Chapter Three

Cognition and Ethnography

The text of this chapter is a translation of a lecture given in French in memory of the French anthropologist Robert Hertz. It contains a defence of anthropology as necessarily involving both ethnographic interpretation and the attempt to generalise about human beings in general. As in Chapter 2, I argue here that the most important aspects of human knowledge must be implicit and I illustrate this by means of an example of the type of kinship that most concerned Lévi-Strauss.

* * *

The few texts which Robert Hertz[1] wrote in his short life have exerted an extraordinarily important influence on anthropology, especially British anthropology. His work reconciles the two characteristic constitutive elements of social anthropology: on the one hand, its scientific purpose in explaining the nature of human beings in society, and on the other, the interpretative work involved in ethnography. The scientific purpose no doubt came to him from Hertz's involvement in the Durkheimian school to which he belonged; his ethnographic skill is perhaps best seen in his study of the cult of Saint Besse, based, as it is, on the practice of ethnography dependent on intimate contact between researcher and the people studied. This uncomfortable combination of social anthropology, the desire to produce generalising work and the desire to understand "from the inside", poses many theoretical and philosophical problems, but nonetheless it is, I believe, what has been the key to its greatest successes. Malinowski provides a supreme example of this strange marriage in the way he combines the theoretical language of functionalism, derived from the natural sciences, with the evocation of the adventurous life of the Argonauts of the Pacific. Lévi-Strauss, too, at one moment seeks to penetrate the ambiguous statements of the magician about his craft, and at another advances theoretical propositions about the neurological organisation of knowledge.

Some anthropologists, however, would now maintain that this epistemological combination is inadmissible. Over the last few years, a spirit of what might be called fundamentalism has developed in the work of anthropologists who identify with only one side of this dual heritage and who consequently wish to "purify" anthropology of the other orientation. We are therefore faced with two movements which only have in common their rejection of the hybrid character of the discipline.

The Two Fundamentalisms

One type of fundamentalist insists on the hermeneutic and literary dimension of ethnography. Traditional ethnographic writing needs to be rethought because, in their view, its scientific claims are inappropriate and invalid. The other form of fundamentalism is aggressively naturalist and manifests itself in a number of ways. Here, I am concerned with only one of its forms in the work of those anthropologists who see in cognitive science, and cognitive psychology in particular, our only salvation. They believe that in order to progress theoretically, social anthropology needs to rethink itself so as to fit within the greater compass of these disciplines. Some even suggest that anthropology should become a kind of ancillary subject to a general science of cognition.

Although equally critical towards traditional anthropology, these two currents of thought rarely meet because their champions generally regard themselves as belonging to such totally antagonistic theoretical camps that they are unable to find any middle ground.

Those anthropologists who, following first Clifford Geertz and later James Clifford and George Marcus (1986) and Michael Taussig (1987), see anthropology as, above all, a literary enterprise, criticise the "objectivist" and scientific pretensions of the discipline. Their aversion to any link between anthropology and the hard sciences is probably due to the fact that they remember with legitimate distaste previous naively reductionist tendencies in anthropology such as cultural ecology or sociobiology; but they also share the general doubts about the independent nature of the scientific method which characterise our time.[2]

At the other extreme are the anthropologists interested in cognition, who are often impatient with the lack of scientific rigour in traditional ethnographic writing. They want ethnographers to supply more "serious" data, which could then be used with confidence in the attempt to build a genuine anthropology in the true meaning of the term. They almost seem to wish that ethnographers become cognitive psychologists working under quasi-laboratory conditions but in exotic settings.

