How We Think
They Think

Anthropological Approaches
to Cognition, Memory,
and Literacy

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Chapter One

Language, Anthropology and Cognitive Science

The text of this chapter was originally given as a lecture in memory of the British anthropologist Sir James Frazer. The arguments it advances are the most general in the book and form the basis for the subsequent chapters. The chapter makes a number of fundamental points. First, it argues for the importance of cognitive science for anthropologists. Second, it argues that what people say is a poor guide to what they know and think. Third, it argues that the knowledge used in practice must be organised in such a way that it can be accessed at amazing speed and cannot therefore be linear in nature. Fourth, it looks at the significance for anthropology of the process of becoming an expert in certain practices. Fifth, it argues theoretically for the importance of participant observation. The chapter turns to connectionist theory to make several of its points, but this does not indicate so much a commitment to any particular form of this theory as an emphasis on the need for this kind of theory.

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I

Cognitive science is usually described as the attempt to bring cognitive psychology, philosophy, neurophysiology, artificial intelligence, linguistics and anthropology together in order to understand cognition. In this alliance anthropology is, in fact, a rather shadowy partner. Only cognitive anthropology is usually taken into account by cognitive scientists and, even then, only the further reaches of cognitive anthropology which many anthropologists, especially European anthropologists, would fail to recognise as their business at all. This state of affairs is unfortunate, because some of the theories emerging in cognitive science are central to the concerns of anthropology, whether social or cultural, and should lead anthropologists to re-examine many of the premises of their work. Other cognitive scientists,
however, have much to learn from the central concerns of anthropology and particularly, if rather unexpectedly, from the traditional method of social anthropology: participant observation.

Cultural anthropologists study culture. This can be defined as that which needs to be known in order to operate reasonably effectively in a specific human environment. Social anthropologists traditionally study social organisation and the behaviour by means of which people relate to each other. Both cultural and social anthropologists, however, are well aware that the distinction between the two branches of the discipline is not absolute. Cultural anthropologists know that they cannot get at culture directly, but only through the observation of communicative activity, verbal or otherwise, natural or artificially simulated. Social anthropologists are aware that they cannot understand action, verbal or otherwise, if they do not construct, probably in imagination, a representation of the culture of the people they study, since this is the only way to make sense of their activities (Winch 1958).

Some concept of culture is therefore essential to all social and cultural anthropologists. However, a further assumption of anthropology, sometimes stated and sometimes unstated, is that this culture is inseparably linked to language, on the grounds either that culture is thought and transmitted as a text through language, or that culture is ultimately 'language like', consisting of linked linear propositions. It is these two assumptions about culture that I want to challenge here.

If culture is the whole or a part of what people must know in a particular social environment in order to operate efficiently, it follows first, that people must have acquired this knowledge, either through the development of innate potentials, or from external sources, or from a combination of both, and secondly that this acquired knowledge is being continually stored in a manner that makes it relatively easily accessible when necessary. These obvious inferences have in turn a further implication which is that anthropologists' concerns place them right in the middle of the cognitive sciences, whether they like it or not, since it is cognitive scientists who have something to say about learning, memory and retrieval. Anthropologists cannot, therefore, avoid the attempt to make their theories about social life compatible with what other cognitive scientists have to say about the processes of learning and storage.

Perhaps all this may seem commonplace but it is striking how often anthropologists' theories of learning, memory and retrieval have not been compatible with those of other cognitive scientists. The exception is the small group of cognitive anthropologists, largely confined to the United States, who have paid serious attention to recent developments in cognitive science. Things are nevertheless beginning to change, and I hope that in this article I am swimming with the tide, but a tide which has only just
started to flow and which has not reached, especially in Europe, the heartland of anthropological theory where it is perhaps most needed.4

Of course I do not claim that other cognitive scientists have figured out how the mind works, and that anthropologists have only to slot culture into this well-advanced model. Cognitive scientists' understanding of the mind-brain is dramatically incomplete and tentative. Nonetheless, some findings are fairly clear and we should take these into account. Moreover, the hypotheses of cognitive scientists, however speculative, fundamentally challenge many unexamined anthropological assumptions in a way that should not be ignored.

II

As noted above, my main concern is with only one aspect of the broad topic of the relation between cognitive science and anthropology, namely the importance or otherwise of language, or language-like phenomena, for cultural knowledge. A good place to start is with a consideration of concept formation, particularly classificatory concept formation, a topic about which much has been written, and which is often recognised as of relevance to anthropology.

In discussing this topic I am not concerned with the traditional anthropological issue, recently reviewed by Atran, of the extent to which classificatory concepts have an innate basis (Atran 1990), though it is quite clear that in the past, anthropologists have grossly exaggerated cultural variation, and that the traditional questions of cultural anthropologists concerning very broad areas of knowledge should be rephrased from 'How are these things learned?' to 'How is culturally specific knowledge produced out of universal predispositions?'. Such rephrasing is too easily obscured in anthropological references to 'cultural models' (Holland & Quinn 1987).

Leaving this fundamental question aside, it is nonetheless clear that all classificatory concepts are at least partially learned, and recent work in this area has brought about certain fundamental changes in the way we envisage the process. The old idea that the child learns classificatory concepts as minimal and necessary definitions, an idea taken for granted in most of anthropology and which was more particularly implied in structuralism and ethnoscience, was shown to be untenable some time ago (Fillmore 1975; Rosch 1977; 1978; Smith 1988). The more generally accepted position now is that such concepts are formed through reference back to rather vague and provisional 'prototypes' which anchor loosely-formed 'families' of specific instances. For example, the concept of a house is not a list of essential features (roof, door, walls, and so on) which have to be checked off before deciding whether or not the thing is a house. If that were so we would have no idea that a house which has lost its roof is still a house. It is rather that
we consider something as 'a house' by comparing it to a loosely associated group of 'houselike' features, no one of which is essential, but which are linked by a general idea of what a typical house is.

Thus, as suggested by Markham and Seibert (1976), classificatory concepts are in fact based on an appraisal of their referents in the world, on how we think of the construction and make-up of these referents, or on our understanding of the way they are constituted. It therefore seems that the mental form of classificatory concepts, essential building blocks of culture, involves loose and implicit practical-cum-theoretical pattern networks of knowledge, based on the experience of physical instances sometimes called 'best exemplars' (Smith et al. 1988: 372). A significant aspect of looking at classificatory concepts in this way is that it makes them isomorphic with what are known as 'scripts' and 'schemata', although these latter may be on a much larger and more elaborate scale. These 'scripts' and 'schemata' are, in effect, chunked networks of loose procedures and understandings which enable us to deal with standard and recurring situations, for example 'getting the breakfast ready', that are clearly culturally created (Abelson 1981; Holland & Quinn 1987; D'Andrade 1990).

