who enjoyed fewer benefits from the tourism industry, and most of them had less formal education and spoke Spanish with some difficulty. After a few months, however, the ethnocartography team developed into a cohesive group to the point of exclusion, organizing social events, communicating independently with caciques and elders from throughout the Gran Sabana, wearing T-shirts and carrying business cards with the project logo they designed, and enjoying their status as "los etnocartógrafos."

As in the case of Kernahan, then, the mapping workshops in the Gran Sabana provided a forum for the contested reproduction of identities, spatialities, and indigeneity through performativity and storytelling. However, the political stakes were higher (the future demarcation of indigenous territory), and more diverse groups of mapmakers were involved: members of a dozen communities, each experiencing different rates of acculturation and exposure to state agents and tourists; teenagers and elders; women and men; local caciques and national leaders; ethnocartographers from different social and economic backgrounds; and, of course, my own white male person, embodying complex memories of domination, promises made and broken, and hopes of future monetary benefits and social status. Also, two versions of the Pemon language are spoken in the Gran Sabana: Arekuna in the north and Taurepan in the south. Although the two languages are mu-

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7. An example of unintended consequences of such mapping projects is the work now conducted by the Venezuelan branch of the Nature Conservancy (TNC) in other Pemon sectors. Although some ethnocartographers are assisting TNC, they are split in their view of the organization's intent. Some are displeased with the more limited participation and cartographic training in other sectors. Some are also concerned that the mapping project carried out by TNC terminated immediately after the termination of the project in sector 5 and without consultation with project participants in sector 5 is conducted primarily to benefit conservation interests. Pemon leaders have not permitted TNC to work in sector 5.

8. Before the start of the project, village caciques in sector 5 and 1 (as "principal investigator and cartographer") signed a formal agreement giving me permission to bring all hand-drawn maps temporarily to Kumarakapay, Caracas, and Cornell University for scanning and laminating. They also gave me permission to use data from the mapping workshops in my maps illustrating indigenous and state fire management approaches (Skelton 2006). During presentation of the final printed map in February 2004, my collaborators and I returned all the now-laminated original sketches to the appropriate communities and also provided copies from all communities with 2 x 2-meter color printouts of the final map and CDs that included the digital GIS map and all photographs and video footage recorded in the respective communities. The final maps were produced in their entirety in Kumarakapay on my own laptop with the GIS program ArcView 3.2 and were printed on archival photographic paper, courtesy of the Unidad de Información Geográfica of the Centro de Ecología, Instituto Venezolano de Investigaciones Científicas (IVIC), and the Centro Internacional de Ecología Tropical at IVIC.
tually intelligible, intermarriage is not common between the two linguistic groups, and capitaneos tend to divide along linguistic lines in elections for capitán general (the mostly symbolic head of sector 5). In what remains of this article, then, I will consider how these social relations were negotiated and (re)produced through memories and knowledge of place and landscape as Pemon "culture" was taking on "new and varied forms of materiality" (Myers 2004, 6) in the form of maps.

Talking Landscapes: Memory, Authority, and What It Means to Be Pemon

Among the once seminomadic Pemon, elders (abuelos and abuelas) are considered the bearers of genuine indigeneity. Elders are the privileged purveyors of traditional environmental and spatial knowledge, respected for their insights into the mystical realm and the pantons of the ancestors. As the elder Antonio Gonzalez once said to me, "It is I who know." This claim to authority is based on movement and seeing; that is, on the number of years spent walking, observing, and producing the landscape through their presence. Most of my elder informants preferred talking about fire management while walking or resting on special spots (pata eredok) on the top of high hills with sweeping views of savanna grasses in shades of green punctuated by dark splashes of forest groves, great sweeps of rivers, and wispy plumes of smoke. However, since the 1950s, the development of the highway to the Brazilian border, coupled with a national strategy to form permanent agricultural nuclei, has led to the formation of large villages such as Kumarakapay. Extensive areas once populated, especially near the borders with Guyana and Brazil, are now visited only during hunting and fishing expeditions. Elders who were born in nomadic settlements and who still walk the old trails thus embody nostalgia for these remote landscapes.

In the mapping workshops, elders quickly took the lead in drawing the more sophisticated second-stage maps, working with the trained ethnocartographers to demarcate natural and cultural features and locations of land-use activities. This was unremarkable, given the respect for elders and their knowledge; in addition, Pemon leaders had also asked me to prioritize the participation of elders in the mapping workshops. However, elders' hegemony has been increasingly challenged by younger community members, especially in acculturated communities where capitaneos are young and unaccustomed to traditional land-use practices. Conversely, elders are often frustrated with younger Pemon because of their lack of "indigenous knowledge." The mapping project brought this criticism of the mis-education of younger Pemon to the fore, partly because it made visible their lack of traditional knowledge and partly because the spatialities of younger Pemon often reflected the influence of modernity on youth culture. Elders therefore used the public stage afforded by the workshops to tell stories of landscape, identity, and belonging, thus reproducing what they saw as the meaning of Pemon indigeneity.

In a mapping workshop held March 4, 2002, in Kumarakapay, Laurencio, a young man in his early 20s, recently married, was working with Rafael, one of the most respected elders and also one of the founders of this modern village, having been born at a time when the Pemon were still seminomadic. Laurencio was drawing rivers on the map per Rafael's instructions, and this conversation ensued:10

Rafael: This place called Apak is where I lived before with my parents. It's north of the Kukenan River. Wuararita was also one of our places [pata, land] when we were children.

Pause.

Rafael (exclaiming): You young people don't know anything! Because those who know, are we, the grandparents. But you don't. This is because the grandparents have been moving back and forth from one place to the next. Because of this we know everything up to the corners. After we do all this, all who come after us will know. They will see all the places, the rivers, the hills.

Laurencio (in a mollifying tone): Yes, that's why we are doing this work. Thinking about our children.

Rafael: We need to put down everything, because in the future, if we leave spaces open, the people who work in tourism might take them from us.

In this conversation, Rafael is expressing the superiority of his knowledge based on his lifelong experience and is adamant in his attempt to educate Laurencio. His claims to uniqueness, then, are associated with persistence. Rafael continues to walk the same distant trails that he walked as a young man. Time is articulated with place through his embodiment of the landscape. However, his stories are performed in the context of discontinuity (young Laurencio has never walked some of these trails), reflecting the paradoxes of indigenous activism, which exhibit characteristics of both continuity and discontinuity with the past (Halley and Wilcoxon 1997). Also important is his emphasis on movement. Rafael is a member of the first generation of permanently settled Pemon, and he still sees walking as a virtue and, most importantly, as a means to see and thus to learn. Even though elders have very little formal education, they have little difficulty drawing pano-

9. Pemon workshop participants (children, women, and men) initially drew sketch ("mental") maps on empty sheets of paper, working in groups based on age and gender. In the following days, self-selected local experts (mainly elder men) worked with the ethnocartographers to systematically draw natural features and locations of land-use activities and sites of cultural significance on large sheets with predrawn visual guides. Workshops were held in 10 different communities in Pemon sector 5 with the participation of about 300 people, resulting in 12 different maps of village territories. The 12 separate maps were digitized in ArcView 3.2 to form a single map of sector 5 using georeferenced satellite imagery and about 2,000 GPS points as additional reference.

10. Informal conversations during mapping workshops were recorded with the permission of participants and later translated by Anibal Herrera and me.
ramic maps to a relatively high degree of accuracy, using ordering techniques to bestow control of the landscape on the viewer (Cosgrove 1984).

Another important point is Rafael's reference to "corners." Pemon visualize their landscape as both finite and infinite: infinite in the sense that one can always move from one place to the next and finite in the sense that a group of families have usufruct to a certain territory and that villages are separated by fluctuating but nevertheless meaningful boundaries. Rafael's use of the term corners alludes to the necessity of knowing not only the stories and meanings of the landscape but also the spatial extent of your space, your kowantok, which roughly translates to "homeland" but also means "life." In a broader sense, his comment implicitly links indigenous knowledge, indigenous land rights, and cultural survival, a rhetorical strategy commonly used among indigenous organizations (Haley and Wilcox 1997; Kirsch 2007). To Rafael, the map is not simply a representation of a land claim but a material object that embodies indigenous identity. Blank spaces on the map would suggest that Pemon are not fulfilling their responsibility to the landscape handed down by their ancestors. (The spiritual connection elder Pemon feel for the ancestors. (The spiritual connection elder Pemon feel for the landscape. By employing traditional structures—privileging indigenous knowledge; at the very least, she suggests that they, the women, express conflicting desires for tradition and modernity and tend to defy elders, just as they voice criticism of state agencies. This is a segment of the conversation among the women drawing their sketch map in the workshop on February 24, 2002, in Kumarakapay; as discussed above, the protagonists are Cristina Rossi (in her late teens at the time), Rosa Emilia Fernández (in her early 40s), and Leticia Fernandez (a grandmother in her 50s and also Cristina’s mother).

Rosa Emilia: Kadawata River empties into Chirimota River.
Leticia: Chirimota River starts at Akurimota.
Rosa Emilia: No, Kurui River starts at Akurimota! Pause.
Rosa Emilia: Claudio knows everything!
Cristina: Sure. Because he travels in helicopters.
Rosa Emilia: We are taking an exam in geography!
Rosa Emilia: After Karawata comes Arapota.
Cristina: Really?
Rosa Emilia: Kamaiwa is a mining area.
Leticia: We ourselves are making our map. Why? It's because we are from here.

I suggest that this passage illustrates the articulations between identity formation, gender relations, and spatialities that characterized the mapping workshops. After a brief argument about what river starts at Akurimota, Rosa Emilia explains that "Claudio knows everything." Claudio, the coordinator for the mapping project, has long been employed by EDELCA and is occasionally flown in the company helicopter to locations throughout the Gran Sabana. Although many villagers resent EDELCA for the domination the company represents, they also grudgingly respect Claudio for his opportunities to enjoy a visual perspective superior to the view from the top of a hill. Cristina, who was still in high school at the time, offers the opinion that of course Claudio knows the landscape better than the elders because his knowledge is derived from modern technology.

Rosa Emilia then suggests that they are "taking an exam." Implied in her statement is the notion that the women are being tested and must live up to standards set by elder men. However, Leticia marvels at the fact that they (or rather "we," meaning the women) are actually making their own map. That is because they "are from here," she says; that is, the women's local knowledge is validated by their birth and lifelong presence within the embrace of their kowantok. In her subtle way, Leticia appears to be countering Cristina’s suggestion that Claudio knows better because of his scientific knowledge; at the very least, she suggests that they, the women, have as much right to make their own map because they are also "from here." The mapping workshop, then, became a stage for women to perform their own stories of place and landscape. By employing traditional structures—privileging elders' knowledge, foregrounding the significance of embodied experience—they were able to cross established social boundaries (see, e.g., Okafor 1994).
Ultimately, the conversation illustrates several axes of tension associated with the reproduction of "genuine" indigenous knowledge and hence "proper" indigenous identity: strongly cemented gender relationships were both reaffirmed and challenged, knowledge claims were contested, and generational conflicts simmered beneath the surface. About two weeks later, these contests intensified in another workshop, where Cristina—now a graduate of the ethnocartography course with a developing identity as an "expert" in a field of modern science—had the following conversation with Rosa Emilia and Antonio Pérez, an elder respected for his physical strength, endurance, and knowledge:

Rosa Emilia: Look, is "Monte Bello" the name [of the village] in Pemon or does it have another name? Antonio (annoyed): No, sure it has its own name. It's called Panaimota.
Cristina (frustrated): What's with all these details? Do we have to correct this? Write the name in Pemon!
Rosa Emilia: So now what's missing is fixing all these mistakes.
Cristina (sarcasm in her voice): So, now, is there anything else missing that we need to put on this map?
Antonio (exclaiming): Yes, yes! What's missing is changing all the names into Pemon!

Ironically, Antonio's emphasis on writing place-names in Pemon rather than in Spanish did not simply originate locally. In the beginning of the project, the ethnocartographers and elders occasionally wrote or told us place-names in Spanish, and Pemon leaders would encourage them to provide the toponym in Pemon, thus contributing to the (re)production of a Pemon identity that privileges traditionalists (see, e.g., Haley and Wilcoxon 1997; Hornborg 1994). In fact, in the Gran Sabana, some Spanish place-names created by nonindigenous state officials are more commonly used than Pemon toponyms. For instance, even when speaking in Taurepan, villagers typically refer to Kumarakapay as "San Francisco," which was its name at its founding in the 1940s. Only in the late 1990s did the village chief at the time, an activist leader with experience in national politics, lobby to change the name to Kumarakapay after the creek located at the site of the original settlement.

Other linguistic inconsistencies abounded. Villagers commonly used the Pemon words para or dopá to label creeks and intermittent waterways but used the Spanish word río (river) instead of tanno tama to indicate rivers. They labeled hills sometimes with the Pemon word tepuy instead of other times with the Spanish word cerro. On the same map, a natural feature might be given both its Pemon and Spanish toponyms. The community of Agua Fria, for instance, is named after the river with the same name, in Spanish. In Pemon, the name of the community and the river is Kuy Yeremepó. On the map drawn by adult men during a mapping workshop in this community, participants used the Pemon name to refer to the upper part of the river—located at the greatest remove from the highway, in a landscape more exclusively embodied by the experiences of Pemon hunters and fishermen—and the Spanish toponym to label the lower part of the river where it crosses the Pan-American Highway.

The ironies continue. Antonio was wrong when he identified Monte Bello as Panaimota, because the name of the village is, in fact, Irewata. This is not to say that Antonio did not know the proper name; this is a short segment of a longer, animated conversation, and Rosa Emilia and Antonio simply miscommunicated. The point is that Rosa Emilia (a politically savvy and "radical" woman chafing at the gender politics that preclude her from leadership in the village) did not know the name of the village in Pemon but sensed the political value of writing the name in Pemon. After she made this point to Antonio, he then rebuffed the unusual challenge from Cristina (who was emboldened by her participation in the ethnocartography course) and again assumed authority because of his "traditional" knowledge. In this way, the significance and form of this particular spatiality was debated, and a decision was made. It became clear that it was necessary to represent the indigenous identity of the community of Monte Bello through words as well as images and that this would show the occupation of space not only by Pemon, but by "genuine" Pemon well versed in their identity.

Grounds for Resistance: Entanglements of the Material and the Symbolic

I have revealed some of the ways in which landscape and identities were mutually constituted through processes of social change. The mapping workshops became a stage for younger people to reveal their ambivalent embrace of the modern and for elders to perform their desires for an indigenous identity derived from historical narratives. However, a political-economic reading of these maps also reveals different perspectives on the materialities produced through these processes of social change. While the contests between elders and teenagers drew on specific knowledge formations and conceptions of indigeneity, the maps also reflected the complex positions of different social groups within broader political-economic structures. The following conversation took place among teenagers drawing their sketch (mental) map in the mapping workshops in Kumarakapay on February 24, 2002:

Andel: We are drawing the tepuyes [flat-topped mountains].
Lino (exclaiming): You're drawing your map as if you're painting a car! Why didn't you draw the power line?
Roger: Yes, we're missing the poles and the wires.
Lino: So after this, are they going to give us the land?
Andel: No, first Bjorn is going to bring the maps to the United States.
Lino: Where is the cattle [grazing] area?
Roger: We're missing the forest.
These five teenagers were working together to draw a map of the territory of Kumarakapay at the same time that elders and women were drawing their own maps of the same area, but before they arrived at their final product, some spatial narratives were present. The group of teenagers went through an impromptu process of negotiation, as suggested by this slice of their conversation. Unlike the elders, who started drawing their final map directly on the 1 × 1-meter paper I had supplied, the teenagers decided to first draw their own map on letter-sized paper and then join the different representations into one map. It was through this contested merging of representations that some spatial narratives were dismissed and others were foregrounded and adopted as the group’s official rendering of Kumarakapay territory to be presented to village leaders and elders. Not surprisingly, this process of negotiation revealed that Pemon teenagers form and re-form their identities in different ways, depending on their relationships with political-economic structures both within and outside the community.

To begin, Anel and Lino are members of relatively poor families who make their living mainly through subsistence practices. They were also active in the demonstrations against the power line that was being built by EDELCA through Pemon lands in the late 1990s. The most radical of the protesters, among them Lino and Anel, tore down the high-voltage towers as soon as they were erected, blocked the Pan-American Highway, and faced the tear gas of the National Guard. By the time of the mapping workshops, a compromise had been reached, and the power line had been constructed, but the conflict had influenced Lino and Anel to the extent that their personal maps present Kumarakapay village territory as a battleground: Anel identifies Kumarakapay as a Zona de Guerra (war zone; fig. 3), and Lino labels his map No al Tendido Electrico (“No to the electric line,” the political slogan of the movement against the electric line; fig. 4). This is one of the reasons why Lino asks whether making the map means the villagers will “get their land.” Anel, on the other hand, foresees a slightly longer process, including a temporary removal of the maps to the United States.

The other participants in the working group were Roger, Leonel, and Tony, whose maps display a perspective on Kumarakapay somewhat different from those of Anel and Lino (figs. 5–7). Roger, Leonel, and Tony are members of large, prominent families engaged in tourism and the growing service economy in the village, and Tony was the natural spokesperson for the group. He was around 20 at the time (the oldest in the group), one of the most fluent speakers and writers of Spanish in the community, and a student in a teacher’s college. More self-identified radical young people like Lino and Anel see Tony, Leonel, and Roger as prey to external influences. Tony, however, argues that the Pemon should negotiate with state agencies with the support of scientific knowledge and verbal eloquence rather than pursue confrontational politics. This is why he pointedly ignored Lino’s suggestions to draw the power line on their map and why the teenagers’ joint map resembles more his own representation and those of Roger and Leonel (fig. 8). As Tony said when he presented the map on behalf of the group of teenagers,

“We made our map like a city map. We think you will understand it well because we made a legend. The green and yellow symbol means it is an indigenous community. And these symbols in blue are the most important tourism sites. And here we have the agricultural zone.”

He then proceeded to point out the location of the power plant, which was built in the 1970s to bring electricity to San Ignacio, a nearby community where EDELCA offices are located. The power plant also serves Kumarakapay because of its location only 8 km from San Ignacio. In the question-and-answer session that followed his presentation, I asked him why they included the power plant on their map. “It’s because it’s important for San Ignacio and Kumarakapay,” he answered. “This is where we get our electricity from.”

At that point, Antonio Pérez abruptly rose and said about the elders’ map (fig. 9),

“The mental map we made is in accordance with our grandparents, who lived in this area. And now you [the teenagers] need to learn [the extent of this area], because those who come after us are going to need more land than we have now, because we elders are not going to live much longer.”

Then Antonio proceeded to recite the place-names that traditionally have marked the boundaries of Kumarakapay village territory, all the while pointing to the horizon. His performance was a form of social action with great awareness of the audience (Myers 1994), drawing on words, gestures, and his own embodiment of histories and landscape in his effort to be persuasive (Stoller 1994a). By reproducing the ritual of pointing into the distance and speaking the names and histories of places from the top of a high hill, Antonio was also illustrating the significance of temporarities for making landscapes meaningful (Tilley 1996).

From the group of teenagers, Anel responded plaintively to Antonio’s recital,

“We have made our map but it is very narrow [covering a smaller area than the elders’ map]. But we have drawn all of what we have lived. For example, when Christmas comes, some of us go hunting. And we have included this distance on our map.”

Finally, Laurencio, the young adult who sketched the elders’ map following their directions, stood up and said,

“As Antonio said, the teenagers have made a map only to serve the interests of tourism. They have drawn the waterfalls and the power plant and the tepuyes, but we have drawn...”
Figure 3. Mental map drawn by Anel Delfranco in a mapping workshop, Kumarakapay, Venezuela, February 24, 2002. A color version of this figure is available in the online edition.
different things. Because what we have drawn in our map are the old settlements of the grandfathers and gardens and other things.

By charging the teenagers with lower authenticity (Hornborg 1994, 255), Laurencio effectively foreclosed any further debate. An obvious but not particularly useful observation, then, is that teenagers' conceptualizations of the landscape differ from those of elders. This could partly be explained by some teenagers' desire to impress the foreign visitor with his cartography skills. The point, however, is that these teenagers could not have drawn a "traditional" map such as that of the elders even if they had wanted to, simply because they lacked the necessary knowledge of histories, place-names, and land-use localities and boundaries. Instead, I suggest that the final map jointly produced by this group of teenagers was the result of negotiations implicated in contested reproductions of indigenous identities. Through these contests, one representation (that of the more formally educated and outspoken Tony) held sway over that of the more rebellious and locally disadvantaged Lino and Anel.

To elaborate, the main point here is that the position of Kumarakapay within the regional, national, and international political economy is conceptualized differently by different social groups and that these conceptualizations are made visible in their maps and revealed in their conversations. While teenagers discussed the conflict over the power line, elders recalled pantons and the places where ancestors lived, walked, and hunted. While elders exclusively drew place-names with historical significance, the teenagers, under the influence of Tony, prominently displayed the power station but left out the power line (which to many Pemon is a complex symbol of domination but also of indigenous resistance). The teenagers represented scattered gardens as an "agricultural area," in keeping with the rhetoric of generations of state development projects; for much the same reason, they sketched "grazing areas" even though the "cattle industry" consists of a few underfed heads of cattle owned by one family; and they...
located "tourist sites" rather than more "typically indigenous" recreation sites, sacred sites, or historical settlements. The elders' map, meanwhile, makes no reference to this material evidence of state incursion in their territory but instead emphasizes the vastness of the space claimed by Kumarakapay all the way to "the corners" of their land-use area.

Eventually, the elders' spatiality won out as the official representation of Pemon sector 5, as it had to because of the priorities expressed by indigenous leaders. The second-stage, "intermediary" map of Kumarakapay (which served as the basis for the final, printed map) came to include "gardens" (locations of slash-and-burn agriculture) instead of "agricultural zones"
Figure 6. Mental map drawn by Leonel Rossi in a mapping workshop, Kumarakapay, Venezuela, February 24, 2002. A color version of this figure is available in the online edition.
Figure 7. Mental map drawn by Tony Rodriguez in a mapping workshop, Kumarakapay, Venezuela, February 24, 2002. A color version of this figure is available in the online edition.
Figure 8. Section of mental map drawn by youths in a mapping workshop, Kumarakapay, Venezuela, February 24, 2002. A color version of this figure is available in the online edition.

and "bathing places" and "scenic views" instead of "tourist sites." No material evidence of state incursion (except for the Pan-American Highway) was included (fig. 10). Except for the highway and the Venezuelan border city of Santa Elena, the final map excludes the power plant, EDELCA installations, National Guard posts, park service structures, and other signs of state interventions in the area (fig. 11).

Even the title of the final map, "Makunaimó Kowamupó Dapon," reflects the significance of continuity in Pemon representation of self and the conscious process of eliminating what were considered nonindigenous spatialities. What to name the map was an important topic of discussion during the weeklong sectoral workshop to edit the draft map in fall 2003. The workshop participants, about 70 elders and capitanes from all the principal communities in the sector, proposed a variety of titles that were put to a vote by the entire assembly. The winning title—which roughly translates as "The flat surface that shows where Makunaimó lived and walked"—reflects a complex articulation of symbolism, materiality, and embodiment: the Gran Sabana is alive with stories about Makunaimó, the mythical predecessor of the Pemon, who conquered this landscape from the supernatural beings who still inhabit its hills and rivers. To many Pemon, the spirit of Makunaimó is embodied by the elders, who have walked the same trails and have seen the places where Makunaimó lived and therefore know what others do not. Thus, the Pemon in sector 5—in collusion with their collaborator, the foreign-trained geographer—employed technologies of concealment and exhibition to convert culture into a new material form (Tilley 1996).

Discussion: Performance, Spatialities (and Resistance?)

I have explored how performances of histories and knowledges were implicated in the production of spatialities and the ways in which the mapping process reflected complex power plays within the communities in Venezuela and in Trinidad. The workshops provided a space for different identities to be performed but also to be contested. Fishermen in Trinidad performed stories of belonging and persistence to claim
rights to fishing grounds and to associate themselves with a winning discourse of environmentalism. Elder Pemon performed in accordance with their memories and embodiment of a landscape free of the burdens of state interventions, while (some) teenagers performed from a script written by modernity. In so doing, these teenagers made visible the material manifestations of political-economic structures while exposing their conflicting desires for modernity and for an idealized indigenous past. So although the teenagers produced maps that celebrated state interventions because of the central role they play in their lives, their map promptly generated a debate informed by the contested knowledges and imaginaries of different social groups.

In Rosa Emilia’s words, the women in Kumarakapay had drawn their map to the limit of their knowledge. In fact, they had moved beyond firsthand knowledge. They had drawn places they had seen only in their imaginations, because stories told of experiences in these places had created what Tilley (1996) calls “structures of expectation.” This suggests that cartography is intimately connected with power and not simply in the sense that some mapmakers have greater means of producing a “professional” map with the rhetorical stamp of authority and making it readily available and seemingly important. Instead, cartography as art (Gell 1998) reflects the ways in which mapmaking facilitates performances and hence action: stories told of landscapes, embodying different temporalities and knowledges, both reproduce and create new forms of indigeneity and social relations.

We must also consider the importance of knowing how to draw a map. Despite the very different political-economic, social, and historical contexts of the Nariva Swamp and the Gran Sabana, both fishermen in Kerhanan and elders in Kumarakapay wanted a map of their own, and my intervention made the map production possible (although only limited knowledge of mapmaking remained within the communities). Only in the Gran Sabana did the ethnocartographers learn how to lead mapping workshops and how to construct mental maps, but the lack of time, funds, and existing infrastructure made it impossible to provide software, computer equipment, or much GIS training. Only recently have the Pemon begun...
using the map in their negotiations for land rights and to produce a more detailed historical map of Kumarakapay. This is the promise and pitfall of participatory mapping. Mapping practitioners will dub their projects “participatory” (in the sense that these maps are not solely the product of an outside researcher but the result of a group activity of some sort), but when these maps enter the global flow of ideas and knowledges, they often move beyond the control and even grasp of many marginalized peoples.

The two case studies presented here suggest that although specific political-economic contexts and processes are important in forming everyday spatial practices, participatory mapping is a performative practice that strives to effect social relations in ways that transcend regional specificities. The mapping projects with both the diasporic South Asians in Trinidad and the indigenous Carib in South America involved performances of histories and landscapes embodied by the activities of humans and spirits, positionings and productions of narratives of belonging and exclusion, and contested visions of indigeneity and the proper ordering of people, things, and activities. Although the local, regional, and national governments were quite different in Trinidad and Venezuela (i.e., the authorities confronted and the organizing strategies were different), the maps ended up looking quite similar. This is partly because the professional cartographer was the same individual in both cases, but also, I would argue, because the maps reflect performances of place, people, and genuineness and therefore converge in style, scale, and the details that are included: place names that reflect local histories, locations of land-use activities within an area easily traversed by foot, places of “cultural” significance.

The convergences we observe between these two sets of maps (and with other community-based, ethnographic, participatory maps) also suggest another influence at work. Participatory mapping remains, in large part, a globalized practice situated within rigid, unequal structures of power and knowledge. Very few mapping projects originate from the margins, and even fewer are conducted exclusively by the local peoples.
whom this methodology is intended to serve. Rather, projects originate in global arenas and are conducted under the guidance (and often in accordance with the interests of) national or local elites. This does not deny the potential of this methodology to further territorial-rights claims or more genuine, participatory approaches to conservation, community development, and other forms of community-based planning. But it behooves practitioners to consider the ways in which participatory mapping is situated within local-global flows of technology, identities, and imaginaries; how participatory maps reflect the imprint of local-global relations of power; and how such mapping projects produce spatialities that reflect both the material and the cultural constructed dimensions of postcolonial landscapes.

Ultimately, participatory mapping becomes something far more complicated than a rational coming together of minds to produce the "best" of all possible maps. Instead, because maps are such desired objects, mapping workshops become theaters for the performances of identities, the reading and interpretation of histories, and the production of material and imaginary landscapes that participants consider "theirs."

Mapping is a performative practice in which individuals speak and act their histories of landscapes and belonging, and through these performances, they negotiate their relationships with each other, with space, and with power. Ultimately, participatory mapping as performative practice provides a space where political-economic structures and controls can be unmasked and participants may be enabled to exert a measure of control over the multiple, contested meanings of indigeneity and authenticity.

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Comments

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Bjorn Sletto’s article identifies and grapples with what could be considered the next frontier in the interdisciplinary area of scholarship known as “participatory mapping.” His argument also applies to the broader realm of methodological innovation—known by various terms such as “activist,” “collaborative,” and “participatory-action” scholarship—to which participatory mapping belongs. Now that we have cleared some ground—challenging the hierarchies and ingrained inequities in conventional research methods, developing alternative practices that emphasize collaboration, dialogue, and horizontal relations with the people who are the subjects of our research—it is time to turn the lens inward, to scrutinize the complexities and contradictions embedded in the new methods we have developed. I use “we” advisedly here in that I have been engaged in participatory-mapping projects for some time, and I also see a great need for this kind of process-oriented internal critique of these efforts. In this spirit of applauding Sletto’s work and using it as inspiration to forge a parallel path for my own, I raise four points of critical reflection.

First, I note a faint implicit “loss of innocence” trope, which gets in the way of the article’s broader objectives. For example, Sletto states that “participatory mapping becomes something far more complicated than a rational coming together of minds to produce the ‘best’ of all possible maps.” Who would have thought otherwise, given that these maps are by design interventions in power-laden fields and that subordinated communities that make and deploy countermaps are always shot through with multiple divisions, which at best they manage to mediate in the interest of a provisional unity in struggle? The challenging edge in Sletto’s argument, in my reading, is not to have discovered these fault lines but to have insisted on narrating them, striking the delicate balance between an ethical-political commitment to the community’s struggle and a thoroughgoing internal critique of their efforts. Sletto has done an impressive job of doing both, but it would have been more convincing still had he replaced the loss-of-innocence trope with more explicit reflection on how he reconciled these two partially contradictory goals.

Second, one element in this delicate balance not well represented here is countermapping as, in Sletto’s words, “a space where political-economic structures and controls can be unmasked.” I agree completely with his focus on internal process, both how counterclaims for territory are constructed and how constitutive inequities, such as gender, age, and economic position, shape this process. Yet one way to make sure that this emphasis does not descend into self-referential cultural critique is to keep the broader contours of the struggle front and center. These are present as the backdrop of Sletto’s article, and such articles work better when they do not try to accomplish too much. Still, especially given his goal of advancing a programmatic statement on the future of participatory mapping, I worry that Sletto has let the pendulum swing to the other extreme, where the only power inequities being unmasked are those within the community in struggle.

Third, given this sharp focus on internal inequities and conflicts, I would have liked to hear more about how Sletto himself enacted his role as activist geographer. The conflict in the first case is less problematic: he was aligned from the start with the traditional fisher folk, in tension with the trappers, and the resulting map reflected this positionality. But in the second process, involving the Pemon, Sletto engaged gender and age hierarchies in more complex ways, as evidenced by his choice to highlight these facets in his ethnography. However, he does not fully explain how his own countermapping methodology influenced these unfolding relations. For example, Sletto actively created spaces for Pemon women and radicalized youth to participate in the mapmaking; but he also eventually decided to back off, so that the maps that ultimately result reflect mainly the perspectives of male elders. Greater (auto)ethnography of this dimension would have furthered his basic goals of process-oriented analysis and, more important, would have allowed him to address a common objection to this kind of activist research. Given the pervasive internal inequities within “communities in struggle,” the critics object, how can the activist scholar avoid taking sides, which thereby elides these inequities? Sletto’s experience seems to contain a convincing response to this critique, but he does not fully share it with us.

Finally, in order to achieve even provisional closure, this analysis of internal process needed to address the “afterlife” question more directly. Once a countermap is produced, it generally plays some role in the community’s struggle to achieve territorial rights, and by extension, it also plays an ongoing role in the internal relations of power and performance that Sletto narrates. This raises a pointed question: if, for example, women or radicalized youth have a lesser voice in the production of the countermap, does it follow that such
inequities will be reinforced as that map is put to use? A fuller statement of Sletto’s programmatic vision for participatory-mapping research would consider this question, balancing his salutary call for attention to the process of mapmaking with greater emphasis on what happens “after the map.”

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Critical cartographers (e.g., Chambers 2006; Pickles 1995) have long been encouraging reflexivity in participatory mapping. Sletto now calls on cartographers to develop this reflexivity specifically by devoting ethnographic attention to “participatory-mapping workshops as theaters for the performance and negotiation of identities.” Focusing his ethnographic lens on the scale of the family and the community, Sletto describes participatory mapping itself as “a performative practice in which individuals speak and act their histories of landscapes and belonging, and through these performances, they negotiate their relationships with each other, with space, and with power.”

What new insights are gained from this designation of participatory mapping as “performative”? Like Tania Li (2000), in her work on Lauje and Lindu identities in Indonesia, Sletto examines the ways in which Pemon foreground indigeneity to gain a particular type of legitimacy. However, while Sletto looks at performativity in mapping, Li conceptualizes political deployments of indigeneity as “positioning.” How does performativity as a concept productively differ from positioning in the context of indigenous movements?

Sletto’s detailed descriptions of intra- and extravillage dynamics highlight the ways in which the different positionalities of Pemon community members are embodied in the diversity of Pemon maps. Focusing on the performance of mapping as an important process within the Pemon community, Sletto describes the ways in which women place elder men’s personal interactions with the entire landscape above their own experiences, for example, and the ways youth see the land as a hotly contested zone rather than as territory composed of a series of traditional relationships. Sletto finds the process of mapping vital for “what it had revealed about the entanglements of identities, social relations, landscape, and power in places on the margins.” He seems most struck by the importance of this process for the researcher developing reflexivity in participatory mapping. I am left wondering how this process affects social relations in the locality over time and the ways the maps are ultimately used.

Like other critical cartographers (such as Wainwright and Bryan 2009), Sletto is attentive to the influence of his own presence. He writes, “As a social scientist, I was concerned about reproducing the essentializing dichotomy between indigeneous tradition and modernity; as an activist scholar, I was aware that indigenous movements often obtain more influence when they employ primitivist imagery.” As a participatory cartographer, I take issue both with this point and with Sletto’s use of the term “primitivist.” Rather than calling for “primitivism,” the growing international environmental-justice audience, for example, is attentive to indigenous people whose livelihoods and identities are threatened by corporate expansion. Cartographers need show not primitivism but an ongoing history of struggle to maintain community and identity in the face of coloniality. Sletto’s juxtaposition of “indigenous tradition and modernity” is essentializing in itself and denies the essential modernity of indigenous people advocating for traditional lands and recognition.

The risks of mapping are well known to participatory cartographers, so Sletto may be preaching to the choir with his warning that “Making indigenous landscapes visible in new material forms, such as maps, is a move fraught with risks, raising complex questions about representation, appropriation, and dissemination.” This is particularly salient in 2009, given the tone of emerging concerns over the México Indigena project (Araujo 2009) and the responses to these concerns (Dobson and Bird 2009). Perhaps the most impressive part of Sletto’s article for the activist cartographer is his description of having trained a dozen indigenous “ethnocartographers” who were empowered by their new knowledge. Looking at the ways in which these newly trained cartographers negotiate “social relations” on the “new ... materiality” of the map offers a ripe opportunity to examine the researcher’s effect on the community.

Using ethnographic methods enables Sletto to investigate the diversity of spatial and cultural perceptions within Pemon country and the structures of possibility that Pemon operate within. Sletto shows convincingly that participatory mapping is a process through which individual Pemon ethnocartographers “perform” place and culture. By including no fewer than nine different versions of Pemon territory in the maps throughout the article, Sletto does not allow one representation of Pemon space to dominate. Is including all of these different Pemon maps convincingly antiessentialist? How does it affect the struggle for territory that the Pemon are facing? I am straying from Sletto’s focus on process rather than outcomes, but there would not be a process without a possibility of an outcome. Sletto may need to marshal more evidence to convince critical, participatory, activist cartographers of the value of understanding mapmaking as performative.

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Bjørn Sletto’s analysis of the meanings and interpretations involved in the events of participatory mapping as theaters...
for the performance and negotiation of identities provides an ethnographically rich and theoretically inspiring examination of the struggles over knowledge and power occurring in participatory workshops. Sletto's case studies of the participatory-mapping workshops in Trinidad and Venezuela offer highly convincing examples of how categorical distinctions between global forces and local resistance are too simple to illustrate the hybridity of meanings and contestations involved in participatory processes. This subject has rarely been tackled in discussions on participatory approaches and thus warrants a careful analysis.

Sletto's call for a greater reflectiveness in the application of participatory approaches is both relevant and challenging. First, critical reflection is necessary for the theoretical revision of the often taken-for-granted assumptions involved in participatory approaches to the harmonic dialogue between the researcher and "those to be researched." Second, careful reflection is necessary for the unveiling of the powerful policy implications often embedded in participatory exercises. Sletto's thorough analysis of his own contradictory roles during participatory mapping provides an important reminder of the ambiguous relations of knowledge and power involved in any research project or human capacity-building program, no matter how participatory their agendas might be.

Sletto's analysis provides a sophisticated examination of the multifaceted negotiations and contestations among the local people on the "right" interpretation of the local landscape. Sletto's examples of gendered conceptions of the environment provide important insights into the socially differentiated environmental knowledges within a society where "vision and walking are privileged ways of knowing." Equally interesting are Sletto's examples of how the Pemon elders challenged the younger generations' right to talk about and interpret the local landscape. These examples clearly demonstrate how landscapes are always imbued with the histories, cultural rules, and social relationships of the people living within them.