However, such "experimental" field methods advocated by cognitive anthropologists can only be put into practice when they relate to extremely circumscribed areas of research: for example, the justly famous studies of plant classification of Brent Berlin, and others, which characteristically only look at the taxonomic status of living kinds (Berlin, Breedlove and Raven 1973). These methods yield data which is usually of marginal interest for answering the traditional questions at the heart of social anthropology. Furthermore, when these studies focus on more central problems, such as the study by D'Andrade of the meta-representations of "mind", it seems as if this kind of enquiry can only be attempted within the context of the researcher's own culture: when they and their subjects share the same background, which they can then take for granted (D'Andrade 1987).[3] This is because the very topic, mind in this case, only exists in terms of already culturally constructed concept, and the result of such enquiries cannot therefore supply the type of rigorous data for cross-cultural comparison which the method originally aspired to provide. Unlike categories within the hard sciences, concepts like mind are not fixed or defined by the world independently of cultural context; our objects of study, as soon as they are complex, cannot really be "known" without an in-depth familiarity with their culturally specific phenomenology. Consequently, we cannot make cross-cultural comparisons of the constituent elements as if they were, for example, metals. Such well-known difficulties, which are fundamental problems in the "idea of a social science" (Winch 1958), explain the poverty of many cognitive studies. These have lost the richness specific to more hermeneutic types of anthropology because the practitioners are not able to constantly redefine their analytical tools in the very process of research and analysis as does an anthropologist using participant observation who is continually reflecting on her relationship with the people studied. By abandoning this particular aspect of anthropological research, cognitive anthropologists—especially American ones—have in the end, through their concern to gain scientific credibility, thrown out the anthropological baby with the bath water.

Fundamentalism also makes its appearance in a very different form in the work of those interested in the interpretative aspect of anthropological writing. These, by contrast, stress the "internal" character of the object of the social sciences. The hermeneutic dimension of anthropological practice, upon which philosophers insist (Winch 1958), has long been recognised in anthropology, as the writings of Evans-Pritchard and Geertz clearly show. Such writers tend to also stress the false "objectivity" of ethnographic texts which claim to present facts "as they are". They rightly point to the large gap which exists between the lived experience of the ethnographer (attempts at participation, uncertain communications, the multiplicity of voices—none of which are explicit, the "imponderability of daily life", as Malinowski put it) and the nicely formulated representations that one finds in ethnographic monographs which give such satisfaction to the author and the reader. It is, therefore, a totally welcome development, though not exactly a new one, that such fundamental questioning of objectivism has penetrated the professional shell of our discipline. But we must also consider critically where the recognition of this problem has led us.

The first effect has been to a disenchantment with the informative capacity of ethnography. This has led to defeatism and, amongst some, a desire to abandon totally pretensions to objectivity and to view our writing as works of "fiction" (Tyler 1986). Secondly, it has sharpened our critical focus on the role of the ethnographic author, a critique which often brings with it a "liberal" or "post-liberal" bent to the denunciation of objectivism. For such authors ethnographic representation is neither different in nature nor better-founded than any other representation, and there is therefore no reason for anthropology to prefer pseudo-scientific language to that of informants. The ultimate conclusion of such an approach is that an honest ethnography should consist of, more or less, the verbatim recording of conversations which have taken place between the ethnographer and his informants. And because of the somewhat showy humility of the author in this type of work, the informant's words hold prime position. Since there is no reason to highlight the words of any particular individual, ethnographic texts ought to become merely an array of quotations. Quotations from women and men, old and young, important or not, all should be juxtaposed without order in a monograph without structure, since organising this text would result in the imposition of an author. By such means, anthropology would return to an "innocent and naive" state in which all scientific pretensions are abandoned (Dwyer 1982).

It is worth noting that this extreme post-modern approach is actually similar to a pre-Malinowskian conception which continues to inform certain contemporary ethnographies of a totally different character. These are ethnographies undertaken after short periods in the field involving no real participation, and which result in the direct publication of more or less structured interviews. Such ethnographies, both the old and the modern, again amount to no more than quotations of what certain informants, often described as experts, confided to a tape-recorder during officially organised visits.

The rationale behind all these ethnographic approaches is the same. It is based on what appears to these authors a self-evident question: Who could tell us better about a culture and a society than those who live within it? Thus the criticism of ethnography's scientific pretensions leads to simple acceptance of what our informants say. Their words supposedly offer direct access to their knowledge, culture, and society. In the end, therefore, this mock naive approach constitutes just as radical a theoretical fundamentalism as that of the cognitivists, though going in an opposite direction.

Cognition and Interpretation

These two tendencies both seem to offer good reasons for abandoning the hybrid character of social anthropology and to retain only one component. Is it possible, however, also to argue in favour of its continuation?