If classificatory concepts such as 'scripts' and 'schemata' are not like dictionary entries, but are instead small networks of typical understandings and practices concerning the world, then the question of their relation to words becomes more problematic than it was with the old 'checklist of necessary and sufficient conditions' view. That there is no inevitable connexion between concepts and words is shown by the now well-established fact that concepts can and do exist independently of language. This is made clear in the many examples of conceptual thinking in pre-linguistic children, first presented by Brown (1973). Children have the concept 'house' before they can say the word. We also have studies which show that the acquisition of lexical semantics by children is very largely a matter of trying to match words to already formed concepts. This is the so called 'concept first' theory.

Contrary to earlier views in cognitive anthropology (Tyler 1969), therefore, language is not essential for conceptual thought. It is possible to go beyond this initial distancing between the lexicon and mental concepts, however, thanks to work on semantic acquisition by Bowerman (1977). This demonstrates a continual back and forth movement between aspects of classification which are introduced through language and mental concepts, as the child learns to express these concepts through words. This dialectical movement is not only interesting in itself but also suggests a much more general process, to which I shall return, by which originally non-linguistic knowledge is partly transformed as it becomes linguistic, thereby taking on a form which much more closely resembles what structuralists, among other anthropologists, had assumed to characterise the organisation of all human knowledge (Keil & Batterman 1984).
This brief review of concept formation enables us to reach the following provisional conclusions: (1) that much of knowledge is fundamentally non-linguistic; (2) that concepts involve implicit networks of meanings which are formed through the experience of, and practice in, the external world; and (3) that, under certain circumstances, this non-linguistic knowledge can be rendered into language and thus take the form of explicit discourse, but changing its character in the process.

III

Another area of joint concern to anthropology and cognitive psychology also reveals the importance of non-linguistic knowledge. This is the study of the way we learn practical, everyday tasks. It is clear that we do not usually go through a point-by-point explanation of the process when we teach our children how to negotiate their way around the house or to close doors. Much culturally transmitted knowledge seems to be passed on in ways unknown to us. Perhaps in highly schooled societies this fact is misleadingly obscured by the prominence of explicit instruction, but in non-industrialised societies most of what takes people’s time and energy—including such practices as how to wash both the body and clothes, how to cook, how to cultivate, etc.—are learned very gradually through imitation and tentative participation.

The cultural specificity, complexity and embeddedness of such tasks, and their character as not linguistically explicit, have often been commented upon by anthropologists, for example by Mauss (1936), Leroi-Gourhan (1943) and Haudricourt (1968), but have rarely been studied satisfactorily. The few studies we do have tend to deal with unusual tasks necessary for specialised crafts which require formal apprenticeships. In these cases, also, anthropologists have noted that language seems to play a surprisingly small role in the transmission of knowledge. For example, in her study of weavers in Ghana, Goody (1978) was amazed at the small part played by questioning and speaking in teaching apprentices. Similarly, Lave, in her study of Liberian tailors, notes that what she calls ‘apprenticeship learning’, which relies on the ‘assumptions that knowing, thinking, and understanding are generated in practice’ (Lave 1988; 1990:310), is more effective than formal teaching based on linguistic, socratic forms. If this is so for these relatively specialised tasks there is no doubt that the same conclusions would be reached even more emphatically in studies of learning more common, though not necessarily less skilled, everyday tasks.

The significance of such findings is much more important than we might at first suppose. This is because the fact that the transmission of knowledge in West African weaving or tailoring is largely non-linguistic may have less to do with the culture of education in these places than with a general fea-
ture of the kind of knowledge that underlies the performance of complex practical tasks, which requires that it be non-linguistic.

That this is so is suggested by various studies of learning in which, by contrast to the examples just mentioned, the original teaching is received through language, or at least in propositional form, but in which the process of becoming an expert seems to involve the transformation of the propositions of the teacher into fundamentally non-linguistic knowledge (Dreyfus & Dreyfus 1986: ch. 1). Thus Anderson (1983) points out how people who are taught driving through a series of propositions have to transform this knowledge into non-linguistic, integrated procedures before the task can be effected rapidly, efficiently and automatically—one might say properly. Only when they do not think about what they are doing in words are drivers truly experts. Probably some teaching needs to be done verbally, but there are also advantages in the non-linguistic transmission of practical skills typical of non-industrial societies since such transmission by-passes the double transformation from implicit to linguistically explicit knowledge made by the teacher and from linguistically explicit to implicit knowledge made by the learner.

It could be objected that my stress on the non-linguistic side of practical activities is somewhat exaggerated. After all, language also plays a role in the performance of many familiar practical actions, though not necessarily driving. Even this fact, however, may bear less on the extent to which knowledge is linguistic than might appear at first sight.

Let me take an example which is in part derived from the semantic work of Johnson-Laird (1988: ch. 18; see also Johnson-Laird 1983: 396–447), modified by Sperber and Wilson’s (1986) theory of relevance and which is also indirectly inspired by Malinowski’s (1935) study of the role of language in Trobriand agriculture. Imagine a Malagasy shifting cultivator with a fairly clear, yet supple mental model, perhaps we could say a script, stored in long-term memory, of what a ‘good swidden’ is like; and that this model, like that of a roundabout in the mind of the expert driver, is partly visual, partly analytical (though not necessarily in a sentential logical way), partly welded to a series of procedures about what you should do to make and maintain a swidden. This Malagasy is going through the forest with a friend who says to him ‘Look over there at that bit of forest, that would make a good swidden’. What happens then is that, after a rapid conceptualisation of the bit of forest, the model of ‘the good swidden’ is mentally matched with the conceptualised area of forest, and then a new but related model, ‘this particular place as a potential swidden’ is established and stored in long-term memory. This stored mental model, however, although partly created by the linguistic event and understood in terms of the relevance of the statement to both the mental model of the ‘good swidden’ and to that of the area of forest, is not likely to include the linguistic statement
tagged onto it. The intrusion of language has therefore not made the mental model any more linguistic.

To return to the example of car driving: we have seen that driving expertly seems to require that the information be stored non-linguistically if it is to be accessible in an efficient way. Why should this be so?