That said, Sletto's analysis of the political economy of participatory processes could have been stronger. Although in the beginning of the essay Sletto briefly observes that participatory approaches have been criticized for their dualistic approach to culture, power, and the locality, it would have been interesting if he had analyzed these issues more carefully in the light of his own ethnographic material. I strongly agree with Sletto that the processes of talking about and drawing landscapes can offer important forums for the revelation of the uneven structures and relations of power that shape places on the margins. However, the unveiling of such structures does not yet mean that these structures will be changed.

In this respect, Sletto's claims that participatory workshops act as counterhegemonic activities or arenas for social agency and political assessment for marginalized people might be exaggerated. Although marginality does not eliminate social agency, poor people's lack of material resources and their deeply felt experiences of marginalization constrain the forms of agency available to them in multifaceted processes of negotiation (Bahre 2007; Cleaver 2005; Nygren 2004; Walsh 2005). Even after Sletto's analysis, the reader is left in doubt as to whether the Pemon performers themselves sincerely felt that the participatory-mapping workshops provided them with chances for actions "intended to change the world" or whether this idea of participatory mapping as emancipatory politics was instead one that the activist scholars themselves wanted to believe in. The fact that all evidence of state intervention was ignored in the map selected as the official representation of the Pemon landscape does not yet imply that the state interventions in the locality will be eliminated or decreased. As pointed out by Nygren and Rikoon (2008), the changes in people's environmental conceptions and in their ways of interpreting the landscape do not automatically lead to material changes in the landscapes or in the power structures involved in the use and control of them.

Unfortunately, Sletto raises this issue only in the conclusion of his essay, where he cleverly points out that the mapping projects often originate in global arenas and are conducted under the guidance and control of national or local elites, thus reflecting the imprint of neocolonial power relations and representations of landscapes. The constraints involved in the participatory approaches' efforts to increase the ability of marginalized people to reshape their lives and to reformulate their living conditions deserved to be explored in greater detail in Sletto's otherwise brilliant analysis.

I completely agree with Sletto that participatory-mapping workshops are "something far more complicated than a coming together of minds to produce the 'best' of all possible maps." By providing a careful analysis of the negotiations and contestations involved in participatory-mapping workshops as theaters for the performances of identities and interpretations of material and imaginary landscapes, Sletto provides a significant theoretical and public policy contribution to anthropology and other disciplines interested in participatory approaches.

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Participatory mapping is commonly used in Latin America for the recognition of territorial rights, planning natural resource use, and analyzing/resolving land use conflicts. Despite its increasing popularity, there is scant discussion about the complexity of the mapping process. In this article, Sletto makes an opportune contribution to this necessary discussion. His work demonstrates that despite our good intentions to strengthen, empower, or emancipate local communities in their struggle for land, natural resource use, and cultural identity rights through self-demarcation projects, community mapping projects are difficult to carry out, not so much because of resistance from the state, as is commonly the case.
in our region, but because of the complex play of power within communities.

The more novel aspect to Sletto's account is not so much the intricacies of community power relations but the few revealing confessions he makes about how awkward, frustrated, and bewildered he felt with the level of local complexity that he experienced in his mapping projects, particularly in the Venezuelan one. I pick on this because I think that a greater reflexivity in the application of participatory-mapping projects, as urged by Sletto, also requires that we discuss how demanding it can actually be for us as mapping practitioners, activists, and researchers to work in these types of participatory encounters. Are we prepared to deal with local complexity? What type of researchers or mapping practitioners does it take to be able to endure all the community tensions, divisions, mistrust, misinterpretation, gossip, and occasionally bad intentions that form part of these processes? This is something we seldom talk about, but it also requires close attention if we want to be more realistic about community mapping.

I have known Sletto for about 10 years through our respective work in Venezuela with the Pemon, and I have known about the mapping project he helped develop in the Gran Sabana since its very beginnings. Despite all the difficulties encountered and also some of the failures honestly acknowledged by Sletto, this is one of the most thorough and complete experiences of land demarcation by indigenous people in Venezuela. But the project was finished at a great cost for Sletto. Four years of his life were devoted to this project, requiring all his patience, physical endurance, creativity, and dialogue/negotiation skills. Not everybody has these skills, and this is perhaps also a reason why not all community mapping projects succeed in helping local communities advance toward self-determination.

This suggests that we ought to be thinking of acquiring skills in addition to those received in conventional academic training when deciding to undertake this type of endeavor. I am thinking specifically of a minimal knowledge of conflict resolution and customary forms of conflict management in order to be able to deal with the tensions that arise when facilitating participatory processes. No matter how respectful and cautious one may be, conflicts will always arise in these types of processes because that is what happens when people's views, interests, and values are negotiated. Thus, apart from accepting the fact that conflicts are normal, we need to be able to work through them without feeling frustrated or overwhelmed by local dissidence. Particularly if the mapping process is viewed as playing a role in shifting power relations, it requires great commitment from external facilitators to help the community overcome mistrust and engage in thorny discussions about identity, tradition, modernity, and the land.

Complexity itself is not as much of a problem as the fact that it often takes us by surprise, hence the importance of being prepared to deal with it. A change of focus in the objective of participatory mapping could also help with this. Rather than viewing participatory mapping mostly as a tool to empower indigenous peoples and local communities in their struggle for the land with "outside" actors, we should start viewing it more seriously as a tool for internal deliberation and the clarification of local views. If we make this a starting point instead of an accidental finishing point, we will be able to play a better role in facilitating dialogue and deliberation when local dissidence arises. Having greater clarity about their internal differences and their desired future will also help indigenous people be in a stronger position to debate development and territorial rights with the state and other national elites.

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7 II 09

Bjorn Sletto's analysis of two participatory-mapping exercises he led in Trinidad and Venezuela sheds fresh light on a set of practices that have become increasingly widespread within development circles over the past couple of decades. The cases he describes originated in very different circumstances. The first was launched, in Sletto's own terms, with seemingly little purpose other than to assist "an eager graduate student" in his quest for a better understanding of the changing spatiality of fishing and rice-growing practices in Trinidad's Nariva Swamp. The second, by contrast, was more explicitly "activist" in its orientation. After recognition of indigenous land rights in the new Venezuelan constitution, Sletto helped demarcate the territories of several indigenous communities along the Venezuela-Brazil border.

Sletto shares with his readers his surprise at the fact that there seemed to be almost as much at stake for participants in the more sterile academic exercise of mapping Nariva Swamp as there was in the overtly political project of claiming indigenous land rights in Venezuela. Despite their different purposes, he found that each of his workshops was actually used by his informants in very similar ways. Unbeknown to Sletto at the time, the Nariva Swamp was home to a simmering controversy between different factions using widely divergent techniques for capturing fish in the swamp. It was also the subject of escalating concern by environmentalists opposed to large-scale commercial rice plantations being established there. In effect, both the Trinidadians and the Venezuelan workshops Sletto ran were converted by his research informants into venues for the performance and assertion of identity and authenticity claims.

I would like to raise just two brief points in response to Sletto's thoughtful and provocative essay. The fact that mapping workshops should be manipulated by locals for political purposes is not at all surprising to me. In my own experience as a development worker before entering the academy, I found myself constantly in play by opposing factions engaged in per-
was true when I eventually took to the field as a researcher. Despite my careful disclaimers, informants often seemed to harbor sneaking suspicions that I was somehow hiding what was really at stake in my research that there were unacknowledged benefits to be had, provided that informants played along with my requests for seemingly esoteric information. The proliferation of community-based mapping exercises and other forms of rapid and participatory rural appraisal has meant that locals have become increasingly shrewd and skilled in shaping development agendas to suit their own purposes, hence, the performative nature of their involvement.

Sletto does a nice job of demonstrating that the mapping workshops constitute an important step in the "process of place making." He also reminds us, however, that the workshops themselves cannot fully realize the political goals of participants. One of the teenage mapmakers in Venezuela frames the issue succinctly: "So after this," he asks, "are they going to give us the land?" His friend replies vaguely: "No, first Bjørn is going to bring the maps to the United States." And then? Sletto's analysis makes clear that while the workshops may be a venue for the performance of identity and authenticity, the political goals and aspirations motivating such performances require second acts. It is not enough to make sure that the map is filled all the way to the "corners." If the broader aims of workshop participants are to be met, the territories represented on the map must be filled with acts of tree cutting and planting, cattle grazing, and fishing. In order for the visions and imagination reflected in the maps to gain political currency, then, they must be combined with deliberate political and legal strategies. These acts of map realization warrant further attention.

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Community-based mapping has been analyzed in its different political uses and as recognition of the collective territorial rights of indigenous peoples. In the past few decades, indigenous mapping has also been part of the reconfiguration of the national territories of Latin American countries that has allowed new ideas of territory, identities, rights, and concepts to emerge that are related to national and local management of symbolic and physical space (Ariza, Chavarro, and Vargas 1999; Offen 2006). Sletto's article is an important contribution to the discussion of participatory mapping, and he proposes new insights in the way it can be used. He points out that the process of making maps is related to inner expressions and cultural representations of the landscape related to dreams, fears, desires, and fantasies, which allow the construction of individual or collective identities. Sletto also argues that participatory mapping is related to performances of spatial appropriations that connect the past, present, and future and evoke cultural referents that go beyond the known territory. Finally, Sletto proposes participatory mapping as a generator and propagator of spatialities related to memory that has the potential of reinforcing emancipatory politics.

Sletto's perspective is very interesting. However, as he notes, maps not only are tools but also are related to symbolism and processes that most of the time make sense only for the people who are involved. Since the seventies, social cartography, as it is called in Latin America, has been used as part of participatory-action research, and it has been very important for indigenous peoples' demands related to their collective rights, but it does not appear in the genealogies of participatory mapping or in academic reports. I want to highlight the case of the so-called talking maps (mapas parlantes). These maps are big representations (murals) of Nasa people's territory in Cauca, Colombia. Six in total, they represent the historical transformations of specific places and the impact of colonial, republican, and modern state policies in the Nasa's territory. They also represent the resistance and the territorial politics of the Nasa people. These maps were made in the seventies by Victor Daniel Bonilla, the Nasa people, and intellectuals who were involved with indigenous resistance movements (Bonilla and Fidji 1986). The mapas parlantes have been used in collective processes of memory recovery because they enable the flow of memories, experiences, and processes that articulate territorial dimensions concerning physical, historical, socioeconomic, political, and cultural practices embodied in specific places. They also facilitate the comparative analysis of local transformations over time. Sletto's article goes in this direction but does not fully develop the role of representations in collective processes. Talking maps put into circulation thoughts, ideas, and historical processes that are tied to specific locations. These maps are still used in educational processes to analyze the current problems of the Nasa people's territorial management. A look at the role of these maps would be useful for the discussion presented by Sletto, and, similarly, it would allow him to trace the genealogy of participatory mapping in Latin America.

Another approach that Sletto proposes with maps is their relationship with art, which he unfortunately does not explore enough. I believe that the maps are linked to intricate aesthetic dimensions because the systems of representation—especially interwoven indigenous knowledges, practices, and concepts—establish a dialogue with their environment in specific moments and places. When indigenous peoples represent their territory, they participate in the construction of their world perspectives and ways of conceiving different notions of space. In this process, they overlay different images (sounds, graphics, movements, and tastes) that collectively feed and interact with local-global cultural representations. Alternative ways of representing the cultural and territorial space (e.g., the body as territory or basketry as articulator of memory processes) that are very common among indigenous peoples have to be explored in order to locate new notions of spatialities. How-
ever, Sletto takes up the conventions and forms of formal mapping (which considers a space of representation in two dimensions), through a process of teaching and training ethnocartographers, without discussing their cultural implications.

Nevertheless, Sletto rethinks social mapping by connecting space with identity and with the circulation of meaning that is generated when maps are constructed, as in the cases of Trinidad and the indigenous Pemon of Venezuela; this work integrates social mapping and reshapes the role of maps. The interesting thing about Sletto’s article is that he is not trying to argue the role of maps as a political strategy in itself (land recoveries, environmental management, resolution of territorial conflicts, or representing inequalities). On the contrary, he perceives participatory maps as generators of spatial dynamics, which may be a strategy of resistance, in the sense of allowing a group to rework their identities and experiences embedded in both individual and collective practices that inhabit specific places.

Reply

I much appreciate the thorough readings and thoughtful reflections by Hale, Middleton, Nygren, Rodriguez, Schroeder, and Ulloa, who have called for further “autoethnography” and discussions of the outcomes and consequences of the mapping projects—the “second acts,” to paraphrase Schroeder—both within the communities and from the perspective of broader relations of power in which these communities are situated. Perhaps the way to start is to consider Middleton’s question whether what is going on in these participatory-mapping workshops would be better understood as “positioning” rather than “performativity.” This question lies at the very heart of the argument being made here, that is, that it behooves practitioners of “participatory mapping” and other “social mappings” to reflect more critically on the social processes that inform and are constituted by such mapmaking processes. The concept of “performativity,” I would argue, more accurately captures the ways in which the interests of different community members are wrapped up in, even subsumed within, culturally and geographically contingent practices of storytelling. By contrast with the more instrumentalist concept of “positioning” (to achieve certain ends), “performativity” allows us to unpack the ways in which the expression of interests is beholden to different rights to speak of landscapes and histories, the ways in which this “speaking-of” is associated with the speaker’s embodiment in and of landscapes, and the ways in which this embodiment is entangled in complex, multidimensional ways of representing time and place, that is, what Ulloa refers to as the “body as territory or basketry as articulator of memory processes.” As I understand and use the term, “performativity,” coupled with the concept of maps as art, offers a productive metaphor for such process analysis, especially because I have argued (perhaps not convincingly) that process is also action (in the sense that art and speaking also are “action”). Hence, the process of participatory mapping and by itself leads to and is shaped by multiple strategies of resistance “in the sense of allowing a group to rework their identities and experiences embedded in both individual and collective practices that inhabit specific places” (Ulloa).

Of course, this does not adequately answer the question of what happened next, especially regarding the Pemon in Venezuela. Many of the complexities of meanings of landscapes, time, and identities were lost as these performances were reduced to two-dimensional cartographic products. The dilemmas I faced as I alternatively led and was pulled along by this reductionist process have been the focus of my essay. What I have not made clear are the numerous decisions I made every day as I was alternatively manipulated (Schroeder), cajoled, prompted, and otherwise influenced—not only by community members, it is important to note, but also by multiple actors in government, NGOs, and the Venezuelan and U.S. academies. As Rodriguez observes, critical cartographers may consider participatory mapping more seriously as a tool for internal deliberation and the clarification of local views.

Especially significant, as Hale points out, was the balancing act I followed to meet my activist commitment to the community while still pursuing what he refers to as my “internal critique.” Perhaps the best (but nonetheless unsatisfactory) comment I can make here is that I came to believe that critical reflection of process, baring the complex articulations between shifting meanings of place, time, and indigeneity that characterize the “essential modernity of indigenous people advocating for traditional lands and recognition” (Middleton), would take us away from the trap of essentializing indigenous peoples through our representations of indigenous cartographies. The more my Pemon colleagues and confidants themselves reflected openly on their complex productions of identities and meanings of place and time, the more apparent it became to me that the mapping process was informed by a broader struggle, that their agency was exercised in multiple ways—not merely through the mapping project—and that this agency deserved further analysis.

Not that this observation obviates further discussion about the consequences and outcomes of the mapping project—as Hale remarks, “if, for example, women or radicalized youth have a lesser voice in the production of the countermap, does it follow that such inequities will be reinforced as that map is put to use?” (Hale). Middleton similarly asks how the training of the “ethnocartographers” might have changed power relations in the communities. Such questions are indeed essential for critical geographers; given the limited space here, another short and limited answer is that for the Pemon, the mapping project was, as I alluded to above, one of multiple, ongoing, more or less consequential engagements with Venezuelan modernity, Western technologies, and non-Pemon...
authorities, and that as such, the mapping process represented one of many ways in which they could articulate their claims to land and rights to cultural particularity within the Venezuelan state formation. These are communities defined in part by their complex appropriations of modernity, and as such, the social processes surrounding the mapping project became part and parcel of these ongoing social changes.

So did the "Pemon performers themselves sincerely [feel] that the participatory-mapping workshops provided them with chances for actions 'intended to change the world'" (Nygren). There is no yes-or-no answer to this question, of course, but Nygren’s observation points to the need for critical, participatory cartographers to combine such projects with deliberate political and legal strategies, to use Schroeder’s words. As I discuss in the article, the mapping project began shortly after the passage of the new Venezuelan constitution, which explicitly and for the first time provided for indigenous collective land rights, and the mapping methodology was designed to meet the requirements and language of the subsequent law of indigenous territorial demarcation. Of course, some of the optimism we felt during the project has now waned; since the mapping project concluded in 2004, Pemon leaders have been stymied in their attempts to claim their land rights because of the inconsistent and shifting requirements for land legalization emerging from the national commission for indigenous land demarcation.

At the risk of glib oversimplification, perhaps this delay in securing land rights ultimately underscores the importance of carefully designing and critiquing process in order to ensure that such mapping projects contribute to ongoing struggles, whether or not immediate political gains are achieved. In the case of the Gran Sabana, the ethnocartographers and other community members closely involved with the mapping project have continued, in a variety of ways, to work on map-making in Pemon communities and to press their claims with the demarcation commission, drawing on both the technical and the deliberative lessons learned from the mapping project. Terms such as "capacity building" and "empowerment" do not fit easily in the vocabulary of critical cartographers, but by all appearances, action in the form of performances of place, time, and identity are continuing in the Gran Sabana, drawing on the agency that shaped the mapping project from the beginning but also informed by new representational tools and strategies introduced via the mapping project.

—Bjørn Ingmann Sletto

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...one cannot “unsettle” the “coloniality of power” without a redescription of the human outside the terms of our present descriptive statement of the human, Man. 
Sylvia Wynter
MORE-THAN-HUMAN
PARTICIPATORY RESEARCH

Tehseen Noorani and Julian Brigstocke
Today we are increasingly seeing calls for universities to collaborate with communities in designing and conducting research. While such calls are to be welcomed they tend to suffer from a historical blind-spot that ignores the fact that research collaboration – partnerships, participation (call it what you will) – is a deep and powerful research tradition that dates back beyond the recent emergence of calls for ‘co-produced’ knowledge.

This series of reviews developed as part of the AHRC’s Connected Communities Programme, sets out to make visible some of these traditions of collaborative research. In doing so, the series aims to:

— help those who are new to the field to understand the huge wealth of history and resources that they might draw upon when beginning their own research collaborations;

— help those who seek to fund and promote collaborative research to understand the philosophical and political underpinnings of different traditions; and

— support those working in these traditions to identify points of commonality and difference in their methods and philosophies as a basis for strengthening the practice of collaborative research as a whole.

The eight reviews in the series were developed to provide eight very different ‘takes’ on the histories of collaborative research practices in the arts, humanities and social sciences. They do not pretend to be exhaustive, but to provide a personal perspective from the authors on the traditions that they are working within. As we worked together as a group to develop these, however, a number of commonalities emerged:

1. A critique of the mission-creep of scientific knowledge practices into the social sciences and humanities, and of the claims to produce universally valid forms of knowledge from specific limited institutional, cultural and social positions.

2. A commitment to creating research practices that enable diverse experiences of life and diverse knowledge traditions to be voiced and heard.

3. A resistance to seeing research methods as simply a technocratic matter; recognising instead that choices about how, where and with whom knowledge is created presuppose particular theories of reality, of power and of knowledge.

4. A commitment to grapple with questions of power, expertise and quality and to resist the idea that ‘anything goes’ in collaborative research and practice. There are better and worse ways of developing participation in research practice, there are conditions and constraints that make collaboration at times unethical.

At the same time, a set of names and events recur throughout the reviews. John Dewey, Paolo Freire, Raymond Williams, Donna Haraway appear as theorists and practitioners who provide powerful philosophical resources for thinking with. Critical incidents and moments reappear across the reviews: the rise of anti-colonial movements in the 1950s and 1960s, of second wave feminism and critical race theory in the 1960s and 1970s; of disability rights movements in the 1970s and 1980s; of post-human and ecological analyses in the 1990s and 2000s. Read as a whole, these reviews demonstrate the intellectual coherence and vibrancy of these many-threaded and interwoven histories of engaged scholarship and scholarly social action.

The first of the reviews, by Kevin Myers and Ian Grosvenor, discusses the long tradition of ‘history from below’ as a collaborative enterprise between researchers, archivists, curators, teachers, enthusiasts, local historians, archaeologists and researchers. They discuss the emergence of the ‘professional historian’ alongside the rise of the nation state, and the way in which this idea was challenged and deepened by the emergence of activist histories in the mid-20th century. They investigate the precedents set by the rise of groups such as the History Workshop movement and trace their legacies through a set of case studies that explore feminist histories of Birmingham, disabled people’s histories of the First World War and the critique of white histories of conflict emerging from the work of black historians and communities.
Two of the reviews explore currents within participatory and critical research traditions. Niamh Moore explores these traditions through the lens of feminist philosophies and methodologies, while Tom Wakeford and Javier Sanchez Rodriguez explore the history of participatory action research (PAR) and its ties to social movements outside the academy.

Niamh Moore’s review highlights the strategic contributions made to participatory research through the traditions of feminist and indigenous methodologies. Drawing on Donna Haraway’s metaphor of the cat’s cradle, Moore explores the way that these different traditions have learned from each other, fed into each other and been in (productive) tensions over the years. Importantly, she makes visible the common threads of these traditions, including a concern with questions of power, matters of voice, agency and empowerment and reflexivity. She identifies examples that include: popular epidemiology and women’s health, the controversies and emerging insights arising from the publication of the book ‘I Rigoberta Menchú’ (a collaboration between Rigoberta Menchu, a Guatemalan activist and Peace Prize winner and anthropologist Elisabeth Burgos-Debray), and the online Mukurtu platform for sharing and curating community stories.

Wakeford and Sanchez Rodriguez’s review is written from the position of individuals who situate themselves as both activists and academics. From a perspective both inside and outside the academy, they make visible the traditions of participatory action research that have evolved in social movements and their interaction with academic knowledge. They explain how PAR emerged as a practice that seeks to intervene and act on the world through disrupting assumptions about who has knowledge, and by building intercultural dialogue between those whose interests have historically been marginalised and those experts and institutions in dominant positions. They discuss the contributions of Paolo Freire and Orlando Fals Borda, as well as the emergence within universities of centres for Action Research and indigenist approaches to research before exploring recent examples of PAR from the Highlander Folk School in the US, to the Cumbrian Hill Farmers post Chernobyl, to questions of Food Sovereignty in India (amongst others).

Central to many attempts to build collaborative research practices is a turn towards the arts and arts methodologies as a means of engaging with different forms of knowledge. Such a turn, however, can often overlook the distinctive and sustained tradition within contemporary arts of reflecting upon the question of how publics can come to participate in arts practices. Our series therefore includes two reflections on this question from different perspectives.

First, Anne Douglas’ review offers a ‘poetics of participation in contemporary arts’, locating the turn to participation in contemporary arts within a wider history of 20th and 21st century arts and politics. She highlights the huge range of work by artists and arts co-operatives who are seeking to make work through participatory forms, and the deep scholarly tensions and debates that surround these practices. She explores through this rich history the debates over whether participation has become instrumentalised; whether the art/life divide should be preserved or eroded; the links between participatory aesthetics and cybernetic ethics; and the capacity for participation to challenge alienation and neoliberalism. Recognising arts practice as itself a form of research and inquiry into the world, she concludes with a set of powerful reflections on the role of the freedom to improvise and the importance of participation as a moment of care for and empathy with the other.

Second, Steve Pool, community artist and academic, reflects on the related but different traditions of community arts as they might relate to social science research. He considers what researchers in the social sciences might need to know and understand about artistic traditions if they desire to mobilise arts practice within the social sciences. He discusses the increasing democratisation of tools for making, the potential for the further opening of artistic practice to publics as well as the importance of recognising that such practices are part of wider traditions and philosophies about the value and purpose of art. In particular, he discusses the tension between the idea of artistic autonomy – art for art’s sake – and artistic democracy – the democratic creativity of all individuals. He foregrounds the way in which the community arts movement was also allied to a wider politics that moved towards cultural democracy and explores the contemporary practice of artists working in and with social science through examples such as Nicola Atkinson’s ‘Odd Numbers’ and the Community Arts Zone’s ‘Being Cindy Sherman’.

More recent traditions of collaborative research characterise our final three reviews which take on, respectively, the way that design theory and practice are playing an important role in reshaping society, products and services; the emergence of new technologies to facilitate new forms of collaboration; and the increasingly urgent injunction to develop research approaches that enable collaboration with the ‘more-than-human’ others with whom we share the planet.
Theodore Zamenopoulos and Katerina Alexiou discuss the field of co-design and its underpinning theories and methods. They argue that Design as a process is always concerned with addressing a challenge or opportunity to create a better future reality, and explore how co-design has evolved as a process of ensuring that those with the life experiences, expertise and knowledge are actively involved in these making new tools, products and services. They observe how the participatory turn in this field has been concerned with both changing the objects of design—whether this is services or objects—and with the changing processes of designing itself. They highlight four major traditions and their distinctive approaches, before exploring the politics and practices of co-design through case studies of work.

Chiara Bonacchi explores how the internet is enabling new forms of collaborative knowledgew production at a massive scale. She locates this discussion in the traditions of citizen science and public humanities, and examines how these have been reshaped through the development of hacker communities, open innovation and crowd-sourcing. In this process, she discusses the new exclusions and opportunities that are emerging through the development of projects that mobilise mass contribution. She examines the cases of MicroPasts and TrowelBlazers that demonstrate how these methods are being used in the humanities. In particular, she explores the ethical questions that emerge in these online collaborative spaces and the need for a values-based approach to their design.

Tehseen Noorani and Julian Brigstocke conclude the series with an exploration of the practice and philosophy of 'more-than-human research' which seeks to build collaborative research with non-human/more-than-human others. They discuss its philosophical foundations in pragmatism, ecofeminism and indigenous knowledge traditions and identify some of the theoretical and practical challenges that are raised when researchers from humanist traditions begin to explore how to ‘give voice’ to non-human others. In the review, they consider how researchers might expand their ‘repertoires of listening’ and address the ethical challenges of such research. To ground their analysis, they discuss the work of the Listening to Voices Project as well as accounts of researcher-animal partnerships and projects that draw on Mayan cosmology as a means of working with sustainable forestry in Guatemala.

This collection of reviews is far from exhaustive. There are other histories of collaborative research that are under-written here—there is much more to be said (as we discuss elsewhere) on the relationship between race and the academic production of knowledge. Each of these accounts is also personal, navigating a distinctive voiced route through the particular history they are narrating. Despite this, at a time when politics is polarising into a binary choice between ‘expert knowledge’ and ‘populism’, these reviews show, collectively, that another way is possible. They demonstrate that sustained collaborative research partnerships between publics, community researchers, civil society, universities and artists are not only possible, but that they can and do produce knowledge, experiences and insights that are both intellectually robust and socially powerful.

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1. INTRODUCTION

At a time of global warming, ecological destruction and mass species extinction, when the texture of everyday life is becoming increasingly mediated by technology, researchers are asking how humans might enter into less violent, destructive and alienating relationships with non-humans such as animals, plants, the earth, spirits, technologies and objects. The humanist ideal of an autonomous, rational, bounded human self is increasingly regarded as a fantasy. According to ‘more-than-human’ and ‘post-humanist’ research paradigms, human life is constituted through a riot of non-human forces, from the microbes in our guts, to the animals, plants and fungi that we live symbiotically with, to the objects that we care for and covet, to the gods and spirits that we summon and which bind us to others. These research paradigms have offered an alternative, ecological picture of social worlds, one in which humans are always constituted through diverse webs of non-human life. Gargantuan inequalities in economic wealth between the richest and poorest people, and a surge in decolonising webs of non-human life. Gargantuan inequalities in economic wealth between the richest and poorest people, and a surge in decolonising webs of non-human life. Gargantuan inequalities in economic wealth between the richest and poorest people, and a surge in decolonising webs of non-human life. Gargantuan inequalities in economic wealth between the richest and poorest people, and a surge in decolonising webs of non-human life. Gargantuan inequalities in economic wealth between the richest and poorest people, and a surge in decolonising webs of non-human life. Gargantuan inequalities in economic wealth between the richest and poorest people, and a surge in decolonising webs of non-human life. Gargantuan inequalities in economic wealth between the richest and poorest people, and a surge in decolonising webs of non-human life. Gargantuan inequalities in economic wealth between the richest and poorest people, and a surge in decolonising webs of non-human life. Gargantuan inequalities in economic wealth between the richest and poorest people, and a surge in decolonising webs of non-human life. Gargantuan inequalities in economic wealth between the richest and poorest people, and a surge in decolonising webs of non-human life. Gargantuan inequalities in economic wealth between the richest and poorest people, and a surge in decolonising webs of non-human life. Gargantuan inequalities in economic wealth between the richest and poorest people, and a surge in decolonising webs of non-human life. Gargantuan inequalities in economic wealth between the richest and poorest people, and a surge in decolonising webs of non-human life. Gargantuan inequalities in economic wealth between the richest and poorest people, and a surge in decolonising webs of non-human life. Gargantuan in
2. HISTORICAL ROOTS OF MORE-THAN-HUMAN RESEARCH

Although the field of non-human participatory research is relatively recent, it draws on diverse traditions that are united in their commitment to challenging Enlightenment ideas of the human, as well as to critiquing humans’ mastery and exploitation of nature. Although there are many different kinds of history we could tell in order to convey something of the intellectual and ethical debts of more-than-human research, here we will focus on the legacies of biopolitical, pragmatist, ecofeminist and decolonial thought. We write self-consciously from our positions as professional academics within the Western university sector—a sector that works within a context of patriarchal, white and middle-class dominance. We have selected the order below to trace the history of the Western academy’s engagement with various forms of more-than-human theorising, rather than a history of when these various forms of theorising emerged.

2.1 Biopolitics and the emergence of ecological understandings of the social

Michel Foucault has traced the emergence in Western thought from the 18th century of a growing awareness of, and interest in governing, the life processes of entire human populations (and connecting these to the life processes of individual bodies). Foucault refers to this as the ‘biopolitical’ constitution of modernity. In fields as varied as statistics, biology, medicine, engineering and economics, there was a growing awareness of the importance of environment and ‘milieu’ in determining the possibilities of human society. Increasingly, power became focused on improving society’s health, vitality and strength. Visions of a society as an organism as an organism became widespread. This contributed to powerful forms of racism that judged some races to be healthy, energetic and advancing, while other races were considered degenerate, sickly and a threat to the health of the species as a whole.

This environmental sensibility travelled across fields and disciplines. In economics, there was a growing awareness that economic life could be subtly manipulated by tweaking environmental variables such as interest rates. Modifying the economic ‘climate’ through subtle adjustments of multiple variables (interest rates, tax thresholds, import duties, etc.) became an important way of controlling human populations without having to limit individual freedoms. Across many spheres of government, a growing awareness emerged of how environments affect human behaviour and determine the healthy vitality (or weak degeneration) of society. These ‘biopolitical’ rationalities of governing generated new forms of racism, power and control—particularly through the control of sexuality—but also lay behind resistance and welfare

Foucault’s account of different ways of thinking about the relation between environments and society, and the importance of rationalities and experiences of life, growth and vitality in modernity, set an agenda for an important, ongoing scholarly effort to re-imagine the concept of life and the different forms of liveliness that animate human societies. His central challenge, which continues to animate more-than-human research, is for us to recognise that what counts as life or non-life, and what value we give to different kinds of life, should be considered a fundamental political question of modern times.

2.2 Pragmatism: knowledge, environment and democracy

In the early 20th century, this interest in humans as embodied, environmentally sensitive beings amongst European intellectuals led to some radical ways of rethinking the nature of the human. The philosopher Friedrich Nietzsche argued for a fundamental overturning of the category of the human, requiring a new morality based on life, vitality and creativity, rather than a life-denying Christian morality of good, evil and endlessly deferred pleasure. Meanwhile, the philosophy of the American pragmatist John Dewey developed an environmental, ‘naturalistic’ theory of knowledge, experience and politics, starting from an account of the development of knowledge as an adaptive human response to external conditions that is aimed at an active restructuring of those conditions. Experience itself arises from an interaction between organism and environment: ‘experience’, he wrote, ‘is heightened vitality… it signifies active and alert commerce with the world; at its height it signifies complete interpenetration of self and the world of objects and events’

This concept of experience also enabled a theorisation of the arts as vital in contributing to an awareness of the tensions between humans and their environment, as well as the resolution of those tensions. For Dewey, art has the capacity to bring to consciousness ‘an experience that is unified and total’. Moving beyond Dewey’s own thinking, we might add that such an experience of interpenetrated self and world is necessarily a more-than-human experience.

Dewey’s thought has had a profound influence on contemporary understandings of participatory research and democracy. Dewey insisted upon the importance of discussion, consultation, persuasion and debate in the enactment of democratic life. He argued that democracy as a public realm involves dealing with conflicts of interest because it is an experimental mode of enquiry through which we can develop a new conception of what our interests are. Central to this view of democratic life was an influential conception of ‘publics’. Against the conventional, abstract notions of democracy as being carried out in an ideal public sphere, Dewey insisted that publics emerge through distinct socio-material entanglements. He argued that in technologically complex societies, in which innovation and change is the norm, the nature of what exactly makes up, holds together and animates a public is precisely the issue that is at stake. Noortje Marres takes this one step further to argue that publics are more-than-human, socio-technical constructions.

5 Foucault 2007.
6 Mears 2012.
7 Blencowe 2012.
9 Dewey (1931) 2009: 19.
10 Ibid: 15.
11 Mears 2012.
12 CONNECTED COMMUNITIES | Foundation Series
13 More-than-human participatory research
Unlike much humanist participatory research, more-than-human research insists on the link between Dewey’s conception of publics, and his ecological way of thinking that always situated knowledge and experience in the context of the interaction between bodies and their environment. Dewey himself remained within a fairly conventional assumption about the differences between human and non-human collectives. A public, Dewey argued, is grounded in the capacity of humans to observe and reflect upon the unintended consequences of collective actions. For Dewey, only humans are capable of transforming an incoherent collective into a self-conscious, reflective public. So whilst Dewey’s thought has had a powerful role in traditions of more-than-human participatory research—particularly in its ecological theory of knowledge and experience, and his recognition of the role of more-than-humans in the composition of publics—his thought does not go far enough in recognising the vital role of more-than-human actors in the constitution of democratic publics.

2.3 Ecofeminism

One of the most powerful traditions of Western thought is the one that associates men with culture and reason, and women with nature, embodiment and emotion. This identification of women and nature has been the cornerstone of Western patriarchy, justifying the idea that men’s place is in the public sphere of reasoned debate, and women’s place is in the private sphere of reproduction and domesticity. It is unsurprising, therefore, that traditions of feminist thought have offered the most important and innovative insights about the relationship between humans and non-humans, and it is feminist geographers, anthropologists, and philosophers who in recent years have produced some of the most compelling insights into more-than-human research. During the 1980s, with foundational texts such as Merchant’s The Death of Nature, a body of ‘eco-feminist’ thought explicitly brought together feminist and ecological politics and emphasized the radical interconnectivity of humans, animals, spirits and the earth. As a political movement, ecofeminism always stressed that its spiritual and cultural dimensions were inseparable from its political actions. It became associated with pagan religious traditions, aiming to develop ways of thinking and experiencing that were based on embodied, intuitive experience. While many researchers worried about ecofeminists’ acceptance of the idea of an intrinsic connection between women and nature, some traditions of ecofeminism have developed ways of thinking about the relationship between feminism, nature, science and technology, and spirituality. These new approaches draw on and extend many of the most important insights of ecofeminism, whilst fully embracing the ‘artificial’, hybrid and technological aspects of the more-than-human. The most famous statement of this departure from ecofeminism is Haraway’s remark in Her Manifesto for Cyborgs: ‘I’d rather be a Cyborg than a Goddess’.

2.4 Decolonizing and indigenous research

It is important to fully recognize that whilst more-than-human research methodologies currently appear new in the canon of Western academic scholarship, there are long, rich histories and traditions of knowledge about non-human relations, which have been subject to important critiques that have helped shape the current landscape of more-than-human research. For example, many researchers worried about ecofeminists’ acceptance of the idea of an intrinsic connection between women and nature. Relatedly, one might find it interesting to think of Kingdon’s notion of ‘natural’ as serving the degraded, ‘instrumental’ rationality of patriarchal, capitalist domination. For example, as we will explore in the next section, the work of writers such as Donna Haraway and Isabelle Stengers has offered new ways of thinking about the relationship between feminism, nature, science and technology, and spirituality. These new approaches draw on and extend many of the most important insights of ecofeminism, whilst fully embracing the ‘artificial’, hybrid and technological aspects of more-than-human worlds. The most famous statement of this departure from ecofeminism is Haraway’s remark in Her Manifesto for Cyborgs: ‘I’d rather be a Cyborg than a Goddess’.
These histories of thought show that academic researchers can learn a lot from indigenous knowledges. Historically, it is well established that much anthropological research concerning indigenous peoples participated in, and justified, colonial violence. It has also been criticized for being ‘extractive’: appropriating the knowledges and experiences of indigenous peoples to further academic careers, rather than to benefit the research participants themselves. However, some research has also engaged with indigenous knowledges in more collaborative and respectful ways that often draw on shared activist and participatory research projects. Such work recognizes the imperative to avoid either appropriating or ‘stealing’ these knowledges, on the one hand, or denying the usefulness of indigenous knowledges for contemporary global ecological problems, on the other. Similarly, it is important not to assume that indigenous peoples have a pure, authentic, unmediated or uncompromised relationship with the natural world. An important series of anthropological works such as Marisol de la Cadena’s Earth Beings, Elizabeth Povinelli’s The Cunning of Recognition and Viveiros de Castro’s Cannibal Metaphysics show how indigenous practices interact in complex and often violent ways with Western rationalities and systems of power. Such research helps illuminate, and seek ways of moving beyond, the structures of reason in Western traditions of thought.