Dan Sperber, an advocate of the cognitive tendency, proposes one scenario for maintaining the link between the two apparently irreconcilable elements of the discipline. He posits the need for a preliminary divorce between ethnography, which must be interpretive, and anthropology, a generalising scientific subject, as an indispensable preliminary to their remarriage. This remarriage would be possible once a "descriptive commentary" is incorporated into ethnography, so that the relation of interpretation to the empirical basis experienced by the ethnographer might be critically evaluated. Ethnography would then provide usable raw material for a scientific anthropology (Sperber 1982). In this scenario, interpretive ethnography would only have an ancillary role.

For my part, I am less optimistic than Sperber about the value of his "descriptive commentary", and I doubt that ethnography, hermeneutic by definition, could ever straightforwardly provide anthropology with the type of data Sperber's anthropology would need. Furthermore, I am, in the first instance, an ethnographer, and I assign to ethnography a central position in anthropology, so I cannot accept the small role which Sperber gives it in his redefinition of the relations between ethnography and anthropology. Like him, however, I want to maintain the Hertzien double-sided character of social anthropology, albeit in a different way.

Sperber asks what it is that ethnography can bring to cognitive science. I prefer to ask here the opposite question: What is it that the various cognitive sciences can contribute to ethnographic practice? I, in fact, want to argue (1) that ethnographers cannot afford to ignore the findings of cognitive sciences if they want their work to lay some claim to objectivity, and (2) that they can do that without taking refuge in the study of phenomena marginal to most social anthropologists.

But why does the ethnographer need cognitive sciences? Simply because one of the essential aims of ethnography is to produce representations of the knowledge of the people we study, even if this knowledge can only be reached implicitly by observing practices and imagining their interpretations. If people's knowledge, in its broadest sense, is an essential object of what we study, it is necessary to reflect on its nature, its psychological organisation, and to be able to explain it in such a way that we can account for one of its most fundamental yet problematic features: the incredible speed and ease with which it can be used. I would argue that all ethnographers employ, whether they are aware of it or not, general psychological theories as soon as they try to make us understand how the people they study see the world and what motivates them in their actions. These theories cannot and, therefore, should not escape from critical examination, especially from disciplines specialising in the study of knowledge in use. In particular, these disciplines will teach us to be wary of the traps laid in our path by the received folk psychology of the ethnographer, which because of its misleadingly "obvious" character evades serious examination.

Lévi-Srauss was aware of this problem when, in the 1950s and 1960s, he used linguistics and cognitive psychology to create what he called structural anthropology. Such a step is still necessary today, but it must be repeated, in particular, since the cognitive psychology he then made use of has subsequently moved on significantly.

Speaking and Knowing

To understand the problems which stem from the use of folk psychology by ethnographers, it is useful, by way of example, to look at the relation of knowledge and language as it is implied to exist in the writings of a number of anthropologists.[4]

Most ethnographic monographs are based on the notion that the language of our informants provides direct access to their knowledge. This is a highly problematic proposition. I have already described the extreme position of those ethnographers who chose simply to record what their informants say and to leave it at that. But without going as far as that, many anthropologists confuse, for example, the rules that informants will occasionally spell out and effective control of social practices. They often tell us that certain words in the language of the people they study are "concepts", ignoring, or ignorant of, the extensive literature which shows how problematic such an equation is. Sometimes merely influenced by lexicographic features, they attribute a "cognitive" quality to relations of hierarchy or opposition which are then grandly qualified as structural. Such implicit but immense theoretical leaps beg many questions. To begin to suggest what these problems might be I turn to two examples, one drawn from a well-known experiment in cognitive psychology, and the other from my own fieldwork.

The experiment goes as follows. Subjects are briefly shown a picture of a totally ordinary office; secretaries are sitting on chairs in front of their work tables, upon which are placed folders, typewriters, computers, and so on, but on one of the typewriters there are two bananas. About half an hour later, the subjects are asked to draw up a list of all the objects in the picture. Nearly all of them, first of all, mention the bananas, and none of them ever forget to mention them somewhere in their list; by contrast, their memory of the other objects tends to be much more inexact (Friedman 1979). The results of this experiment are not surprising, but they illustrate nicely the kind of thing which cognitive psychology can teach the ethnographer.