In order to begin to answer this question we need to turn again to the process involved in becoming an expert. It is not surprising that practice in performing a complex task makes the practitioner more efficient, but studies of expertise show that the increase in efficiency is more puzzling than might at first appear. For example, when people are repeatedly asked to read a page of text upside down they gradually do this faster and faster, but the increase in speed is not continuous, nor does it go on for ever. At first there is a rapid increase in efficiency which continues for a while, then it begins to slow down, until eventually there is no further increase. The shape of the curve of increased efficiency suggests (Johnson-Laird 1988: 170, citing Newell & Rosenbloom 1981) that the process of learning involves the construction of a cognitive apparatus dedicated to cope with this sort of task. The establishment of that apparatus is slow, and while it is in construction there is significant improvement; however once it has been set up no further improvement becomes possible. A chunk or apparatus concerned with a familiar activity has thus come into existence in the brain as a result of repeated practice (Simon 1979: 386 sqq.).

A more complex and much discussed example of what happens when someone becomes an expert comes from studies of master chess players. It has been convincingly argued that expert chess players do not differ from novices (who are not complete beginners) in knowing the rules of chess or in performing such motor tasks as moving one piece without knocking the others down. What seems to distinguish the expert from the novice is not so much an ability to handle complex strategic logico-mathematical rules, but rather the possession, in memory, of an amazingly comprehensive and organised store of total or partial chessboard configurations, which allows the expert to recognise the situation in an instant so as to know what should be done next (Dreyfus & Dreyfus 1986: 32–3). However, bearing in mind the example of driving, what is surely happening is that the expert is not just remembering many games but that she has developed through long practice a specific apparatus which enables her to remember many games and configurations much more easily and quickly than the non-expert. She has learned how to learn this kind of information. This would explain how the expert can cope, not only with situations which she recognises, but also with situations which are new, so long as they fall within the domain which she has learned to cope with efficiently.

Learning to become an expert would therefore be a matter not simply of remembering many instances, but of constructing a dedicated cognitive
mechanism for dealing with instances of a particular kind. Such a mechanism, because it is concerned only with a specific domain of activity, can cope with information relating to that domain of activity remarkably quickly and efficiently, whether this be information about chess-piece configurations, motorway scenarios, or potentially useful areas of forest, and even though the specific cases of chess, motorway or forest have not been previously encountered in exactly that way (Dreyfus & Dreyfus 1986: ch. 1).

If becoming an expert does involve the creation of apparatuses dedicated to handle families of related tasks, then this is surely something which an anthropologist must bear in mind. For what she studies is precisely people coping with familiar yet ever novel situations (Hutchins 1980). It seems reasonable to assume that the construction of dedicated apparatuses for dealing expertly with certain areas of activity is going on in the process of cultural learning of all common practical tasks. Indeed some recent work suggests that learning to become an expert in familiar areas is a necessary preliminary to other types of learning and to being able to cope with the less familiar and less predictable. The reason seems to be partly neurological.

In the case of car driving, it seems that as a person becomes an expert, not only does she drive better, not only does she transform what was once linguistic propositional information into something else, she also seems to employ much less neurological potential in doing the necessary tasks (Schneider & Shiffrin 1977), thereby freeing her for other mental tasks, such as talking on a car phone. Similarly, the extraordinary feats of memory of the chess master seem to be made possible by the efficient packing of information through the use of the expert apparatus for coping with novel situations of play.

Such observations suggest the general conclusion that the ability to learn more is largely a matter of organising what one has already learned in packed chunks so that one has room for the new. Some cognitive scientists have therefore argued that the problems young children have in doing the tasks which, as Piaget showed, only adults can do, stem not from the immaturity of the children’s brains, but from the fact that this ‘packing’ has not yet taken place. Once the essential preliminary procedures have been sufficiently organised, their implementation will only require limited neurological capacity, leaving enough ‘room’ for the child to perform further supplementary tasks. These are the tasks which the child had earlier seemed unable to perform; but really the problem was that they could not normally be performed simultaneously with their necessary preliminaries (Smith et al. 1988).

There is therefore considerable evidence that learning is not just a matter of storing received knowledge, as most anthropologists implicitly assume when they equate cultural and individual representations, but that it is a matter of constructing apparatuses for the efficient handling and packing of
specific domains of knowledge and practice. Furthermore, as suggested by the case of learning to drive, evidence shows that once these apparatuses are constructed, the operations connected with these specific domains not only are non-linguistic but also must be non-linguistic if they are to be efficient. It follows that much of the knowledge which anthropologists study necessarily exists in people's heads in a non-linguistic form.

Before proceeding further, an ambiguity in what has been argued so far must be removed. To say that knowledge concerned with the familiar must be non-linguistic could mean one of two things. It could mean simply that this knowledge is not formulated in natural language. On the other hand it could mean something much stronger. It could mean that this knowledge is in no way 'language like', that it is not governed by the characteristic sentential logic of natural and computer languages. Here I adopt the stronger of the two alternatives because I believe that the studies on expertise discussed above suggest that the knowledge organised for efficiency in day-to-day practice is not only non-linguistic, but also not language-like in that it does not take a sentential logical form. To argue this and to make my argument less negative I now turn to the admittedly controversial assumptions of what has been called 'connectionism'.

IV

What is particularly interesting for anthropology in connectionism is not so much connectionism itself, but the reasons why a theory like it is necessary. Simply put, a theory such as connectionism is necessary because a sentential linear model of the mind-brain, sometimes called the sentence-logic model (Churchland & Sejnowski 1989), which is broadly similar in form to the semantics of natural language, cannot account for the speed and efficiency with which we perform daily tasks and cope with familiar situations.

The sentence-logic or sentential linear model is intuitively attractive for a number of reasons, and these explain why it is implicit in anthropological theory and was accepted unchallenged for a long time in cognitive psychology. First of all, and probably most significantly, it is the model of folk psychology, as anthropologists have nicely demonstrated (D'Andrade 1987). Sentential logical forms are how we think we think. Secondly, sentential logical models work well for the semantics of natural language; and thirdly, they worked well for those metaphors for the mind—the von Neumann or digital computers.

However, these three arguments in favour of the sentence-logic model are very weak. First, folk psychology, whatever the majority of anthropologists may say, need not, as Churchland and Churchland (1983) point out, be any more valid than folk physics as a basis for scientific accounts. Secondly, what applies to language need not apply to other forms of mental activity.
Thirdly, digital computers come nowhere near to doing the jobs we humans can do, and so they must be, in some way, inappropriate as metaphors for the mind-brain.