For example, Deborah Bird Rose, working with the Yarralin people in the Northern Territories of Australia, has shown how Indigenous views of human identity create the foundations for an ethos of ecological respect, restraint and recognition, which has much to teach dominant cultures. Rose shows how, in contrast to the future-oriented rationalities of the West, which frame the past as having already finished, Yarralin society orients itself towards origins. The past – the ‘Dreaming’ – is not finished, but continues in all living bodies whose origins are in the Dreaming, through ceremony, creation and music. Memory, place, dead bodies and genealogies hold stories that are painful but also constitute relationships of moral responsibility. This way of experiencing time makes possible a way of relating to death that is less alienating and more sustainable than Western rationalities that desire to ‘overcome’ death or hold it at bay for as long as possible. Death is part of life, a return to the land that nurtures life. This vision of death, Rose argues, enables a way of thinking about the land as a ‘nourishing terrain’, and of death as a nurturing, material continuity with ecological others.

Academic researchers in the field of more-than-human research have much to learn from decolonizing traditions of research on the one hand, and indigenous worldviews on the other. Contrary to extracting methodologies, concepts, or theories, this entails joining forces with decolonizing and indigenous ethics of care and responsibility, sharing intellectual and political commitments and developing modes of ‘border thinking’ that escape the dominant forms of rationality of Western reason.

3. CONCEPTUAL ORIENTATIONS IN ACADEMIC MORE-THAN-HUMAN RESEARCH

Although research engaging with more-than-human worlds is very diverse, we identify three broad conceptual approaches that have emerged in the Western academy, which place emphases, respectively, on: (1) socio-technical relations; (2) experience beyond the human; (3) more-than-human communication. Because they are attempting to overturn the whole tradition of Western thought that makes European man the measure of all things, these approaches can seem counterintuitive. Each has a rich and often complex conceptual architecture. In what follows, we will briefly analyse key points from each approach.

3.1 Socio-technical relations

In recent decades, otherwise divergent theoretical paradigms including Structuralism, Post-Structuralism and Actor Network Theory have outlined radically relational views of the world. According to this viewpoint, everything (whether human or non-human) is created through, and made meaningful by, its relations with other things. There are many ways of interpreting this idea. However, it is potentially radical because it enables us to reject at least two central assumptions of Enlightenment thought. First, it rejects the idea that relations between humans are in any way more ‘real’ or meaningful than relations between humans and non-humans (and between non-humans and other non-humans). Second, it rejects the idea that the human ‘self’ is autonomous, bounded and self-contained. In fact, the self is merely a complex bundle of relations, not intrinsically different to any other bundle of relations. The ‘human’ therefore no longer exists on a different plane of social reality to the non-human. Rather, this relational view of the world articulates an entirely ‘flat’ view of what makes up the world – sometimes referred to as a ‘flat ontology’.

Perhaps the most well-known relational theory comes from Actor Network Theory (ANT), associated with writers including Bruno Latour, Michel de Certeau, Pierre Bourdieu, Gilles Deleuze and Bruno Latour. ANT views the social as being constructed through creative associations between varied human and more-than-human agents. More of a ‘sensibility’ or way of seeing the world than a theory, ANT brings certain characteristics of the world into view. First, it highlights the constitutive role of non-humans in social life. Second, it avoids seeing agency as an essential capacity that some kinds of entity (like humans) possess and others (like stones or clouds) don’t, but identifies agency as being an outcome of the relations between all kinds of different social and material entities. It is these ‘actor networks’, not subjects or objects in isolation, that get things done.
One crucial point that researchers of more-than-human worlds could take from ANT is that ‘agency’ – the capacity to act and to be responsible for those actions – is not something that only belongs to humans. Agency isn’t concentrated in a single human body, but is a relational, distributed, more-than-human achievement. This leaves open, however, a series of questions, the most important of which relate to power and responsibility. How can this ‘flat’ conception of social worlds account for the unevenness of power relations? What scope is there for making normative distinctions between ‘better’ and ‘worse’ networks? What happens to our notions of responsibility and accountability when agency is distributed so widely? These are questions that researchers working within this tradition are still working through today.

In their influential book *Acting in an Uncertain World: An Essay on Technical Democracy*, Callon, Lascoumes and Barthe develop an approach to participation that – drawing on ANT’s conceptualisation of the social as a dynamic, fragile and heterogeneous assemblage of various human and non-human agencies – takes ‘controversy’ as its primary mode of explanation. Controversies, they write, create *overflows* that are at once technical and social. Controversies help to reveal hidden events and processes by bringing forward groups that are involved with the overflows. Socio-technical controversies are important spaces of learning, making it possible to overcome the gap separating laypersons and specialists, and also between ordinary citizens and their representatives. They conceptualise this potential of controversy through the notion of a ‘hybrid forum’ which brings together multiple actors into a mutual space of exploration, learning and construction, and which scrambles distinctions between experts and laypersons, and the power asymmetries that these distinctions entail. In a similar vein to the concept of controversy generating a hybrid forum, we could evoke Callon’s ‘hot situations’, Latour’s ‘matters of concern’ or Stengers’ ‘things that force thought’, to name the moments of disturbance in which the unexamined, situations’, Latour’s ‘matters of concern’ or Stengers’ ‘things that force thought’, to name the moments of disturbance in which the unexamined, material fabric of everyday life starts to deform and reform itself. Such situations, matters or forces make expert knowledge claims the subject of intense political interrogation.

Another way of thinking about the relational nature of the more-than-human world comes from the feminist philosopher Donna Haraway. In her *Cyborg Manifesto*, which develops a socialist-feminist account of women under advanced capitalism, Haraway theorises a notion of the ‘cyborg’ as a figure that rejects any rigid boundaries separating humans from animals and machines. We are all cyborgs, in the sense that we are all made up of a multitude of human and non-human forces. The clothes we wear, the technologies we use, the emotional relations that attach us to others, the bacteria in our gut – all these are not at all external to our identity but form an essential part of it. All humans are hybrid, monstrous, cyborg, more-than-human beings that share kinship with many non-human beings. Crucially, the cyborg does not aspire to unify all its parts into an organic whole. Rather, the cyborg ‘is not afraid of joint kinship with animals and machines... of permanently partial identities and contradictory standpoints.’ The cyborg forms close bonds of love, care and respect across the boundaries separating ‘self’ from ‘other’.

How might these ideas help us think about participatory more-than-human research? Haraway’s book *When Species Meet*, which addresses the interactions and mutual dependency between humans and other species, offers some useful pointers here. In a moving account of human-dog training in agility sport, Haraway shows how the intense training required for the sport creates a ‘contact zone’ in which human and dog are forced to confront important philosophical questions. ‘Who are you, and so who are we? Here we are, and so what are we to become?’ Although some people might think of training as a process where the human acts to make the dog fully obedient and do whatever he/she commands, Haraway shows how training involves plural relations of mutual trust, respect and authority between dog and human. The human trainer has to learn to trust the dog and to recognise and respond to the authority of the dog’s performance. There is much that more-than-human researchers can learn from this insight. When researching with non-human ‘others’, human researchers have an ethical responsibility to avoid treating non-human research participants as passive objects. Instead, human researchers can look for ways of recognising and responding to the authority of the non-human participants, and of entering into shared, playful spaces of interspecies co-becoming and care.

### 3.2 Experience beyond the human

Another tradition of thought seeks to expand the place of experience outwards, looking to understand experience from a more-than-human perspective. When we fully recognise that social worlds are always more-than-human, the seemingly self-evident concept of ‘experience’ – a foundational starting point for most research methodologies – becomes much more complicated. This is because when we think of experience, we almost automatically tie it to our own senses of self. When I consider animal sentience, for example – it is very hard not to turn this into a question of whether animals feel and think like I do. But why shouldn’t the mouse have feelings and awareness in ways that are not like mine? Part of the problem here is that we almost automatically tie it to our own senses of self. When I consider animal sentience, for example – it is very hard not to turn this into a question of whether animals feel and think like I do. But why shouldn’t the mouse have feelings and awareness in ways that are not like mine? Part of the problem here is that we feel as if we ‘own’ our experiences, and as if these experiences are somehow private and inaccessible to others. Once we recognise that humans come into existence through their relationships with human and non-human others, we can come to an expanded way of thinking about experience that does not tie it exclusively to the interior of a bounded human subject. We may consider that experiences are not ‘owned’ exclusively by a stable, self-contained subject. We might even have to think of experience without a subject altogether.

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34. Latour 2005.
These ideas of non-human or non-subjective experience can seem very counter-intuitive. However, writing in this area of thought, such as from the orientations of ‘non-representational theories’ or ‘post-phenomenology’, insists that producing a genuinely more-than-human knowledge of the world requires us to face these propositions head-on. Rather than attempting to describe experience directly (which was the goal of the philosophical tradition known as phenomenology), post-phenomenology concentrates on how experience is mediated by more-than-human relations. One strand of this work, associated with Don Ihde, has focused on technoscience. Other strands, perhaps more useful for thinking about participatory more-than-human research, have emerged from areas such as cultural geography, drawing on contemporary European philosophy. These focus on the ways in which subjects come into existence through experience, rather than existing prior to experience. Self and world emerge together through their co-constitutive being together. This leads to an impulse to understand the autonomous existence of non-human objects, outside of the ways in which they appear to, or are utilised by, people. It requires attempting to get at the ‘otherness’ of non-human experience and consciousness, rather than assimilating these to human frames of reference. This is sometimes described as a methodology of ‘attunement’: a methodology where the researcher looks for ways of sensitising their bodily responses to non-human registers of experience and inhabiting the contact zones of multi-species experiences. The philosophers Deleuze and Guattari referred to this through the notion of ‘becoming animal’.

Methodologically, this tradition of thought demands creative and speculative practice, since its goal is a contradictory one – understanding non-human experience, and accounting for it through human practices (for example, writing), without assimilating it to human modes of thought. For this reason, it demands creative and speculative research practices that thrive on apparent contradictions, rather than denouncing them as meaningless or futile. Recent philosophical schools such as ‘speculative realism’, ‘object-oriented ontology’ and ‘new materialism’ have taken these speculative thoughts in exciting directions. As illustrated through case studies in the next section, when explored through creative, politically engaged research methodologies, the resulting journeys of thought and experience can be revealing.

3.3 More-than-human communication

Another key conceptual approach is found in ‘multi-species studies’. Such work aims to produce rich, detailed, ‘thick’ descriptions of the distinctive experiential worlds, modes of being and social and cultural attachments of other species. This often involves forms of ethnography with indigenous people who already recognise the world to be made up of more-than-human, multi-species communities. Anthropologists who have spent time with people who have kinship with non-humans (animals, plants, rivers, mountains, land, spirits, and more) have attempted to analyse the forms of sociability that are embedded within a more-than-human world. This work is heavily influenced by the anthropological perspectives of writers such as Eduardo Viveiros de Castro and Philippe Descola, whose work with Amerindian peoples from lowland South America takes seriously indigenous and other non-Western cosmologies which attribute selfhood not just to humans, but to diverse non-human others.

Viveiros de Castro, in an influential article on ‘Amerindian perspectivism’, has documented Amerindian ways of seeing the world which escape conventional nature/culture distinctions. In these cosmologies, animals and spirits are understood to view themselves and act in the world in the same way as humans do – from the perspective of a jaguar, the jaguar is a self with an experiential world and a cultural life involving hunting, kinship, home and a heterogeneous distribution of cares and preocupations. Perspectivism may be rendered consistent with the scientific search for objective knowledge insofar as the phenomenal world of each self is delimited by objectively-ascertainable capacities of perception, affection and so forth. Nevertheless, it invites a radical shift in orientation to the more-than-human, offering a ‘perspectival multinaturalism’ that inverts the standard [ Eurocentric ] formulation of ‘multiculturalism’: instead of one (material) nature and many cultures, non-humans such as animals and spirits are understood as differing costumes hiding culturally-similar interiors – selves with phenomenal worlds similar to ours.

One of the most important ways in which Western intellectual traditions have described humans as unique and fundamentally different from other forms of being through the human capacity for language and communication. Therefore, undoing the idea that humans are totally set apart from non-humans requires theorising further how to cross the boundaries between human and non-human communication. Amidst a broader turn to the interdisciplinary study of biolinguistics and biosemiotics, Eduardo Kohn’s book How Forests Think has revitalized multi-species ethnography. As Kohn argues, within the cosmology of perspectival multinaturalism, trans-species communication is possible through boundless-human intersubjectivity and communication through the self of another species. In Amerindian cultures, shamans provide the figure of the boundary crosser; psychedelic plants and dreaming enable crossings. Kohn’s analytical entry point into human-non-human communication draws on the semiotics of the Amazonian peoples, Charles Peirce, which distinguishes between several forms of representation. Symbols are one form of sign, gaining meaning purely through human convention. However non-symbolic signs (for example: ‘icons’ such as the colouration of [izard’s skin representing its backlit connection, ‘indices’ such as tracks indicating the presence of animals) are also available to non-humans. If we follow Kohn and Peirce in recognising that language exceeds symbolic communication, we can document how non-human actors participate in abundant and lively language systems. Kohn thus proposes an ‘anthropology of life’, which embeds humans within webs of more-than-human lifeworlds replete with symbolic, iconic and indexical languages.

Undoing the idea that humans are totally set apart from non-humans requires theorising how to cross the boundaries between human and non-human communication.
4. RESEARCHING MORE-THAN-HUMAN WORLDS

In this section we present a number of examples of more-than-human participatory research. The case studies are delimited by our own experiences and gaze, which in turn are heavily shaped by the English-speaking academy. Nonetheless, they do showcase a variety of ways in which researchers have taken up and worked with the concepts and historical traditions described above.

4.1 The fruits of ‘giving voice’

A key challenge to all attempts to enrol non-humans in participatory research is the idea that participation requires having a voice, and as non-humans are incapable of speaking they cannot therefore participate. From this perspective any attempt to ‘give voice’ to non-humans could be characterised as anthropocentric fictionalising – at best, producing an empathy that reveals something about ourselves, while at worst, legitimizing modes of domination over others whom we characterize on our own terms. We may always anthropomorphise when we give voice to non-human others. Additionally, giving voice remains fraught even in a strictly human context, from legacies of disenfranchisement of slaves and women, to continuing to speak on behalf of the subaltern, including those protected by legislation as ‘vulnerable peoples’ such as those lacking in mental capacity, where the giving of voice risks reinscribing the voiceless in their position as voiceless. Rather than seek resolution of this seemingly-intractable problem, one response for more-than-human researchers is to analyse what wider phenomena are revealed when we attempt to give voice.

Gwendolyn Blue offered such a commentary in describing Bear 71, an interactive documentary created by the National Film Board of Canada, which explores the surveillance of animals in the Canadian Rockies, where the eponymously named female grizzly bear moves through the enclosure and speaks to us in a first person imagined narrative form, evoking our identifications and our sympathies. Blue did not look to Bear 71 to gain an ‘accurate representation’ of the subjective experience of a grizzly bear, but to develop post-phenomenological insights into how the experience of Bear 71 and other animals in the enclosure are brought into our awareness through a plethora of surveillance and representational technologies. The value of her research lies not in excavating the ‘voice of the bear’, but in highlighting the contradictory coexistence of two logics: the technological multiplication of media and mediation, and an increasing felt sense of immediacy, intimacy and connection. This enabled Blue to conclude that digital information systems ‘augment the capacity for collective care and concern in public life while simultaneously facilitating the surveillance of and intervention into private lives’.

In a second example of experimenting with giving voice, the artist-researcher Kat Austen sought to call forth empathy with, and embodied knowledge of, a marine environment that is being altered in anthropogenic ways. Austen used sound recorders to map an underwater environment in Bergen, Norway, also measuring the levels of microplastics detected in nearby algae. She transduced her recordings into analogue vibrations of sound, touch and smell, creating an embodied interface in the form of the ‘Coral Empathy Device’, a multi-sensory headset worn in order to re-present the processes through which the coral is affected by its environments. Austen’s aim was to create a conversation between humans and coral, allowing us to perceive other worlds and very different spatio-temporal scales.

As an experiment in interspecies empathy, Austen avoids the thorny claim to know what the coral is actually experiencing. Rather, by feeling sensations generated by the changing measures of what the coral itself was ‘feeling’ over time, Austen experimented with cultivating an empathy that grounds the possibility for revitalizing care in the coral, the marine environment and beyond. Moreover, Austen suggests such empathy can bypass mental representations altogether – inspiring responses to the crisis of climate change without the need for deliberative consensus. Finally, the ability of users to remove the device reminds them of humans’ capacity for motility – a capacity unavailable to the coral. Thus, removing the coral empathy device is itself an important moment in engendering empathy, suggesting empathy is produced not by ‘becoming’ another but in the interplay of similarity and difference in our encounters with non-human others.
4.2. Expanding repertoires of listening

Another response to the problems of ‘giving voice’ has been to shift the emphasis from giving voice to learning to listen differently. Learning to listen differently is not easy: it requires learning to recognize, and be interrupted by, non-human agencies, forces and forms, and note the role that they are already playing in the construction and disruption of publics. It is unsurprising that questions of listening and voice have fostered participatory practices that cut across the senses. In particular, sound art and music – as temporal forms of expression – have proved particularly effective at communicating non-human temporarilities, including the times of geological transformation, climate change and the *anthropocene*. A number of artists and researchers, for example, have experimented with using innovative recording practices in order to listen to the voices of the earth.

The sonic is connective, rendering commensurable different modalities of data, allowing us to place them side-by-side so that they can reverberate and echo, linking up spatially and temporally distant agencies and places. Sounds are immersive, engaging people deeply and emotionally. They also articulate and dramatize the experience of place and landscape, situating bodies within complex events such as the processes of climate change. These qualities make sound an effective medium for including non-human agencies in contemporary research practices. Researchers have sought ways of enabling human communities to participate in the sounds and voices of the environment. For example, George Revill leads an ongoing research project on *Listening to Climate Change*. In part, the project is driven by the imperative to use participatory methodologies to move beyond the impasse of whether climate change exists or not. Instead, they foreground the question, ‘What kind of world do we want to live in in twenty years time?’

Imagination exercises such as this allow us to take hold of the future rather than be passive before it.

The project is set in Blakeney, a UK coastal community with a highly dynamic coastline that is an important reserve for marine mammals and migrating birds and is also susceptible to extreme weather events and climate change. The researchers, including social scientists and sound artists, enlisted stakeholders with particular expertise in the research site, together with wider communities of local residents, as human participants in the research. To these stakeholders, they introduced the term ‘living landscapes’ to convey that the landscape is always changing – (re)made by the people who live on the land, forces of nature and the lives of plants, non-human animals and birds. Participants were then asked two questions: (1) to identify two other ‘voices’ they would want heard, where one of these has to be a non-human voice; (2) to imagine that they had to think about the future and make a decision on what acoustic recordings to solicit in order to do so. From these conversations, groups decided what will happen in the future, factual observations about what has happened in analogous situations elsewhere and at other times. Revill describes this as a ‘sleight of hand’ that serves to trouble the assumptions of the human participants as they emerge. Recognising the need to treat dialogue be created across these divergent registers? Anticipating this challenge, the researchers weave into exercises facilitated to imagine what will happen in the future, factual observations about what has happened in analogous situations elsewhere and at other times. Revill describes this as a ‘sleight of hand’ that serves to trouble the assumptions of the human participants as they emerge. Recognising the need to treat imaginative work seriously as a praxis, this lends imagination exercises a dialogic quality whereby what has happened can come to interrupt constructions of what will happen.

The project design makes space for a rich public debate concerning the place of non-humans in their lives, as well as attempting to listen to these non-human voices in new ways. At the same time, the project also raises challenges that are common to many projects involving non-humans. As with Austen’s Coral Empathy Device, can the non-human voices ‘speak back’ to the research process, or do humans end up speaking on their behalf? Does the research design presuppose that it is only how non-humans matter to humans that is the important thing, or can genuine dialogue be created across these divergent registers? Anticipating this challenge, the researchers weave into exercises facilitated to imagine what will happen in the future, factual observations about what has happened in analogous situations elsewhere and at other times. Revill describes this as a ‘sleight of hand’ that serves to trouble the assumptions of the human participants as they emerge. Recognising the need to treat imaginative work seriously as a praxis, this lends imagination exercises a dialogic quality whereby what has happened can come to interrupt constructions of what will happen.

Inspired by critical post-phenomenological approaches, Revill approaches agency as that which shapes what can be thought and said. The project design makes space for a rich public debate concerning the place of non-humans in their lives, as well as attempting to listen to these non-human voices in new ways. At the same time, the project also raises challenges that are common to many projects involving non-humans. As with Austen’s Coral Empathy Device, can the non-human voices ‘speak back’ to the research process, or do humans end up speaking on their behalf? Does the research design presuppose that it is only how non-humans matter to humans that is the important thing, or can genuine dialogue be created across these divergent registers? Anticipating this challenge, the researchers weave into exercises facilitated to imagine what will happen in the future, factual observations about what has happened in analogous situations elsewhere and at other times. Revill describes this as a ‘sleight of hand’ that serves to trouble the assumptions of the human participants as they emerge. Recognising the need to treat imaginative work seriously as a praxis, this lends imagination exercises a dialogic quality whereby what has happened can come to interrupt constructions of what will happen.

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54. The epoch of the Earth’s history that began with the industrial revolution and the consequent emergence of the human being as a geologic force. See Chakrabarty 2009.

55. For example, see Ken Goldberg 2006: ‘Ballet Mori’.


57. From personal communication. See also Pearson 2016.
4.3 Building stages for new encounters

Another way to side-step naive attempts to give voice has been to identify and work with the capacities of non-human others to participate in meaningful ways by constructing ‘stages’ for non-human voices to speak, and developing modes of receptivity that allow us to be able to respond to them. An innovative collaboration has been documented between researchers at Oxford University, led by Sarah Whatmore, and residents of Pickering, a small town in the UK that suffered regular flooding, making flood risk management a subject of a great deal of controversy. The project design involved assembling new ‘competency groups’, where natural scientists and social scientists collaborated with volunteer residents in the localities where flood risk management plans were a powerful source of tension and disagreement. Each competency group was comprised of project team members and residents. During bi-monthly meetings, hands-on flood modelling – usually the province of appointed experts – became the key practice through which ‘expert’ and lay members’ knowledge claims could be tried out. Ever-present within the discussions was the non-human agency of water itself: its paths, dynamics, (over)flows and capacity to push back against poorly-conceived models. Equally important, however, was the agency of artefacts such as photos, video footage, computer models, policy documents and maps.  

Drawing on the work of Stengers, Whatmore and Landstrom contrasted a conventional participatory ethos of empowering local people with an ethos of empowering the situation, where the aim is to ‘force thought’ in those affected by it and to ‘slow down’ the reasoning of the established experts, in order to enable a redistribution of expertise. The competency groups used residents’ situated knowledges of the flood catchment area, including memories of floods dating back to the 1940s, to modify existing Environment Agency flood models that had in the past been the cornerstone of the ‘top-down’ expert view of flood management in the area. These existing flood models insisted that a large and costly flood wall was needed. The modification of the official flood map was followed by flood modelling exercises in which everyone in the competency groups could try out modelling different solutions to the flooding, with the help of the flood modellers to programme the software. New solutions were explored, and exhibited in the local area, generating substantial debate and, eventually, the take-up of a new solution, which did not involve flood walls but a series of more inexpensive upstream ‘bunds’. Through intentionally building stages and spaces for the intermingling of human and non-human agencies, and slowing practices down, Whatmore and Landstrom document how hybrid forums of knowledge and expertise can offer innovative practical and political responses (Figure 3).

Figure 3
Upstream bund diverting flow into field.
4.4 Ethics in more-than-human participation

A large body of distinct yet related research, particularly in human geography, anthropology and philosophy, has explored ways of understanding the participation of non-human animals and plants in research in Western contexts. Moreover, a number of researchers are engaging in creative experiments in inviting non-human animals into participatory research processes. Michelle Bastian et al’s recent collection of essays marks a highly significant intervention in this field of research, exploring a range of methodological practices for including entities as varied as dogs, birds, plants and trees in research processes.

In one such example, Tim Hodgetts, a human researcher, and Hester, a springer spaniel, described their role in a research project that brought humans, Hester the dog and pine martens in rural Wales into a specific set of conservation practices. Small carnivorous animals, pine martens are very rare in this area, and ongoing conservation projects assess the size and location of any remaining populations, whilst also preparing for a ‘re-stocking’. Searching for these elusive animals often relies on scat surveys but these surveys are themselves difficult, since pine marten scat is so similar to the scat of certain other animals. The project aimed to combine canine smell with human sight to identify pine marten scat. Both dog and human had to learn the skill of collaboratively identifying the scat.

In contrast with thinking of the dog as a ‘tool’ to enable the research to be carried out, Hodgetts and Hester emphasised the vital role in their research of embodied empathy and attunement, as different feeling, seeing and thinking bodies undo and redo each other, reciprocally but not symmetrically. Given this rich attunement between human and dog, one might consider analysing the research practice as involving different kinds of collaboration between human and dog. This raises issues of representation, ethics and power that are of course central to participatory research. Did Hester consent to being a research participant? According to Hodgetts and Hester, Hester’s tacit consent could be judged from the enthusiasm and joy with which she engaged in the activities. However, even accepting that the research process was enjoyable for her, it is harder see how the outcomes of the research (distribution maps of pine martens) benefit her. Indeed, Hester is documented as having shown little enthusiasm and joy with which she engaged in the activities. However, seeing the dogs perspective.

Contrast this with the zoomusicologist Hollis Taylor’s experiments with co-producing music with birds, in particular, pied butcherbirds in Australia. The use of birdsong in music is well established, but Taylor strives to develop a genuine co-production of sonic outcomes. Taylor writes:

As a violinist/composer I do more than incorporate avian vocalizations into my practice: I trust the musicality of pied butcherbird song, and many of my (re)compositions are almost direct transcriptions. My ability to transcribe pied butcherbird vocalizations improved by playing them on the violin – with me entering into the physicality of the experience. This became for me part of the analytical process and not merely what preceded or followed it. I study pied butcherbird vocalists, but I also study under them.

In terms of participation, Taylor gives examples of birds declining to participate. She was attacked by a ‘bird-musician’ twice during nesting season and, concluding that the bird was quitting the project, no longer recorded there. Similarly, on one occasion eight pied butcherbirds evicted her from their territory via harsh calls and beak claps. Taylor suggests that the birds both were fully involved in the co-production of key project outputs, namely the music, and also had a genuine choice to decline to participate. Taylor does not offer a view, however, of whether the birds were empowered in any way by this participation.

It is clear that recognising and enhancing the role of more-than-human participation in academic research will ultimately need a much fuller reworking of ethical language, norms and standards. Beyond ethical objections to outright exploitation of animals in experimentation, projects involving animal participation raise difficult philosophical questions about the nature of power and empowerment. Participatory research is a research practice that is dedicated to empowering stakeholders in the research. What empowerment might mean in relation to non-human animals, however, remains unclear and contested. The above examples could be construed as problematic attempts to bridge the difference between humans and non-humans, by re-articulating non-humans as being like humans, and granting them rights to informed consent and so on. Most discussions of ethics, empowerment and participation in research are almost entirely anthropocentric, and rather than trying to fit non-human participation within the ethical categories of human research (informed consent, empowerment, the difference between ‘genuine and ‘pseudo’ participation, control, decision-making and so on), which can seem like trying to slot a square peg into a round hole, new frameworks are clearly needed. This will be a complex affair, requiring a wholesale reworking of many of the embedded institutional assumptions about the nature of social research. Until then, researchers engaging in more-than-human research will have few universal norms to refer to. Given intersectional critiques of universality, this might be recast as an opportunity to strive instead for ethical sensibilities that are sensitive to their specific research practices and conceptions of empowerment.

Recognising and enhancing the role of more-than-human participation in academic research will ultimately need a much fuller reworking of ethical language, norms and standards.

59 Interesting examples include: Barua 2014; Callicott 2013; Haraway 2008; Kitts and Helmreich 2010.
60 Bastian, Jones, Moore and Rex 2016.
61 Hodgetts and Hester 2016.
63 Ibid. 48.
4.5 Documenting ecologies of more-than-human selves

The examples of Hester and the pied butcherbirds beg the question: to what extent, if at all, is it legitimate to ascribe like-minded selves to non-human others – as if beneath all that difference we would find selves ‘just like us’? In an example of more-than-human ethnography that takes forward a synthesis of more-than-human research with a recognition that non-humans do not share the same capacities for ethical deliberation as humans, Naomi Millner conducts a project on community forestry in the Maya Biosphere Reserve in Guatemala. In order to try to research the ‘pre-history’ of sustainable forestry – or ‘sustainability before Sustainability’ – Millner approaches the implementation of sustainability practices as imperfect attempts to translate the pre-existing signification of non-human selves into the symbolic language of resource management protocols and practices.  

Millner thus avoids the trap of believing that the reserve was a \textit{terra nullius} awaiting human intervention, a term used in the justification of settler colonialism. Following Eduardo Kohn, Millner articulates the challenge of documenting the historical layering of human and non-human interaction as ‘interlacings of networks all trying to know each other’.  

Millner draws upon oral history to develop a more-than-human oral history methodology. Where oral history is traditionally an account of an individual person’s life, more-than-human oral history starts from the recognition that ‘a life’ is never just an individual human life but is also a crossing point for many other entangled lifefroms. In order to do this, Millner has conducted workshops and recorded the oral histories at particular sites, so that her (human) participants could show her important aspects of their life that extend beyond the individual. For example, one participant showed her how he makes craft objects out of mahogany. Another showed her how the community used to extract a form of natural chewing gum called chicle from trees in the forest, until petrochemical gum destroyed the industry (Figure 5). In a third, a guide showed Millner around the ruins of Mayan architecture, explaining the importance of Maya architecture to the community. In each case, the individual’s life provided the framing, but interruptions from the site itself (the sounds of the forest; a sudden downpour; the silence of a two-thousand-year-old stone structure) were used as prompts to broaden from the personal story to the imbricated living networks that the interviewee participated in and was shaped by. Through these research practices, the project aims to adapt an existing methodological tool in order to allow it to register more fully the multiple networks of non-human life that are entangled within the lives of participants. In this way, Millner suggests that a fuller description of the more-than-human ecologies of community forestry can be developed.

4.6 Engaging psychic multiplicities

Turning to consciousness itself, what William James in 1890 described as a ‘teeming multiplicity of objects and relations’ has proven a rich site for research into intra-psychic forms of participation. Recognizing with the phenomenologists the impossibility of fully escaping our own experience, those interested in navigating altered and/or multiple states of consciousness employ techniques such as dreamwork, the use of psychoactive substances, meditative techniques, breathwork, fasting and drumming. The philosopher Aldous Huxley drew on Henri Bergson’s subtractive notion of consciousness in positing that a ‘cerebral reducing valve’ exists in the brain in order to channel the vast totality of sensory experiences into manageable experience, and that this valve can be opened up through various techniques including the use of psychedelic substances.  

This led to the widely-held claim in the counterculture of the 1960s and subsequent ‘New Age’ that we can more fully participate with more-than-human entities, and even the infinite itself, through practices of cleansing our Blakean ‘doors of perception’.  

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64 Personal communication.
65 Ibid.
66 Huxley 1954.
67 See Smith 2009. The titles of Huston Smith and Aldous Huxley’s books draw from Blake’s poem: ‘If the doors of perception were cleansed every thing would appear to man as it is, infinite. For man has closed himself up, till he sees all things thro’ narrow chinks of his cavern’ (Blake 1701).
Combining an inquiry into the varieties of conscious experience with attention to the political challenges posed by the subaltern, in 2014 Gail McConnell, Jo Collinson Scott and Deborah Maxwell conducted Listening to Voices: Creative disruptions with the Hearing Voices Network, a community-based participatory research project with the Hearing Voices Network (HVN). The latter is an international peer-led network of local self-help groups attended by people who hear voices that only they hear. The researchers describe how medical professionals and healthcare support providers have tended to encourage voice-hearers to silence their voices, in particular through psychiatric drugs, rather than listen to them. Joining with the HVN, the researchers – themselves with expertise in poetic, musical and narrative voice – attempted to foreground not individuals with voices but ‘voice’ itself, in all its manifestations – for instance, as common human experience, as pathology, as friend, as agitator, as advisor and as aside. Recognizing that listening is a more active process than merely hearing, the project asked how creative listening practices could enable individuals and communities to become more attuned to voices previously marginalized, repressed or ignored, to disrupt academic and medical hierarchies of knowledge and power. Instead of ‘giving voice’ to the voice-hearers, the researchers sought to re-imagine academic writing practices themselves, by bringing the multiple voices of academia itself (such as the subjective, doubting, meandering, hyper-critical and comic voices one finds relegated to footnotes) into conversation with the voice-hearers who were recognised as experts in voice-hearing.

The project was participatory and unfolded iteratively, centred around a retreat for researchers, artists and voice-hearers where they explored the relationships between HVN members’ expertise in listening to, and engaging, multiple voices, and musical, poetic and storytelling-based artistic practices. Participants co-created a Listening to Voices guide, outlining best practice when listening to voices and voice-hearers. The initial text of the guide was overwritten in numerous voices – living, dead, imagined, self-critical, angry, reflective, analytic – until the play of voices became more important than any original message. The methodology was one of collectively ‘writing on the object’, rather than ‘writing about the object’. By making visible and audible the creative disruptions (e.g., for example, ‘overwriting’, annotations and footnotes), the final texts attempted to foreground what was challenging and meaningful about the collaborative process and the politics of authorization. The palimpsest of responses in the guide illuminated process, the struggle for meaning and the numerous iterations the guide had undergone (Figure 6).

Listening to Voices showcases the futility of hoping to fully or comprehensively represent experiences in participatory research practices where subjects continuously react to how they are represented. This highlights the importance of appreciating the performative register in contexts where representative projects call forth infinitely regressing loops. Listening to Voices responds to the exasperation in seeking authorial finality through its techniques of constant narrative disruption.

See http://www.listeningtoviches.org.uk/
For the Hearing Voices Network, see https://www.hearing-voices.org/


Figure 6
Listening to Voices guide
5. SUMMARY

More-than-human research does not seek to reveal the minds of non-humans, as if non-humans could suddenly speak. Nor does it mean necessarily imputing a special kind of subjectivity to non-humans. While perspectival naturalism (see section on ‘more-than-human communication’ above) does claim this, science studies and post-phenomenological approaches do the opposite, being more concerned with the objective capacities of a wide range of inter and intra-psychic agents to perceive, act and react, while multi-species ethnographies turn to material semiotics to adumbrate the richness of language systems. All the case studies referenced here offer ways of conducting more-than-human participatory research enabled by speculative leaps of various kinds, whether the intention of concepts, the stating of working hypotheses or inferences about other worldviews. We suggest that attending to the more-than-human encourages participatory methods to rub up against their limits in generative ways. This is what makes more-than-human participatory practice an exciting research intersection.

Yet the nomenclature of more-than-human, and a yearning to research the ‘other’, will always risk devaluing the emancipatory, rights-based politics of the liberal bounded human self. Moreover, we have sought to collaborate with community partners who were rightly concerned that the language of ‘more-than-human’ and ‘non-human’ carry the normative connotations of ‘sub-human’ and ‘inhuman’. This can feed into the politics of coloniality and the subaltern in unintended and toxic ways. All efforts at going beyond the bounded, univocal subject problematized at the beginning of this review must therefore be done with care if they are not to reproduce the conditions for undermining progressive rights claims or slipping into an exclusionary normative register.

In describing speculative approaches, Isabelle Stengers draws upon the metaphor of dancers, hands joined and leaning back, spinning in a circle. No one dancer can achieve this on their own and yet together upon the metaphor of dancers, hands joined and leaning back, spinning in a generative world. All efforts at going beyond the bounded, univocal subject must be done with care if they are not to reproduce the conditions for undermining progressive rights claims or slipping into an exclusionary normative register.

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Stengers 2015: 239. For similar configurations, see the plateaux in Deluze and Guattari 1987 and the ‘constellation’ in Benjamin 2009.


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GLOSSARY

Actor Network Theory
Views the social as being constructed through creative associations between varied human and more-than-human agents. Associated with Bruno Latour, Michel Callon and John Law, Actor Network Theory (ANT) highlights the constitutive role of non-humans in social life, describing agency as an outcome of ‘actor networks’, not subjects or objects in isolation.

Agency
Traditionally defined as an ability to act and think. It is generally seen as something that comes from consciously held intentions, and as resulting in observable effects in the human world. This definition makes agency something that only humans exercise. More-than-human research problematizes this by suggesting, for instance, that human ‘agency’ is actually a composition of the agencies of many different entities, and/or that non-humans (including animals, materials, and objects) can also exert forms of agency.

Anthropocene
The name of a purported new geological age, replacing the Holocene, that is marked by the point in history where humanity became a geological agent, acting as a key determinant of the environment of the planet, through the burning of fossil fuels, carbon emissions, nuclear radiation, and other geophysical processes. The term is significant for its challenge to modern understandings of nature as a stable domain that is separate from the realms of history, culture and society.

Biopolitics
A way of governing that takes life, especially biological life, as a key value and target of intervention – attempting to make societies healthier, more vigorous, more full of vitality. Biopolitical rationalities have supported visions of an organic, vital, healthy human society. However, biopolitics can also result in a politics of death, when certain groups are believed to be so damaging to collective vitality that they must be destroyed. One focus of contemporary biopolitics is not only on how to promote life, health and vitality, but on what counts as life, and how that life is to be valued.