We can easily explain the results of the experiment. The furnishing of the room is a familiar spectacle, and so attention is drawn towards the unusual, in this case, the bananas. This psychological capacity of paying attention to the uncommon is clearly useful: It allows us, in our daily life, to focus our interest immediately on what might require a less foreseeable and "automatic" response than an action motivated by objects whose presence is "taken for granted". But what is familiar to each and everyone in a particular historical context is precisely what anthropologists call culture.

To know a culture is therefore to have successfully stored in our memory knowledge of the type as what is a normal "office". This type of knowledge is often called a schemata in psychology (Schank and Abelson 1977).[5] Such a schemata permits us not only to recognise an office—the various elements which make up its furniture—but also to know how to react towards it in an appropriate manner. In fact, this type of schematic knowledge is a more complex phenomenon than it might first appear. It is clear that even in a relatively homogeneous culture, all actual examples of office furniture are different. Holding such a schemata enables the individual to recognise not just a particular office but all the occurrences of what could be an office and to act according to all the possible requirements of this category in a quasi-automatic fashion, without paying much conscious attention to the actions which an office is likely to entail for them. Paying attention, as here, also often implies speaking about it because, as we saw in the experiment, people speak about the unexpected and not about the familiar. The fact that the subjects of the test did not easily mention the usual furniture of an office, however, did not mean that the subjects of the test had completely forgotten the various elements which make up an office when they were asked what they had seen. In a sense, it could be said that they remembered these things too well but not in an explicit verbalised way. Thus even in such a straightforward case, as in this experiment, we see that "knowing" involves different types of activities. To know what offices are like within our own culture is to stock a whole series of implicit and closely interlinked theories. These theories enable us to recognise the occurrence of "offices" and to record rapidly a multitude of phenomena which are then "taken for granted", without normally having to consciously pay any great attention to them or speaking of them. Furthermore, these theories enable us to react extremely rapidly in terms of the schemata, "without thinking". On the other hand, knowing that there are bananas in this office is a different type of knowledge; it entails storing this information in one's memory in such a way as to be able to mobilise it consciously with ease, to speak of it, and to act consciously in response to it.

A great deal of work in cognitive science is relevant to this observation. For example, according to connexionist theories, the difference between these two sorts of knowledge takes on a very special significance. This theory, which admittedly does not enjoy universal acceptance, helps to formulate this type of problem better and to understand the reasons why it is so difficult to speak about familiar schema, or in other words, to provide an account of one's own culture. Knowledge of schemas, such as the office schema in our example, is probably organised in the brain in a radically different manner to the linear and sentential logic of language. In particular, such nonlinear organisation, in connectionist networks, would allow for the mobilisation of "fundamental" knowledge, at the very instant that we act in a familiar environment. Moreover, this process happens at sufficient speed for this knowledge not to occupy too much "space" in our brain and thus not to be easily put into words, and, by this means, leaves enough room for coping consciously, and therefore linguistically, with the unexpected.

The significance of this type of consideration for ethnographic practice is immense. The first lesson to be drawn is that one must not confuse what people say with what they know. Different types of knowledge are organised in different ways, each with its own specific relation to language and action. Normally, the most profound type of knowledge is not spoken of at all. Indeed, speaking of it transforms its nature, since it is because one is unable to speak of it that it can be used as such a basic guide, with such speed and suppleness. This type of knowledge must be implicit, which is a great nuisance for the ethnographer, since it is precisely knowledge of this sort that anthropological research claims as its subject matter.

Secondly, schema theory may help us understand better what it means when informants appear to hold different beliefs from one another. Without wishing to deny the existence of real differences, many of the differences that the ethnographer comes across might, in fact, hide more fundamental agreements, simply because informants do not speak about what is fundamental in their culture and which they therefore most likely share. What they will talk about might, on the contrary, simply be about what is most unexpected to them, which, by definition, is not shared.

Similarly, schema theory explains why it is that informants are generally incapable of explaining to us what they should do in rituals which the ethnographer cannot witness but that they can, nevertheless, perform with ease when the time comes. Such inexplicitness is partly due to the incongruity of the anthropologist's questions, but mainly because such knowledge is organised in such a way as to be simply accessible for practice, and thus speed, but not for verbal exposition. There is thus a contradiction between the ease with which we use knowledge and the extreme complexity involved in explaining linguistically the mechanism which enables us to do so.