The case for connectionism is best made by considering an example, and by using this type of example we can see the relevance of the theory for anthropology. Remember the Malagasy peasant. When the man said to his friend, 'look over there... that piece of forest would make a good swidden', an unbelievable mental feat seems to have then been achieved by the man addressed. He recalled from long-term memory the complex yet highly flexible mental model or schema 'the good swidden', then he conceptualised the piece of forest indicated, taking in information about the vegetation, the slope, the surrounding countryside, the hydrology, the soil, etc, then he matched the two intricate conceptualisations in what could not be just a simple comparison but a highly complex set of transformations. When put in this way, the total task seems Herculean, but in reality even the moderately talented Malagasy farmer would come to some assessment in just a few seconds. Furthermore, this computational feat is no more difficult than many other similar tasks which human beings perform all the time. Why then does such a task appear so impossibly complicated when we think about what it entails? The reason is that we are explaining the behaviour of the Malagasy farmer in terms of our own folk psychology, including a model of language-like semantics. This makes an easy task, which we know the farmer can perform in an instant or two, seem absolutely awesome. There must, indeed, be something wrong about how we think we think.

Connectionism is an alternative theory of thought which makes such commonplace feats as that of our Malagasy farmer possible to envisage. It suggests that we go about the whole process of thought in a quite different way from what we had previously and loosely assumed. The problem with the folk way of describing thought procedures, for example of how a decision is reached, is that we tend to see the activity as a serial process of analysis carried on along a single line by a single processor. For complex yet familiar tasks such processing would be impossibly clumsy and lengthy. Instead, connectionism suggests that we access knowledge, either from memory or as it is conceptualised from perception of the external world, through a number of processing units which work in parallel and feed in information simultaneously. It suggests, too, that the information received from these multiple parallel processors is analysed simultaneously through already existing networks connecting the processors. Only with this multiple parallel processing could complex understandings and operations, like those about the swidden, be achieved as fast as they are. Otherwise, given the conduction velocities and synaptic delays in neurons, it is a physiological impossibility for the number of steps required by a logical-sentential model of the mind-brain to be carried out in the time within which even the simplest mental tasks are ordinarily performed. A connec-
tionist brain, on the other hand, could (at least hypothetically) work sufficiently fast (Feldman & Ballard 1982).

It is much too early to say whether connectionism will prove to be an accurate analysis of the working of the brain, and, in any case, I am not in a position to be able to evaluate its neurological validity. What matters here, above all, is that the theory offers the kind of challenge to sentential logical models which anthropology requires, and it offers the kind of answers which would cope with the situations we seek to understand.

Support for connectionism does not, however, all come from first principles. Some psychological experimental work seems to confirm the theory (Rumelhart et al. 1986). The fact that computer programs which enable digital computers to work on something like parallel processing seem to be able to get them to do tasks which ordinary digital computers working with classical programs are unable to do, such as reading and reproducing in three dimensions grey shaded images, is also encouraging. There are, however, two other aspects of the theory which make it particularly attractive to the anthropologist.

The first is that connectionism can cope well with what Johnson-Laird (1983: 438–47) called the ‘provisional’ character of mental models, while sentential logical models imply a much greater rigidity which is quite unlike what we find in natural situations. The mental model of ‘the good swidden’ cannot be a checklist of characteristics to be found in a particular configuration or even an example of the kind of ‘fuzzy’ digital models recently proposed. With such fundamentally fixed models the Malagasy farmer would never find the right plot. The model cannot require absolutely any particular characteristic or configuration; just a general flexible theoretical-practical hypothesis. Connectionism can handle this type of ‘fairly loose’ practical-theoretical thinking, which as we saw is also implied in prototype theory, while other theories cannot.

Secondly, a connectionist model can account for the length of the process of becoming an expert at a particular task, a fact which itself is quite a puzzle. With such a model we can understand what would be happening when a person is learning to handle a family of related tasks, such as learning how to learn chess configurations. The person would be creating connected networks dedicated to specific domains of cognition, and procedures which, once set up, could be accessed quickly and efficiently by multiple parallel processing. Such a process for complex tasks, such as becoming a car driver or a chess expert, would require a good deal of packing and quite a bit of connecting, but once the job was done it would be highly efficient.

Since much of culture consists of the performance of these familiar procedures and understandings, connectionism may explain what a great deal of culture in the mind-brain is like. It also explains why this type of culture cannot be either linguistic or ‘language like’. Making the culture efficient requires the construction of connected domain-relevant networks, which by
their very nature cannot be stored or accessed through sentential logical forms such as govern natural language. Furthermore, as the discussion of apprenticeship learning shows, it is not even necessary for this type of knowledge ever to be put into words for it to be transmitted from one member of the community to another. The highly efficient non-linear packing in purpose-dedicated domains, formed through the practice of closely related activities, also explains why the transfer of one type of knowledge from one specific domain to another is so difficult. Lave's (1988) observation that there is no carry-over between school maths and coping with mathematical problems in a supermarket may well be due to the fact that the latter are dealt with in such a well chunked and connected domain that it cannot easily admit knowledge of such a different kind as sentential-logically organised school maths.\(^{11}\)

V

To claim that much of culture is neither linguistic nor 'language like' does not imply that language is unimportant. Nevertheless, contrary to what anthropologists tend to assume, we should see linguistic phenomena as a part of culture, most of which is non-linguistic. Instead of taking language for granted, we should see its presence as requiring explanation. I cannot, in the space of this article, review all the circumstances and reasons for the occurrence of language in cultural life. Nonetheless, even here, cognitive science can offer provocative suggestions.

As we have seen, everyday practical actions and knowledge are probably packaged fairly hermetically into networks that take the form presumed by connectionist models. This packaging works very well for quick and efficient operations in familiar domains, but occasionally these networks can also be unpacked into linear sentential sequences which can then be put into words. If such transformations are commonplace, as I believe they are, we should then see culture as partly organised by connectionist networks and partly consisting of information organised by sentential logic, with a fluid transformative boundary between the two.\(^{12}\)

The process of putting knowledge into words must require such a transformation in the nature of knowledge that the words will then have only a distant relationship to the knowledge referred to. But the process may also involve gains in different areas, as suggested by work, cited earlier in this article, on the transformation of prototype concepts into classical concepts (concepts which can be defined by a checklist of necessary and sufficient features).