Decolonial
Approaches that seek to confront and overcome colonial matrices of power. Existing as long as colonialism itself, decolonial approaches draw on ‘subaltern’, marginalized knowledges and practices originating from outside of, or in opposition to, European hegemony and the ‘Western canon’. They are committed to exposing, opposing and subverting the racialization, instrumentalization and dehumanization wrought by ongoing legacies of colonialism.

Ecofeminism
Brings together feminist and ecological politics to emphasize the radical interconnectivity of humans, animals, spirits, and the earth. As a movement, ecofeminism stresses the inseparability of spirituality, culture and politics. It is associated with pagan religious traditions, aiming to develop ways of thinking and experiencing that were based on embodied, intuitive relations with the earth.

More-than-human
Describes how human societies are always composed of varied relations between humans and non-human forces and agencies such as objects, animals, microbes, and technologies. It challenges the idea that humans are separable from their worlds, or society is separable from nature. More-than-human can refer to realms or entities beyond the human, or to larger ensembles that include the human.

Multi-species ethnography
Aims to produce rich, detailed, ‘thick’ descriptions of the distinctive experiential worlds, modes of being, and social and cultural attachments of other species. Drawing upon and adapting methods developed for research with humans alone, multi-species ethnography calls for ways of listening to, and building stages for voicing, the interlaced agencies of humans and non-humans alike.

Perspectival multinaturalism
Advances cosmologies in which non-humans also have selves or souls when understood from their own perspective. Drawn from the work of Eduardo Viveiros de Castro, all selves are understood to partake of similar phenomenal worlds or ‘cultures’, despite having very different bodily manifestations or ‘natures’. The resulting ‘multinaturalism’ contrasts with the Western notion of ‘multiculturalism’ by proposing that it is our mindedness rather than our physicality that we share with non-human others.

Post-phenomenology
A form of thought that is indebted to, but in some ways departs from phenomenology. Phenomenology focuses upon the human subject as an embodied vessel of experiences and sensations. Post-phenomenology retains this interest in embodiment and experience, but views experience as distributed across, and mediated through, both human and non-human bodies, technologies, objects, and worlds.

Pragmatism
A philosophical movement that argues that what counts as true knowledge is determined by its usefulness. It is a philosophy that takes practices as its starting point. Ideas are labelled true when they enable humans to get things done, and to cope with the world. More-than-human researchers point out that practices involve many different kinds of actor, and not just human achievements.
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CHAPTER 5

Communal Customary Land Rights in Sudan: The Need for a Comprehensive Reform of Statutory Land Laws

Mohamed A. Babiker

Introduction

This chapter focuses on legal norms concerning customary or communal land rights and their use in Sudan, in terms of ownership and transfer of land and as recognized and practised by local communities who apply their own customs to land. In this context, the first part sheds light on customary rights to land and usage, while the second part focuses on statutory laws governing customary ownership of land and how tribal and communal lands are affected by various statutes or enactments in the context of contemporary legal framework in Sudan. The objective of this part is to highlight some milestones in Sudanese land legislation and to give some background concerning the legal framework. In this respect, the chapter not only provides a historical overview of land legislation in Sudan as pertaining to land laws, but also looks at how such legislation (in this process) has seriously encroached upon the traditional and communal ownership of land. This has negatively impacted on the peaceful co-existence between tribal communities and affected traditional land ownership.

Accordingly, the chapter examines existing legislations dealing with land. The aim here is to highlight some milestones in Sudanese land legislation and to give some indications about Sudan’s legal framework governing land ownership and use. In addition, the chapter examines the legal status of customary laws and rights governing communal and tribal ownership of land, and the extent to which such rights have been encroached upon or marginalized from the colonial era to the present day by statutory and Islamic laws. The chapter also examines the developed jurisprudence and the recognition of customary rights to land as applied by courts. It also touches on “land grabbing” and investment laws and policies, and their negative impact on traditional and communal ownership of land.
The chapter also focuses on law reforms after the signing of the CPA, which calls for the establishment of land commissions mandated to adjudicate on traditional customary land rights and land polices. However, it concludes that the land reforms in the post CPA era have been a failure, as communal land ownership rights are still marginalized by statutory laws and the Constitution. Land legislation in Sudan is confused, complicated and arbitrarily used for purposes of land expropriation. Land reforms and practices to strengthen customary land rights have not taken place to date in the post CPA era, despite constant calls for reform. Hence, the chapter concludes by arguing that there is an urgency for Sudan to undertake a comprehensive review of its land laws to ensure the customary rights of local communities are respected.

**Customary Land Ownership and Usage in Sudan**

Historically, land in Sudan (as the most important asset of the society) is vested in the tribe as a whole, and is administered on its behalf by chiefs or tribal leaders (Macek 1993). An individual's right to land derives from his right as a community or tribe member (*ibid.*). Customary law also exclusively reserves specific areas for communal usage, which may be used by individuals but they do not have ownership rights over them nor can they exclude other members from them (*ibid.*). These areas are incapable of individual ownership and are regarded as public places for the whole tribe. These areas include: forests, livestock pastures, fishing areas, cattle camps and so on. Members of neighbouring tribes may be allowed access to these public areas, however, on a basis of reciprocity (*ibid.*). These are customary law rules which correspond with easements under English Law, such as the right of way: a footpath designated for use by members of the public who may pass across land owned by individuals. The latter are not entitled to obstruct any member of the public from using such a footpath. People may also acquire the right to pass across individual's land to go and obtain water, for example, from a well.

In eastern Sudan, for instance, each Beja tribe has its own land for farming and cultivation. They also breed cattle and dig wells. Any persons from any other tribe are allowed to pasture their cattle on their land and are not forbidden from drinking from their wells. However, it is customary that one can only have access to tribal land provided that no structural changes are made such as digging a well or farming the land without permission; these acts will be regarded as trespassing on their tribal land which may lead to fighting. Accordingly, agricultural land, wells and cattle become the main causes for fighting if tribal customs related to land are not observed by members of the host tribe. Non-Beja “guests” who dwell or settle on Beja land for a short or long
time can use the land for cultivation and for pasture or access to water, provided that they do not establish permanent structures, digging wells or cutting trees (Abdel Ati 2005, p. 12). In return for using the land, guests are expected to pay to the sheikh or the 'omda of the area what is traditionally known in the Beja language as *guadab*: the chest-meat of an animal. The *guadab* symbolizes “recognition” of title over land rather than rent or a tax levy, since it is not regular or defined in quantity, but depends on when guests slaughter an animal. In some areas, the *guadab* is paid in cash (*ibid*.). This small or nominal amount given may be regarded in contract law as a “consideration”. In case of dispute, the legal title to the land or the right of to occupy it can be proved through a variety of ways including witnesses, signed documents (i.e., the Hadandowa and the Rashaida agreement\(^1\)), neighbours and tribal maps. Tribal disputes over land are normally resolved by tribal maps. Such maps are recognized between tribes and easily demarcated or identified according to the *nazāra* (extension of the tribe’s geographic boundaries). However, disputes over rights to land between sub-clans and cousins, rather than between different tribes, have proved to be difficult to resolve. In this case, the *guadab* must be applied in addition to witnesses. If the dispute over the land leads to serious fighting between sub-clans, the *nāzir* will intervene and issue what amount to an interim legal “injunction” (*ḥajz*) until the dispute is resolved. In the case of a dispute between different tribes, the *majlis* or *ajawīd* council, headed by the *nāzir* of each tribe, will resolve the matter according to the recognized custom, to be agreed upon between the two tribes. Regarding the right of pastoralists to herd their cattle on the land of other tribes, the Hadandowa, for example, require other tribes to use certain routes. They also ask pastoralists not to travel with their families so that they can guarantee that they will return to them. The Hadandowa have signed agreements in this respect with the Rashaida and Shukriyya tribes under which they have hosted the Rashaida in their land and specified the tribal routes for the pastoralists.\(^2\)

**Courts Recognition of Customary Land Ownership and Use**

Courts in Sudan historically recognized customary law (Mustafa 1971, p. 163). Unlike other areas of customary, criminal or personal law, land ownership and

\(^1\) For example, the Hadandowa signed an agreement in 1933 with the Rashaida allowing them to use the land on specific conditions.

\(^2\) In Darfur, customary ownership is also different from land ownership in eastern Sudan and other parts of Sudan such as Blue Nile, Nuba Mountains and northern Sudan. For details on this see Osman (2012).
use have been recognized and codified in statutory laws. Customary rights related to land have also been codified and land usage and customary ownership integrated into the statutory laws (El-Mahdi 1979). Section 13 (iii) of the Land Settlement and Registration Ordinance 1925 adopts the right of amāra. Moreover, section 27 of the same law has codified customary law where the ownership of the land is distinct from the ownership of trees.

During the 1960s, Sudanese courts recognized local customs and practices, particularly customs relating to land in the northern parts of Sudan. During this period, as soon as courts ascertained the existence of a custom or practice, they accepted it and enforced it on the understanding that it afforded the most equitable solution, since it had been accepted for a long time by the community in which the dispute arose. It is pertinent to note here that, outside land law, there was no development in the field of customary law with regard to other legal branches at all (i.e., criminal, personal laws) during that period (Mustafa 1971, p. 218). The courts were much more reluctant to recognize and give effect to customs and practices outside the realm of land law (ibid., p. 164), and have largely failed to build up any body of law or set of legal rules with any roots in local customs and practices outside the field of land law (ibid., pp. 164–165).

The courts widely applied a custom known as karū (rights over government land) and rights of sīd al-amāra. Other customary rights, though not specifically provided for by legislation, are also applied as law and recognized by Sudanese courts. Most of the litigation concerned the ownership or right to cultivate islands emerging in the Nile in northern Sudan. For example, customs

3 The right of amāra is a customary right whereby the person who holds such a right (sīd al-amāra) cultivates the land owned by another (sīd al-aṣl) and has the right to a share of the crop.

4 These included a custom known as idāfa practised in the Merowe District. See Heirs of Abdel Gabbar Mohamed v. Fatma Mohamed (1964) SLJR 156; mudāyara, practised in Damer District (see Abdall Elhussein v. Safya Ali Abu Ali, 1964 SLJR 62); karū applied Shendi District (Heirs of Fadlalalla Oklah v. Hussein Ali Brar (1964) SLJR 96); haqq al-ard practised in various parts of Northern Province (Osman Omar v. Mohamed Idrsi (1958) SLJR (1958) 62); haqq al-miswaq applied in Manasir area (Heirs of Abdel Aziz Khamees v. Heirs of Habib Habram (1967) SLJR 64).

known as *ḥaqq al-quaṣād*, *ḥaqq al-arḍ*, *ḥaqq al-torīa*, *ḥaqq al-miswaq*, and *mudāyara* have been recognized by the courts especially in the Northern Province. However, for these customs to be valid and applied by the courts, they must not be contrary to justice, equity and good conscience, as recognized in English law, as no Sudanese common law had emerged at the time. The policy behind this limitation was to preserve or observe the customs of the local communities while at the same time giving the courts power to scrutinize such customs, to ensure that they were in harmony with the principles of justice, equity and good conscience (Zakaria 1991, p. 70).

6 The *quaṣād* principle was applied in the case of *Heirs of Eltoma Bint Ali v. Ali Mohammed El Sayed*, (DCCS - 472–5, 519, 805–1942 (Khat.). See also the case of *Taha El Egeil v. Suliman Dawood Mandil* (AC rev-35-8-1953, at pp. 3–4). The learned judge in this case applied the *quaṣād* principle and stated: “We have no hesitation in agreeing with the learned judge of the High Court that the *quaṣād* principle does in general apply to this area. It is an equitable principle governing the rights of riparian owners, which in one form or another is widely accepted in many countries and is commonly known to be similarly accepted in many areas in the Sudan, although the form in which and the extent to which the principle is applied may vary locally. The presumption therefore in ... any case involving riparian rights is that some such principle applied unless a contrary local custom or practice is proved in a particular are or is so well known in that area that judicial notice has been taken of it” (Mustafa 1971, p. 163). See also *Atta El Manan Ahmed Falag v. Owners of Sagia No 6 El Damer*, hcs-401946; *Mahmoud Elnur Omer v. Ibrahim Mohamed*, HC-REV-65-1955.

7 A local custom in Merowe District, where the land owner is entitled to share any date tree planted by another on his land. For a relevant case see *Osman Omer and others* (1964) SLJR, p. 29.

8 A local custom in al-Damer District that gives the right to one-half of the produce of the date trees on the land. It does not give the ownership of an undivided share in the trees but merely a right to one-half share of the crop. See *Omer Ali Omer v. Fadl El Mula El Hussein and others*, SLJR (1962) p. 42.

9 A recognized custom in Manasir Area in the Northern Province, which gives the right to cultivate the land, besides the original right to *ḥaqq al-aṣl* (i.e., the owner of the land). Such a right is registered as an encumbrance. See *Heirs of Abdel Aziz Khames v. Heirs of Habib Habram* SLJR (1967), p. 64.

10 The system of *mudāyara* is a custom applicable in Berber and Shendi whereby the shares, as numbered and shown in the register, are given no recognition by the owners, and that the one who owns a number of ‘*uds* (sticks), each one of them appearing in the register in different shares on land which cultivates the same number of ‘*uds* as one piece of land allotted somewhere in the *sāqiyya* according to *mudāyara*. See *Abdulla El Hassan Hamza v. Safiya Ali Abu Ali and another* SLJR (1962), pp. 53–54.
Town and Rural Courts

The formal judiciary has established Town and Rural Courts (both have civil and criminal jurisdiction) at the lower level of the judiciary and at the state and locality level. Such courts apply customary laws related to land disputes. The Rules of Regulations of the Town and Rural Courts 2004 create certain types of Middle Rural Courts and Rural Courts. These courts apply the predominant customs in their geographic jurisdiction in addition to other laws and regulations, provided that the custom does not contravene the law, the principles of justice and *sharīʿa* law. Unlike the application of customary law during the sixties, here customary law should not contradict *sharīʿa* law or English common law. These courts are recognized as part of the formal structure of Sudan’s court system and have been accorded certain powers. The jurisdiction of these courts and the appointment of their members are also defined. These courts can therefore be classified as part of the judiciary, statutorily mandated to apply customary laws, and are an integral part of the formal court structure. They are managed or supervised by judges from the Judiciary who are familiar with the customs of the area. These courts, because they are manned by prominent influential tribal figures, play important roles in most situations.

However, it is pertinent to note that such courts have limited civil and criminal jurisdiction: they are only mandated to look at a limited category of cases pertaining to land disputes and pasture. For example, the Town and Rural Courts have no criminal jurisdiction over civil claims related to land ownership or any claims against the government or any public corporation. This is one of the serious gaps in the body of the law, as most disputes relate to communal land and are claims against the government or corporations or companies, particularly those allocated vast agricultural schemes under investment laws. Furthermore, such courts have civil jurisdiction over small cases only involving small sums. This can also be considered as a further limitation in the law, particularly in various cases involving conflict between farmers and pastoralists in some areas where a large sum of money is involved. The Middle Rural Courts and Rural Courts have specific civil jurisdiction over: (a) claims related

11 Article 22 (2) (author’s translation) of the Rules of Regulations of the Town and Rural Courts 2004.
12 Article 12.
13 Article 14.
14 For example, cases involving the estimated damages to crops caused by pastoralists on agricultural land that exceed more than 20,000 USD are outside the jurisdiction of these courts. Rural and Town Courts will not have jurisdiction, as the civil case may involve large sums of money.
to pasture and farm damages or injury to cattle, without specifying the amount of damages; (b) cases related to land and boundary disputes; (c) appeal powers over decisions and orders issued by the Rural Courts related to civil cases.

Under the provisions of the Rural and Town Courts, it is apparent that customary law is relegated to a minor status. For example, customary law must not contradict statutory laws. In cases related to unregistered usufructuary land, whoever has land registration certificates over the land, will get a positive decision from the court, even if the other party proves his ownership of the land through possession (ḥiyāza) or occupation. This is because Sudanese laws and practices still ignore the interconnection between customary and statutory law with imported legislative blueprints such as the Civil Transaction Act 1984, which allows land grabbing by new and old elites and ignores unregistered usufructuary rights of tribes or local communities, which can be confiscated for the public good.

Currently, all states in Sudan apply the weak Rules of Regulations of the Town and Rural Courts 2004 outlined above. With the exception of North Kordofan State, states do not enact any laws to establish a powerful native judiciary to apply customary law. It is pertinent to refer here as an example to the Native Judiciary Act of North Kordofan State, 2006, which can play a vital role in land disputes. The Act establishes Rural and Town Native Courts to look at cases of a customary nature. These courts consist of a head, a deputy head and a sufficient number of members, to be chosen by the Head of the Judiciary at state level after consultation with the wālī (Governor). The candidate who presides over the Native Court must be aware of the customs and traditions of the area. The court has jurisdiction over ownership in cases of land disputes and any claim against the government or a public corporation. The Act also decrees that rural courts have jurisdiction over cases of damages to farms or pastures or of injured cattle. Under the Act, native judges deliver justice to all people in the state in accordance with the law and customs. Rules of Procedures and Regulations were also issued under Article 29 of the Act. However, it remains to be seen whether this Act will make a difference in practice.

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15 Act No 19, 12 April 2006.
16 Article 6.
17 Article 7 (2).
18 Article 10 (2).
19 Article 17.
Statutory Regulation of Land Ownership and Use

The statutory regulation of land in Sudan has been a complex product of various sources. It is a unique combination of Sudanese legislation, judge-made rules, customary laws, and sharīʿa law. Many of these rules are wholly of local origin, whereas others have been received from abroad with a varying degree of modification in Sudan (Thompson 1965, p. 5). Therefore, examining land legislations is an important starting point in an attempt to determine the applicable rules and procedures and their impact on customary ownership and usages.

Replacement of Diversified Indigenous Systems of Land Tenure with a Uniform State-Controlled System

One of the important legislations regulating land ownership and rights in Sudan is the Civil Transaction Act, 1984. The CTA stipulates that its provisions shall apply to all obligations and rights arising, inter alia, out of lease, ownership, incidents of ownership and mortgages. It also governs possession, ownership and rights, including contracts with respect to land. Prior to 1970, the registration and settlement of rights and interests in land had not been effected throughout Sudan. On 6 April 1970, the Unregistered Land Act 1970 was promulgated, stating that all land of any kind, whether waste, forest, occupied or unoccupied, which was not registered before the commencement of this Act, would on such commencement, be the property of the government and be deemed to have been registered as such. The Unregistered Land Act 1970 reconfirmed in the CTA, 1984, was nothing less than a major act of land reform, which vested ownership of the greatest majority of Sudanese land in the government (Gordon 1986, p. 148). The Act has made the government the biggest landlord in Sudan and the state is declared to be in control of the land as well as the owner of all land that was not registered in the name of a private individual prior to 6 April 1970. As a consequence, no right or interest may be obtained in state-owned land unless such right or interest is registered at a government registry. Thus, widely diversified indigenous systems of tenure and land management were replaced by the implementation of a uniform

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20 Civil Transactions Act 1984, s. 4.
21 Ibid. S. 11 (12) and (13) (b).
23 The Act states that “all waste, forest and unoccupied land’ shall be deemed to be property of the government until the contrary is proven’.
24 See CTA, 1984, s. 559 (1) (3).
and state-controlled system (de Wit 2001). Here, the state replaces the local community as the guarantor of access to land and security of tenure. This replacement could be problematic if the state does not behave as a credible and neutral grantor of land; an issue that will be critically explored in this chapter, particularly with regard to land grabbing for investment in major projects at the expense of local communities’ entitlement to customary land ownership.

Although the government becomes the legal owner of all land not registered before 1970, it may, of course, permit private parties to use state-owned land. In this respect, the CTA of 1984 provides that the central, regional, and National Capital authorities shall adopt the necessary procedure for granting the usufruct of land to private persons, through the establishment of committees consisting of no less than three members.25

However, just as the government may grant land, Sudanese legislation similarly provides for forced acquisition by the state of land privately held when a determination has been made that such land is “required permanently or temporarily for any public purpose”.26 However, no person shall be deprived of property except through the payment of prompt consideration and in accordance with due process of law.27 The procedure to be followed in state acquisition is that an expropriation officer is to be appointed, who gives notice to the occupiers of the land and other interested persons, identifying the land and stating its intended acquisition “for a public purpose”.28 Where the land in question has not previously been registered, it shall be registered before the acquisition is carried out.29 The expropriation officer is to attempt to reach an agreement on compensation with the persons having rights or interests in the land.30 In the absence of an agreement, the amount of compensation is to be determined by a board of arbitration according to a set of standards,31 with provision made for appeal.32 Compensation may be made either through the payment of money or the grant of other land.33

The registration of rights and interests in land is the process by which a government agency keeps a record of titles to land, whereby no dealing in respect

25 CTA, 1984, s. 556(1) (2).
27 CTA, 1984, s. 517 (2).
28 Ibid. s. 10.
29 Ibid. s. 9.
30 Ibid. s. 14.
31 Ibid. s. 19.
32 Ibid. s. 23.
33 Ibid. s. 22.
of any parcel of land in that record is invalid unless registered. The purpose of registration is hence to facilitate proof of title and so make the transfer of land simpler, quicker and certain. The Act is still in force to date and sets out how land rights are identified and registered.

In terms of procedures, the Land Registration Act 1970 empowers the Attorney General, whenever “it appears expedient”, to publish a notice in the Gazette stating that it is intended to affect a settlement and registration of land within a specified area. Following such publication of the notice, the head of the judiciary in the governmental region in question appoints a settlement officer and demarcation officers to carry out the demarcations, settlement and registration of the settlement area. The Act specifies in detail the principles to be followed in determining outstanding rights to and interests in land, and provides dissatisfied parties with a method of appealing the decisions reached. Provision is also made for the correction of information contained in land registries in the event of mistake or fraud.

**Limited Recognition of de facto Customary Rights of Land Ownership – Ḥiyāza**

Tribes, clans, families and rural dwellers could consider land as de facto their “own” in a communal or cooperative context. This de facto ownership (to be called ḥiyāza in Arabic) is recognized by Sudan’s current laws and jurisprudence as entitling communities to customary land rights. The term ḥiyāza is parallel to some degree with the concept of “occupation of unregistered land” in English law. Under the 1970 Land Registration Act, proof of ownership relies heavily on “occupation”, i.e., highly visible land use, as in agricultural fields and residential areas.

The Civil Transaction Act, 1984 has also dealt with the customary or communal ownership of land: it specifies certain criteria to be observed by the authorities when granting land. The CTA provides for the right to use government land or land belonging to another person through the legal right of usufruct (manfaʿa). This right of usufruct may be acquired through deed, inheritance, will or the exercise of possession. Whoever uses rural waste land, by cultivation, building or irrigation, becomes entitled to the usufruct of the land in question. The government also has an important role to play in granting usufruct on public land. As a landlord, the central and state governments in Sudan are charged with promoting land use, and towards that end have the power to

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34 Section 5 (as amended in December 1984).
35 **Ibid**. s. 84.
36 **Ibid**. s. 85.
divide, survey and register land. In terms of procedural law, committees at the federal and state levels, comprised of three to five members, are to handle the grant of usufruct on public lands. In granting the usufruct regarding agricultural land, the government authorities shall consider certain criteria related to pastures, agricultural enterprises and modern production techniques. The following shall be taken into consideration:

a. Preservation of villages, natural resources, animal health and natural pasture areas.
b. Preservation of small agricultural enterprises.
c. Large tracts of land should not be granted without a determination that the grantor will exploit the whole of such area in the most favourable way.
d. Protection of drainage and other services.
e. Particular land may be granted to more than one person, family or company for the purpose of agricultural production by modern techniques.

At the lower administrative levels, land allocation procedures stipulate a series of important responsibilities, although it is doubtful whether these local activities are adequately implemented under the law. The local level is instrumental in resolving land disputes. A Land Conflict Resolution Committee at the mahalliyya (locality) level deals with and resolves disputes over land. The customary authority must occupy a central role in these committees. In the case of an unresolved land conflict, the dispute is referred to the Civil Court. However, in terms of actual practice, it is pertinent to mention that there is a need for further investigation into how the law is applied in practice.

Land Grabbing and Encroachment on Communal Customary Land Rights under Sudan’s Investment Laws

There is a dynamic relationship between customary law and rights, and land grabbing, which normally encroaches upon the customary rights and ownership of rural populations. Land grabbing is one of the main causes of conflict: it amounts to a violation of human rights including the right to food. The UN Special Rapporteur on the right to food recommends a minimum set of

37 CTA, 1984, s. 560 (6).
38 Ibid. s. 566(1) (2).
39 Ibid. s. 561.
principles and measures based on human rights, with regard to large scale transnational land acquisitions and leases, more commonly referred to as “land grabbing” (UN Special Rapporteur 2009). These principles and measures are intended to assist both investors and governments in the negotiation and implementation of large-scale land leases and acquisitions, to ensure that such investment works for the benefit of the population including the most vulnerable groups, and is conducive to sustainable development, with the progressive realization of the right to food (*ibid*.). The measures are grounded in the principles of international human rights law, including the right to food and the right to development (*ibid*.).

According to estimates, between 15 and 20 million hectares of farmland in developing countries have been subjected to large-scale land acquisition involving foreign investors. This already represents the size of France’s farmland. Among the main target countries are sub-Saharan African countries such as Cameroon, Ethiopia, the Democratic Republic of Congo, Madagascar, Mali, Somalia, Sudan, Tanzania and Zambia.40 As far as Sudan is concerned, from the end of the 1960s onwards there was a massive horizontal expansion of agricultural production, accompanied by an expropriation of *de facto* community land and land grabbing associated with Mechanized Farming Schemes and the Arab Breadbasket Policy. It is estimated that approximately 25–30 million feddans were placed under mechanized cultivation (de Wit 2001). The SNCS for 1992–2002 played a part in marginalizing the rural household sector in favour of large-scale commercial and mechanized farming (*ibid*.). There is the assumption that there is still plenty of land available for horizontal expansion, and that this land can be placed under production without causing social tensions (*ibid*.). Sudan’s mechanized agriculture policy, which makes grants of vast areas of agricultural land for investment, sprang from a central desire to realize Sudan’s agricultural potential but did not give sufficient consideration either to the viability of the policy or, most importantly, its impact on local stakeholders, particularly the rural populations who have historical customary rights to land. The result is that it has not only failed to achieve its development objectives, but has actually served to undermine existing livelihoods and local development. There is no doubt that large-scale land investments can create opportunities for development, given their potential for creating infrastructures for employment, increasing public revenue and improving farmers’ access to technologies and credit. However, they may also have negative consequences. Potential impacts include: the eviction of land users who have no formal or statutory security of tenure over the land they have been cultivating.

40 For details see UN Special Rapporteur (2009).
for decades; the loss of access to land for indigenous peoples and pastoral pop-
ulations; competition for water resources; and decreased food security if local
populations are deprived of access to productive resources (ibid.). Also, the
policy of making large scale commercial farms available for investment is one
of the main factors leading to conflict between pastoralists and farmers, as the
expansion of such land has led to social tensions.

Unfortunately, Sudanese investment laws relating to the granting of land
for agricultural and development schemes do not consider the negative im-
pacts of allowing large scale commercial farms on local communities’ custom-
ary rights to land. The Encouragement of Investment Acts 1999 and 2013, for
instance, call for the promotion of investment, and, accordingly create favourable
conditions and privileges for investors, without taking into account the
customary land rights of local communities, particularly small farmers and
pastoralists. The Encouragement of Investment Acts 1999, for example, encour-
ages investment activity in areas such as agriculture and animal production.41
Furthermore, it categorizes these investment activities as “strategic activities”.42
In order to encourage investment, the Act endows the Minister of Investment
at the federal level the power to grant land for strategic projects free of charge,
or for nominal or “encouraging fees” for non-strategic projects.43 Strategic proj-
ects are exempted from paying taxation profits for ten years and the Minister
can increase this period if he or she determines it appropriate to do so.44 States
or localities are not allowed to impose taxation or dues on any investment proj-
ect. Similarly, at the state level, the Act gives the relevant Minister the power to
grant land free of taxation and dues (when they are imposed by a state law or
locality) for a period up to five years, renewable for a further five years.45 Such
projects are also exempted from any other state taxes and further land can
be allocated to the project on payment of only nominal or encouraging fees.46
Furthermore, the Minister may provide other favourable conditions for inves-
tors if projects are diverted to less developed areas: increasing the exports; real-
izing rural development and; creating work opportunities.47 Most importantly,
the law provides legal guarantees for the investor against nationalization or
confiscation of the project or land in the public interest without payment of

41 Article 7.
42 Article 9 (Strategic Projects).
43 Article 12.
44 Article 10.
45 Article 13 (1).
46 Article 13.
47 Article 16.
just compensation. Furthermore, confiscation of monies and the freezing of assets can only be implemented by judicial order.

Thus, the traditional smallholder sector of traditional land owners has been marginalized by policies and legislation since the Condominium Period, through Independence and up until the present, with encouragement of the territorial expansion of commercially based agriculture at the expense of this traditional sector. Along the way, this has caused evictions from land and forced resettlements (de Wit 2001). Thus, the granting of land and land management is a sensitive issue, as land should be distributed in a transparent manner and in accordance with well-defined legal procedures that guarantee the rights of customary ownership of land and ensure that land cannot be encroached upon by large-scale mechanized farming or land grabbing. The right to allocate land by the government is therefore a subject of primary importance, particularly in the light of the large amount of land owned by the state and it is unfortunate that the state in Sudan is not perceived as a neutral agent that distributes land in a transparent manner. Furthermore, the absence of rule of law, entrenched corruption, and the capacity of state institutions to administer land are all factors that encroach upon communal customary land rights. Sudan investment law outlined above provides many guarantees to the investors while completely ignoring communities’ customary rights, as well as the rights of small farmers and pastoralists. Land represents not only the main means to access and produce food for millions of stakeholders and their families, but is also an essential element in the very identity of certain peoples and communities. From a human rights perspective, any negotiations leading to investment agreements should be conducted in full transparency and with the participation of the local communities whose access to land and other productive resources may be affected by the arrival of an investor. Any shifts in land use should, as a matter of principle, be made with the free, prior and informed consent of the local communities concerned. In Sudan, it is noticeable that large-scale mechanized farming and other development projects owned by some powerful individuals have been granted huge tracts of land, which sometimes encroach upon ḥarām al-ḥilla (boundaries of villages and communities).

Recently, there has been increasing concern over development projects that result in forced displacement, such as the construction of the Merowe and Kajbar dams, which also prompted protests that were met with excessive force (EIPR 2013). The government of Sudan has also pursued a policy of selling off

48 Article 17 (1).
49 Article 17 (b).
land to Sudanese and foreign investors (Alden 2010). In addition to the loss of customary land rights, this practice (which is reportedly often accompanied by forced evictions), has impacted adversely on a series of rights of those affected, including the right to property and the right to housing (ibid.).

Thus, Sudan’s legal system, far from providing adequate protection against public acquisition and expropriation of land, actually facilitates these practices, particularly the Civil Transactions Amendment Act, 1990, and the National Investment Encouragement Act, 2013 (Babiker and Oette 2015, pp. 22–25). This practice also raises concerns over its compatibility with the rights of the Sudanese people to freely dispose of their wealth and natural resources under international human rights law and the regional African human rights regime.50

**Constitutional Guarantees of Customary Land Tenure under the Interim National Constitution 2005 and the CPA**

The Interim National Constitution 2005 provides in Article 186 (3) that “all levels of government shall institute a process to progressively develop and amend the relevant laws to incorporate customary laws, practices, local heritage and international trends and practices”. This important provision authorizes states to undertake a process of incorporating customary laws as part of their legal system. The Constitution also provides that the regulation of land tenure, usage and exercise of rights shall be a concurrent competence, exercised at the appropriate level of government.51 Furthermore, rights in land owned by the government of Sudan shall be exercised through the appropriate or designated level of government.52 The Constitution further provides for exclusive and concurrent powers to be exercised at the state level for the management of land resources. This includes state land, state natural resources and traditional and customary law.53 State governments, too, have legislative and executive competencies related to land tenure, agriculture, water and pastures to be

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50 Article 21 of the African Charter on Human and Peoples’ Rights (to which Sudan is a party) provides that “all peoples shall freely dispose of their wealth and natural resources. This right shall be exercised in the exclusive interest of the people. In no case shall a people be deprived of it”.

51 Article 186 (1).

52 Article 186 (2).

exercised concurrently with the National Federal government, including the regulation of land tenure and usage and exercise of rights in land.54

Although the Constitution provides for the aforementioned powers, no legislation is in place to reform existing land laws, despite the INC 2005 calling upon all levels of government to institute a process to progressively develop and amend the relevant land laws to incorporate customary laws, practices and local heritage.55 The enactment of new laws and the incorporation of customary law, as well as the institutional mechanisms to manage land rights, shall be developed, in the new context of decentralization of state powers guaranteed by the INC 2005. In particular, customary practices related to rural land tenure systems and land rights management are to be developed to enable individual or communal rights to be registered and protected. This is a priority, as disputes over land and related resources normally ignite violent local land conflicts. Unfortunately, the state has so far often failed to establish functioning land tenure systems.

Some states’ Constitutions call for the enactment of laws to regulate land ownership, land use and land rights and have the competence and powers to regulate government land as provided for in Article 186 (2) of the INC 2005.56 For example, the Sennar State Interim Constitution provides that the state “shall initiate procedures and gradual measures to develop laws related to land to include customary practices, local heritage and international trends and practices related to land.”57 This Constitution further provides for the establishment of a Land Commission with competences to adjudicate between disputing parties willing to be finally bound by the Commission’s decision, which shall be registered and enforced by courts. The Land Commission has also powers to adjudicate on land claims in the state and between parties who have interests in the disputed land but without prejudice to the competences of the judiciary. In performing its work, the Land Commission is required to take into consideration the principles of equity and justice.58 Similarly, the White Nile State Constitution provides for the enactment of legislation to govern land ownership and usage, as well as the integration of customary practices. However, no legislation has been enacted at state level to address customary

55 Article 183 (3).
56 See Article 88 (1) of Sennar Interim Constitution 2005.
57 Article 88 (3) of Sennar Interim Constitution 2005.
58 Article 88 of Sennar Interim Constitution 2005.
land rights. At the level of localities, legal orders can only be issued for approval by the state legislative orders.

Both the CPA and the INC 2005 provide for concurrent and exclusive powers regarding the regulation of land matters. Both documents call for the establishment of a National Land Commission and other state commissions to deal with issues pertaining to land ownership rights, land use, land tenure systems and land arbitration. The recently enacted Land Commission Act, 2009, deals with the relationship between the Commission and the ongoing rules and acts, the Commission’s membership, its technical mandate on land use, its semi-judicial mandate and its arbitration powers. However, the Commission is not yet operational to tackle urgent issues related to communal customary land rights and usage.

Sudan’s 2015 Constitutional Amendments and Their Impact on Land Tenure Regulations

As outlined above, questions surrounding the use of land have been at the heart of many communal conflicts. The CPA and the INC 2005 were meant to address at least some of these issues, but are largely acknowledged to have failed to bring about satisfactory solutions (Alden 2010). The INC 2005 was amended on the 4th of January 2015 and, unfortunately, article 186 was amended despite it forms a basis for developing a legal regime that considers good practice, with due regard to customary laws, practices and local heritage. The revised article 186 signals a markedly different approach (Babiker and Oette 2015). It stipulates that:

(1) Acquisition and exploitation of land and enjoyment of rights over it is a common capacity exercised across the relevant level of government according to the provisions of the law.

(2) The National Assembly shall approve the National Investment Plan.

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59 Article 186 stipulates: “(1) The regulation of land tenure, usage and exercise of rights thereon shall be a concurrent competence, exercised at the appropriate level of government.” Article 186 (2) provides: “Rights in land owned by the Government of the Sudan shall be exercised through the appropriate or designated level of Government.” Article 186 (3) provides: “All levels of government shall institute a process to progressively develop and amend the relevant laws to incorporate customary laws, practices, local heritage and international trends and practices.”
(3) The President of the Republic may, from time to time, issue Presidential
Decrees to define the lands to be exploited as investment, how the invest-
ment returns shall be used and the level of government concerned with
running it and enjoy rights over it, observing citizen rights [sic] and the
social responsibilities of investors.

The language of the amended article 186 (1) signals a shift in emphasis, mov-
ing from regulation, which ideally considers all relevant rights and interests,
to “acquisition and exploitation of land”, which is given textual priority over
the “enjoyment of rights over it” including customary land rights (ibid.). The
amended article 186 (2) and (3) places heavy emphasis on investment, and ex-
plicitly links land exploitation to investment. It thereby risks giving constitu-
tional approval to a policy of selling off land that gives scant respect to local
rights and interests (ibid.). This concern is reinforced by the provision in the
amended article 186 (3), which grants the President of the Republic the power
to use Presidential Decrees for the purpose of land exploitation, which, in light
of recent practice, enhances the risk of executive abuse of powers. The formula
“observing citizen rights and the social responsibilities of investors”, which the
President is to take into consideration when issuing Presidential Decrees, is
insufficient to provide adequate safeguards against abuse. “Citizens’ rights”
are not clearly defined. “While article 43 of the Bill of Rights of the INC 2005
provides for the right to own property and to prompt and fair compensation
in case of expropriation, it is widely acknowledged that the legal framework
relating to land rights is seriously flawed” (ibid.)