Thirdly, what all this means is that the hermeneutic process which is most problematic because of the distance between knowledge and interpretation is not, as it is often assumed, that of ethnographic writing, but that which has to take place in the head of an informant when he or she is asked to explain a practice and its significance. If basic knowledge normally remains implicit and cannot be directly expressed in words because of its nonlinear organisation, then the informant who tries to answer us in language—which is necessarily linear—must proceed to a fundamental reinvention since no translation is possible. Anthropologists, such as Geertz, have correctly stressed the existence of two levels of interpretation in ethnography, but by refusing to take into account what cognitive psychology could teach us, they have lacked a framework with which to adequately deal with the problem which informants have to undertake when faced by an anthropologist. Similarly, anthropologists who simply reproduce informants' words are not getting any closer to their knowledge than those who apparently interpret most freely.

The foregoing remarks, although merely indicative, are intended to illustrate the relevance of cognitive psychology for ethnographic practice, even when ethnographers claim to be merely recording "naively" what others have said to them. We have to face the fact that we cannot speak about the knowledge of others, if we have not also seriously considered the nature of "knowledge". For such a task the implicit folk psychology of most anthropologists simply misleads.

A Malagasy Example

Some anthropologists might object here that the above discussion might well be true of the kind of phenomena dealt with in the office-and-banana experiment but that it has nothing to do with their usual concerns. To show that this is not so, I now turn to one of the most classical of anthropological subjects: the study of a kinship structure which Lévi-Strauss would characterise as an elementary structure with direct exchange, and which others would call Dravidian.

My example concerns the Zafimaniry of Madagascar, which I have been studying for over twenty years (Bloch 1992). In common with that of a number of South East Asian peoples, Malagasy society seems at first disconcerting in that it seems to lack clear organising social principles. As a result, a number of anthropologists have alleged that it is practically impossible to give satisfactory ethnographic accounts of such societies (Wilson 1977).

I could not help sharing these sentiments when I first arrived in Zafimaniry country. I was then interested in their marriage system, but it was impossible to obtain precise information on the subject from the Zafimaniry. Their explicit discourse was limited to a very few negative rules common to the whole island: The marriage of descendants of two sisters is forbidden (over a number of undefined generations), and one should not marry a classificatory mother or father—that is to say one should marry within one's own generation. The kinship terminology of reference is of the Hawaiian type; it operates with a minimum number of distinctions, and the terms which do exist to designate parents-in-law, sons-in-law, and daughters-in-law are rarely used. The terminology of address is simpler still: It does not even distinguish between parents and affines, and there is no term with which to refer to affines as a group. Other explicit principles are vague: It is good for a brother and a sister to marry a sister and a brother; couples should get on well together; partners should love each other, and so on. In other words, nothing explicit indicates the presence of an elementary structure.

It was therefore with much surprise that after tracing genealogies, I realised that the two parts of the village in which I worked, sometimes called "up" and "down" by the inhabitants, formed two quasi-exogamous moieties, which were exchanging spouses in a systematic and regular fashion. When I spoke to the Zafimaniry about this "discovery", they told me that they too had noticed this phenomenon and that they knew it existed in other villages in more or less the same way. My discovery did not interest them very much. I was not teaching them anything new; for them, it was totally natural to marry in this way, but they could not explain the pattern of alliances to me, just as they could not understand my interest in knowing about it.

After much uncertainty, I had to face a common ethnographic problem. Either I ignored the existence of a structure which was not spoken about and which the kinship vocabulary seemed to deny, and thereby implicitly attribute the marriage pattern to a statistical accident, or I had to try to somehow account for it. Choosing the latter option, I needed to explain how such a well-known pattern could occur without the presence of the rules and the vocabulary that we have all been taught necessarily accompanies such a structure.

Actually, the office-and-bananas experiment helps us in a preliminary way make sense of this type of situation, since it enables us not to be surprised by the lack of an explicatory discourse about a schemata, something which we now know are not normally verbalised.