For example, the extension of aspects of knowledge, normally chunked in a particular domain, into another domain may be one of the processes that require verbalisation, and such extension may well be linked with the
process of innovation, as some of the work on the significance of analogical thinking for creativity suggests (Sternberg 1983). Indeed, we should perhaps see culture as always balanced between the need for chunking for efficiency, on the one hand, and, on the other, linguistic explicitness which allows, inter alia, for innovation and for ideology. With such a perspective we might be able to envisage a kind of general 'economy' of knowledge, which social anthropologists could then place in specific social and historical situations.

VI

In mentioning one of the possible reasons for explicitness I am suggesting a direction which would take me far from the limited concerns of this article. But even without going into these issues our discussion of the ways in which knowledge is organized has fundamental implications for anthropology. The first point is that culture is probably a different kind of phenomenon from what it has previously been thought to be, with the result that our understanding of culture has remained partial and superficial. Up to now, anthropology has tried to analyse culture through folk models of thought applicable only to sentential logical knowledge which, as noted, is but a small part of all knowledge. Dreyfus and Dreyfus (1986: 28) point out how social scientists use a 'Hamlet model' of decision making where the actor is assumed to weigh up and analyse alternatives in a self-conscious, logical fashion. In this respect it is interesting to note that unlike most of our informants doing familiar tasks, when Hamlet was trying to decide what to do next he was putting his thoughts into words. It is striking how many of the theories which have been popular in anthropology, such as transactionalist theories and other forms of methodological individualism, fall foul of Dreyfus and Dreyfus's strictures.

Then there are methodological implications. If the anthropologist is often attempting to give an account of chunked and non-sentential knowledge in a linguistic medium (writing), and she has no alternative, she must be aware that in so doing she is not reproducing the organisation of the knowledge of the people she studies but is transmuting it into an entirely different logical form. To effect such a transmutation is not impossible—after all we can describe things which are not linguistic. But in the attempt to evoke such knowledge we should avoid stylistic devices which turn attempts at description into quasi-theory, as was the case with structuralism and transactionalism. Perhaps we should make much more use of description of the way things look, sound, feel, smell, taste and so on—drawing on the realm of bodily experience—simply for heuristic purposes, to remind readers that most of our material is taken from the world of non-explicit expert practice and does not only come from linear, linguistic thought.
Above all we should beware the temptations of folk psychology. Folk psychology is indeed a form of competence employed in certain limited circumstances by the people we study, and it is therefore an object of research for anthropologists, but it is not something we want, or need, to carry over into anthropological explanation.

Thus, when our informants honestly say 'this is why we do such things', or 'this is what this means', or 'this is how we do such things', instead of being pleased we should be suspicious and ask what kind of peculiar knowledge is this which can take such an explicit, linguistic form? Indeed, we should treat all explicit knowledge as problematic, as a type of knowledge probably remote from that employed in practical activities under normal circumstances.

Of course such conclusions raise the question of how we are ever going to get at this connected, chunked knowledge. But here I believe anthropologists have an advantage over other cognitive scientists in that they already do have a technique advocated by Malinowski but perhaps never followed by him: participant observation.

Because of its long-term character, involving continuous and intimate contact with those whom we study, participant observation makes us learn the procedures which these people have themselves learned and enables us to check up on whether we are learning properly by observing our improving ability to cope in the field with daily tasks, including social tasks, as fast as our informants. For example, as a result of fieldwork I too can judge quickly whether a bit of forest in Madagascar would make a good swidden. Indeed, I find that as I walk through the forest I am continually and involuntarily carrying out this sort of evaluation. Once this level of participation has been reached we can attempt to understand chunked knowledge through introspection.

Introspection is, of course, a notoriously dangerous procedure, and fear of introspection is what led anthropologists, especially cognitive anthropologists, to adopt research procedures which imitate the laboratory studies of others in order to 'harden' their findings. To me this approach seems misguided for two reasons. First, even in psychology, now that it has emerged from its behaviourist phase, the value of 'ecologically' based studies (Neisser 1975) and the merit of introspection as a theoretical starting point are once again becoming recognised. Secondly, much 'hard' evidence has proved on critical examination to be much 'softer' than the information obtained through participant observation. For example, one lesson which seems to follow from my argument is that ethnography backed up by our informants' 'actual words' may often be quite misleading.

In any case, I believe that anthropologists who have done prolonged fieldwork have always obtained the basis of their knowledge about the people they study from informal and implicit co-operation with them, what-
ever they might have pretended. I am fairly sure that the way I proceed in giving an account of the Malagasy cultures I study is by looking for facts, and especially for statements, that confirm what I already know to be right because I know how to live efficiently with these people, or, if you will, because I have established in my brain non-linguistic chunked mental models which enable me to cope with most things in daily life at great speed. Like other anthropologists, I then pretend that the linguistic confirmations of these understandings, which I subsequently obtain from what my informants say, are the basis of what I understand, but this is not really so. My knowledge was established prior to these linguistic confirmations.¹⁴

To recognise this is not shameful and should help us to avoid what is our greatest betrayal as anthropologists. One fact we always and rightly stress when explaining how our way of going about things contrasts with that of other social or cognitive scientists is the importance we attach to the everyday and how we believe that the most important aspects of culture are embedded in the basic mental premisses of action. Anthropologists are particularly aware of this because many work in foreign cultures and so the exotic-in-the-everyday cannot but be prominent. We are also reminded of the importance of everyday practical culture because we do long-term fieldwork and participate for very long periods indeed. This learning about the practical is the best thing about anthropology, yet it often hardly features in our ethnographies; rather we rush to studies of rituals which could have been done in a week, or to analyses of economic organisation which could be done better by geographers and for which participant observation is, in any case, unsuitable.

Anthropologists worried about the difficulty of applying the kind of rigorous confirmation procedures of other disciplines should not worry too much. They certainly should not give up altogether and pretend, as has recently been fashionable, that they are merely involved in some literary exercise. They should learn to respect their own work rather more, and in cognitive matters they should remember that the basis of cognitive science is a combination of different disciplines, each with a contribution to make, but with a single aim.

The greatest contribution of anthropology to this totality can perhaps be to specify how cognition is employed in a natural environment, and in this way to help to decide what kinds of hypotheses and findings are required from those other cognitive sciences which work in more controlled conditions. It is because I am familiar with daily life in a Malagasy village, and also because I look for general explanations of what is going on, that I see the central problems to be solved as those of accounting for such intellectual feats as that of the peasant looking at the forest in search of a swidden site. Dreyfus and Dreyfus are quite right to point out that, by failing to pay attention to the way real human experts operate, cognitive scientists have
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attempted to create artificial intelligences along lines that ignore the very characteristics of human practice, but this is not a reason for arguing, as they do, for giving up the attempt altogether. It is a reason why, in the same way as anthropologists need other cognitive scientists, these other cognitive scientists would also benefit from co-operation with anthropologists having experience of participant observation.