Conclusion

Land laws and the legislation that regulates access to land and land use has
invariably been designed to defend the interests of the state and not of the
rural communities. Land legislation in Sudan is excessive, confused and com-
 complicated; “arbitrarily used”; “excessively used for purposes of expropriation
of private land”; “something is wrong with our land laws and we need to ad-
dress this mischief” (de Wit, Tanner and Norfolk 2009, p. 8). It is apparent that
land reform and practice strengthening customary land rights have not taken
place to date in Sudan post the CPA era, despite constant calls for land reform.
Hence, there is a need for Sudan to undertake a comprehensive review of its
land laws, including investment laws, with a view to ensuring customary rights
of local communities, as well as conformity with Sudan’s obligations under in-
ternational human rights law.
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Colonial Moral Economy and the Discipline of Development: The Gezira Scheme and “Modern” Sudan

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While Evans-Pritchard was conducting his now-famous studies of the Azande and the Nuer in southern Sudan, changes were taking place in the great plains between the two Niles south of Khartoum that would forever alter the course of Sudanese history. In partnership with multinational capital, the British were turning millions of acres inhabited by farmers and pastoralists into a vast irrigation project dedicated to the production of cotton. The Gezira Scheme, which ultimately became the largest centrally-managed irrigation project in the world, started operations in 1925 and continues to operate today under the management of the Sudanese government.

Although countless studies have focused on the economic organization and policies of the Gezira Scheme, this article yields a new understanding of the Gezira Scheme, and perhaps of development projects in general, by shifting attention to the symbolic dimensions of such monumental projects. If, following Mitchell (1988), one views the mission of colonialism as that of literally and figuratively creating a world order and ordering the world, the Gezira Scheme is a stunning exemplar. Its miles and miles of irrigation canals and uniform fields stretched out in a huge grid dominate space, its rigid schedules for agricultural operations command time, and above all, its hierarchy of inspectors and bureaucrats supervising, documenting, and disciplining strive to control the people of the Gezira. In Foucault’s terms, the Gezira Scheme must be understood as a “disciplinary institution” (1979:139). For this and its implications to become clear, however, requires us to decenter the ostensible economic purpose.

[T]he government... decided... to reorganize their lands into a large agricultural project which the government itself, with all its power and authority would supervise.... Suddenly they found their village alive with land surveyors, engineers, and inspectors.

—Tayeb Salih, The Wedding of Zein

of the scheme and to attend to its cultural qualities, viewing economic interaction as merely one medium through which complex social relations are enacted and negotiated. Recognizing the agricultural project as a disciplinary institution reveals the role of the Gezira Scheme in inscribing colonial social relations, most particularly relations between rulers and ruled.1 In this sense, the Gezira Scheme formed part of the larger colonial effort to establish a political order and constitute relations of authority. My interest here is not so much in the role the Gezira played in the colonization of Sudan, however, but rather how such a project—apparently concerned with specific economic goals—is suffused with noneconomic values and cultural meanings.

Studies of peasant moral economy have drawn attention to the meanings and values inherent in the economic behavior of peasants. Their behavior is often implicitly contrasted with Western economic behavior, which is assumed to be guided by rational calculation and narrowly economic goals (Ouroussoff 1993; Scott 1976). Such assumptions are evident in colonial statements about the Gezira Scheme and much of what has subsequently been written about it. Attention to the cultural meanings expressed by the British through and about the Gezira Scheme suggests, however, that such projects embody far more than the simple economic interests of a ruling power. They can be seen as expressions of what might be called a “colonial moral economy.”2 Rationality and calculation, moreover, are revealed to function not as the principles governing colonial actions, but rather as tropes through which the colonizers represented Western culture and distinguished themselves from those they colonized. Colonial discourse on agricultural development can be read as cultural assertions having as much to do with the social relations of empire as with the cultivation of crops. Furthermore, while the Gezira Scheme was not very successful in terms of economic performance, it succeeded in the colonial era as a monument to economic modernization and the values of rationality, discipline, and order, and has continued to function as a potent symbol of progress and state power until today.

Central to my investigation of the colonial moral economy of the Gezira Scheme is Arthur Gaitskell, who began work in the Gezira in 1923 and managed the Scheme from 1945 until his retirement in 1952.3 Gaitskell’s 357-page book, Gezira: A Story of Development in the Sudan, was published in 1959 shortly after Sudan achieved independence and draws on Gaitskell’s own experience, as well as on other colonial sources. If for no other reason, the case of the Gezira Scheme presents an interesting site to explore colonial moral economy because one of the key actors wrote so voluminously about it. Indeed, the very fact that Gaitskell believed that the Gezira Scheme merited such a book is telling. The Gezira Scheme was not just business; it was a monument of great material and symbolic proportions. In the preface to his book, Gaitskell states that the fundamental theme of his account is the question, “What does the western world stand for?” (1959:22).

Although the Gezira Scheme has been the subject of much research (several books [Barnett 1977; Barnett and Abdelkarim 1991; Gaitskell 1959], numerous articles [for example, Barnett 1981; Ebrahim 1983; Taha 1973; Versluys 1953];
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Voll 1980], and countless theses), the questions explored have been relatively narrow in scope (see Bernal 1990 for one critique). Gaitskell's book is widely cited as a reference on the Gezira, yet scholars extract tidbits of information from it without any critical analysis of the text or its premises. Much of the contemporary social science literature on the Gezira has been dominated by debates over the class position of tenants and by neo-Marxist concerns with relations of production and exploitation (Abdelkarim 1985; Al-Arifi 1975; Barnett 1977; O'Brien 1984; Tait 1978). The concern with the culture of power that is addressed obliquely in this literature is taken as a central focus here.

The analysis presented here takes Gaitskell's text as a product of colonial culture and draws on it, along with other sources, to explore both the symbolic meaning of the Gezira Scheme for the colonizers and the ways in which the Gezira inscribed the social and political relations of colonialism. While colonial accounts accord little attention to Sudanese perceptions of the cotton scheme, we are nonetheless able to gain some sense of the conflict between the colonial order and its Sudanese subjects from these texts. Further insights into how Sudanese experienced such schemes are drawn from oral histories of farmers in the village of Wad al Abbas, where I conducted fieldwork in the 1980s. Wad al Abbas is located across the Blue Nile from the Gezira and was incorporated into a cotton scheme patterned after the Gezira in 1954. The case of the Gezira Scheme suggests that development projects are not solely nor even primarily economic in nature, but disciplinary institutions that establish authority, encode moralities, and order social relations. The Gezira Scheme shares these characteristics with many postcolonial development projects.

Imperialism, (Agri)culture, and Modernity

The opening page of Gaitskell's book is a map. This is not an ordinary map, however, but one that shows key features of the Gezira region set within the map of England (see Figure 1). On this map, the Blue Nile runs from the Sennar Dam at London to meet the White Nile, some distance east of Liverpool, at Khartoum. While the ostensible function of the map is to acquaint British readers with the vast scale of the Gezira, this is not all it accomplishes. By relocating the Gezira in England, the map simultaneously claims the Gezira for Britain and removes it from any larger Sudanese context. In this respect, the map illustrates something about the way the Gezira plain and its inhabitants were treated by colonial policy makers. While British colonial discourse about the development of irrigated cotton production in the Sudan represents agricultural development as a scientific and economic matter, the product of purely rational calculations, British plans for the Sudan were intricately bound up with colonial assumptions about world order and Britain's right to order the world. In this context, science and rationality were key elements of a discourse through which the British represented themselves as progressive and modern, and by doing so, set themselves above what they considered to be the inferior "traditional" world of the Sudanese.

Gaitskell's narrative begins by likening Sudan in the early 20th century to "childhood impressions of the Old Testament" (1959:28–29). He mentions the
“poor man’s goat, the rich man’s herd of camels, ... the patriarchal dignity of elders,” and the “sense of immemorial custom repeated and fate accepted.” Gaitskell then declares, “It was upon this setting that a uniquely new system of agriculture was to descend” (1959:29). His biblical references and use of the word descend almost lead one to believe that the irrigated cotton schemes fell to Sudan from heaven rather than from the drawing board of British colonial officials, engineers, and assorted businessmen. Gaitskell, in fact, constructs a manifest destiny for the Gezira region as divinely intended for an irrigated megaproject. The Gezira plain, he writes, “seemed predestined by nature for the purpose [of irrigation]” and “the country seemed especially suited by nature for cotton production” (Gaitskell 1959:39–56). That Sudan’s economy at the time was organized around the production of grain and livestock was, apparently, of no particular significance.

The origins of the Gezira Scheme and the imposition of a new productive regime upon rural Sudanese are naturalized by Gaitskell’s account, in which the
forces of nature and destiny take the place of human agency. In much the same way, other colonialists of the period, such as Major Lee Stack, write of "the interests of the Sudan" as if the territory itself demanded to be acted upon in certain ways (quoted in Gaitskell 1959:64). The people of Sudan as agents of their own destiny are noticeably absent from phrases like "poverty-stricken Sudan, searching to raise its standard of living" (Gaitskell 1959:58). Phrases such as these invest the anthropomorphized country with the human capacities denied its people.

In fact, if Sudan-the-country was to realize its manifest destiny, the people of Sudan, who appeared unaware of nature’s or God’s grand design for their territory, posed something of an obstacle from the British standpoint. Ignorance, superstition, and fanaticism are common themes in colonial descriptions of the Sudanese, suggesting a contrast to the rationality of “civilized” man. As a British journalist expressed it in 1904, “The chief difficulty [facing the British in Sudan] is the universal ignorance and superstition that prevail” (Peel 1969[1904]:271–272). Government reports on the people of the Gezira describe them as apathetic, lazy, and fanatical (Gaitskell 1959:89).

In 1900, Sir Reginald Wingate, governor-general of Sudan, saw the Gezira plain covered with sorghum and wrote, “If a system of irrigation were feasible in the Gezirah, it would become a huge granary capable of supplying not only the whole Soudan but other countries as well” (quoted in Gaitskell 1959:36). Sudan was in fact exporting grain at the beginning of this century (Daly 1986). But if, in the British view, manifest destiny demanded the irrigation of the Gezira, the capital-intensive irrigation plans (which called for damming the Blue Nile at Sennar) could only be justified economically by the production of a lucrative cash crop. Through this circuitous reasoning, irrigated cotton production emerged as the only “rational” use of the Gezira plain. Cotton production made economic sense because of an active world market in the crop and because of its use by British industry. However, there was little consideration of any alternative, even after cotton proved to be more difficult to produce under Sudanese conditions than expected and world market prices fell far below those used in the original calculations of profitability.

Despite the rational planning and technical expertise involved in designing the Gezira Scheme, this megaproject had a powerful lure for colonials quite apart from what it could produce. Indeed, the Gezira project captured British imaginations, being compared to the Panama Canal (Himsbury 1923) and even to the pyramids, in an article in the Manchester Guardian (Ransome 1925). The Gezira Scheme represented the triumph of modern civilization over nature and ignorant tradition, which in practice meant the imposition of colonial order on the Sudanese landscape and society. Colonial officials could hardly describe the Gezira project without some reference to its “rational,” “scientific,” and “modern” qualities. For example, Sir Geoffrey Archer, governor-general of the Sudan in the 1920s, described the Gezira Scheme as the application of “western science to native economic conditions” (quoted in Gaitskell 1959:91). Gaitskell himself wrote,
By establishing control over the land use and insisting on terms of tenancy related to an economic unit on a sound agricultural rotation the Government introduced to a peasant society advantages normally available only to large-scale estate management, for on to such a base could be grafted efficiently the instruments of modern agriculture. [1959:86, emphasis added]

Science, rationality, and efficiency were present more in spirit than in substance, however; indeed, one might say they were invoked like Western deities whose names alone could confer blessings on the endeavor. Colonial calculations of both the cost of capital works and the value of the cotton the Gezira Scheme could produce were far off the mark and had to be revised repeatedly. F. T. Hopkinson, an engineer sent from England to report on the construction of the scheme, found rampant mismanagement, overstaffing, and inefficiency (Daly 1986). According to Hopkinson, official estimates bore "no relation to the actual cost" of the scheme's construction (quoted in Daly 1986:424). He called the statistical data "voluminous, but unconvincing" and accounting procedures "hopelessly bad" (quoted in Daly 1986:423). Cranes stood idle while human labor was used to move tons of stone. Twenty-five thousand men were hired, when 7,000 would have sufficed (Hopkinson, quoted in Daly 1986:441).

The cotton produced on the Gezira ultimately benefited the British textile industry, but for the colonials involved in administering Sudan or the Gezira Scheme itself, the realization and perpetuation of this mammoth project were goals in their own right. The overall profitability of the scheme was never clear in strict economic terms. Every time the actual cost of the Sennar Dam and associated major works appeared out of proportion to what the scheme might be expected to yield, the proposed size of the irrigated area was increased, thus reducing the relative cost of the dam and making balance sheets appear more favorable (Daly 1986; Gaitskell 1959). What is more, as it turned out, the colonial government ran a loss on the Gezira Scheme nearly one out of every two years from the Gezira's inception in 1925 up to 1946, when there was a world market boom in cotton prices (Gaitskell 1959:268).6

Understanding the Gezira Scheme as, among other things, a means of constituting and expressing power relations, goes a long way in explaining the perpetuation and expansion of the Gezira and similar schemes under colonial rule and after it, despite their checkered records of economic performance.7

Erasures and Inscriptions: Establishing the Gezira Scheme

Its aim was to establish presences and absences, to know where and how to locate individuals, to set up useful communications, to interrupt others, to be able at each moment to supervise the conduct of each individual, to assess it, to judge it, to calculate its qualities or merits.

—Michel Foucault, *Discipline and Punish*

The establishment of the Gezira Scheme was both an expression and an extension of British power. One distinctive feature of the Gezira (and of the smaller schemes that preceded and prefigured it) is the extent to which the British ignored
preexisting patterns of land use and social organization. If knowledge is power, so too is the more insidious power to remain ignorant that is the privilege of rulers. The Gezira Scheme represented an attempt not simply to remake or reform rural Sudanese society, but to create a (colonial) Sudanese society: a homogeneous society of hardworking and disciplined peasants. The British did not try to transform local practices so much as obliterate them, starting literally from the ground up, with new systems of production and productive relations of their own design. The schemes were enclaves where colonial control permeated the very conditions of people's daily lives to an extent the colonizers were unable to achieve elsewhere.

The first colonial cotton scheme in Sudan was started in 1906 by a U.S. industrialist, Leigh Hunt, who was granted a land concession at Zeidab by the colonial state. Hunt formed a private company, the Sudan Plantations Syndicate, which, in partnership with the colonial government, developed irrigated cotton schemes in Sudan, including the Gezira. After various failures, including Hunt's proposal that "skilled negroes" from the United States be encouraged to immigrate to Sudan as "technical demonstrators" for Sudanese peasants, a tenancy system was adopted at Zeidab. (Unfortunately, we learn nothing more about Hunt's fantastic plan from Gaitskell than that "Only one batch of negro emigrants was tried" [1959:52].) The tenancy system, however, was not an immediate success. Sudanese farmers refused to participate, and the initial tenancies were given to Egyptian peasants instead. But in 1908, the Zeidab Scheme gained Sudanese tenants by incorporating land that local farmers had been irrigating by sagia (animal-driven waterwheel). According to Gaitskell (1959:53), having seen the scheme in operation, these farmers now welcomed the prospect of pumped water. But farmers got much more than simply a new water supply.

In 1911, another scheme was established at Tayiba in the Gezira plains jointly by the British government and the Sudan Plantations Syndicate. Other pump stations in the Gezira, under the same British-Syndicate management, followed: in 1914 at Barakat, in 1921 at Hag Abdulla, and in 1923 at Wadi al Nau. While the early pump schemes were relatively small, leaving local inhabitants with viable resources outside their bounds, the Gezira Scheme claimed thousands of hectares previously inhabited and exploited by Sudanese peasants and pastoralists. When the Sennar Dam finally was completed in 1925 and the Gezira Scheme opened, the scheme covered an area of about 240,000 feddans, or 100,800 hectares (ILO 1976). Local inhabitants lost control of their land, receiving tenancies as compensation. By appropriating land and instituting a new agrarian system, the British essentially destroyed indigenous farming systems and disrupted pastoral systems. This did not mean, however, the definitive triumph of colonial discipline. In fact, new forms of social relations and economic diversity were developed by the Sudanese in and around the cotton schemes (Bernal 1991, 1995).

Research on the Gezira plain prior to the scheme, unfortunately, is scanty. Even rather basic facts are in dispute. It is evident, however, that the Gezira plain was part of a complex and diverse economy. In contrast to the colonial vision
of a self-contained society of cotton-producing peasants stretching for miles and miles of monotonous and precise irrigation canals, Sudanese used the Gezira plains in a variety of ways, and the region was part of a larger system of social and economic relations. While the British regarded Sudanese farmers as ignorant, the few accounts that we have of indigenous agriculture in the Gezira (Randell 1958) and neighboring areas such as Wad al Abbas (Bernal 1991) indicate that local farmers had developed a rather complex system. Riverain farmers took advantage of three different seasonal and ecological zones: rainlands, riverbank floodlands, and island land that emerged from the Blue Nile when the water level dropped. Nomads used the Gezira as seasonal pasture. Trade routes traversed the Gezira, connecting it to Omdurman and parts north and to Ethiopia to the southeast, as well as to the Red Sea in the northeast (Bernal 1991; Randell 1958). The Gezira also lay on the overland pilgrims' route from West Africa to Mecca. The movement of nomads, traders, and pilgrims connected the Gezira to a much wider area and made it a multiethnic crossroads of sorts (Barnett 1977). The area participated in an active trade in the sorghum and livestock it produced, and gave rise to local market centers (Shaw 1987).

The British, however, treated the Gezira plain as a tabula rasa on which they could inscribe their vision of a society of yeoman farmers organized around the central function of cotton cultivation. The Gezira Land Ordinance of 1921 essentially abrogated existing land rights and reallocated land as irrigated tenancies for cotton production. Under the terms of the Ordinance, land was compulsorily rented from its original owners by the scheme (at the rate of 10 piastres per feddan); in return, landowners were entitled to irrigated tenancies. In Gaitskell's view, "the Ordinance provided an ingenious way of establishing control over land without outraging the traditional right of proprietorship" (1959:85). Sudanese scholars see it somewhat differently: Khalafalla argues that land tenure regulations of the Gezira Scheme "amount to nationalization of land by the colonial administration" (1981:67). And Saeed writes, "The Gezira Scheme marked the beginning of the alienation of a large number of Sudanese peasants (cultivators and herdsmen) from the means of production they hitherto owned" (1982:88). A Wad al Abbas villager I spoke with in 1981 recalled the establishment of the scheme there with what might be termed "outraged proprietorship": "They didn't buy the land. They said it belonged to the government. No one talked to them; they just came and took the land.... People said, 'Give us money for the land,' but they said, 'No, we will give you tenancies.'"

The effect of the Gezira Land Ordinance was not to create a landless population but a highly controlled peasantry. Standard tenancies of 30 feddans (12.6 hectares) were allotted to Gezira natives and others, including West African immigrants. Within each tenancy, 10 feddans were to be under cotton, 10 fallow, 5 under sorghum, and 5 under lubia, a fodder crop, in yearly rotation. Scheme regulations required tenants to cultivate cotton and permitted the cultivation of the other crops. Irrigation, plowing, and other inputs were supplied by the scheme, which controlled the ginning, grading, and marketing of cotton (Barnett 1977).
Gaitskell himself wonders at how colonial authorities were able to take over the land of the Gezira and establish the scheme without direct violence. He speculates that the inhabitants "accepted the fact that the land had been conquered in war and was being administered with equity in peace" (Gaitskell 1959:90). This one line of Gaitskell's entire 357-page account situates the imposition of the Gezira Scheme in the context of brute military force. It is one of the great artifices of the Gezira that it could simultaneously make British power manifest, yet invisible. Barnett's description of the Gezira scene in the 1970s is worth reading here for the sense it conveys of a man-made order so totalizing that it becomes natural:

As an environment, the Gezira is an area which is entirely dominated by the use to which man has turned it. It is so as much as any of the grimy Lancashire cities whose needs [for cotton] brought it into being. As the mill loomed over the artisans' mean dwellings in Manchester or Bolton, so the technology of production is an ever present feature of the scene in the Gezira—so omnipresent as to become almost invisible and unnoticed. After a while one has to remind oneself that the geometry of the irrigation channels is a symbol of man's ordering of his environment, rather than a natural part of that flat expanse of clay. [1977:28]

British power was physically represented by the vast gridwork of Gezira canals and miles of ordered fields. Moreover, through the daily operation of the scheme, the relations between rulers and ruled were hidden in the routines of irrigation cycles, farming schedules, and accounting practices.

Accounting and the Colonization of Tradition

While treating the Gezira region in practice as a cultural and agricultural tabula rasa, the British selectively drew on local "tradition" as a rationale for colonial policy. Even as they represented the Gezira Scheme as "modern, scientific" agriculture, colonial authorities sought to legitimate some of their policies by locating their origin in Sudanese "tradition." The creation of tradition by colonial authorities was common in Africa (Ranger 1983) and is well documented in British India as well (Cohn 1983). "Tradition" was useful to the colonial architects of the Gezira Scheme because it could conceal (from themselves and perhaps, they hoped, from the Sudanese) the essentially arbitrary nature of the economic arrangements regarding cotton production, which ultimately were premised on nothing but the power of the colonizers to impose their terms on Sudanese farmers. The realities of economic and social life in the Gezira region prior to the irrigation scheme were not well documented. Thus colonial actors could alternatively ignore or invent the local past and Sudanese "tradition" as it suited them. This "tradition" was, however, entirely a part of the colonial order. Divorced from its context, distorted, and rigidly fixed as part of a vast bureaucracy, such "tradition" represented imperialist understandings of social and economic relationships rather than Sudanese ones.

One example of how Sudanese tradition was invented to support colonial authority is the policy on paying farmers for cotton. Initially, tenants at Tayiba...
(the pump station in the Gezira that preceded the large-scale scheme) were charged a flat rental rate for land and water and received the rest of the cotton profits. However, the Inspector of Agriculture, W. A. Davie, who ran the Tayiba project, proposed to colonial authorities that rent be abandoned in favor of a "share partnership" (Gaitskell 1959). The character of this "partnership" is illustrated by the fact that Sudanese tenants were not consulted about this change; it was simply imposed while cotton cultivation was underway.

The profit-sharing system first introduced at Tayiba is no small matter because it remained in effect on the Gezira without significant alteration until the early 1980s and was adopted on other colonial and postcolonial schemes (Ali 1983). According to Davie, profit-sharing on cotton was based on a traditional crop-sharing agreement that governed sagia (cattle or camel-driven waterwheel) cultivation. "Local sheikhs" at a place called Fedassi described this to Davie as follows: landowner one-tenth, owner of sagia one-tenth, owner of cattle two-tenths, supplier of fodder two-thirtieths, supplier of seed and implements four-thirtieths, and farmer four-tenths of the harvest (Gaitskell 1959:69). Sudanese tenants, like the farmer, would receive 40 percent of the profit from the cotton they produced, while the British Government and the Syndicate would receive the remaining 60 percent, which they further divided into shares of 35 percent and 25 percent respectively. (This was later modified to give the government 40 percent and the Syndicate 20 percent [Daly 1986:425].)

Sagia cultivation was, however, only a small element of the Gezira economy, and the relations governing sagia cultivation were thus not those under which the majority of Gezira farmers operated.15 Moreover, the division of cotton profits on the schemes, while seeming to conform to "tradition," departed from it significantly. For one thing, the sagia agreement dealt with the division of crops, not profits. The sharing of profits introduced several new elements, the effect of which was to alter the practical meaning of share partnership profoundly. One new element was the great risk (absent from the crop-sharing arrangement) of the uncontrollable fluctuation of world market cotton prices. Furthermore, while the division of a crop into shares is a fairly simple matter, the calculation of production costs and sales profits is not. The accounting system to determine these costs and profits was designed and controlled by colonial authorities, and the importance of accounting in this "profit-sharing" system gave colonials a power over tenants' cotton returns that no party to the sagia arrangement could possibly have had.16 We learn something about how Sudanese experienced "profit-sharing" from Gaitskell, who reports that some Gezira tenants had the "impression that the proceeds were some external mystery managed by others for whom they did the work" (1959:188).17 A 1937 article in a Sudanese-owned newspaper describes the tenant as submerged by doubt "when he sees that the Syndicate's 20 per cent share results in palaces and luxury ... while he suffers from the greatest penury in spite of his 40 per cent" (Ai Nii 1937, quoted in Barnett 1977:121).

With the highly variable yields that characterize Sudanese irrigated cotton production, arrangements for the sharing of risk are as important as those for the
sharing of profit. Davie does not mention how risk was shared under the sagia agreement. I suspect it was borne equally by all parties, as it is today in sharecropping arrangements in Wad al Abbas. There, whether large or small, the yield is simply divided, and if the crop fails entirely, landowner and sharecropper both endure the loss; no debts are incurred. Under the colonial system, however, a farmer could end up in debt to management after a year of laboring on cotton, if the sale value of the cotton failed to cover the cost of inputs supplied by the scheme. Debt was, in fact, quite common among tenants even from the early days of the Zeidab Scheme. Furthermore, on the scheme, the individual tenant was assigned a disproportionate share of risk. Under the accounting system created by the British, instead of tenants receiving 40 percent of the profit from their own cotton, the Syndicate was permitted to deduct any debts owed by tenants from any money due tenants. This meant that the cotton profits of the most productive tenants were taken to cover the cost of scheme-supplied inputs to the less successful or more resistant farmers. (The debts were, nonetheless, still liable to collection from future profits of the individual debtors [Gaitskell 1959:147,157].) Among other things, this artificial construction of all tenants as a community whose profits and losses are pooled stands in contradiction to the economic individualism the British saw themselves as fostering.

The British, moreover, outlawed the Sudanese system of agricultural credit, a crop-mortgage practice called shayl (Shaw 1966a); but they did not eliminate the problem of peasant debt so much as centralize it under their own control, setting up colonial bureaucracy in place of the local moneylender. Unlike a farmer’s relation to the local moneylender, the relationship between tenants and scheme administration was not governed by shared membership in a community but was unilaterally defined by the British. This they did in strict legal terms that were spelled out in a contract comprised of two documents, the “Tenancy Agreement” and “Standard Conditions of Tenancy,” that each individual tenant had to sign or, in most cases, mark. The rigid, unilateral definition of economic obligations by colonialists contrasted markedly with indigenous economic practices that, through flexibility and ambiguity, acknowledged the wider context of relationships in which economic activities were embedded. Given this cultural difference, Barnett points out that the profit-sharing arrangement on the Gezira Scheme certainly departed from tradition, since “in whatever way the traditional sharing arrangement had operated, one thing is certain. Rights and obligations within it would have been diffuse” (1977:93).

The contingent and negotiable character of indigenous economic relationships is evident in the practice of shayl, which reportedly entailed “an intricate social relationship between the moneylender and the cultivator. [The lender] is in constant touch with the farmer and is a built-in feature of village society. [T]here develops a kind of moral obligation on the part of the lender to help out in times of need” (Shaw 1966a:D58). By contrast, tenants on the Gezira Scheme received credit only for expenses of cotton production. The credit was, furthermore, limited to fixed amounts for particular agricultural operations, and payable only after cotton fields had passed an inspection certifying that the operation
had been satisfactorily performed. (Despite these restrictions, Wad al Abbas farmers managed to divert credit to consumption expenses and other pressing needs. It is likely that some Gezira farmers did likewise.)

Despite their selective references to "tradition," the British imposed a new system of economic and social relations upon Sudanese farmers, one that in key respects was at odds with Sudanese practices and understandings. It would be wrong to construe this, however, as a case of simple opposition between the moral economy of the colonized and the "rational" economy of the Western colonizers. The British objected to Sudanese ways on moral grounds as well as on grounds of efficiency. The British saw the flexibility and the personal relations of dependence and authority that characterized Sudanese society as inherently inferior and corrupt. Thus, for example, a District Commissioner in the Blue Nile Province declared that the institutions fostered by indirect rule were inefficient "because they are based on the hereditary system and suffer from the disease of nepotism" (quoted in Gaitskell 1959:215). A British journalist, comparing the tax policies of past regimes with that of the British in Sudan, asserts,

The uncertainty of the amount [of tax] to be paid had, however, an attraction for the Oriental. It varied with the circumstances of the year. The Government wished to get all they could, and in a good year they exacted a most excessive amount; but in a bad year they took little or nothing at all. According to the Western system a fixed moderate amount has to be paid every year, and a whole or partial failure of the crop is not considered any excuse. The latter system is, of course, far the most just and economically sound. [Peel 1969(1904):248-249]

While it is easy to see how the rigid, legalistic form of scheme regulations was derived from British colonial culture, the content of those regulations cannot be derived solely from the dictates of efficiency or profit maximization. However definitive once in place, scheme regulations were somewhat arbitrary and, as in the case of Davie's suggestion of "share partnership," seemingly implemented with little forethought. Nor were scheme procedures subject to the regular review and revision that efficiency would seem to demand; on the contrary, scheme regulations codified an unquestionable authority. (Indeed, it is striking how little reform the schemes have undergone to this day.) The administrative structure and policies of the Gezira Scheme were not so much governed by some pure economic rationality as they were governed by a different moral principle: that of British supremacy over Sudanese.

The imposition of colonial authority, whether cloaked in "tradition" or not, was contested by Sudanese farmers in various ways. Farmers at Zeidab initially declined tenancies. And when (prior to the introduction of profit-sharing) the cotton crop at Zeidab failed, farmers were so adamant in their refusal to pay rent that the Sudan Plantations Syndicate resorted to cutting off irrigation to some farmers and suing others in court (Gaitskell 1959:68). Gaitskell also reports that "to get tenants at all rents had to be low, for the idea of a fixed rent, regardless of the crop yields attained, was strange to the local cultivators, accustomed as they were to the considerable fluctuation in yield from natural hazards, locusts
and variations in the river” (1959:68). When the first pump scheme was set up in the Gezira in 1911 as a precursor to the Gezira Scheme, once again not a single local farmer accepted a tenancy. While Gaitskell attributes this to locals being “quite averse to change,” he nonetheless reports a quick reversal after a drought made tenancies look appealing (1959:59).

Counter to colonial assumptions that Sudanese farmers were bound by tradition, tenants’ awareness of their self-interest was evidenced by their vehement opposition to the introduction of profit-sharing at Tayiba in 1913. Far from finding comfort in “tradition,” tenants protested the shift to profit-sharing, which by one estimate would reduce tenant returns by 70 percent (Gaitskell 1959:71). Tenants submitted a petition of protest and caused a “disturbance” of enough magnitude that the provincial governor, Major Dickinson, was moved to investigate. In his report, Dickinson quotes a local farmer as saying, “We don’t know what may be done next. We may be handcuffed and marched off to prison for all we know” (Dickinson, quoted in Gaitskell 1959:71). Tenants in fact felt imprisoned by the scheme itself, as expressed in a saying Gaitskell recorded, which goes:

The man on the East Bank [of the Blue Nile across from the Gezira Scheme] is free like a nomad,
The man on the West Bank [that is, on the Gezira Scheme] is like a soldier in a camp. [quoted in Gaitskell 1959:156]

Since we have little record of how Sudanese experienced the imposition of the Gezira Scheme, these fragments have great value. Further insight into Sudanese perceptions can be gleaned from a memorandum by W. P. Clarke, who in 1920 was the first colonial commissioner to contact local people about the introduction of the Gezira Scheme. Clarke paraphrased opposition to the scheme this way:

“The Sheikhs will be nobodies, the Syndicate inspectors will be kings of the country.” ... “I now own and control large areas of rain land, under the new scheme I shall have only 30 faddans just the same as my slave.” ... “We hate these straight lines, we would rather be hungry once every few years, with freedom to range with our cattle unconfined, than have full bellies and be fined if we stray outside these horrid little squares.” ... “Our children and our slaves will become swollen headed and no longer regard our authority.” [Clarke, quoted in Gaitskell 1959:202]

From these various sources, it is clear that while the colonizers justified the scheme primarily in terms of economic rationality and secondarily in terms of “tradition,” Sudanese farmers consistently perceived the scheme as a new system of political and social relations. Prior to the scheme, Gezira inhabitants of some stature (such as the shaykhs and slaveowners quoted by Clarke) feared its impact upon their authority. We do not know how less powerful Sudanese such as slaves and women perceived the scheme, because the sources privilege elite
voices. Once the scheme was established, tenants all stood in much the same subordinate relation to scheme officials, and tenants experienced the scheme as a system of domination. The tenants quoted by Dickinson and Gaitskell above, for example, clearly were not simply complaining about the arrangements for determining cotton profits, but were critiquing the unequal power relations at the heart of the scheme. Whether represented in the guise of modernity or of tradition, the colonial order of the cotton schemes had the same meaning for the Sudanese: subjugation to British authority.

Emancipation and the Construction of Colonial Authority

[The scheme] provided him [the tenant] with seed, fertilizer, spraying, and supervision.

—Arthur Gaitskell, Gezira: A Story of Development in the Sudan

The colonial vision for the Gezira was that of a stable, economically homogeneous society of yeoman farmers. Sudanese farmers, rather than colonial planters, were to be the backbone of the Sudanese economy, for, as Lord Edward Cecil put it, “The Sudan is not, and never will be, a country suitable for permanent European habitation” (quoted in Gaitskell 1959:46). In 1904, the governor-general reported that the majority of British officials in Sudan favored “creating a peasant proprietary class” (Gaitskell 1959:43, emphasis added). And as J. A. Gillan, civil secretary in the 1930s, expressed it, “The Sudan (thank God) can never be a naturally industrial country” (quoted in Sanderson 1985:115, emphasis in original). Just as the British considered Sudan especially suited for cotton production, they considered the rightful occupation and status of the Sudanese to be those of a peasant, regarding any Sudanese of stature with suspicion.

Colonial officials generally disdained “traditional authorities” and were even more contemptuous of Sudanese who, through education, urban living, or other means, acquired Western-style knowledge or sophistication (Collins 1984; Daly 1986). Towns were regarded as “nurseries of corruption,” subversion, and a ‘bastard’ Westernization that was culturally and morally damaging to Sudanese” (Sanderson 1985:115). The British specifically sought to avoid creating an “effendi class” of indigenous administrators (Daly 1986:362). Indirect rule was used to limit the need for educating Sudanese and to isolate educated Sudanese from political power (Daly 1991). In British terms, schools produced not educated Sudanese, but “half-educated natives” (Daly 1986:379). Implicit in this expression is a racist notion that, in effect, there is no such thing as an “educated native,” since regardless of training a native is always a native, and therefore always less than a (Western) person. The British likewise considered well-to-do Sudanese, such as merchants and landlords, to be thoroughly reprehensible. Local elites were seen as backwards and lacking in integrity. British leadership was thus necessary to set Sudan on the path to progress (Fuller 1984). While the British debated among themselves about the form their leadership should take (the role of private capital versus government and the role of
Sudanese—particularly educated Sudanese—in administration were key issues), Sudan’s “need” for British rule was unquestioned.21

The Gezira Scheme can be regarded as an important material representation of the kind of “modern” society the British wished to create in Sudan. In some respects, it contrasts with British policies toward areas not targeted for development, where efforts were made to strengthen or even create “traditional” systems (a project for which indirect rule was especially suited). Gaitskell, in fact, derides some colonial officials for interpreting their function as that of “game wardens in a Garden of Eden” (1959:203). As a paramount colonial creation, the Gezira Scheme brings colonial social relations into relief, revealing the contradictions between the drive to incorporate subjects yet distance them (Thomas 1994), and the need to transform the economy, yet keep social and political change in check.

From the 1920s on, the British followed a policy of indirect rule in Sudan under the rubric of “native administration.” Sudanese were kept out of all but the lowest level of colonial administration, with Egyptians filling the positions not held by British administrators (Daly 1991). Although the colonial state had granted land for pumping stations to prominent Sudanese in the White Nile Province, this policy, in Gaitskell’s view, “had an affinity with the oil and land sheikdoms of the Middle East and it carried the great danger of corrupting traditional authority rather than revitalizing it, producing pashas in Packards rather than a progressive peasantry” (1959:203, emphasis added).

On the Gezira Scheme, an even stricter policy was pursued than elsewhere in Sudan, one that reserved all positions of authority for British colonialists. In colonial discourse, this was justified in terms of equality and individual freedom.22 The Gezira Scheme was praised for emancipating the Sudanese from “traditional” bonds of subordination. An anonymous colonial document states, “Before the Gezira Scheme was started the type of native life gave very little opportunity to individual enterprise. The people were very much under paternal authority, the father holding the whole property of the family, often even after the marriage of his sons” (quoted in Barnett 1977:90). The colonial architects of the scheme sought to reduce all tenants to similar circumstances and avoided creating any positions of authority among tenants. In Gaitskell’s words, the administration of the Gezira Scheme was “a direct administration consciously emancipating, by the opportunity of economic independence, the individual tenants from the control of masters and fathers to whom otherwise they would still have been subservient” (1959:199, emphasis added). The high value the British placed on equality among Sudanese, however, proved to be quite compatible with great inequalities between the colonizers and the colonized; in fact, the one reinforced the other.

Moreover, despite British discourse on the emancipation of the individual, the Gezira Scheme was premised on a model of the peasant household as that of a male landowner whose wife and children worked the land with him. Women generally were denied tenancies when the Gezira was established, even if they owned rainland. Among British complaints about tenants was the refrain that
“their women” did too little field work and that their sons sought occupations away from the land (Gaitskell 1959).