But another aspect of the ethnographic enigma remains to be explained. Why does the kinship vocabulary not reflect, and why does it even, apparently, contradict a form of matrimonial alliance which has long existed among the Zafimaniry? According to kinship textbooks, kinship terminology and the alliance system should represent two sides of the same coin. But since nothing like this occurs among the Zafimaniry, we must ask how such a state of affairs could possibly be? However, this question originates in precisely one of those commonsense psychological theories which cognitive psychology warns us against. The notion that a given kinship terminology and an alliance system are closely related is based on a strong, but unproved, hypothesis that terms express clear and categorical concepts which, because of their classificatory nature, are logically interrelated and hence organise practices; in this case marriages. But cognitive psychologists' recent work on conceptualisation reaches two conclusions which cast fundamental doubt on such presuppositions.

First, anthropologists need to remember that concepts and words are not the same thing (Smith 1988).[6] This difference can be shown in different ways; the easiest is to note that some concepts are not verbalised. The Zafimaniry can therefore easily possess and use the concept of "group of affines amongst whom we normally seek our spouses" without having a word to designate such a group; indeed, their very behaviour testifies to the existence of such an unnamed concept. For the same reason, the fact that they do not distinguish terminologically in address between father and father-in-law in no way excludes the possibility that the same word designates two very distinct concepts.

Furthermore, contrary to the assumptions implicit in structuralism and traditional ethnoscience, concepts are not defined by a list of abstract distinctive features (Smith 1988) which are locked into a closed system of contrasts and oppositions. Rather, we should understand concepts in terms of the analogy with a dazzling light, with an uncertain centre, which diffuses a multitude of aureoles and beams. Concepts are merely loosely bound mental associations and bits of knowledge according to which we can recognise certain phenomena as similar to each other and others as different. Above all, concepts allow us to organise actions which are well-adapted and foreseeable; they are not definitional tools. If concepts were organised according to the structuralist model and corresponded to words, they would indeed establish significant contrasts and definitions. Thus kinship terminology would allow one to know categorically whether an individual was conceptually a cross-cousin or not; it would be impossible that the same person could be considered both a cross-cousin and a parallel-cousin if the two terms existed. But if, as suggested above and as many psychologists now believe, concepts are vague in spite of significant cores—that is to say if they are organised around prototypes, i.e., ideal-typical occurrences to which empirical phenomena more or less correspond—it becomes possible for one individual to be conceived more or less as a cross-cousin and for another to be regarded as both cross-cousin and parallel-cousin. This is the case with the Zafimaniry, who often treat the same individual differently—as kin or as a affine—depending on the context, and changing in this, from one moment to the next. The implacable and quasi-mathematical classificatory logic of classical kinship studies would make such examples highly problematic, but in fact there need be no problem if our understanding is informed by what cognitive psychology can tell us and not by the kind of folk psychology that is buried in anthropological theory.

Cognitive science can also, however, sometimes offer positive teachings to the ethnographer. For example, the above-mentioned theory of concepts and schemas also suggests a methodology for fieldwork that can make us observe and study with particular attention phenomena and practices of which we might otherwise not have taken much notice.

Let us return to the example of Zafimaniry marriage. Many cognitive psychologists believe that concepts and schemas are linked to prototype situations mostly defined in early childhood. If this is so, we need to study the process of socialisation to understand both the development of the conceptualisation of a kinship system and the formation and existence of nonverbal concepts. The study of the socialisation of children is not a new area of study in classical anthropology. But the latter, because of its lack of reference to cognitive psychology, has uncritically adopted the vague commonsense notions of behaviourism. In particular, the traditional ethnography of learning rarely addresses what is a central ethnographic problem; that is, the indirect relation between socialisation and the formation of concepts, verbal and nonverbal.[7]

Here I briefly indicate the type of phenomenon that would need to be studied in order to deal with such a question. Everything happens as if just after birth, a Zafimaniry child begins to acquire a notion of the kinship system in the form of prototypical concepts well before learning to speak. Such knowledge obviously owes nothing to language but seems to be derived from certain practices which create those concepts, which then implicitly come to organise the world of alliances.