Notes

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1. I do not want to imply that all members of a community need possess all cultural knowledge. Discussions of 'distributed cognition' by Cicourel and others suggest that this may not be so.

2. See Sperber (1985) to find this point fully argued.

3. Among American anthropologists who have discussed connectionism are Quinn and Strauss (1989), Hutchins (1988) and D'Andrade (1990).

4. It is true that structuralism, at least in its Lévi-Straussian form, paid attention to memory in that it assumed that the source of structuration was the need to encode information so that it could be kept and retrieved. In retrospect, however, as was shown by Sperber, the structuralist's view of memory was much too simple. In particular it assumed, with other types of cognitive anthropology, that what an individual received from others was stored in the same form as it had been communicated and that all information was equally memorable (Sperber 1985).

The theories of learning implicit in structuralism are even more problematic since they cannot account for the inevitably gradual construction of structured knowledge, a criticism which has been made in a variety of ways by Piaget (1968), Turner (1973), Sperber (1985) and myself (1985). Admittedly, such writers as Bourdieu (1972) have tried to remedy this situation, but in his case with a theory of learning habitus which is psychologically vague and which, because of its reintroduction of the notion of individual 'rational' choice, runs into some of the difficulties which I go on to discuss.

5. Holland and Quinn (1987: 19) draw attention to the significance of these for our understanding of culture.

6. Work on deaf and dumb children also seems to show that advanced conceptual thought does not require language (Petitto 1987, 1988). I am grateful to L. Hirschfeld for pointing out the significance of this work for my argument.

7. Although I am relying extensively on Dreyfus and Dreyfus's characterisation of expertise (1986), I do this to reach quite different conclusions.
8. I am told that in Norway, although the normal driving test involves both a practical section and a question and answer section, some people seem totally unable to do the language part of the test, and so a more difficult practical test has been devised for them which eliminates the need to do the language part. There is apparently no evidence that people who have obtained their test in this way are worse drivers than the others.

9. The point has already been made by Schank and Abelson (1977).

10. After all, it is possible to argue, as Fodor does, that although thought is not a matter of speaking to oneself silently, it still is ultimately 'language like' and involves series of 'grammatically' (though not the grammar of the surface structure of natural languages) linked representations and propositions. This suggestion enables Fodor to talk of a 'language of thought', though it might be better to say a 'quasi-language of thought' (Fodor 1987).

11. The fact that people who are unable to perform Wason's task (a logical test performed under laboratory conditions with two-sided cards) when faced by his cards can do it when it is reproduced in a context familiar to them points to similar conclusions (Johnson-Laird & Wason 1977). See the discussion of this point in Dreyfus and Dreyfus (1986: 19).

12. Such a reconciliation of rule and representation models with connectionist ones has been proposed by Bechtel (1990).

13. These explicitly linguistic forms may have different relations to knowledge (see Barth 1990).

14. An attempt to write an ethnography along these lines is to be found in Bloch (1992).

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Chapter Two

What Goes Without Saying: The Conceptualization of Zafimaniry Society

This chapter is an attempt to put into practice some of the ideas concerning ethnography and cognition outlined in Chapter 1 and to apply them to the ethnography of the Zafimaniry, a small group of forest dwellers whom I have been studying for more than 20 years. The basic concept used here is that of the mental model, which is close to the notion of schema discussed in Chapter 3.

* * *

A problem which lurks uneasily in the prefaces of most anthropological monographs and worries, or should worry, all fieldworking anthropologists is that the way anthropologists conceptualize the societies they have studied in their ethnographic accounts almost always seems alien, bizarre, or impossibly complicated to the people of those societies. Perhaps this would not matter if ethnographies claimed only to be description from the outside; however, most accounts attempt, at least in part, to represent a society and ways of thinking about it from the insiders' point of view. Perhaps, then, we could get rid of the difficulty by saying that this disturbing lack of recognition was just a problem of vocabulary; after all, most people in most parts of the world are unacquainted with the technical terms and literary conventions of academic anthropology. But one has to face the fact that, if this were all there was to it, anyone who was reasonably good at paraphrase would surely be able to cross the communication gap and produce a non-technically worded ethnography with which informants would largely agree. Clearly, this is not often the case. The problem of lack of validation by the people with whom anthropologists work begins, then, to look like a very serious one.
In fact, the basis of the problem lies in something much more damaging than is normally recognized. Anthropological accounts, I believe, work from a false theory of cognition. As a result, when they attempt to represent the way the people studied conceptualize their society, they do so in terms which do not match the way any human beings conceptualize anything fundamental and familiar in any society or culture. In imagining how the people they study conceptualize society anthropologists use the common folk view of thought current in both Western and many other societies. But there is considerable evidence that this folk theory is as wrong about psychological processes as the folk theory of physics is wrong about the nature of energy (see Bechtel 1990; Churchland and Sejnowski 1989).

The folk model, which is also widely assumed in Western philosophy, is that thought is logic-sentential and language-like. We tend to imagine thinking as a kind of silent soliloquizing wherein the building blocks are words with their definitions and the process itself involves linking propositions by logical inferences in a single lineal sequence. By contrast, much recent work in cognitive science strongly suggests that everyday thought is not ‘language-like’, that it does not involve linking propositions in a single sequence in the way language represents reasoning. Rather, it relies on clumped networks of signification which require that they be organized in ways which are not lineal but multi-stranded if they are to be used at the amazing speed necessary to draw on complex stored information in everyday activity. If that is so, anthropologists are presented with two problems.

The first is that the people we study are unlikely to be able easily to describe their thought processes for anthropologists through what they say, since language is an inappropriate medium for evoking the non-lineal organization of everyday cognition. (This, like several other points I make in this chapter, was pointed out in a different way by Bourdieu in 1972.) Furthermore, since having to explain to others what one thinks through an inappropriate medium, viz., language, is a familiar problem in all cultures, informants asked to give retrospective accounts of their thought processes—a common enough occurrence in normal life—are able to fall back on the conventions by which the problem is normally avoided. These conventions of everyday discourse usually involve reinventing a hypothetical quasi-linguistic lineal, rational thought process which appears to lead satisfactorily to the conclusions reached. But it does so deceptively in terms of the way we, and they, think we think because it follows the folk theory of thought shared by the anthropologist and the informant. In other words, the problem of explaining to others how we reach a decision is solved by what we would normally call post hoc rationalizations, and these are what anthropologists are given when they too ask for retrospective explanations of actions.