The efforts of the British to impede the rise of inequality among the Sudanese resulted in one of the most hierarchical agrarian structures imaginable. The official structure of the Gezira Scheme was a rigid bureaucracy, with the tenant at the lowest level. A 1937 article in *Al Nil* describes the relations between management and tenants as one of “absolute despotism” (*Al Nil* 1937, quoted in Barnett 1977:121). While the massive infrastructure and capital investment required to establish the Gezira Scheme and set up its irrigation system may have required central planning, the regulation of water was not the main function of the authoritarian administrative structure instituted by the British. This structure had much more to do with control over Sudanese farmers than with control over water. On the Gezira Scheme, decision making was highly centralized, and peasants were permitted very little scope within which to operate their farms. Tenants were told which crops to grow, how much to plant, and when to carry out the various agricultural operations. Farmers had no say over the purchase of inputs to cotton production such as plowing and insecticide-spraying. Scheme regulations and officials told farmers what to do and when to do it, and inspectors checked the fields to insure the timely and correct completion of the various tasks involved in cotton production. Tenants had no voice in how or where their cotton was ginned and graded, or when and for what price it was sold. Decisions were either taken directly by management or severely curtailed through rigid land use regulations, agricultural schedules, and inspections.

Similarly, the regulations ostensibly governing land on the schemes (the “Tenancy Agreement” and “Standard Conditions of Tenancy”) were very much about the control of Sudanese farmers, stipulating that cotton production was to be done under the direction and supervision of scheme authorities. This was a contract very much about status. With agricultural decision making concentrated in the hands of colonial authorities, farmers’ official role was reduced to that of supplying labor to cotton production. The degree of regimentation led some observers to liken Gezira farmers to workers on an assembly line (Beer 1955:49). Farmers who were deemed negligent in cotton production were penalized financially (by being charged for cultivation of their land by wage-labor under scheme direction) and could even face eviction.

The centralized structure of decision making and the rigid schedules imposed by the colonizers stand in stark contrast to the flexible, individual, and diverse agricultural strategies of Sudanese households prior to the schemes and in areas outside the schemes (Bernal 1991; H. Ibrahim 1979). Before the schemes, farmers in the Gezira (and at Wad al Abbas) timed their sowing with the onset of the rains and the receding flood waters of the Blue Nile, each farmer assessing and responding to conditions according to his or her situation and judgment. As tenants on the scheme, however, the Sudanese were required to farm according to a growing season determined by the availability of irrigation water (which was controlled by management) and to perform specific agricultural operations according to a preset calendar. Among other things, the colonial order thus imposed...
a new sense of time on Sudanese farmers: a yearly agricultural timetable divorced from farmers' intimate knowledge of their environment and its seasonal changes. The official agricultural calendar of the schemes was both a symbolic and material expression of relations of power; through ordering time (instituting a schedule), colonial authority exercised control over Sudanese labor.24

In the understanding of the British, "modern" farming was characterized not only by certain inputs but by its very form, schedules and discipline (Cooper 1992). Even as the British saw themselves as "helping" Sudanese farmers, the colonial mission was actually more about creating "good farmers" by directing their activities. The very nature of this project meant that a "good farmer" by definition was one who would carry out British orders. The imposition of the colonial order in this respect was about disciplining a rural population to accept not only the rigors of irrigated cotton production but also British authority. Thus, the "Standard Conditions of Tenancy" required not only that the tenant cultivate the requisite feddans of cotton, but that the tenant do so "to the satisfaction of the Syndicate and ... in all things obey the reasonable orders of the Syndicate's officials" (quoted in Gaitskell 1959:340).

While the colonizers methodically created a subordinate and dependent population of farmers, the colonizers also represented and treated farmers as if they were free agents for certain purposes. Thus, in the official discourse of colonial authorities, tenants were "partners" of the colonial government and the Sudan Plantations Syndicate. Sir Geoffrey Archer, governor-general of Sudan in 1925 when the Gezira Scheme opened, described the scheme as "a partnership in which the native, the Government, and the Company managing the concern on behalf of the Government, each take an agreed percentage of profits" (quoted in Gaitskell 1959:91, emphasis added). The ritual signing of the "Tenancy Agreement" by each individual tenant, moreover, suggests that Sudanese farmers and colonial authorities were merely parties to a legal contract, ignoring the larger political context of the compulsory conditions under which the British established the scheme and unilaterally set the official conditions of tenancy.25 The idea of Gezira tenants as independent parties to the tenancy "agreement" and as "partners" of the colonizers operated as a central legal fiction of the scheme, perhaps reflecting in microcosm the colonial fantasy that the Sudanese population (and other colonized peoples) as a whole willingly consented to British rule.

In order to maintain the idea of a give-and-take between partners, colonial representations of the Gezira Scheme also sought to deny the degree to which the scheme disrupted life in the Gezira and subjugated Sudanese farmers to colonial authority. Governor-General Archer continued,

The native cultivates land which is his own property. The social system to which he is accustomed remains undisturbed. In fact just as we endeavour to improve existing native institutions by the addition of consistent elements from more civilised countries, so have we endeavoured in the Gezira project to improve native cultivation with the aid of scientific methods without alteration of the normal social development of the community. [quoted in Gaitskell 1959:91, emphasis added]
While in colonial discourse the British mission in the Gezira was about "instructing" the Sudanese to be good farmers, most of the British agricultural inspectors who staffed the Gezira Scheme lacked agricultural qualifications (Fuller 1984:142). In fact, the head of Sudan's Department of Agriculture during the planning of the Gezira Scheme was a military man, Colonel Wilkinson, who apparently brought little knowledge of agriculture to his post beyond that of "keen gardener" (Gaitskell 1959:63). Like the "half-educated natives," Sudanese farmers, it seemed, were inferior by definition, while British colonials were assumed to possess expertise.

In contrast to the discourse on improving local farming through extending the benefits of "scientific methods," the actual structure of the scheme indicates that colonial authorities did not trust Sudanese farmers to exercise sound judgment or even to carry out agricultural operations as they were ordered to do without strict British control. Colonial authorities in fact regarded Sudanese farmers with disdain. Among other things, the British compared the Sudanese unfavorably with Egyptian fellahin (peasants), apparently because the latter were more accustomed to the drudgery of intensive agriculture (Gaitskell 1959). A 1929 governor-general's report on the Gezira Scheme refers to "the somewhat scanty initiative of a simple and fatalistic people" (quoted in Gaitskell 1959:146). Gaitskell himself describes "the character" of Gezira tenants in the following terms: "They were not naturally attracted by a steady routine. Delighting in ostentatious show and despising the miser they were impatient of all arguments in favour of thrift. And in the end they were lazy, as was to be expected in such a climate" (1959:225). Gaitskell's representation is particularly interesting because it casts Gezira farmers as virtual opposites of the disciplined, thrifty, and hardworking peasant that was the colonial ideal.

Sudanese farmers contested forced cotton production in numerous ways. Since the cotton schemes were largely structured around the attempt to control peasant labor, struggles between Sudanese peasants and colonial authorities were waged largely in terms of labor inputs to cotton production. Tenants had no control over the decision to cultivate cotton and no control over their cotton crop once it was harvested. Farmers could express resistance primarily in the production process itself—by withholding labor. That tenants did resist by slacking is evidenced by, among other things, a continual stream of assertions by colonials that tenants were not doing enough work on cotton.

Rather than acknowledging Sudanese resistance, however, colonial discourse represents Sudanese farmers as lazy and incompetent. A 1937 Sudan Plantations Syndicate report states, "The lethargy of Gezira natives makes independence of immigrants very improbable" (quoted in Voll 1981:242). A 1942 entry in the Provincial Monthly Diary (an administrative record) characterizes tenants in terms of "the lethargic inertia of Equatoria" (quoted in Daly 1991:190). And Gaitskell comments that the threat of eviction and other sanctions "were of limited value when alternative tenants were no better," as if "bad" farming was simply a genetic characteristic of the Sudanese rather than a response to oppressive conditions on the scheme (1959:103). The British also...
used "tradition" to explain tenants' lack of enthusiasm for cotton production and their use of hired labor. Thus, for example, Gezira society before the scheme is described as having been composed of large landlords who had many slaves to do the farming, and Sudanese are said to have a cultural disdain for agriculture "owing to its former association with slavery" (Culwick 1955:177).

Colonial authorities represented tenants not only as lazy, incompetent, and disdainful of agriculture, but as "idle rich." Gaitskell uses this phrase in a 1928 correspondence (quoted in Daly 1986:426), while a 1942 entry in the Blue Nile Province Monthly Diary calls tenants "leisured landed gentry" (quoted in Daly 1991:190). Sir Harold Wooding derides tenants as "gentleman farmers" and "gentleman loafers" (quoted in Gaitskell 1959:257). The British were particularly incensed by tenants' use of hired labor for cotton picking, the most labor-intensive task, and one carried out partly during the hottest months of the year, when temperatures above one hundred degrees Fahrenheit are common. Hired labor was a routine part of the cotton production process at Zeidab and on all subsequent schemes, and was formally institutionalized to the extent that scheme management routinely provided credit to tenants for paying laborers. The use of hired labor by tenants was, nonetheless, dismissed by the colonizers as an unnecessary luxury. Gaitskell writes,

However much others might be planning the future society as one of hard-working peasant proprietors they [the tenants] showed a marked preference themselves for a future as gentleman farmers, and one of the first uses to which they put the additional money which the Scheme brought them was to hire others to do all the work. [1959:225]

The representation of tenants as not just lazy but also as leisured, along with the use of gentleman as a term of reproach, reveals something about the moral economy underlying colonial discourse about tenants. According to this moral economy, if tenants behaved differently than the colonial planners and scheme administrators intended, it was because of inherent Sudanese moral failings. From the colonial perspective, neglecting cotton, employing hired labor, or aspiring to be other than simply a cotton-farming tenant (or the cotton-farming wife or son of a tenant) was not simply inconvenient for the realization of colonial goals, it was evidence of bad character. Such behavior on the part of tenants contradicted fundamental colonial beliefs about the Sudanese peasant society the British were supposedly creating on the Gezira Scheme. The moral tone of the discourse on tenants makes it clear, moreover, that it was not simply cotton yields that were at stake, but an entire colonial order, one in which subject peoples knew their proper places and did not aspire to live like "gentlemen." A 1920s incident in which a district commissioner went to a school and sent every boy wearing Western shoes home to change into merkoub (local-style slippers) conveys some of the ambivalence of the colonial task: to "improve" the natives, without ever allowing them to feel entitled to the same things as the colonizer.

The refusal of the colonizers to officially acknowledge tenant resistance to the imposed (cotton) regime reinforced the British view of Sudanese farmers as
incapable of responsibility, and this in turn helped to justify the authoritarian structure of the scheme. Colonial discourse on Sudanese farmers' "need" for supervision can thus be understood as another way of talking about coercion. While the British saw themselves as emancipating Sudanese from traditional authority, they regarded any involvement of Sudanese in the administration of the scheme as fraught with danger. Nonetheless, in the 1930s, growing nationalist movements in the Sudan and elsewhere in the British empire compelled British authorities to consider giving some positions on the Gezira Scheme to leaders drawn from the tenant population and to educated Sudanese. As Sir Douglas Newbold wrote in 1940, "unless the Sudanese can soon have some more intelligent participation in the Scheme than that of a labourer, we are bound to have trouble" (quoted in Henderson 1974[1953]:516).31

The policy goal of including Sudanese in administration was called devolution, a term that in itself conveys misgivings. While evolution is clearly associated with natural processes and "progress," one can only guess what images devolution conjured up for colonials—evolution in reverse, perhaps. One of the word's dictionary definitions is, in fact, "degeneration." Colonial fears of allowing Sudanese to assume positions of authority were expressed in terms of the dangers of creating "local despots" (Barnett 1977:127) or, in Gaitskell's words, "petty autocrats" and "petty official[s] of the worst type" (1959:210). These phrases suggest that the colonizers did not even imagine that Sudanese could act in any official capacity beyond a "petty" one. Like the "half-educated" native and " 'bastard' Westernization"—dangerous hybrids that threatened to blur the racial/colonial boundaries between rulers and ruled—the petty Sudanese official was inherently inferior and therefore could only be of "the worst type." Outside the scheme, "devolution" was rejected in the same terms. As late as 1944, the governor of the Blue Nile Province could not envision any reduction of British staff before 1964, while another official thought 40 or 50 more years were needed before Sudanese could move into positions of responsibility (Daly 1991:144-45). Gaitskell, sounding several colonial themes, describes the Gezira Scheme population in the 1940s as "a society of half-knowledge ... increasingly resentful of nursery orders yet with little mechanism to understand the complex technical background to such orders and with none of the responsibility of association with decisions" (1959:225, emphasis added). While by current standards Gaitskell seems to epitomize imperialist attitudes, he may actually have been relatively progressive for his time; some colonial government officials regarded him as "too modern and socialistic" (Daly 1991:308). Gaitskell's self-definition as superior to the Sudanese yet in service to them was, however, quite typical (see also Shoettler 1984:129–131).

Just as the resistance of Sudanese farmers to the cotton regime was taken as evidence of laziness and incompetence, necessitating colonial discipline, the hierarchical administrative structure of the Gezira Scheme was justified in terms of a lack of initiative on the part of Sudanese. Discussing prospects for Sudanese involvement in managing the scheme, Gaitskell states, "Spoon-feeding had been wise in the early years, and drive and direct control essential through the depression,
but if the tenants were ever going to stand on their own feet the habit of waiting to have everything organized for them had to be changed" (1959:206, emphasis added).32 A former District Commissioner justified British colonialism in the Sudan in similar terms, arguing that "the peoples of the Sudan had done little on their own to make their passage through this world more comfortable" (Sandison, quoted in Shoettler 1984:108).

Of course, the British colonial project was not to make the Sudanese "more comfortable," nor had the population of the Gezira simply been waiting for "a new system of agriculture to descend." The Gezira Scheme was established by colonial fiat and, through its hierarchical bureaucracy and rigid production routines, the scheme imposed colonial authority on farmers to such an extent that they had little opportunity to exercise managerial ability or initiative. The important exception to this was tenant initiative outside of, or in opposition to, scheme regulations. This included tenants giving the cultivation of sorghum (their subsistence grain) priority over cotton and pursuing nonagricultural sources of livelihood, such as pastoralism, trade, and various commercial activities, all of which were less directly under colonial control than cotton production (Bernal 1991, 1995).

The Sudan Plantations Syndicate, which directly managed the Gezira Scheme, was even more resistant to sharing power with Sudanese than the colonial government (Daly 1991). This split has been described as a debate between those who saw the British role as that of trusteeship and those for whom economic profit was key (Barnett 1977). But it can also be understood as expressing tensions between a government with limited resources and a capitalism that could not be contained by administrative structures. Some British officials in England and Sudan apparently felt their own authority threatened by the power of private capital in setting Sudan’s course. In seeking to protect the Sudanese from the Syndicate, officials were also asserting that they were ultimately in charge. Yet despite debates among colonialists, policies and practices on the Gezira were monolithic and enduring. Colonial and Sudanese critics of the scheme’s design and administration had little impact. (In fact, to this day the call for greater farmer participation continues to be heard.33) As it happened, World War II created a shortage of British staff, which made Sudanese participation in the lower levels of scheme administration a necessity.

Gezira tenants also began to organize collectively in the late 1940s, leading to the creation of an official representative organization, the Tenants’ Representative Body. What tenants chose to demand is significant; they did not simply ask for a larger share of cotton profits, but demanded participation in decisions about cotton production, grading, and sales, as well as the right to audit the scheme’s accounts (Barnett 1977). Tenants did gain a larger percentage of cotton profits, but they were unsuccessful in the other demands that would have fundamentally altered the power structure of the scheme.

Colonial arguments against Sudanese participation in running the scheme were (predictably) phrased largely in terms of the dangers of inefficiency and poor performance that might result (see Swan 1954:33, for example). Underlying
these arguments, however, it is clear that a moral order was also at stake: the British right to rule and the rightness of British rule. Thus Gaitskell apparently saw no contradiction between “independence” and British domination when he wrote in 1940 that it would be “indefensible to relegate tenants who have tasted economic independence back under selected natives and to pass over to these latter the power over their food and their money which is today exercised by British officials” (quoted in Barnett 1977:126–127). British loathing for Sudanese merchants and landlords, their disdain for “traditional” authorities, and their fears of “petty autocrats” and “half-educated natives” suggest that any Sudanese whose status was more exalted than that of a peasant constituted an implicit threat to the colonial order in which wealth and power were to be British prerogatives.

For the British in colonial Sudan, it was a truism that “there were only a handful of Sudanese who could fill posts of responsibility” (Dr. John Bloss, quoted in Fuller 1984:162). As Sir Hubert Huddleston, governor-general of Sudan, put it in 1945, “a high standard of efficiency” was required, and for important jobs, “a good knowledge of English and a period of education in the U.K. were absolutely essential, [moreover,] it was most unfair to put Sudanese into jobs which they clearly could not hold down” (quoted in Daly 1991:199). In the words of another British colonialist, it was “illogical and undesirable” that a British official serve under a Sudanese (quoted in Daly 1991:145). Gaitskell reports that the British staff of the Gezira Scheme in the 1940s, including himself, opposed placing any Sudanese in a managerial position: “A manager, we felt, could not learn his job by starting at the top [but] by working his way up” (1959:324). He and his colleagues apparently ignored the fact that under the colonial system in place up until that time, British men had occupied all positions of authority, while thousands of Sudanese had worked on the Gezira Scheme for 20-odd years with no possibility of “working their way up.”

Colonial discourse on individual economic freedom and emancipation focused on relations among Sudanese, precluding examination of the inequality between the British and the Sudanese, which for the British was part of the taken-for-granted order of the world. This allowed them to represent as emancipating what was in essence a system of forced labor.

Conclusion

The postcolonial world is one in which we may live after colonialism but never without it.

—Nicholas Dirks, Colonialism and Culture

The Gezira Scheme and cotton production are not simply the stuff of colonial memory; they occupy a central place in Sudan today. The Gezira Scheme itself has operated continuously for over 70 years without substantial modification. Moreover, this single development project has had a dominant role in setting the course of subsequent development initiatives in Sudan and in shaping the Sudanese economy as a whole. The legacy of the Gezira Scheme is not only
the profound economic transformations it entailed, but also the vision of modernity that it represented. In the most literal sense, the Gezira Scheme was a model of development for the Sudanese. Successive Sudanese regimes and Gezira Scheme administrators not only maintained scheme structures and policies uncritically, but developed new areas using the Gezira framework.

At independence in 1956, some 840,000 hectares were under irrigation in the Gezira (Frost 1984); by the 1980s, the total irrigated area of Sudan (mostly in Gezira-like schemes) approached 2 million hectares, and it may reach 2.5 million by the year 2000 (Horowitz 1989; A. Ibrahim 1984). Over 2 million Sudanese live or work on such schemes. In recent decades, cotton has contributed about half of Sudan’s export revenues (Zakaria 1986), and the Gezira Scheme remains the centerpiece of development in this agricultural economy. The lack of other productive investments makes the Gezira Scheme vital to the Sudanese state, since the cotton it produces is one of the only secure sources of export revenues (Barnett and Abdelkarim 1991). Cotton, however, is a secure source of revenue not because tenants are able to produce it profitably, but rather because the state is able to exercise such complete control over the cotton that is produced (Bernal 1988a). Like their British predecessors, moreover, Sudanese bureaucrats and field inspectors continue to see problems (such as poor cotton yields) as stemming from the laziness and irresponsibility of farmers, rather than as negative reflections on the schemes themselves (Barnett 1977; Bernal 1991).

The Gezira Scheme and irrigated cotton production are not solely the concern of statesmen and technocrats, however. They have entered the national imagination as a part of Sudanese identity. References to the schemes crop up, for example, in the fictional accounts of rural life by Sudanese writer Tayeb Salih. In one of Salih’s stories, the colonial government plans to establish a pump at the very site where a majestic tree stands:

[T]he government . . . said that the best place for setting up the pump was where the doum tree stood . . . However, when [the villagers] heard about cutting down the doum tree they all rose up as one man and barred the district commissioner’s way. That was in the time of foreign rule. The flies assisted them too—the horse-flies. The man was surrounded by the clamouring people shouting that if the doum tree were cut down they would fight the government to the last man, while the flies played havoc with the man’s face. As his papers were scattered in the water we heard him cry out: “All right—doum tree stay—scheme no stay!”

[1970:4]

In fact, it is the schemes that have stayed. Moreover, for some Sudanese, the schemes have become a source of national pride and a symbol of “modern Sudan.” Thus, for example, a Sudanese postage stamp depicts women harvesting cotton. And a Sudanese acquaintance, in response to my critical comments about the Gezira Scheme, muttered in frustration, “Yes, there are some problems with it. But the Gezira Scheme is the best thing we have in the Sudan.” He seemed to feel Sudan once again was being measured against Western standards and found wanting. Visitors from the West might be expected to find little in Sudan
that conformed to their sense of order and efficiency, but surely they could at least appreciate the Gezira Scheme. The scheme still serves as much more than a production site; it is a monument to modernity and civilization. A pamphlet published by the Sudan Gezira Board in 1980 boasts that the scheme is a "magnificent example of the ability of the Sudanese people to plan, execute, and run Agricultural Schemes" (SGB 1980). The scheme has come to represent the character of the Sudanese; the pamphlet makes no mention of colonial rule even in a section called "Historical Background," and its cover displays a portrait of then-President Jaafar Nimeiri. However, the colonial legacy echoes in the text, where, for example, the government, the Gezira Board, and the tenant are still referred to as "partners" who carry out "their obligations according to an ordinance which allows the maximum manoevrability [sic] for the highest possible production" (SGB 1980).

The analysis of the Gezira Scheme presented here challenges the narrow economic view of colonialism and reveals the limitations of conceptualizing power simply in terms of exploitation (or resistance to it). Neither colonialism nor capitalism stand outside of culture. And in some contexts, like that of the Gezira, science and rationality are best understood not as principles of action, but rather as bases for claiming power and authority. In the case of the Gezira, science and rationality, representing modernity in opposition to "mindless tradition," can be read as elements of colonial discourse through which the British sought to constitute and legitimate relations of inequality between themselves and the Sudanese.

While much work has been done on political symbolism and colonial culture as reflected in colorful spectacles and exhibitions (Corbey 1993; Hobsbawm and Ranger 1983; Mitchell 1988), development projects are less obvious sites for the creation and expression of cultural and political symbols (see, however, Brow 1988; Tennekoon 1988; Woost 1993). Yet it is clear that productive relations are always culturally constructed and can never be solely economic arrangements (Downs 1995). Indeed, even in the West, where capitalism might be thought to exist in unadulterated form, it has been argued that, far from being free of cultural baggage, "the economy is the main site of symbolic production" (Sahlins 1976:211). The Gezira Scheme certainly lacks the pageantry of a coronation or an exhibition: in fact, it is striking for its drabness and monotonous routine. But it is no less an artifact of culture. For how is such ordinariness of routine constructed and perpetuated over decades if not by the naturalization of the political order? Seen in this light, the Gezira Scheme is as much about discipline as it is about development. In fact, from the colonial perspective it would seem that discipline was development, since discipline was regarded as intrinsically beneficial to the colonized regardless of whether it increased their income or productivity. The case of the Gezira Scheme challenges the view of capitalist institutions as necessarily rationalizing, and calls into question the central role of the market in disciplining economic behavior under capitalism. The Gezira Scheme was, in fact, largely a command economy in which the paramount value was not so much a capitalist value like efficiency or profit-maximization,
but rather a moral and political value, that of colonial authority over subject peoples and the world’s natural resources.

The Gezira Scheme is not unique; its qualities as a technology of power that above all established relations of authority over rural populations give us insights into contemporary as well as colonial development projects (see Ferguson 1991). The case of the Gezira suggests that development projects cannot be fully understood unless they are recognized as systems for ordering social relations and as vehicles for expressing and encoding moral values.

Notes

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1. Sudan was not technically a colony of Britain, but administered by Britain jointly with Egypt as the Anglo-Egyptian Condominium. For all practical purposes, however, Sudan was ruled by Britain. Sudan’s history, moreover, must be seen in the context of the European colonization of Africa. For these reasons, I use the term colonial throughout this article when referring to Britain’s relationship to Sudan.

2. I first developed the idea of a colonial moral economy in my article “Cotton and Colonial Order in Sudan: A Social History with Emphasis on the Gezira Scheme” (Bernal 1995). Some of the material presented there is reprised here as I amplify my analysis of the symbolic and cultural dimensions of the Gezira Scheme and situate my discussion in terms of anthropological concerns with development, modernity, and colonialism.

3. Upon leaving the Gezira, Gaitskell (never one to leave well enough alone) headed a mission to investigate the possibility of developing ranching in the Kalahari.

4. “Few British agents concerned themselves to any degree with consequences for the Sudanese of these [economic and social] changes” (Fuller 1984:166).

5. Like many of the phenomena described in this article, such practices are by no means unique to British colonialism, and in fact, they continue in the postcolonial era. See, for example, Raikes’s (1988) wonderful unmasking of the Food and Agriculture Organization’s agricultural production figures for Africa.

6. The Sudan Plantations Syndicate fared considerably better, running at a loss in only one year, 1931.

7. The expansion of irrigated cotton production by postcolonial Sudanese regimes has continued until the present. The stagnation of productivity on existing schemes (rather than discouraging further development of this type) drives the state to incorporate new populations into the system of cotton production (Bernal 1988a).

8. One of the aims of the schemes was to make Sudan support the cost of its colonial administration. Ironically, given the great involvement of the colonial government in the
schemes, the colonizers did not believe government should invest in the economy (Sanderson 1985). Thus, private investors like Hunt were sought.

9. Egypt played an important role in Sudan’s colonial history. The strategic importance of Sudan to Britain, in fact, was largely to secure the southern border and Nile waters of British-dominated Egypt. Egyptians served in the Condominium administration, and many of the British who served in Sudan were drawn from Egypt.

10. For example, an anonymous colonial document states that wealth was concentrated in the hands of a few large landowners and merchants on whom the general population were dependent clients (Barnett 1977). Others say the Gezira was populated mostly by small owner-cultivators, or predominantly by seminomads, and still others argue that the Gezira was cultivated mainly by slave labor (Daly 1986; Issawi 1966). There is little evidence of a large landlord class in the Gezira or of land shortage or fractionalization. One source of misunderstanding may be colonial land registers, since multiple Gezira families sometimes registered their lands collectively to save on registration fees (Miskin 1950). Some local leaders also registered communal lands as their own property at the establishment of the scheme (Niblock 1987). Even so, Gaitskell reports that large holdings were rare among those whose land was incorporated into the scheme. Individual land tenure itself was not introduced by the British, however, but it has a long history in northern and central riverain Sudan (Awad 1971). The extent of women’s participation in agriculture also is a matter of debate (Bernal 1988b).

11. The Wad al Abbas scheme was started by Sudanese merchants licensed and regulated by the colonial government after the model of the Gezira.

12. Over time, there was some experimentation with different rotation systems and tenancy sizes. Lubia was eliminated from the crop mix. Tenancies were increased to 40 feddans on the Gezira, but set at 15 feddans on extensions of the Gezira and most of the other schemes, typically with 5 feddans cotton, 5 feddans sorghum, and 5 feddans fallow each year. This was the cropping pattern instituted at Wad al Abbas.

13. Many Sudanese communities had suffered greatly during the Mahdiya, which caused massive population movements, the disruption of economic life, and famine. Riverain populations, in particular, were treated harshly by the Baggara and Taisha ethnic groups, who formed the core of the Mahdi’s supporters.

14. Another example is the rent paid for land that was incorporated into the scheme. Rent was set at ten piastres (two shillings) per feddan, a rate supposedly based on the highest rent paid for rainland prior to the scheme (Gaitskell 1959:85). However, Gaitskell himself states that fixed rents were “strange” to the Sudanese. Abdel Salam (1987) argues that there never was a ten piastre per feddan rent prior to the scheme, but rather a payment of ten percent of the harvest. Rental of land, moreover, probably was not common in the Gezira, where land shortage does not appear to have been a problem before the scheme.

15. Moreover, sagia arrangements themselves varied (Shaw 1966b; Thornton 1966).

16. Furthermore, just as accounting maneuvers were used to give an artificially bright picture of the Gezira Scheme’s potential profitability to authorities in England, control over accounting allowed the British to manipulate tenants’ profit levels. Sometimes tenants’ profits were inflated so that they did not give up on cotton altogether, and sometimes profits were withheld without tenants’ knowledge (Gaitskell 1959).

17. Gaitskell describes the payment system as “complicated and altogether remote” (1959:86). The actual payment of profits was spread out in installments with at least a year’s delay. Furthermore, the payments of past cotton profits were interspersed
with advances against future profits (to help cover tenants' expenses in producing the current crop). These practices made it difficult for tenants to keep track of cotton returns and avoid falling into debt.


19. While one might expect irrigation to reduce variation, such was not the case as pests and other problems plagued the Gezira Scheme, and the price of cotton on the world market rose and fell precipitously.

20. This is presumably a reference to fines for cattle trespass on the schemes.

21. See, for example, the debate in British Parliament of July 10, 1924, and Daly 1986:360–379.

22. This view of the individual being emancipated through connection to colonial rule was expressed by the journalist Sidney Peel, for example, who praised the tax system introduced by the British for the fact that "the Government deals directly with the individual taxpayer" (1969[1904]:251).

23. The tenant initially was responsible for sowing, weeding, and picking. The pulling out of cotton plants and sweeping up of debris after the harvest were added later.

24. Agricultural timetables on the schemes, moreover, were expressed according to Western (solar) calendar dates rather than according to the Islamic lunar calendar familiar to the Sudanese.

25. And, in fact, the only legally binding form of the contract was the English-language one, which very few tenants would have been able to read.

26. Postcolonial literature on the schemes similarly abounds with references to the need to instruct farmers in their duties and enforce compliance to scheme regulations. At the end of a 1966 volume devoted to agricultural development in the Sudan is one short paragraph entitled "Farmer," which concludes, "Until the farmer himself and others are taught to realize their national responsibility, control and supervision are essential to the policy of Do it at the correct time and do it properly" (Shaw 1966c:D27, emphasis in original). Another article on the Gezira originally published in 1972, nearly 50 years after the establishment of the scheme, still speaks about the possibilities for growth of "motivation and ability to farm" among tenants (Thornton 1987:350). Kontos describes Gezira farmers as having a "seemingly self-defeating attitude" (1991:138).

27. The immigrants referred to were primarily migrants of West African origin and people from the western region of Sudan, some of whom were granted tenancies, while others made up a large portion of the agricultural laborers, required particularly for picking cotton.

28. Many colonial sentiments continue to echo more than half a century after the establishment of the Gezira Scheme. A 1973 World Bank report on the Gezira states, "Experience has shown that tenant families do relatively little for two reasons. Firstly, there is a strong tradition against landowner families doing farm labour. Secondly, farm wages are so low that most tenants can afford more labourers than necessary" (World Bank 1973, quoted in Olsson and Keddeman 1978:17). Along the same lines, Warburg asserts that the heritage of slavery in Sudan led to "gentlemen farmers" in Gezira (1978:242). Osman and Suleiman write, "In the Gezira and elsewhere ... tenants apparently prefer to devote a large proportion of their cash incomes to employing casual labour" (1969:455), and the authors go on to equate expenditure on hired labor with the "consumption of leisure" (1969:458).

30. This incident is recounted by Francis Deng as told to him by Yusaf Badri (Deng 1984:217).

31. By this time, moreover, Syndicate management was drawing to a close, as the colonial state was scheduled to assume full control of the Gezira Scheme in 1950.

32. One subsequent Gezira administrator defined devolution as “the handing over of power to the local people, thus encouraging them to take more interest in running their own affairs” (Swan 1954:31), as if the Sudanese had no interest in their own affairs without British encouragement.

33. Barnett states, “Attempts to devolve decision-making power on to the tenants have always failed, because effective achievement of such a goal would contradict the entire rationale of the Scheme” (1977:114).

34. Indeed, as Barnett and Abdelkarim (1991) note, the greatest significance of the scheme may lie in the transformations it brought about outside its borders through migrant labor and other influences.

35. The Gezira Scheme and its extensions in Managil and Guneid account for about half of Sudan’s irrigated area.

36. In 1978, it was estimated that over one million people lived or worked on the schemes (Keddeman and Abdel Gadir 1978). A more recent study estimates that seasonal workers alone number one million, while tenants comprise another 200,000 (Shaeldin 1986). Given that tenants are usually members of households that include several landless individuals and that there are settled communities of laborers and sharecroppers on the schemes, the resident population of the schemes easily comprises another million.

37. A British engineer’s report and the British Treasury are mentioned, but given no context.

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Toward a Reconstitution of Ethnicity: Capitalist Expansion and Cultural Dynamics in Sudan

In this article I argue that, despite recent advances in anthropology, prevailing images of ethnicity in academic discourse overemphasize units at the expense of interconnections and tend toward ahistorical formalism. Through an examination of the dynamics of ethnic segmentation of the agricultural labor force in Sudan in this century, I seek to show that both the modern character of ethnicity as an organizing principle and the content of particular ethnic identities in Sudan emerged in the process of peripheral capitalist development. Ethnicity in these circumstances is thus fundamentally different from the ethnicity of the Sudanese past and from ethnic principles articulated elsewhere under different social conditions.

DISCOURSE ON ETHNICITY AND RELATED ISSUES seems to be problematic, both for its anthropological guardians and especially for their cousins in neighboring academic and policy-making fields. While real advances in the understanding of ethnicity have been made in anthropology in recent years, fuzzy primordialist notions seem still prominent in other disciplines and predominant in popular conceptions. When I ask students in my introductory sociocultural anthropology classes (every one born in the United States) how many are German, Irish, Chinese, French, and so on, there is always a show of hands, some of them raised more than once. If I go on to ask about the proportions of the mix among those responding more than once, knowing snickers always greet my suggestion of such fractions as thirds or fifths. It seems that everybody knows the arithmetic of biological reproduction—and also knows that it is the same arithmetic that gives you your ethnic identity.

Anthropologists are, of course, much more sophisticated, and few would now accept that ethnicities are immutable primordial identities. Many anthropologists have contributed to the development of an understanding of the mutability of ethnic identity. Evans-Pritchard's (1940) analysis of segmentary opposition among the Nuer provided a basis for understanding situational identification, an insight followed up and elaborated in the work of the Manchester School in Central African urban areas (cf. Mitchell 1956). Barth and his associates (1969) investigated ethnic boundaries as delimiting separate coherent systems of values and documented individual movement across such boundaries (e.g., Haaland 1969). Colson (1968), Asad (1970), and others discovered that many African tribes were actually colonial constructs deliberately created to meet the needs of administration through indirect rule. Richards (1969) pushed this discovery further to argue that ethnic identities and "tribalism" became more prominent in East Africa as the result of colonial policy and the form taken by decolonization there than they had been previously. Drawing on insights such as these, ethnohistorians began to investigate the origins of newly emergent ethnic identities and coined the term "ethnogenesis" to describe such processes (see Sturtevant 1971; cf. Newcomer 1972). As a result of such developments in anthropological thinking about ethnicity, the concept has largely been shorn of its connotations of immutability and persistence of primordial loyalties. Ethnic identities and ethnic groups are seen to be fluid, to appear, undergo transformation, and disappear in
response to changing distributions of resources and other conditions. In the meantime, individuals move back and forth across ethnic boundaries.

Despite these important advances within anthropology, the primordialist notion remains strong outside the discipline, and sometimes even seems implicit in the concrete analyses of some anthropologists. It could be that this circumstance reflects an incompleteness in our rethinking of ethnicity. Indeed, two important recent books by Wolf (1982) and Worsley (1984) offer strong criticisms of the units and modes of cultural analysis current in anthropology. Wolf characterizes contemporary anthropology as preoccupied with dividing its subject matter into distinctive cases, or societies, “each with its characteristic culture, conceived as an integrated and bounded system, set off against equally bounded systems” (1982:4), concluding that

By endowing nations, societies, or cultures with the qualities of internally homogeneous and externally distinctive and bounded objects, we create a model of the world as a global pool hall in which the entities spin off each other like so many hard and round billiard balls. [Wolf 1982:6]

What is missing, according to Wolf, is an appreciation of the interconnectedness of social and cultural phenomena and the historical contingency of the units that present themselves for analysis. While these programmatic statements from the introduction to Wolf's book can justifiably be said to overdramatize things and to ignore the permeability of boundaries, for example, in a Barthian analysis, the more nuanced analysis in Wolf's later chapters should lead us to take seriously his general complaint about the overvaluation of units and the undervaluation of interconnections in anthropological analysis of cultural processes. Our concern with Galton's Problem would seem to be a case in point. Wobst (1978) has made a forceful case that ethnographic preoccupation with identification of units has hampered the ability of archeology to develop methods and concepts appropriate to grasping the fluidity of organization of paleolithic hunter-gatherers.

Part of the problem is illuminated by Worsley in his criticism of contemporary anthropology's situational understanding of ethnicity (1984:246). This understanding, he argues, tends to assume a market model in which individuals make choices, such as which ethnic identity to embrace, without constraint in much the same way that American consumers select which make of car to buy. The result is that the role of inequality and power relations in restricting the field of choice, and ultimately in shaping the larger cultural constellation, is left out. In particular then, for Worsley, such a model of ethnicity is incapable of grasping the nature of cultural dynamics within and between societies divided by class. Worsley also detects formalism in anthropological discourse on ethnicity and related phenomena, in which ethnicity is treated as qualitatively the same kind of phenomenon regardless of historical period, social context, or level of operation (1984:246-249). Thus, the ethnicities of the contemporary San, the classical Roman, and the Welsh nationalist are viewed as functional equivalents. Similarly, for analysts such as Colson (1968; cf. Worsley 1984:367), ethnicity expressed through “tribalism” is qualitatively the same as ethnicity expressed through “nationalism,” only on a smaller scale.