Thus, young babies are often encouraged to breastfeed not only from their mother's breast but also from other women who nearly always belong to the same moiety as the mother. Similarly, small children are often systematically placed on the backs of older children, who also belong to their own side of the village. Such practices seem to contribute to the formation of a nonverbal conceptualisation of the contrast between the two moieties. One can even sometimes notice external manifestations of this psychological process. For example, even though most of the time, babies are passed from breast to breast, from hand to hand, and from back to back, within their own moiety, as soon as they are handed over to a person who is not familiar, which usually means from the other moiety, they immediately begin to struggle and cry and as this is expected of them this is encouraged through teasing. It is thus probable that babies begin to form one or more concepts or schema of their moiety, which incorporates a series of typical and expected behaviour from people belonging to their own moiety, and which they accept only from them.

Similarly, one can see in the child's behaviour the progressive development of a concept of the "other" moiety. This process is without doubt linked to the fact that adults from the child's moiety treat adults and children from the other moiety very differently. Indeed, many of them are treated as brothers-in-law and sisters-in-law, with whom a joking relationship, marked by a lack of respect, is maintained. Thus at a very young age, children adapt themselves to these differences in behaviour. These differences are often extremely subtle, but by providing an ever-present backdrop to village life, they are all the more powerful.

Children's progressive familiarisation with these subtle differences rapidly leads to behaviour which is more directly alliance-related. From early childhood, children play at "being married" and simulate sexual relations. These games are always organised in relation to the division of the village, and soon they become increasingly serious. In other words, childhood "alliances" are already governed by exchange between moieties. I have seen, for example, boys of about thirteen tease their playmate whose lover was a girl from his own moiety.

I do not mean to say by all this that Zafimaniry children reinvent the kinship system of their society by deducing the logic of practices they observe. Rather, they unconsciously take part in these practices, and in this way the system is incorporated and transmitted. The Kantian principle which postulates that categories are always prior to practices is thus reversed. In this respect, a practice so common that it can pass unnoticed by the observer proves illuminating. Young Zafimaniry children, up to the age of two or three, are nearly always carried in a piece of cloth which makes their whole body adhere to the backs of boys and girls, and men and women, who in their manner of speaking and of moving their bodies, produce and reproduce the implicit classification of kinship according to the conceptualisation which was transmitted to them. When a child is stuck to a back, his or her body is an integral part of another body, "connected" to another brain. It is thus through the activity of their own bodies that Zafimaniry children discover and integrate conceptualisations transmitted through culture. A child does not first learn the concepts which govern kinship and then put them into practice. On the contrary, by being part of another body, the child practices kinship even before knowing its principles.

There is therefore nothing mysterious about a kinship structure which operates without people knowing its rules or possessing a vocabulary to describe it. Such a kinship structure is thus the product of concepts and schema which are nonverbal and about which it is not necessary to speak. Unspoken, these concepts are nonetheless integrated into daily practice and organise knowledge and behaviour.

Conclusion

Was it necessary to have recourse to the cognitive science in general, and cognitive psychology in particular, to reach such conclusions? At least it seems that without such recourse our reasoning would have been quite different. My argument rests on the claim that observation must be guided by the dialectic between empiricism and theoretical hypotheses, in this case borrowed from the cognitive sciences. These hypotheses here concern the nature of schema and of concepts, and the relationships that exist between concepts and language, between concepts and practices, and between the learning of specific social practices. Such toing and froing between scientific theories and ethnography is precisely what the dual nature of anthropology has involved, and losing it would make us lose the possibility of this type of reasoning.

My fieldwork amongst the Zafimaniry followed the traditional approach inspired by Malinowski. This type of research is what cognitivists criticize because of its lack of precision and because of its anecdotal character. I did not ask myself a priori questions at the outset, which being defined outside of any ethnographic context could have provided a basis for cross-cultural comparison. On the contrary, like most other anthropologists, I let myself be guided to a great extent by the Zafimaniry themselves, towards what interested and mattered to them most. Thus, as my research and understanding progressed, I constantly redefined my questions. In this way, my informants participated in the definition of my objects of enquiry.