The second problem which arises from the impossibility of matching the organization of everyday thought to the semantics of natural language relates
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to the anthropologists' own accounts of their informants' thought processes. Anthropologists write books, in which information is inevitably presented by means of language, and so their medium makes them slip far too easily into representing the hypothesized thought processes of those they study as though these also inevitably assumed the organizational logic of the semantics of language. Furthermore, the problem is not just one of medium; anthropologists naturally attempt to produce accounts of intellectual processes which will prove persuasive to their readers, and readers, along with the anthropologists' informants, expect accounts of the thought of the people studied to match the folk theory of thought. As a result, a kind of double complicity is all too easily established between anthropologists and their readers and between anthropologists and their informants—a double complicity which leads to representations of thought in logic-sentential terms.

But in fact, although a plausible account is thereby produced, it leaves all the main participants uncomfortable. Informants feel that anthropologists' accounts are not right. Readers are also suspicious of accounts of the culture of others which, although plausible, are quite unlike the way they experience their own culture. Anthropologists themselves are worried by the fact that the acceptable ethnographies they produce with such effort have somehow lost 'what it was really like'. This is something which they sometimes wrongly attribute to the difficulty of rendering one text into another, while what they should be thinking about is the problem of rendering into a text something which is not a text.

Fortunately, as suggested above, recent work in cognitive science helps us resist the insidious influence of the folk theory of thought by suggesting an alternative to it. This work, although still tentative, provides scientific ammunition against the logic-sentential folk model of thought implied by language and suggests another way in which thought is organized which, furthermore, is intuitively attractive to a field-working anthropologist. This is so because, while the post hoc overlinguistic rationalizations of most ethnography seem distant from what one feels is going on in real situations in the field, the newer theory of thought intuitively seems to correspond to the way informants actually operate in everyday situations.

It is impossible to discuss fully here the theoretical basis of this alternative view of cognition (but see Churchland and Sejnowski 1989; Bloch 1991). The core of the approach, usually known as connectionism, is the idea that most knowledge, especially the knowledge involved in everyday practice, does not take a linear, logic-sentential form but rather is organized into highly complex and integrated networks or mental models most elements of which are connected to each other in a great variety of ways. The models form conceptual clumps which are not language-like precisely because of the simultaneous multiplicity of ways in which information is integrated in them. These mental models are, what is more, only partly linguistic; they
also integrate visual imagery, other sensory cognition, the cognitive aspects of learned practices, evaluations, memories of sensations, and memories of typical examples. Not only are these mental models not linear in their internal organization but information from them can be accessed simultaneously from many different parts of the model through ‘multiple parallel processing’. This is what enables people to cope with information as rapidly as they, and probably other animals, do in normal, everyday situations.

There is, of course, no question of anthropologists’ studying these mental models directly in any detail. However, the awareness that cultural knowledge is likely to be organized in this way should modify the way in which we represent actors’ ways of thinking in general and their conceptualizations of society in particular. It should make anthropologists suspicious of overlinguistic, over-logic-sentential conceptualizations and prompt them to search for alternatives which could correspond to the clumped models just discussed. Furthermore, in going in this direction I believe we will find that much of the often-expressed discomfort with ethnography may disappear and that the problem that the people we study cannot relate to our accounts of them may be diminished.

This paper is an attempt to go some way towards writing ethnography in such a way that actors’ concepts of society are represented not as strings of terms and propositions but as governed by lived-in models, that is, models based as much in experience, practice, sight, and sensation as in language. In trying to do this there can be no avoiding the problem that inevitably this information is presented in a medium, language, whose semantic organization leads back to the kind of presentation from which I am trying to escape, but it is also true that language can be used, if not without difficulty, to talk about processes and patterns which are not in any way language-like. We should not mistake our account for what it refers to.

There is also another difficulty. Normally anthropologists who are trying to persuade their audience that what they are saying is a fair account of the concepts of the people they study tend to fall back on quoting their informants. This apparently innocent procedure is, however, for the reasons just discussed, potentially misleading, since people’s explanations probably involve post hoc rationalizations of either a conventional or an innovative character. So, although I do use what people say in attempting to convey these mental models, these statements are merely purpose-specific periphery to the foundations of conceptualization. But then where do my data come from, and how can I persuade my readers of their relevance? Here I propose an awkward solution.

Through intimate participant observation over long periods of time, anthropologists learn how to live in a relatively coordinated way with their informants. In order to do this they must learn and internalize a great deal of the knowledge that the people they study must themselves have learnt
and internalized. Now, if indeed anthropologists have learnt these clumped non-logic-sentential mental models which organize the cultures they study, they should be able to make at least plausible assertions of how their informants conceptualize the world as a result of their own introspection. I believe that readers who are convinced that anthropologists have carried out the kind of fieldwork necessary for this kind of understanding should be willing to give them the benefit of the doubt. This may seem to be asking a lot; in fact it does not call for more intellectual generosity than is normally required from the readers of academic texts, and sometimes perhaps not for such good reasons. Anthropologists' accounts of the thought processes of their informants accompanied by many verbatim statements, which therefore superficially appear based on irrefutable evidence, will on examination turn out to require almost as much trust from the reader because of the arbitrary way in which these statements must be selected. Furthermore, such language-based accounts are likely to be misleading because the style of presentation will inevitably suggest that the core of the actors' conceptualizations is these few selected verbal statements.