The common theme that seems to unite the concerns of Wolf and Worsley is that our present conceptual tools for analyzing cultural dynamics are insufficiently attuned to the role of social relations—particularly those characterized by inequality—within and between groups in shaping ethnic identities and, indeed, the variable principles by which ethnicity is defined in different circumstances. In the remainder of this article I take up this challenge in the specific field of the social relations of the agricultural labor force developed in Sudan under the impact of capitalist penetration in the 20th century. This labor force came to be structured and expressed in ethnic terms on the basis of principles which I argue were fundamentally constituted in the context of capitalist incorporation (see O'Brien 1980, 1983, 1984; Ali and O'Brien 1984).

The Formation of the Agricultural Labor Force in Sudan

The British occupied Sudan in 1898 and opened the vast irrigated Gezira Scheme for cotton production in 1925. Considerations of security and social control led the British to
establish a smallholding tenancy form of organization of the Scheme rather than allowing conditions for the formation of a large landless proletariat to arise. There was nevertheless a need for large supplementary supplies of wage labor for peak season agricultural operations, which could not be performed by tenant family labor alone. In response to this need, the colonial regime fostered the development of a large, seasonally migrant wage labor force composed of peasants and pastoralists who maintained their village plots and their herds in addition to working for wages for a few months of the year (see O’Brien 1983).

The form of involvement in wage labor, and in markets more generally, which the British authorities promoted therefore had to be one that was compatible with the maintenance of peasant and pastoral productive systems, indeed, it had to presuppose their continued functioning as a prerequisite to the successful operation of the export sector. This condition meant that participation in wage labor varied from group to group depending on their differing divisions of labor, conditions of social reproduction, techniques of production, and so forth. The policies implemented to stimulate the appropriate type of labor supply are familiar from other parts of the continent in the colonial era: taxation in cash, undermining of craft production (particularly textiles), and aggressive marketing of a few key consumption goods of broad appeal (tea, coffee, sugar, manufactured cloth). These and other policies engendered cash needs which could not be met within existing productive systems without basic alterations, yet did not necessitate full integration into labor markets for their fulfillment. Neither did the conditions of the labor market, attuned to peak seasonal labor demands, allow much scope for full proletarianization (see O’Brien 1984).

For some, seasonal migration for wage labor became a regular part of their annual work cycle. For many more, it became an irregular part of life, occasioned by crop failure, disadvantageous livestock prices, special expenses (such as for marriage), or was a temporary necessity for youthful households. The mosaic of patterns of incorporation into the agricultural wage labor force in these and other ways mirrored differences between local and social groups which were generally conceived in terms of cultural differences among them. If one knew a person’s ethnic identification, one could fairly reliably predict what form her or his incorporation would take, including type, patterns, and intensities of work. The result was a highly segmented labor force structured on a basis that was expressed in terms of ethnic identities. Groups whose internal division of labor involved women in agricultural production, or allowed it in principle, tended to migrate to Gezira in family groups and put all family members to work in activities such as cotton picking. Other groups, particularly those that practiced a strict seclusion of women, often preferred to intensify village production and to meet their cash needs through production of a cash crop demanded by the British, a preference that sometimes required relocation to more favorable areas. When they did engage in seasonal wage labor, it tended to be only adult men.

The situation was, however, not a simple matter of one-way cultural determination of social forms of production. Indeed, once incorporation had become widespread, the process seems more generally to have moved in the other direction, from social form to ethnic identity. The following two cases are offered as illustration of different ways in which this occurred.

West African Immigrants

Due to the relative sparseness of population in Sudan and the lack of developed wage labor markets, the colonial regime encouraged immigration into Sudan of West African Muslims, especially poor Hausa peasants from the former Fulani sultanates (see Duffield 1979, 1981, 1983). In the early 1930s the government and the Gezira Scheme management organized an active coordinated program of settlement of such immigrants in the scheme (see O’Brien 1980, 1983, 1984). As a virtually landless population they served as a stable pool of cheap wage labor year round. Other groups of West African immigrants were encouraged to settle in underpopulated areas outside the Scheme, particularly in
the Rahad/Dinder region east of the Blue Nile from Gezira, where they cultivated their own rainland plots and could be drawn upon as seasonal wage laborers for Gezira.

The groups involved in this immigration and settlement showed considerable cultural diversity. The largest number of them were Hausa-speaking peasants who affirmed distinct, named ethnic identities among themselves. There were numerous other groups as well, including Fulani, Borgu, Bornu, and various Chadian peoples. All of them were Muslims and most of them spoke Hausa at least as a second language, but many different languages and cultural forms were represented among them.

Right from the beginning resentments against these groups began to develop among the indigenous populations with whom their work brought them in contact. The Gezira authorities used them in two important ways to discipline tenants in the Scheme. If a tenant failed to carry out an agricultural operation on schedule, the Inspector hired settled labor to do the job at double the going wage and charged the expense against the tenant's account. Any tenant who failed to cultivate his plot to the satisfaction of the British Inspectors or who absconded (as many did during early cotton blights and the depression) were replaced by immigrant settlers. More generally, the long experience that most of the immigrant groups had of disciplined agricultural work under conditions of exploitation predisposed them to adapt to the rigors of market relations efficiently and impersonally. They bargained hard, and collectively, over wage rates and then worked very hard to maximize returns to their labor time. In contrast, many of the local, mainly Arab, populations—many of whom were only part-time or recently settled cultivators—held agricultural work in low esteem, avoiding it when possible and working at it desultorily when it became necessary. Despite the dramatically higher labor productivity of West African settlers, tenants often tended to prefer hiring less hard-working locals with whom they could establish family-style patron/client relations and create long-term obligations to work.

The settlers came to be known generally in Sudan as "Fellata" (from the Kanuri word for Fulani). This term was applied indiscriminantly to all "Westerners" and quickly took on basically pejorative connotations linked to stereotypes of these people as hard-working and slavish. In a context in which the contacts local populations had with these diverse people were socially homogeneous, the cultural differences among them were glossed over and ignored.

The settlers responded to these conditions of hostility, discrimination, and confinement to the lowest rungs of the social ladder through a process of cultural realignment. Some material differences with the local populations were reinforced. Some of the settlers moved into previously vacant economic/ecological niches, such as riverbank vegetable cultivation and commercial fishing, which they had occupied in West Africa. In regions where cultivation had previously been mostly restricted to sandy ridges, they moved out on the heavy clay plain where their broad-bladed, short-handled hoe, destined to become known in central Sudan as a "Fellata" tool, was more suitable to weeding the muddy soil in the rainy season. In addition they did make some changes in their material culture in adjusting to Sudanese conditions. The small-bladed, long-handled hoe used in dry weeding on sandy ridges by their Sudanese neighbors was added to their tool kit. Generally, they also adopted local styles of dress and house-type.

Along with these changes, they began to elaborate a number of key symbols to differentiate themselves from the culturally dominant Arabs in ways that served to legitimate the differences and endow them with dignity. In particular, they tended to adopt fundamentalist, ascetic Islamic practices and beliefs which served as a counterpoint to the "paganism" of the surrounding Arabs, who practiced spirit possession (zar), ecstatic trance, and veneration of saints, and drank alcoholic beverages. In rural areas at least, they practiced an increasingly strict seclusion of women within the household compound and confined participation in wage labor to adult men only. They articulated an ethic of hard work and moderate consumption. Gradually, the name "Takari" came into use among them as a term applied, regardless of ethnic origin, to all of the people otherwise called Fellata. The term derives from the respectful name applied irrespective of ethnicity.
in the Hejaz to pilgrims from West Africa. As the adoption of this name indicates, there was a tendency toward the obliteration of subgroup differences among the Hausa and the incorporation of non-Hausa groups under a specifically Sudanese identity of Takari.

Mark Duffield (1979, 1981) has argued that this process represents the formation of a new specifically Sudanese ethnic identity corresponding to a definite location in the colonial social system. Partly in defensive adaptation to circumstances of discrimination and lumping together by others, these diverse cultural groups have drawn on commonalities of their past heritage and contemporary circumstances to forge a more or less coherent ethnic identity. It is of course uneven, with small groups living in more remote rural areas showing less integration than others. I have encountered some villages of people who steadfastly affirm their separate identity, yet have met some of their relatives in more central areas who claim Takari identity.

The Joama’ of Central Kordofan

The Joama’ people are another group that has played a prominent role in the agricultural wage labor force. From the beginning they have been prominent as regular suppliers of cotton-picking labor in family groups. They occupy the transitional zone of central Kordofan where the sandy ridges of northern Kordofan penetrate the heavy clay plain of the rich central agricultural zones. Settled cultivators identified as Joama’ have been recorded as living in this region since at least the 17th century. All are Muslim Arabic-speakers and they generally claim Arabian origins, claims that are much disputed by their neighbors and by Western scholars (most notably MacMichael 1912). Such evidence as exists suggests that this zone has been a scene of cultural blending for at least a couple of centuries. The zone straddles the great east-west “highway” of the precolonial trade routes linking Sudan and the Ottoman Empire with the Western Sudanic states, a route that was followed by thousands of African pilgrims to the Hejaz and along which the British laid an important railway line soon after conquest. This has long been an area of movement and mingling of diverse peoples. The name of the Joama’ itself roughly translates as “gathered together,” further reinforcing the impression of mixing. In villages across this zone one encounters considerably varied stories of the origins of the Joama’ and how they came to occupy their present homes. The account current in a particular village or cluster of related villages often sounds remarkably similar to the reputed origin of some nearby Arab group. For example, in some eastern Joama’ villages, the favored account traces their origins to a little-known brother, Jumi’ (the “in-gatherer”), of Jimi’, the Arabian founding ancestor of the Jima’ group that lives in White Nile Province immediately to the east of the Joama’ and whose claims to Arabian ancestry are respected.

Whatever the cultural dynamics of the earlier history of these people, their contemporary identity is clearly bound up with their modern position in the wage labor force. This position is characterized by annual family group migration for cotton picking, initially in the Gezira Scheme, but latterly more often in the pump-irrigated schemes along the Niles. Women and children are centrally involved in the wage-earning labor but, unlike the case with many Arab pastoralists and recently settled former pastoralists, Joama’ men generally appear to work as hard and as long (at least) as do the other members of their families.

The Joama’ soon became famous in central Sudan as good, reliable cotton pickers and were highly sought after. With the rapid expansion and differentiation of capitalist agriculture beginning in the 1950s, an elaborate recruitment system for seasonal labor evolved and the Joama’ belt became a prime recruiting ground. Representatives of tenants or management would travel to the region in advance of the picking season and negotiate with prominent men to supply stipulated numbers of pickers at agreed rates. Recruiters would supply transportation to the scheme, cash advances and food for each family while at work in addition to fixing piece rates for work performed. As this recruitment system became entrenched, newcomers to the agricultural labor market—or people who sought to move into a different sphere of it—increasingly found it necessary to be in the
places recruiters usually went to find such labor. It also helped to be identified to the recruiter as a member of an ethnic group, such as the Joama', reputed to be good workers at the particular sort of job being recruited for.

In response to these conditions, some of the thousands of immigrants and seasonal migrants from further west who annually moved through the Joama' area began to settle on the fringes of Joama' villages and insert themselves into the Joama' pattern of seasonal migration. Joama' villagers were quick to take advantage of the abundant labor floating through their neighborhood. Many of them cleared as much land as possible and used their earnings from wage labor to hire labor to cultivate larger fields. In a very short space of time all available land was privately held by individuals and cultivated more or less continuously. There was thus no vacant land for latecomers to cultivate for subsistence purposes during the long part of the year when wage labor in capitalist agriculture was unavailable. Instead, Joama' landowners began to offer migrant families small grants or loans of land, take them on as sharecroppers, and in other ways to give them access to land for subsistence crops in exchange for their labor. Then, after the local harvest both would migrate for the cotton-picking season to Gezira. Eventually, the more successful larger landowners were able to withdraw from wage labor altogether.

In research in a Joama' village in 1977, I found a number of families with known origins outside the village who appeared to be in different stages of assimilation to the group. Four families, related to each other through their male heads of household, identified themselves to me as Joama' and were referred to by other villagers in conversation as Joama'. After some weeks in the village, however, I learned from one older man that these families "used to be Fellata" (i.e., West African immigrants) who had come to the village in their youth and been granted plots of land by the speaker's father. In this man's view, the "Fellata" families had proved their worth through hard work and cooperation and had become legitimate Joama'.

Another group of three brothers and their families had settled in the village more recently and occupied a somewhat different status. They had settled a separate small hamlet with a number of relatives (Ballala, from Chad) about 200 m from the village and had been given very little land. Most of these families had sharecropped or rented land from the Joama', until all but the three remaining families had moved a few kilometers away to a new Ballala village a few weeks before I arrived. The families who remained behind had moved their houses into the main village and continued to cultivate the small plots that had been given them by a large landowner. Villagers referred to these people as "good Ballala" who were "just like the Joama'."

The comparison between the situations of these two groups of families is suggestive of a process of ethnic assimilation in which each represents a different stage on the way to becoming Joama'. If so, it is a process of formation of an ethnic identity that corresponds to access to a specific location in the agricultural wage labor force. Not all those assimilated to the ethnic identity of Joama' occupy the same position with respect to the labor force. Some are independent, landowning peasants, while others are virtually landless agricultural laborers both in the village and on the capitalist farms. But the ethnic identity that each affirms is conditioned by a local social structure articulated to a regional and national social structure primarily through the participation of most villagers in wage labor according to a definite and distinct pattern identified as the Joama' pattern. There are also a few other individuals and families from different ethnic backgrounds who have settled in the village and become Joama', including one of the two richest merchant-moneylenders and an Islamic healer, each of whom received a grant of land and patronage from one of the village's leading families.

An indication that such a process of assimilation has been going on longer than the few decades reflected in the cases discussed is given by the division of the population of the village roughly in half between the two principal Sufi tariqas represented there. The Sammaniya tariqa was the sect most popular in rural areas of precolonial central Sudan and to which the Mahdi adhered, as did the bulk of his Joama' army. The Tijjaniya tariqa was brought to significance in Sudan by West African immigrants, the poor among whom
were almost uniformly Tijjaniya. It could be that the Tijjaniya adherents in el'Igayla are people of West African descent who did not feel the same sorts of pressures on their religious identifications as they experienced with respect to their ethnic identity.

Ethnic Processes and the Labor Force

These are but two examples of a diverse set of ethnic processes that have characterized the formation of the agricultural labor force in 20th-century Sudan. As these cases illustrate, there is not a single unitary ethnic process at work, but many. The dynamics of each, as I have tried to suggest here, derive from the specific intersection of precolonial local characteristics and capitalist encroachment. In rural areas the social composition of a particular ethnic identity has tended to be more or less heterogeneous, but to take its central character from a predominant form of market participation. In urban areas and some rural trading centers, a more narrow occupational definition of ethnic identity—or ethnic definition of occupational identity—has occurred. In both sorts of conditions, access to certain locations in the labor force and markets has tended to become regulated by ethnic identity, often involving substantial ethnic change.

In rural areas the social composition of a particular ethnic identity has tended to be more or less heterogeneous, but to take its central character from a predominant form of market participation. In urban areas and some rural trading centers, a more narrow occupational definition of ethnic identity—or ethnic definition of occupational identity—has occurred. In both sorts of conditions, access to certain locations in the labor force and markets has tended to become regulated by ethnic identity, often involving substantial ethnic change. Whether through coalescence and synthesis of a new identity, assimilation and accommodation of individuals and small groups to shifting established identities, or through other means, people came to participate in labor migration circuits and other markets as “ethnics” of a particular sort.

The ethnic segmentation of the labor market and the ethnic processes which were associated with its development in Sudan corresponded to conditions of capitalist expansion on the basis of absolute surplus value—that is, by insertion into existing communities in ways that required people to work longer to maintain themselves at the same level (see Marx 1976[1867]:643-672). By the mid-1970s capital had fully penetrated Sudanese society in the relevant sense and its further expansion had begun increasingly to bring it into direct competition with subsistence production for land and labor. Under these radically changing conditions, exacerbated by the impact on Sudan of international economic trends and internal political crisis, the dynamic of the labor force began to change dramatically. The needs of rural populations for cash in order to meet subsistence requirements expanded and deepened rapidly. Forest and pasture land disappeared, eliminating direct sources of building materials, fuel, and supplementary sources of food. Terms of trade shifted drastically to the disadvantage of small producers. People generally experienced increasing pressures on their labor time, leading to the adoption of ever-narrower calculations of returns to individual labor time as the sole criterion of work. A shift toward capitalist expansion on the basis of relative surplus value began to occur, based primarily not on direct technical improvements in labor productivity but on socially dictated intensification of work effort, spurred indirectly by technical changes in agricultural operations other than the main peak labor-demanding harvests.

The result of the combination of these forces has been a breakdown in the ethnic structure of the labor force (see O'Brien 1980, 1983; Ali and O'Brien 1984). Individuals who previously migrated with their families to pick cotton began to split up between different jobs in order to maximize income from the work they did. Thus, men, plus women unencumbered by small children, began to work in the higher-paying sorghum harvest while children, old folks, and women with children continued to pick cotton. As workers generally sought higher returns to labor time, increasing numbers eschewed payments in kind of transportation, food, etc., in favor of higher piece rates. Employers in turn, faced with escalating recruitment costs attendant upon inflation in petroleum and other prices and a generally rising wage bill, sought to cut costs wherever they could, and began to cut back the long-distance recruitment effort. The structure of the labor force has consequently taken on an increasingly direct social form, with people of all ethnic groups seeking the highest paid work available to them at the lowest cost. Age and sex are becoming more reliable predictors of patterns of labor force participation than is ethnicity.

An important consequence of these recent trends has been a tendency for long-distance seasonal migration to decline in significance and for labor markets to become more localized within distinct regions. Increasingly, seasonal workers seek to minimize costs of
job searches and to work as close to home as possible. Those who find no wage-earning opportunities close to home, such as many people of Darfur, resettle more or less permanently in areas closer to centers of employment, such as Gedaref District. Paralleling these developments in labor markets, the government began in the late 1970s a program of administrative decentralization based on regions created out of two or more provinces each (Rondinelli 1981). As employers and employees increasingly confronted each other on the basis of class, class struggles became regionalized. The new regional administrative framework provides a basis for capitalists operating on a regional scale to attempt to divert local class conflicts into interregional competition and opposition to the central government and the import/export interests that dominate it. If this strategy is successful, regionalism may come to replace ethnic rivalry and tribalism as a political strategy for small capital and the petty bourgeoisie. Such a development does not imply the disappearance of ethnicity as an important form of identification, but only a change in its structure and significance as class and regional identities take over some of its functions in organizing political and economic relations.

Crosscutting these emerging axes of conflict, however, is an increasingly important dimension of nationality. The rigidly restrictive Nationality Law denies citizenship rights to immigrants and most of their descendants born in Sudan. Such rights are indispensable to gain secure access to independent farmland, higher-paying jobs, and other means of upward mobility. Takari groups are thus effectively frozen in the bottom layers of the unskilled cheap labor force. Recently, thousands of Eritrean and Ethiopian refugees have been joining them through a resettlement program which has been moving them from refugee camps to labor-supplying villages around the major schemes. The social disabilities of these groups rest on their statutory position as legal foreigners deprived of the rights of Sudanese.

Toward a Reconstitution of Ethnicity

The chief intent of this analysis has been to indicate—through specific cases taken from Sudan—the way in which ethnicity as it has been encountered in the contemporary Third World, particularly by anthropologists, has been constituted by the same world-historical process that has produced modern capitalism, wage labor, and class structures. As historically constituted social identities, contemporary ethnicities have fundamental determinations which are as modern and capitalist as are those of the giant multinational corporations. They are not natural objects, slightly modernized traditional identities, relics, or billiard balls. This observation further implies that accounts of the impact of capitalist encroachment on Third World peoples which have taken ethnicity as an artifact of pre-colonial tribal structures have been little more than pseudohistories, based implicitly on oppositional models of noncapitalist society. This is the case because the method of reconstituting the precapitalist past of these societies has often consisted of subtracting supposed capitalist effects on them and then analyzing the abstract effects of adding back in the subtracted elements. That is to say, the starting and ending points of analysis, regardless of theoretical stance, have tended to be identical; analysis itself is pseudohistorical, being based on imputed absences of capitalist characteristics in the past or in existing supposedly autonomous units. The results vary depending only on the oppositional models employed: e.g., market/nonmarket, high technology/low technology, exchange value/use value, etc. Many anthropologists and ethnohistorians have transcended the view of ethnic identities as relics of a traditional past, but to the extent that they continue to treat them as bounded objects they also tend implicitly to employ oppositional models of capitalist and noncapitalist forms in which ethnicity as an organizing principle remains undifferentiated and particularly characteristic of noncapitalist forms. Reading of their work by nonanthropologists almost automatically transposes the opposition back into the terms of modern and traditional. Wolf's project in *Europe and the People Without History* (1982) represents an important step in challenging such oppositional models.

The limitations placed on our analyses by pseudohistorical construction upon a basis of oppositional models seem to me crippling. In the case of African studies, these limi-
tations appear to account for the difficulties encountered and the tortured models developed in attempts to reconstruct precolonial states, class structures, and market systems which are known to have existed but whose bases are poorly understood. Thus we are given "peripheral" or "nonmarketplace" markets latent within "nonmarket societies" (Bohannon and Dalton 1962), an "African mode of production" in which exploitation takes place only between (classless) societies through trade (Coquery-Vidrovitch 1978), and "tributary" modes of production dominating social formations that comprised numerous distinct modes of production (Amin 1976). Capitalist penetration is thus reduced to market creation and/or replacement of one external dominating structure by another, with qualitatively unchanged "traditional" local communities either quickly dissolving or persisting in stunted form.

Such impoverishment of theory leaves us incapable of contending with the complex dynamics of modern ethnic processes, and of finally transcending the apologetic tribal atavism thesis that ascribes contemporary political fragmentation in African countries to the effects of primordial ethnic loyalties. It also renders us unable to anticipate or adequately analyze fundamental transformations within the bounds of capitalist political economy as that which has been restructuring the agricultural labor force in Sudan over the past decade. Analytically, tribalism, regionalism, and class struggle come to appear as mutually indeterminate alternative forms of social conflict linked by an implicit evolutionist schema. Social forms such as the ethnic segmentation of the Sudanese labor force tend to be seen—despite theoretical advances in anthropology—as the result of traditional resistance to change or simple colonialist manipulation, a fundamentally ethnocentric view that misses the most central determinant of the process. It obscures the fact that the extractive economy imposed by colonialism foreclosed complete market integration as an option for all but a very few Africans by developing a sharply seasonal demand for wage labor and maintaining a need for people to meet most of their consumption requirements outside markets, primarily through their own direct efforts. Under these circumstances ethnicity represents an organizing principle articulated by the Sudanese people through their creative struggles to respond to the challenges of colonial capitalist encroachment, and thus also was not simply imposed by colonial divide and rule policies. Ethnicity was, of course, not invented by capitalists or people reacting to them. In the contemporary Third World, however, we can only fully comprehend ethnic organization—and its historicity—in relation to modern social struggles in the context of peripheral capitalist development. Such an understanding must avoid substituting a new formalism for the old, and therefore must be sensitive to the variable character of peripheral capitalism as well as to the specificities of central capitalism and socialism as they affect the principles of ethnicity.

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PASTORALISTS UNDER PRESSURE IN PRESENT-DAY SUDAN: AN INTRODUCTION

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PASTORALISTS UNDER PRESSURE IN PRESENT-DAY SUDAN: AN INTRODUCTION

Barbara Casciarri and Abdel Ghaffar M. Ahmed

On a global scale, Sudan perhaps ranks first in terms of pastoralist population size (Markakis 1998: 41). About 66 per cent of the country is arid land, which is mostly pastoral habitat. It is estimated that pastoral activities involve 20 per cent of the population and account for 40 per cent of livestock wealth. The livestock sector plays an important role in the economy of the country, accounting for approximately 22 per cent of the Gross National Product (GDP), meeting the entire domestic demand for meat, 70 per cent of national milk requirements and contributing almost 18 per cent of the country’s foreign exchange earnings by the late 1980s. It is also a very significant source of employment for about 80 per cent of the rural workforce.

Table 1: Total and nomadic population in Sudan, 1955–1993

<table>
<thead>
<tr>
<th>Census year</th>
<th>Total population</th>
<th>Nomadic population</th>
<th>Percent</th>
</tr>
</thead>
<tbody>
<tr>
<td>1955</td>
<td>10,263,000</td>
<td>1,405,000</td>
<td>13.69</td>
</tr>
<tr>
<td>1973</td>
<td>14,819,000</td>
<td>1,630,000</td>
<td>10.99</td>
</tr>
<tr>
<td>1983</td>
<td>20,564,000</td>
<td>2,191,000</td>
<td>10.56</td>
</tr>
<tr>
<td>1993</td>
<td>N/A</td>
<td>N/A</td>
<td>13.80*</td>
</tr>
<tr>
<td>1998</td>
<td>N/A</td>
<td>N/A</td>
<td>18.00*</td>
</tr>
</tbody>
</table>

Source: Ahmed et al., 2002: 12. Note: *estimate

The different census figures referring to nomadic people in Sudan can be considered an underestimation, starting with the first one from 1955/56. This was, and continues to be, a result of the unsatisfactory definition of "nomads" adopted by the authorities, which was based on the type of dwelling that these people inhabited. This being the case, a large number of nomads were excluded. Most of the time, censuses are carried out during periods of agricultural operations, hence nomads who used to cultivate small farms and stay on them for short periods in the course of their movements were considered to be a sedentary population. Census officials did not easily reach those who moved with their animals. Attempts to remedy this situation resulted in estimated figures rather than actual figures. This explains why the figures for 1993 and 1998 were just estimates, the basis for which was not explained. Even though the figures from the 2008 census have not been released, much cannot be expected. In fact, even if the question on nomads was included as one of the seven categories of family or social groups in the short questionnaire, the criteria for defining ‘who is a nomad’ remains ambiguous.
Pastoralist groups in Sudan are enduring multiple marginalization processes, escalated by strict land laws and misguided development plans promulgated by the state. This situation is further exacerbated by an administrative vacuum resulting from the abolishment of indigenous mechanisms that used to govern relations between individuals and groups in rural areas and organize their systems of utilization of available natural resources. Those who studied these groups from the 1950s onwards recognized three types of systems of organization of pastoralism, namely: (a) pastoral nomadism, which is the regular movement of people and entire families with their animals in search of pastures and water. Pastures are often discontinuous and connected by access routes. Each group has traditional and exclusive rights of residence and exploitation of a territory. These rights are referred to as the group dâr (homeland). Their capital is kept mainly in the form of animals. It is unfortunately apparent that this territory is shrinking due to the rain-fed agriculture favoured by the planning strategies adopted by state; (b) semi-nomadism or agro-pastoralism, in which part of the family is left in the dâr while the remainder moves with the animals in search of good pastures and water. Those who are left behind engage in a variety of occupations, the most dominant of which is agriculture; (c) transhumance, which is a form of pastoralism practised by sedentary cultivators whose major economic activity the majority of the time is agriculture and movement is generally from a permanent base. However, these categories can no longer be clearly distinguished as separate entities. The most common activity is agro-pastoralism, which is practised by almost all those we refer to in the following text as ‘pastoralists’.

Since the mid-1940s, land policies first designed by the colonial administration and later adopted by the national governments have tended to marginalize pastoralists. The Soil Conservation Committee Report of 1944 recommended that ‘where nomadic pastoralists were in direct competition for land with settled cultivators, it should be the policy that the rights of the cultivators be considered as paramount, because his crops yield a bigger return per unit area’ (El-Tayeb 1985: 35). National planners who are exponents of sedentarization followed suit and adopted a nearly total disregard towards nomads. By the early 1960s, investment in rain-fed semi-mechanized schemes was strongly promoted and took over most of the land that pastoral groups utilized in most parts of the country. This in turn led to serious conflicts between pastoralists and scheme owners, and settled communities close to the grazing areas. This was further exacerbated by the civil war, which denied pastoralists who used to move south access to dry season grazing areas.

With reference to planning strategies, progress in the development of the pastoral sector can be explained through the types of paradigms that have dominated the scene in the past sixty years. Three major paradigms have influenced the rural production systems in which pastoralists played a significant role. The settlement paradigm, propagated by the exponent of sedentary civilization, dominated in the late 1950s and 1960s. This paradigm regarded the pastoral system of livelihood as rudimentary and encouraged pastoralists to settle and integrate into sedentary society. The service driven paradigm followed in the early 1970s,
recognizing the pastoral sector's contribution to the national economy. It argued for the need to support and develop the pastoral sector through the provision of services to both the human and animal populations to help ensure the continuity of pastoralist contributions to the gross domestic product and export sector.

The market driven paradigm propagated by the state policies of the 1970s considered Sudan as the breadbasket of the Arab world. The World Bank and other interested parties followed suit, mainly focusing on the expansion of agricultural production. Expansive areas of land were appropriated in favour of irrigated and rain-fed agricultural schemes, which strengthened the link between the national and global economy. World Bank and other foreign investment in areas traditionally used by pastoralists led to the rise of the competing mechanized schemes paradigm, bringing about a reduction in grazing lands and forcing most pastoralists to settle.

This development is now paving the way for the gradual emergence of a human development paradigm. Though not yet fully having taken shape, this paradigm, in which genuine attention is paid to human capital, seems to have found ground among pastoralists who have chosen to transform their production systems through the diversification of resources and activities that enable them to maintain some aspects of their traditional systems of livelihood and traditional culture, while at the same time improve the quality of life for future generations. However, it has to be noted that another paradigm is also active at this time. That is the conflict paradigm, which has been ushered in by the civil wars. Pastoralists are among the major groups suffering from the prolongation of these civil conflicts in different parts of the country. These conflicts are negatively impacting the daily life of pastoralists, limiting the distance of their movements and destroying the symbiotic relationship they use to have with settled neighbours.

The papers in this issue, each in its approach to the topic, address the context in which these paradigms have emerged and assess their success and failure, their impact on the pastoral sector and its contribution to the national economy and its engagement with the global system. They also consider possible future scenarios for the development of pastoral systems of livelihood under such pressures.

The focus on 'Sudanese pastoralism' in scientific literature: a persisting neglect?

In a short article about 'Arab and arabized' nomads in Sudan (living between the 22nd and 10th parallels), historian Nicole Grandin raises the question of the longstanding lack of research concerning the groups within the socio-anthropological studies devoted to various Sudanese peoples (Grandin 1980). This gap was even harder to justify given the demographic importance of pastoral groups who were, according to the first population census from 1955/1956, estimated as 31 per cent of the global population in the six northern provinces. Even if the article focused more on the debate about nomad sedentarization and development, the explanation given
by the author concerning this negligence is quite interesting. Grandin argues that, beyond the general attitude of central states towards minimizing the presence of nomads within their borders, there lies a question of approach. Since colonial times, the British have differentiated between politics adopted vis-à-vis Southern Sudan (governed by a paternalist direct administration) and nomadic groups governed by a decentralized native administration in what has been improperly labelled as 'Northern Sudan'. Thus, by fostering the separation between the two parts of the country, an ideological bias came to determine the development of social studies in both parts (also due to the strong link between colonial policies and scientific studies). While the 'primitive' peoples of Southern Sudan were deemed pertinent to anthropological studies, the more complex societies of the Arab-Muslim area were designated as the domain of orientalists or historians. This historical trend was difficult to change, and yet Grandin stresses that at the beginning of the 1980s, only three relevant monographs published between the 1960s and 1970s (Cunnison 1966, Asad 1970, Ahmed 1974) focused on the nomadic groups of the region.

Nonetheless, as has been recently remarked upon (Hassan 2003), the development of Sudanese post-colonial anthropology is closely linked to the study of pastoral nomadism. If the latter were still rare during the colonial phase – and what Hassan calls the ‘transitional period’ – most of the Sudanese university staff within the domain of social sciences were later involved in fieldwork among various nomadic groups. Thus it is assumed that the birth and development of Sudanese anthropology, at least up until the 1980s, cannot be separated from a focus on nomadic societies. Moreover, we should add that for a long time anthropological theory benefited from the contributions of Sudanese studies on pastoralism. The analyses issued by case studies among Sudanese pastoralists nourished various crucial modern anthropological debates, from historical debates about the 'segmentary model' and political structures (Cunnison 1966), to Marxist debates about class and the formation of the elite (Asad 1970, Ahmed 1974, Saeed 1982) up to more recent debates concerning sedentarization, change and development (Abbas 1980, Sørbe 1985). Given this panorama, it is quite surprising that few critical reviews have been devoted to an overview of (academic) studies on the Sudanese nomadic peoples. The book edited by Ahmed in 1976, Some Aspects of Pastoral Nomadism in the Sudan, stands as an important contribution to matching specific case studies with a wider reflection on pastoral nomadism. But despite its undeniable value, this book remains an isolated case and after thirty years – during which time deep transformation has affected nomadic societies in Sudan as elsewhere – both the scope and content need updating. Thus, if works concerning pastoral nomadic groups can be found in various books edited with either a regional focus (Horn of Africa, but also the 'Arab-Muslim' or 'North Africa and Middle East' regions) or with thematic issues (such as development, drought, crisis, war, poverty, and so on), no similar attempt has been made to specifically focus on 'Sudanese pastoral nomadism'.
We admittedly cannot assert that there is a real lack of literature on pastoral nomadism in contemporary Sudan. Indeed, in recent decades, we witnessed a ‘booming’ production in documents that deal with Sudanese pastoral groups. Yet this multifarious literature reflects the particular trends that influenced research on Sudan during this period. On the one hand, pragmatic issues linked to war and post-war contexts (besides the civil wars in the South, the Nuba Mountains, the Eastern Region and Darfur) added an ‘emergency’ character to the general issue of development in studies and interventions among Sudanese (pastoral) groups. On the other hand, global trends determined the rising importance of NGO/international agency (and their recruitment of academic staff) in providing aid and development within the country, which led to a shift in the balance between academic and ‘applied’ studies. This particular trend has meant that the rich number of texts in which the issue of ‘pastoral nomadism’ is dealt with do not translate into an equivalent complexity in the discourse that needs to be developed. This is not simply the fault of the authors of the aforementioned texts. Rather, it is the difference between actors, their objectives, notions and methods of research that creates this gap. And even when ‘consultants’ who are writing reports have a good academic background, the general framework in which their contribution is included prevents them from approaching the targeted (nomadic) groups within the complex reflection that the scientific historical background on social studies on (Sudanese) pastoralism would require. Thus, Sudanese nomads continue to be perceived of as ‘a problem’ – and to be approached as an interesting focus by virtue of this status. They are also considered poor and in need of relief, not yet integrated into the wider societies (and economies) within which they experience the process of urbanization or sedentarization and they are considered a crucial actor in armed conflicts. The attempt of this issue of Nomadic Peoples is to go beyond the multitude of reports produced over the last few years for the use of NGOs or international agencies (with a more applied concern, and using ‘rapid methods’ of data collection) to achieve a deeper analysis of that which is crucial to social scientists developing their fieldwork research on pastoral groups.

The general approach of an issue on nomadic pastoralism in contemporary Sudan

The above observations brought us to the idea of proposing a monographic issue on ‘pastoral nomadism in contemporary Sudan’, which without the ambition of being exhaustive, aims to bring together scholars who have done recent fieldwork among such groups. By way of empirical case studies, such an issue aims to enter into the wider debate of social sciences on this topic. Two other main aspects of the current Sudanese context illustrate the pertinence and interest of a similar contribution. The first is that the supposed ‘end of pastoralism’, which for decades has been claimed by various states, international agencies and scholars, seems not to be a reality in contemporary Sudan. Pastoral nomadism is one of the most
adapted forms of resource exploitation in the country. It is historically rooted as a basis for the livelihood of different communities in the arid, semi-arid and savannah zones. Various factors have contributed to the weakening of the basis of production and reproduction of pastoral peoples, as well as to a decline in their contribution to the national economy (Mohamed Salih 1990). These factors include state pressures and its appropriation of land (often backed by the intervention of international investment agencies), the promotion of sedentarization as the privileged solution to the ‘nomad problem’ and the proposal of wider national economic policies neglecting the nomadic and agro-pastoral sector. An unfavourable ecological and economical global environment further aggravates this situation. Nonetheless, at the beginning of the twenty-first century, pastoral production still constitutes a fair part of GDP (22 per cent) and global national export (18 per cent), and to different degrees, a fair part of the Sudanese rural population continues to rely on extensive mobile herding for its subsistence (16 per cent). Whereas slight changes did occur in the discourse of different (non-pastoral) actors regarding nomadic pastoralism in Sudan during the last years, it is still possible to see – from independence up to today – a constant trend towards the marginalization of pastoralism and the inability of conceiving of it as an integrated social and economical system.

The second aspect concerns the rapid and deep changes from the political, economic and ideological points of view that the country has undergone at the national level over the last two decades and that have inevitably affected the production and reproduction of pastoral societies living within Sudan’s borders. For these reasons, and because of the fact that pastoralists’ strategies themselves have changed and renewed their options in recent times, we consider a review of the contemporary situation of pastoral nomadism in Sudan a useful step in the general debate on nomadic peoples. Here, we resume the main issues – flagged separately, but intrinsically linked in the processes of social dynamics – that constitute the focuses of a more general analysis based on various case studies from Central, Eastern and Western Sudan.

Land issues
The dispossession of nomadic land rights is not a recent phenomenon, but the last decades have intensified and accelerated this process. Thus, the reduction of available space for herd grazing or subsistence complementary agriculture – coupled with an ecological degradation of natural resources – today constitutes a major threat for pastoral Sudanese groups. Recent modifications of land laws consolidated an already unfavourable legislation for nomads, and the expansion of private ownership and mechanized farming contributed to the restriction of pastoral access to land as well as to the proletarianization of the nomadic labour force. The recent abolishment of the principle of dâr (defining tribal territorial rights) contributed to an increased competition and the anarchical exploitation of land. The growing constraints of access to land (and to water sources as well) is one of the main
hindrances in the management of pastoral production as well as a source of conflict between pastoralists and farmers, or between different groups of pastoralists.