For me, as for Geertz, a hermeneutic process was essential and integral to ethnographic practice. But interpretation also has to be informed by a scientific tradition: It cannot simply be guided, as Geertz and even Weber have it, by vague intuitions of uncertain origin; hence the importance of cognitive sciences in the enterprise I seek to contribute to. By using cognitive science, we can analyse from an explicit and considered standpoint hypotheses we implicitly make about knowledge, about motivations for action, and about the actions of the people we study, to pay attention to phenomena in the field which we might otherwise have neglected or at least interpreted differently.

Notes

1. This text was given as the Robert Hertz Memorial lecture in Paris in 1993.

2. Marvin Harris's cultural ecology and sociobiology provide good examples of such theories.

3. I think we have to accept that we have failed to develop fieldwork research methods which succeed in combining the rigour of a psychology laboratory with the anthropological tradition of participant observation. Even if this were desirable, it would be too much to expect from just one person. A field worker studying the people with whom he lives cannot create events; he waits for them to happen. He holds conversations with others, but only when the right moment occurs. Should several ethnographers work together in the field? It would not be possible to divide the workload so that one person learns to know intimately the language and culture under study, while the other carries out psychological experiments. Both these researchers would need to undertake both aspects of the work, but then the advan-

tage of being two disappears. This idea is therefore not practicable. Only one researcher, Toren (1990), seems to have succeeded in undertaking, in an exotic setting, research informed by cognitive psychology but relevant to the central concerns in classical anthropology.

4. I am taking up a theme which I have argued elsewhere (Bloch 1991 and 1992).
5. Schank and Abelson (1977) use the term *script* for what I call here *schemata* (sing.) and *schema* (plur.).
6. Even if language necessarily uses words to express concepts.
7. For a counterexample, see Jean Lave 1988, *Cognition in Practice*.

References

Berlin, B., Breedlove, D. and P. Raven. "General principles of classification and nomenclature in folk biology". *American Anthropologist*. vol. 74. 1973: 214–242.

Bloch, M. "Language. Anthropology and Cognitive Science". *Man*. vol. 26. n 2. 1991: 183–198.

______. "What goes without saying: the conceptualisation of Zafimaniry society", in A. Kuper (ed.). *Conceptualising Society*, London, Routledge. 1992.

Clifford, J. and G. Marcus (eds.). *Writing Culture*. Berkeley, University of California Press, 1986.

D'Andrade, R. "A Folk model of mind" in Holland, D. and Quinn, N., *Cultural Models in Language and Thought*, Cambridge, Cambridge University Press, 1987.

Dwyer, K. *Maroccan Dialogues: Anthropology in Question*. Baltimore, The John Hopkins University Press. 1982.

Friedman, A. "Framing Pictures: The role of knowledge in automatised encoding and memory for gist". *Journal of Experimental Psychology*, General 108, 1979: 316–355.

Hirschfeld, L. "Is the acquisition of social categories based on domain specificic competence or on knowledge transfer?", *in* L. Hirschfeld and S. Gelman (eds.), *Mapping the Mind*, Cambridge, Cambridge University Press, 1994.

Lave, J. *Cognition in Practice*, Cambridge, Cambridge University Press, 1988.

Schank, R. C. and R. P. Abelson. *Scripts. plans, goals and understanding*. Hillsdale, N. J., Lawrence Erlbaum Associates Inc., 1977.

Smith, E. "Concepts and thought", *in* R. J. Sternberg and E. E. Smith (eds.). *The Psychology of Human thought*, Cambridge, Cambridge University Press, 1988.

Sperber, D., *Le Savoir des anthropologues*, Paris, Hermann, 1982.

Taussig, M. *Shamanism, Colonialism, and the Wild Man*, Chicago, Chicago University Press, 1987.

Toren, C. *Making Sense of Hierarchy*, London, The Athlone Press, 1990.

Tyler, S. "Post-modern ethnography: From document of the occult to occult document", *in* J. Clifford and G. Marcus (eds.), *Writing Culture*, Berkeley, University of California Press, 1986.

Williams. P. *Nous, on n'en parle pas*. Paris, Éditions de la Maison des sciences de l'homme, 1993.

Wilson, P. "The problem with primitive folk", *Natural History* 81 (10). 1977: 26.

Winch, P. *The Idea of a Social Science*, London, Routledge and Kegan Paul, 1958.