Conflict and war
Despite the signing of the CPA (Comprehensive Peace Agreement 2005), which ended the long war between Northern and Southern Sudan, the country has not yet attained peace and the implementation of the Peace Agreement is far from being achieved. To the contrary, the application of the CPA raised various questions concerning nomads’ livelihoods. These include the return of IDPs (Internally Displaced People) in their original regions, the demarcation of borderlines, the criteria of wealth sharing and the redefinition of land issues in the ‘new Sudan’. Moreover, in recent years regional armed conflicts have emerged in Darfur, Southern Kordofan and Eastern Sudan. All of these conflicts witnessed a strong implication of local nomadic groups, either on behalf of the governmental army or the ‘rebel’ side. War and armed conflicts notably altered pastoral transhumance routes and access to resources, which together with the inter- and intra-tribal relationship of nomadic groups, must be taken into account in understanding the current situation of these groups.

Liberalization
After the Structural Adjustment Plans of the 1980s, Sudanese national policies since the mid-1990s have taken a more definitive liberal direction. In recent years, and more strongly after the beginning of oil exploitation, the opening up of investment opportunities to national and international capital has occurred in parallel to the Sudanese State’s disengagement from supporting social development. The privatization of social services has led to sharp inequities and growing poverty levels. Already marginalized within the national context, Sudanese pastoral groups are particularly affected by these trends. In a context otherwise dominated by market logic, nomadic pastoralism has been disrupted by the spread of the commoditization process, leaving pastoralists more and more dependent on selling their labour force. The result of this development is uneven, as some pastoralists have benefited as a result of being integrated into the market economy, a fact that weakens the traditional networks of solidarity and introduces differentiation and conflict within the pastoral groups themselves.

Reshaping political institutions
The coming to power of Ingâz (1989) supported a reshaping of local political institutions and administrative units while reinforcing a federal system. Even if nomads were affected after the urban population, the process has had a remarkable effect on them too. The ‘revival’ of native administration institutions (formally abolished in 1970) promoted by the government in the mid-1990s resulted in a proliferation of native leaders and competition between new and old elites, and thus did not produce an efficient local administration system and popular participation.
as was claimed. Consequently, the spreading of the *lajna sha'abiya* (popular committees) – an institution created by the Islamic government – from urban to rural contexts determined among nomads a further overlapping of the sources of local power and the increasing social control by the state on local communities. Already suffering for the vacuum of authority created since the 1970s by the abolishment of native leadership, nomadic communities more and more face the negative effects that power competition and leadership fragmentation brought forward as a result of the lack of appropriate mechanisms for conflict mediation.

Two general remarks need to be made before going through the review of contributions to this volume. The first concerns the ‘absence’ of case studies from Southern Sudan. The first reason for this limitation could be related to the different form of pastoralism that characterizes the groups of the arid semi-desert zone, the savannah belt and the southern areas of the country. These groups are possibly more accurately labelled as ‘semi-nomads’, ‘agro-pastoralists’ or ‘transhumant’. The second and most relevant reason actually rests in the fact that Southern Sudan was affected by war up until 2005. Aside from the serious disruption to local pastoral livelihoods, this element explains why it is rare to find social scientists that have conducted fieldwork in the South over the last decades. Following the peace agreement there were a myriad of interventions in the South (including those concerning pastoral peoples), although the latter (and the reports related them) are more grounded in the sphere of international agencies, NGOs and similar perspectives not targeted by our issue. Nonetheless, we refused the idea of presenting this issue as one focused on ‘Northern Sudan pastoralism’. This is because, first of all, contributions include a wide range of marginal (neither ‘Northern’ nor ‘Central’) groups and areas such as Western Sudan (Darfur), Eastern Sudan and the transitional areas of Southern Kordofan and Blue Nile. It would be inappropriate to class these areas as ‘Northern Sudan’ simply to emphasize that Southern Sudan is not included. Secondly, even if often used, the label ‘Northern Sudan’ sounds reductive and in some way ideological, in as much as it allows one to think of the geographical northern and central Sudan as the exclusive domain of ‘Arab-Muslim Sudanese groups’ assimilated into the Middle East and North Africa. This is also inaccurate to the (pastoral) Sudanese context, and would require much more debate. Thus, while regretting the lack of contributions on Southern Sudan, we took this option for the above-mentioned reasons.

The second remark concerns most authors’ use of a controversial notion that has been previously criticized by social scientists: namely that of ‘tribe’, which normally corresponds to the vernacular Arabic term *gabîla*. Starting from the 1990s, a huge anthropological literature (Bonte et al. 1991 and 2001, Abdul-Jabar and Dawod 2003) again ‘opened’ the debate about the legitimacy of the notion of ‘tribe’. This literature takes into account the earlier criticism of this notion that had begun in the 1960s and 1970s. Thanks to fieldwork evidence and theoretical analysis, the scholars engaged in this debate affirm that, particularly in the area roughly defined
as ‘Arab and/or Muslim’ (stretching from Maghreb, northern Africa and the Middle East up to Afghanistan and Pakistan), ‘tribal model’ still has pertinence both in the use and conception of peoples defined (by themselves and by outsiders) as ‘tribal’ and in the actual local dynamics of power relations. More specifically, as far as Sudan is concerned (at least the regions of Sudan focused on by those articles), the complexity defined by the term gabila is something still highly significant in economic, political, socio-cultural processes, and is far from denoting a fixed, ‘traditional’, isolated reality. Thus, even if we may agree that this notion has been (and continues to be) controversial in anthropological theory, we consider that at least for the context that is dealt with by the contributions of this volume, its use constitutes an important analytical tool for understanding the historical and current social dynamics of Sudanese pastoral groups.

The contributions of this volume

The seven articles in this volume focus on one or more of the main common issues outlined in this introduction. They each present a regional case study pertaining to a specific pastoral group whose dynamics are analysed by underlining their significance within the recent socio-economic, political and cultural transformations in contemporary Sudan, and with reference to the wider debate by social sciences on modern pastoral nomadic societies.

Hassan Abdel Ati’s article provides a detailed description of the methods of conflict prevention and management embodied in the salif customary law of the Beja. The author argues that the attention paid to similar local models on topics pertaining to the socio-political organization of pastoral peoples could be useful in avoiding the failure of national and international ‘peace building’ interventions, which often impose models borrowed from other situations and unsuitable for the local case. Far from being a simple idealization of ‘traditional’ customary systems, this issue remains particularly relevant for the Beja who recently experienced the escalation of conflict leading up to armed regional civil war (2004–2006). Sandra Calkin’s article deals with the same area, Eastern Sudan, but with another pastoral group known as the Rashâyda, whose status as ‘newcomers’ from the Arabian Peninsula has strongly conditioned their access to land and political representation. She describes how this pastoral group entered the political arena within the context of the recent armed conflict in the east, and developed a subtle game of alliances both with neighbouring Beja groups and the state, which has been used to support livelihood strategies and cope with the serious consequences of drought.

The article by Casciarri focuses specifically on the conflict between communal practices and market logic that characterizes most Sudanese pastoral peoples, particularly in this recent phase of global capitalism diffusion. She uses two case studies – the Ahâmda of Central Butâna and the Awlâd Nûba (Hawâzmâ) of Southern Kordofan – to illustrate common features of these pastoral groups. These include the embeddedness between the
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Map 1. Pastoral Sudanese groups included in the issue

access and management system of the material resources and the socio-political symbolical system of the *gabila*, stressing the contradiction inherent in national and international 'peace building' interventions that pretend to protect common interests yet ambiguously support the dominance of neo-liberal globalization in Sudan. El-Hadi Ibrahim Othman deals with another important pastoral Sudanese group, the Fulbe of the Blue Nile State, to whom little attention has been paid by social scientists. Here as well

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the strategies for accessing economical resources are examined for their strong link with political ones. The Fulbe, who were also marginalized as relative 'newcomers' (from Western Africa) by local populations and authorities, progressively converted from 'exit' to 'voice' strategies. Aided by the events of the recent civil war in which the Blue Nile was a major front, they experienced militarization and put into place an alliance with the Northern Government, which resulted in material and political advantages for their group. In the same region, Abdel Ghaffar M. Ahmed presents an update of the Rufa’a Al-Hoi (Arab) pastoralists, who he has been working among since the 1970s. The Rufa’a have been affected by a well-known set of factors that have disrupted pastoral modes of production for decades: detrimental state policies, land encroachment, ecological stress and civil war upheaval. An in-depth analysis of select household strategies in responding to such a transformation indicates that they could avoid general impoverishment and marginalization by integrating ‘new’ opportunities of economical engagement, tribal restructuring and access to social services.

The land issue, with its strong link to micro- and macro-political and economic dynamics, is at the core of the analysis of Mustafa Babiker’s article on Northern Kordofan pastoral groups. Conflict over land resources, the main base of production and reproduction of such groups is illustrated through various case studies, with marginalizing and incoherent state policies held primarily responsible for this conflict. Land tenure reform is identified as the crucial issue at stake for guaranteeing both viable livelihoods and peaceful inter-tribal relations in the area, where challenges linked to ‘resources access’ are more and more relevant since the 2005 Peace Agreement. The last contribution, an article by Munzoul Assal, focuses on pastoral groups in Western Sudan, a region where armed conflict between groups has been – and still is – one of the most relevant questions in need of address. By stressing the fact that (despite early criticism by some anthropological works) the ‘dichotomy’ between pastoral and sedentary people - with its underlying trend of ‘naturalization’ or ‘ethnicization’ of the conflict – continues to be a dominant paradigm for reading (and inspiring related interventions in) the current crisis in Western Sudan, Assal sees state policies and internationalization as the main causes of conflict escalation. At the same time, he invites anthropologists to develop a more accurate and deeper analysis of the dynamics of complementary relations between the nomadic and the sedentary. The two elements, Assal argues, should be conceived as global systems, with the complexity of their historical, political and economical interaction free from the misconceptions about pastoral nomadism and their ideological weight in an internationalized context of local conflicts engaging pastoral groups.

Far from pretending to be an exhaustive illustration of ‘nomadic pastoralism’ in contemporary Sudan, collecting these articles into a common issue is an attempt to respond to various needs. First, it stresses the importance of in-depth and long-term fieldwork research in order to understand the current dynamics of pastoral Sudanese societies. This approach opposes the otherwise increasingly favoured ‘rapid research
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methods' imposed by the recent turn to consultancy works with applied concerns. In this sense, each article can be read as a separate contribution to the knowledge of specific pastoral groups within their regional context. Second, by focusing on selected topics relating to the 'pastoral question' in Sudan (land issue, liberalization, conflict and war, political institutions resetting), micro-level empirical cases shed light on wider crucial issues relating to the transformational processes affecting the lives of pastoral groups today. The latter claims an integrated analysis of both the actors (pastoral people, their sedentary or nomadic neighbours, the state, the market and international agencies) and of the interrelated spheres of social dynamics (ecological, economical, political, socio-cultural and ideological). At this level, the articles in this issue can be read as being linked by transversal common general issues whose manifestations are seen in local contexts, yet also elucidate a global perspective which transcends that relating to their specific case.

Finally, this issue argues against some persistent visions of the 'end of nomadic pastoralism' in the twenty-first century and its related misleading conceptions of the binary opposition between nomadic and sedentary, of nomadic responsibility for war and conflict and of the conservative economic and political attitudes of pastoralists. On the one hand, the analysis of Sudanese nomadic groups seems to prove that their persistent pastoral option (including the socio-political and cultural patterns of their economic model) is far from being alien to transformation or from being irrational within the 'modern' context of globalization. On the other hand, by showing that social reality is much more complex than national and international actors (who still aim at co-opting pastoralists as subordinated supporters for a dominant market economy, or at their 'domestication' for the objectives of political stability) would suppose, the case of Sudanese pastoral groups calls for the development of a critical anthropological analysis.

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Note on transliteration

Although some of the criteria for the transliteration of Arabic (Fulbe and Beja) terms have not be taken into consideration (for example, the use of diacritical dots for the
emphatic ‘d’, ‘t’, ‘z’, ‘h’), we homogenized the transcription of vernacular Sudanese terms as closely to the local pronunciation and writing as possible according to common coherent criteria. This is done in all cases for the names of regions, ethnic groups and other terms, except when a current use is consolidated with a simplification of such criteria (for example, ‘El-Obeid’ and not ‘El-Ubaid’, ‘Fellata’ and not ‘Fellâta’, ‘Darfur’, and not ‘Dârfur’, and so on).

References


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A SUDANESE RELIGIOUS BROTHERHOOD: 1

AL-ΤΑΡΙQA AL-HINDIYA

The Hindiya տարիqa is probably the only religious brotherhood founded in the Sudan which has a status completely independent of any parent organization. It is, further, one of the few such organizations originating in this century.

The տարիqa founder, Şarîf Yûsuf Muḥammad al-ʾAmin al-Hindi, was born in the Jazîra area in al-Rubwa near Wâd Medani in 1865. 2 His great-grandfather came from the Hijâz and claimed Sharifian descent. 3 Yûsuf’s father was a religious scholar and teacher who acquired a reputation as such in the Jazîra. During the Mahdist period (1880–1898) Yûsuf became an officer in the government succeeding to a post vacated by his deceased brother. He also participated in several battles in the Eastern Sudan. 4 After the British occupation he was active in Sudanese nationalist activities and gained notoriety as a religious teacher, gradually gathering a following which became the nucleus for al-Τarıqa al-Hindiya. The Sharif had been trained in the Sammâniya տարիqa and his organization is an offshoot of this although today it is wholly independent. In 1913 he left the Jazîra and settled in Bûrî al-Lâmâb, a village in the Khartoum suburbs. Among Bûrî al-Lâmâb residents, the most widespread of several conflicting stories is that he was forced to reside near Khartoum by the British so that they might more easily observe and control his nationalist activities. Another version is that he settled here voluntarily in order to be in view of the British after certain Sudanese had reported him as conspiring in the Jazîra against the occupation government.

Beside his home in Bûrî al-Lâmâb the Sharif established a rest house (dīwâšîn) and a religious school (khalwâ). In the latter young boys of the village were trained in Qurʾān and older men were prepared for the role of khalīfa—representatives of the Sharif who functioned in the more remote districts as religious teachers and missionaries of the "path" (տարիqa). Sharîf Yûsuf is the author of the hymns and litanies which form the ritual of the տարիqa. He also wrote a mawlid, a biography of the Prophet, the recitation of which has a central place in the religious services of the brotherhood. In his later years Sharîf Yûsuf began the preparation of a history of the Arab tribes of the Sudan, but

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1 This article is based on anthropological research undertaken in the Sudan during 1959–60 in the village which is today the headquarters for the Hindiya տարիqa. The research was supported by the Social Research Center, American University at Cairo, and by the American University at Cairo. For this support and for the hospitality and co-operation of Sharîf ʿAbd al-Rahmān and the officers of the տարիqa the writer wishes to express his thanks and appreciation.
3 Ibid.
4 Ibid.
it was never published and the unfinished manuscript remains in the
Sharif’s library.
Sharif Yusuf died December 25, 1942, and was buried in Būrri al-
Lamāb where a large tomb (qubba) was later erected beside his house.
He was succeeded by a son ʿAbd al-Rahmān whom he had selected and
trained as his successor.
The Hindiya tariqa has never been too well integrated into the
village life of Būrri al-Lamāb. Most of the villagers had been affiliated
with the Sammâniya tariqa, although only a minority were in any way
active. Members of the major lineage (ḥilā) in Būrri al-Lamāb have
often viewed the Sharif and his family as a threat to their prestige and
power which extended not only over this village but to others in the
vicinity. No more than ten percent of the men over puberty in Būrri
al-Lamāb are members of the Hindiya tariqa. Only a few of those of
the old, established families of the village have joined the organization,
while in contrast much of the local village following is derived from
men who have migrated to it largely because of the presence of the
Sharif. They have tended to marry local village women to a lesser
extent than have other strangers who have settled in Būrri al-Lamāb.
Instead they more frequently marry individuals from their home
districts, or servants or relatives of the Sharif. The Sharif’s compound
in the north end of the village, with its rather large population consisting
of most of the Sharif’s family, and the servants, officers and other
retainers of the tariqa, is viewed by some inhabitants as almost a sepa-
rate village. For many years all the Muslim feast days, the Friday
prayers, as well as the daily prayers, have been observed in two distinct
places in the community: the Sharif’s compound and the local mosque.
The latter represents the non-Hindiya element dominated by what may
be termed the village’s Sunna clique (since they hold that the only
tariqa is the Qurʾān and the Sunna of the Prophet) and the remnants
of the Sammâniya, which as an active organization is now defunct in
Būrri al-Lamāb. These various factors have contributed to division
within the village which, however, has never resulted in any prolonged,
over expressions of animosity.
Social Organization of the Tariqa. The Hindiya tariqa, like so many
other such organizations, is not only a mystical association but it is also
a saint cult. Its uniqueness is derived from the reverence members
bestow on its founder as well as from its special ritual. Sharif Yusuf
inherited some degree of holiness or baraka as a member of a Sharifian
family and as a son of a rather well-known religious teacher, but this
alone does not warrant “canonization.” One must give evidence of a
special degree of religious power before his position as a saint is
validated by the community. Signs of holiness are expressed through
exemplary and superior behavior and through the development of a
“path” which can direct others to the achievement of what they believe
to be more satisfying lives. Any behavior by the holy man which is
interpreted as having or expressing a supernatural quality is of considerable importance. Sharif Yusuf, for example, was accredited with occult powers for divining future events. Through a combination of these signs Sharif Yusuf proved his special baraka and so earned deference from the community and reverence from his followers. From his death emerged a saint cult centered around his tomb. In life, followers approached him with bowed heads, knelt before him, and kissed his hand. In death his tomb is an object of pilgrimage (siyāra) to offer prayers and to request the Sharif's intercession with the divine. Death initiates a saint cult somewhat independent of the tariqa organization itself in that the tomb and the holiness of the Sharif continue to attract a large clientele only part of which is otherwise associated with the tariqa and its activities. Women, for example, none of whom are tariqa members, are frequent visitors to the Sharif's tomb. Individuals who otherwise have no association with the tariqa may visit the tomb on such occasions as a circumcision procession.

As a holy man Sharif Yusuf is a major focus of a saint cult. As the founder of the tariqa and as the author of its rituals, he is a major focus of the brotherhood. He is a most important link in the chain between God and the brotherhood member. Holiness flows through him and is inherited from him, either through direct physical kinship with him or through an apostolic succession from him or both. His successor, Sharif 'Abd al-Rahmān, derived his holiness from the circumstances that he is the son of Sharif Yusuf and that he was selected by the Sharif to succeed him. Sharif 'Abd al-Rahmān does not have the stature or the degree of holiness that his father had before him. Sharif Yusuf was the charismatic leader, Sharif 'Abd al-Rahmān the holder of a position that has become legitimized, the successor who sits in the shadow of a greater man. Informants in Būrī al-Lamāb stated that the tariqa lost a number of followers after the death of Sharif Yusuf. Many of these, however, persist in their veneration of the Sharif and thus as participants in the more amorphous saint cult.

Members of the tariqa greet Sharif 'Abd al-Rahmān as they did his father, and the other sons of Sharif Yusuf and the sons of the present head in only a slightly less subservient manner. These men share in the holiness emanating from Sharif Yusuf by virtue of their kinship with him. However, the amount of holiness so inherited decreases as one is removed kinship-wise from the direct line of tariqa leadership. Thus, grandsons of Sharif Yusuf by sons other than Sharif 'Abd al-Rahmān do not receive such highly deferent treatment.

Immediately under Sharif 'Abd al-Rahmān is a position called khulafa al-khulafa'ā which, however, has been vacant since 1955 when its holder died. The khulafa al-khulafa'ā acted as head of all the khulafa'ā and as the chief representative of the Sharif who appointed him. He was installed in a ceremony in which the candidate kneeled before the Sharif, grasped his right hand and made a vow ('ahd) to be a good
Muslim and a faithful member of the tariqa, that is, to obey the will of the Sharif. The khalifa al-khulafa received the greetings of the assembled group (salāmāt); sheep were slaughtered and a feast held. Religious services followed in which the mevlid written by Sharif Yūsuf was read; hymns were sung and a dhikr performed. Today in the absence of the khalifa al-Khulafa the administration of the khulafa is apparently more directly in the hands of the Sharif and his immediate aides.

The rank subordinate to the khalifa al-khulafa is that of the khalfi. In former times the khalfi candidate attended the khulfa in the Sharif’s compound at Būrri al-Lamāb. Here he studied the Qur’an, Hadith and Fiqh and demonstrated to the Sharif that he not only had an understanding of the principles of Islam but was also a good and worthy man faithful to the tariqa. In addition he was expected to know its litanies and be able to read the mevlid. It is said that the Sharif interviewed all potential candidates for khalfi-ship before appointing them to office. Since 1955 the khulfa in Būrri al-Lamāb has been closed and candidates must receive their training elsewhere. Officers of the tariqa report that attendance at the Sharif’s khulfa, when it was operating, was not compulsory for khalfi candidates; they could attend another khulfa. While the khalfi is expected to be of a rather mature age before being inducted to the office, today younger men are accepted to this rank.

At present a khalfi may be appointed personally by Sharif ‘Abd al-Rahmān or an individual may be asked to fill the position by the people in a given place. All candidates are, however, finally approved by the Sharif.

The ritual of investiture follows the same pattern as the installation of the khalifa al-khulafa but lacks any great public celebration. The nominee kneels before the Sharif or khalifa al-khulafa and proclaims that he is a good Muslim and swears allegiance to the Sharif. After this, a document (ijāza), is presented to him. This is a letter signed by the Sharif stating that the bearer is a good Muslim and conferring on him khalfi-ship and the right to admit members to the tariqa. Notification of the new appointment is given to all the khulfa and members of the tariqa.

The khalfi is expected to train laymen in the principles of Islam and in the “path” laid down by Sharif Yūsuf. The khalfi is a master of ceremonies in religious services of the brotherhood and accepts gifts or zakāt from members for the purpose of maintenance of the organization and carrying on its activities.

Large villages where there are members of the brotherhood may have more than one khalfi, while smaller ones may have none at all. Būrri al-Lamāb, as the center of the organization, is therefore atypical, having several khulafa, most of whom are aids to the Sharif. One acts as the treasurer of the waqf that is connected with the Būrri al-
Lamāb headquarters. This consists of a garden and orchard lands adjoining the Sharīf's house, the proceeds from which were assigned by Sharīf Yūsuf to support the guest house and kitchen maintained by the ṭarīqa for the benefit of visiting brothers.

Below the khalīfa is the murīd or ordinary lay member. Membership is acquired by making a pledge ('ābd) before the Sharīf or his agent, the khalīfa, which states:

I admit God as my creator and Muhammad as my Prophet and Islam as my religion and accept the way that leads to belief.

After making this pledge the new member shakes the hand of the khalīfa who gives him a wirād, a short litany written by Sharīf Yūsuf which the member is expected to memorize and repeat thirty-three times after the morning and evening prayers. He may learn other awrād as well, but this one is obligatory for a ṭarīqa member.

Special groups of muridin known as the Shabāb are organized in various villages for the purpose of holding regular meetings at which they march behind the flag of the ṭarīqa and sing hymns. A Shabāb group in Būrri al-Lamāb meets each week in the Sharīf's compound. Each Shabāb group has a leader (muqaddam) who does not march himself but directs the movements and formations of the marchers. While the Shabāb organization is primarily for young men, several older men belong to it in Būrri al-Lamāb. The Shabāb groups are also organized in the Khattīya ṭarīqa and the Mahdiya in the Sudan. It is apparently a recent innovation under development, according to some informants, as a means to attract and hold younger men. The obvious implication is that there is some fear that religious fraternities do not appeal to youth today and something must be introduced to attract them.

The membership of the Hindiya ṭarīqa has been confined to Blue Nile Province with a scattered following around Khartoum, in western Kassala Province, and al-Rahad town in Kordofan. Leaders in the ṭarīqa assert that at one time they had a half million members and 12,000 Khulūfā'. Even at its peak this number certainly appears to be highly exaggerated. A most liberal estimate might be one fifth of this number for its peak, while today there can be no more than 50,000 members.

The structure of the Hindiya ṭarīqa is a hierarchical one. The ranks are ordered according to the possession of holiness or baraka and this is determined according to one's nearness to the founder, Sharīf Yūsuf, who stands at the apex of the organization. Immediately beneath him is Sharīf ʿAbd al-Raḥmān, followed by the other sons of Sharīf Yūsuf and the sons of Sharīf ʿAbd al-Raḥmān. The next ranks are filled by the Sharīf's specially qualified agents, the khulūfā'. At the bottom are
the ordinary members, some of whom are most active and devoted and, therefore, have informally a higher ranking than the less active and more indifferent members.

In addition to a hierarchical structure, the *tariqa* has what approaches a priestly type of organization. While no one holds the “path” of the *tariqa* to be essential to salvation, it is considered by members as an important aid. The proper path must be learned from the head of the organization or from a *khaliifa*. The latter is essential to the entire system and in turn has a certain amount of professional training and is duly ordained. While most do not make a living from this occupation, it is a full time post for a few. Thus, it is arguable that a *khaliifa* is a kind of priest. In any case the general organization of the *tariqa* contrasts with that of the regular orthodox Muslim community. The *tariqa* is “episcopal”; the latter is “congregational.” Where the *tariqa* tends to priesthood the latter is priestless.

**Financing the tariqa.** It has been reported above that financial and other support for the *tariqa* is derived from gifts or *zakat* and from the operation of gardens and orchards belonging to the *waqf* of the *tariqa*. In addition some of the pilgrims and other visitors to the tomb of Sharif Yusuf and home of Sharif ‘Abd al-Rahman on departing leave contributions which may be in the form of money (from fifty to seventy-five piasters) or livestock. One informant maintained that in the Jazira where the Sharif has most of his following each *khaliifa* travels around his district collecting as *zakat* one tenth of the sorghum crop from members who can afford it. This informant is a former member of the *tariqa* in the Jazira and left it because he disapproved such practices. The writer is unable to comment on the Jazira situation except that other religious leaders in the Sudan have been known to acquire funds in this manner. In Burri al-Lamab contributions are a voluntary matter. To what extent such gifts go purely for maintenance of the organization or into the pockets of its officials remains another unanswered question. *Tariqa* members definitely deny that any gifts go towards enhancing the wealth of the leaders of the organization while some informants, who are not members and tend, in addition, to be ill-disposed towards it, believe that gifts are used to enrich them. In fact, a frequently heard criticism is that members of the *tariq* and of the Mahdist Movement are exploited by their leaders.

Presumably all the income from gifts and *waqf* go to the maintenance of the brotherhood. These expenses include keeping the guest house and kitchen at Burri al-Lamab. Meals are provided here for visitors. Every Friday, breakfast is served to those who attend the *mawlid* reading, and on the ‘Id al-Kabir (The Great Feast), the ‘Id al-‘Saghir (The Little Feast) and the annual celebration of the death day (hamliya) of Sharif Yusuf large numbers of followers—sometimes ranging into several hundreds—are fed and housed for one or two days. A small number of *khulafa* who act as aides to the Sharif and
do not have other occupations also probably live from these gifts. These men incidentally have a small income from the preparation of charms (ṣiḥāb), especially those worn by infants.

*Rituals and ceremonies of the Hindiya Ṭariqa.* The "path" of the Hindiya brotherhood enjoins the member to know and practice the principles of Islam, to recite certain private prayers and litanies at stated times and to participate in the religious services of the organization.

The minimum ritual requirement of any member is the recitation of the *wird* mentioned above following the morning and evening prayer. In addition, individuals are encouraged to learn and recite others. It is commendable to read or recite the ṛāṭib of the ṭariqa as frequently as possible. This is a long litany of almost 1700 words which was prepared by Sharif Yūsuf. It comprises the *Fāṭiha*, ḍuʿāʾ (prayers requesting the mercy and forgiveness of God, making confession of sin, and praising God), the Ṣalāt al-Nābi or prayer for the Prophet and his family, the ninety-nine names of God, and some short Qurʾānic passages. Finally, one is encouraged to perform the private dhikr in which the creed or some name of God is continually repeated by the individual worshipper with the aid of the prayer beads (ṣubḥa). Only the most devout members attempt to fulfill any more than the minimum ritual requirements.

The regular religious service of the ṭariqa is the laylīya, so-called because it is held at night, every Thursday and Sunday. It is, however, also held on Friday mornings in Būrri al-Lamāb. In this village the service is conducted in front of the Sharif's guest house and those attending sit on brush mats that are laid out in a square so that none of the participants has a central position. The laylīya service lasts for about two hours and in it the *mawlid* is read, or more correctly, chanted. Each of several participants reads a chapter. At the end of each, the ṭaylāt al-Nābi is recited by the congregation. Readers include anyone who has been trained to read *mawlid*; they are not confined to the *khulafāʾ*. Sometimes as many as ten share in the reading and so gain *baraka*. At each laylīya tea is served. When the *mawlid* is completed, prayers (ḍuʿāʾ) are said, thus bringing the meeting to a close. In some *ṭuruq* the laylīya includes a dhikr, but such a practice is rare for the Hindiya. In Būrri al-Lamāb, at least, no dhikr is performed at a laylīya.

Attendance at the Būrri al-Lamāb laylīya varies between fifteen and thirty, depending on the number of guests or visitors from other villages who may be present, largely as part of their pilgrimage to this religious center.

The *Shabāb* meetings have already been mentioned and represent another type of religious service of the ṭariqa. Between ten and fifteen young men and boys and a half dozen older men participate in these ceremonies in Būrri al-Lamāb.
One of the characteristic features of any ṣariqa is its dhikr, the group religious ritual aimed at inducing a state of mystical union with God. The Hindiya dhikr pattern is a circle (ḥalqa) of men holding hands and repeatedly flexing the knees and arms as they stand in place. As they move they repeat the phrase: Al-hayy al-qayyim (The Living One, the Self-Subsistent One) while Shabāb members stand in the circle singing hymns. The leader (muqaddam) moves around inside the circle where he can watch all the participants, clapping his hands to maintain or change a desired rhythm. Unlike most ṣarūq in the Sudan the Hindiya does not employ the nawsba, a large bass drum similar to that used in an American brass band. The usual dhikr is unaccompanied by any musical instruments, although occasionally a smaller hand drum is used. Furthermore, the Hindiya members in Būrī al-Lamāb indulge in this ritual only on certain limited occasions. These include the Birthday of the Prophet (Mawlid al-Nabi), the anniversary of the death of Sharīf Yūsuf, the twenty-seventh of Rajab, and the ‘Id al-Kabīr and ‘Id al-Ṣaghir. In some years a dhikr is not held on all of these occasions.

Pilgrimages to the tomb of Sharīf Yūsuf and home of Sharīf ʿAbd al-Rahmān are important religious acts of the ṣariqa member. Pilgrimages are most frequent on the festivals mentioned above. Pilgrims sleep in and around the Sharīf’s guest house and are fed by the Sharīf. Members of the ṣariqa in the village and those more in sympathy with it also contribute food and lodgings for pilgrims who, especially on the hawliya of Sharīf Yūsuf, number two to three hundred.

For the two major Muslim feasts there is the usual morning prayer and sermon beside the tomb followed by breakfast and the special rituals of the ṣariqa, including the marching of the Shabāb, the recitation of the mawlid, the singing of hymns and the dhikr. The Mawlid al-Nabi celebrations are observed on the actual birthday rather than on the usual eve of the Mawlid because extensive ceremonies in Khartoum and Umdurman are observed at the latter time and attract the bulk of the populace. The same rituals that occur on the ‘Id al-Kabīr and ‘Id al-Ṣaghir are performed with the exception of the sermon.

For the Hindiya ṣariqa the most important festival is the hawliya of Sharīf Yūsuf, the celebration of the anniversary of his death. This has been made a fixed date, December 25, rather than one according to the Muslim calendar. Thus the date remains at a time of year when the weather is most pleasant; it is a slack period in the Jazīra between the sorghum and cotton harvests; and it has been an official government holiday—Western Christmas—which gives most residents in Khartoum and Umdurman a free day.

The hawliya is accompanied by considerable secular activity, as are other festivities in this area. Booths for selling sweets, mawlid sugar dolls, fried bean cakes and other foods are set up outside the walls of the Sharīf’s compound. There are also swings and small “Ferris”
wheels. From time to time, whenever music is provided, some men dance the 'arda, an Arab war dance, and in years gone by a special type of horse racing and camel races were held. Some local men indulge in 'araq drinking in the privacy of their homes and attend the festivities in an intoxicated condition.

In addition to the secular activities there are the religious services, beginning with the marching of Shabab. Distinguished guests, including representatives from the government and the heads of the Khatmiya tariqa and Mahdiya, arrive with their attendants and retainers to pay their respects to Sharif 'Abd al-Rahmān. Following this the secretary to the Sharīf delivers a sermon recounting the life of Sharīf Yūsuf. The mawlid is read and, after the singing of a few hymns by the Shabab, the dhikr circle is formed and continued throughout the evening, sometimes until the dawn prayers.

In these festivals women too have their role. They stand in the darkened background observing the male activities and make visits to the Sharīf's tomb where they enter the mausoleum, kneel and kiss the frame cover under which he is buried. Many also place their hand in an opening in the frame to remove a bit of the holy sand within it. As the men monopolize the dhikr and related rituals, so women monopolize the visitation of the tomb during this time.

The Hindiya brotherhood represents a type of tariqa which we might call "Apollonian," employing the terminology which Ruth Benedict borrowed from Nietzsche, in contrast to others which tend to be more "Dionysian." 6 Ruth Benedict writes:

(Nietzsche) discusses two diametrically opposed ways of arriving at the values of existence.

The Dionysian pursues them through "the annihilation of the ordinary bounds and limits of existence"; he seeks to attain in his most valued moments escape from the boundaries imposed upon him by his five senses, to break through into another order of experience. The desire of the Dionysian, in personal experience or ritual, is to press through it toward a certain psychological state, to achieve excess. The closest analogy to the emotions he seeks is drunkenness, and he values the illumination of frenzy. With Blake, he believes "the path of excess leads to the palace of wisdom." The

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Apollonian distrusts all this, and has often little idea of the nature of such experiences. He finds means to outlaw them from his conscious life. He "knows but one law, measure in the Hellenic sense." He keeps the middle of the road, stays within the known map, does not meddle with disruptive psychological states. In Nietzsche's fine phrase, even in the exaltation of the dance he "remains what he is, and retains his civic name."  

In this dichotomy the striving for mystical experience and, thus, Muslim furūq in general, are "Dionysian," particularly when contrasted with the highly formalized ritual of Muslim orthodoxy. Some furūq appear to stress unrestrained states of individualistic abandon or "spiritual drunkenness" and various masochistic rites. Others de-emphasize such behavior. The Hindiya is a tarīqa of the latter, or Apollonian, type. While it has a dhikr, it actually plays only a very peripheral role in religious activities. It is conducted on limited occasions throughout the year and is noticeable for the absence of musical instruments. In addition, compared to the dhikr of many other furūq, that of the Hindiya is more sober and restrained. The most important rituals of the Hindiya tarīqa (mawlid reading, the marching and hymn singing of the Shabāb) parallel the rituals of Muslim orthodoxy in their formalism. The Hindiya members, in fact, lay great stress on their role as Muslim missionaries—teaching the uneducated the principles of Islam. Their "path" or tarīq is not greatly elaborated beyond those rituals identified with the general Muslim community. Some of the more educated members of this tarīqa believe, in effect, that their organization is purely a Muslim missionary society which appropriated the older and available pattern of tarīqa organization. Two such informants explicitly stated that when Islam can be properly disseminated and instilled through other means there will be no need for the Hindiya tarīqa. Obviously this is a minority opinion; one may expect the Hindiya tarīqa to be perpetuated as long as there exists sustained veneration for the persons of Sharif Yūsuf, Sharif ʻAbd al-Rahmān, and their potential successors.

This sober or "Apollonian" character of the Hindiya tarīqa may in part be derived from the influence of the Mahdiya, a conservative and puritan fraternity which has always opposed the furūq and with which the Hindiya leaders have had some close associations. Secondly, the more specifically missionizing role of the tarīqa is, of course, far from innovative; yet the role of these organizations as more purely mis-

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sionaries of Islam, not only to the uneducated Muslim, but to the kāfir as well, has long been of crucial significance in a country like the Sudan—relatively recently Islamized and possessing an extensive "pagan" frontier. Finally, it may be suggested, although the importance of this factor may be questionable, that the rise of the tariqa in the early part of this century, at a time when Western urbanizing and secularizing influences were beginning to be felt in the Sudan, may have dampened any ecstatic mystical emphasis in favor of more "rational" or sober emphases.

The Hindiya tariqa is, nevertheless, in a state of decline. Membership is considerably less than what it was twenty years ago. According to informants participation in festivals has been greatly curtailed in recent years. The religious school at the headquarters in Būrī al-Lamsāb has been closed for almost a decade. Part of this decline may be attributed to the death of Sharīf Yūsuf and, thus, the absence of the charismatic leader. More important is the fact that many of the functions of the brotherhoods in the Sudan have been assumed by the government or have found secular alternatives. Increasingly government primary schools are becoming the free source of religious education. This means that part of the rationale for the Hindiya is gradually becoming superfluous. With the extension of education in general and the advance of secular attitudes spurred on by industrial urbanism, increasing numbers find little or no satisfaction in the brotherhood and its activities. They are less willing to accept "holy" men and submit to their wills. Particularly, partially or fully urbanized young men look upon the brotherhood as old-fashioned. They would far rather play football (i.e., soccer) than march in the ranks of the Shabāb behind the flag of al-Tariqa al-Hindiya.

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