In this paper I shall therefore try to give, for the sake of demonstration, an ethnographic account of the conceptualization of society by a small group of people I studied in Madagascar called the Zafimaniry.¹ My description relies on the evocation of a few linked central mental models² which I believe are, when put together, sufficient to organize their conceptualization and practice of society. These models, as connectionist theories would lead us to expect, are not principally propositional in the traditional sense of the term, though they can be accessed in part through language, but partly visual, partly sensual, partly linked to performance. They are all anchored in practice and material experience, and this is what makes them 'obvious' to anybody, anthropologist or informant, who participates in Zafimaniry life. For this reason, my account would not, I am sure, appear in any way strange to my informants. Indeed, their reaction is that since what I am talking about is merely about what things 'are like'—people, trees, sex, gender, houses, and so on—it is a waste of time to talk much about them.³

In doing this I am implicitly criticizing some aspects of my earlier attempt at giving an account of their society (Bloch 1975). This article dealt with many other topics than the Zafimaniry's conceptualization of their society, and by these I stand. I tried to give an account of their social organization, especially their kinship organization, largely in terms of a fairly hazy moiety organization, complex marriage and filiation rules, and kinship terminology. After subsequent fieldwork I now find this attempt, although on the whole acceptable in terms of the facts it presented, to suffer precisely from giving the impression that the Zafimaniry's conceptualization of society could be given in the logic-sentential form criticized above, though this is precisely how social anthropologists traditionally proceed.⁴
The Zafimaniry are a group of shifting cultivators numbering about 20,000 who live in the eastern forest of Madagascar. Although they have in the past been incorporated into various states and kingdoms, they have, by and large, maintained a remarkable degree of autonomy up to the present day, and in most matters their villages, varying in size between 300 and 3,000 inhabitants, are practically self-governing. Here I attempt to give an account of how they conceptualize their society in terms of five linked mental models from which all the main principles of their social organization seem to flow: (1) the mental model of what people are like and how they mature, (2) the mental model of the differences and similarities between women and men, (3) the mental model of what a good marriage is like, (4) the mental model of what trees and wood are like, and (5) the mental model of what houses are like. These are all very simple models which misleadingly appear to the participants as merely emanations of the empirical, but when they are put together they produce the highly specific conceptualization of society which characterizes the Zafimaniry’s view.

What People Are Like and How They Mature

The Zafimaniry conceptualization of the maturation of the body focuses not so much on growth as on hardening and straightening. Thus Zafimaniry often play with the soft bodies of their babies and laugh over their bendability, calling the children, in an amused fashion, by the common Malagasy term for babies, which literally means ‘water children’. In a similar mood they show each other the baby’s fontanelle and the watery substance it covers. The change from bendable wetness to straight hardness is rarely commented upon in discourse, but its importance can be guessed at from continual allusions to the straight leg and arm bones of elders and ancestors and from the fact that people talk of the elders’ ‘straightening’ the young as they show them the proper ways of behaving according to ancestral rules. Less direct is the way people expect to break their leg and arm bones more and more easily as they get old because, they sometimes say, the bones are ‘harder’. Equally suggestive is the way people note and sometimes comment on the drying out of the skin of the old in relation to that of the young. All these cognitions, practices, and chance or more formal remarks indicate a general understanding of the maturing body which is unproblematically communicated without the anthropologist’s necessarily being aware of the exact manner of this transmission.

This physical change in the body accompanies psychological development. Babies cannot talk and cannot be expected to do much for themselves. They do not exercise any moral judgement. This amoral unpredictability continues through youth and in some ways increases as the children get bigger and cause more chaos. Their behaviour soon becomes tinged with bois-
terous and unstable sexuality, and the boys tend to become aggressive. But a change occurs for both boys and girls at marriage. Marriage calms people down; their minds turn to practical matters to do with making their marriage successful, in particular rearing and being able to support children who themselves produce children, and so on. Later the psychological state typical of the married person is, and should be, gradually replaced by the gravitas of elders. Elders are calm, very stable people whose psychological disposition is the exact opposite of the playful quarrelsomeness of the young in that they are peacemakers who value unity and morality above all things.

This general process of psychological development is often commented upon and even explained by the Zafimaniry, but it is not the basis of the model any more than statements are the basis of the cognition of bodily development. The basis is the demonstrated and observed behaviour of people of different ages, together with the disapproval or surprise expressed when people display psychological behaviour inappropriate for their ages.

The model of maturation also has an aspect which concerns occupation. When the young are old enough to get about, they soon become little foragers. At first this happens in the village, and then, little by little, their activities take place ever farther away, sometimes deep in the forest, involving ever bigger finds and game. The children start with berries and insects, then move on to small fish and crustacea and then to birds and larger mammals. This period of foraging ends with marriage, but because boys marry later than girls (since girls mature earlier) the foraging stage continues longer and develops towards an extreme for young men when their hunting becomes associated with larger animals such as wild boar. This last stage of the foraging period also involves wage labouring; this consists exclusively of forestry work, which is easily assimilated to hunting and foraging both because it takes place deep in the forest and because the young male workers behave as if they were on a hunting trip, with boisterous mock aggression and the singing of hunting songs.

The foraging of the young is for the Zafimaniry an adventurous but not a serious form of activity; they say and demonstrate by their actions that it is a form of play. Consequently the product of such activity, although it is very important nutritionally and economically, is not, nor in their evaluation should it be, taken seriously. For example, when little girls sold some delicious forest fruit at a market for quite a lot of money, everybody commented that this was ridiculous. With marriage, the foraging of the young gives way to agriculture, which is and is recognized to be the typical activity of the married middle-aged. Unlike foraging, this is a serious business, and it is closely associated with the need to support children and grandchildren. Finally, middle age, dominated by agriculture and marriage, is gradually replaced by elderhood. In elderhood other types of activities dominate. At first carpentry and the carving of the wood which will strengthen and
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beautify the house become central activities for the relatively young male elder. Then, for both genders, various forms of highly valued oral activities come to the fore. These include making formal speeches, amongst them requests or thanks to the ancestors, speeches involved with formal visiting, church addresses, and above all the oratory of dispute settlement.

Maturation is therefore not just a matter of physical and psychological development but also a matter of changing occupation. This totality can be more completely grasped by briefly looking at two further facets of the model: language and locality.

The language of the young is and is often noted to be a tumble of rushed, often unfinished sentences. Their conversational style is marked by continual interruptions and what Karl Reisman called ‘contrapuntal conversation’ (Reisman 1974). In many ways it too is an aspect of play. The language of the middle-aged also relates to the character of the activities which dominate their lives. It is typically earnest conversation in which the different speakers do not interrupt each other and in which the intention of conveying information and of negotiation seems to govern the style of intercourse. Finally, the speech typical of elders is highly formalized, highly decorated, and largely formulaic in that it follows predictable models for thanks, greetings, prayers, etc. The manner of speaking is quiet and as if not addressed to anybody; it seems beyond dialogue. However, in contrast with the styles of conversation typical of the young and the middle-aged, the style of elders is employed only when they are being elders; the rest of the time they speak like middle-aged people.

Finally, there is a spatial aspect to all this. The young are always running about, as soon as they can they go off in search of forest products and adventure. The middle-aged also move about a lot, but they are ever more anchored to the house of their marriage. This stabilization and localization increase still further with the elders, who, as we shall see, gradually merge with the house itself.

The Differences and Similarities
Between Women and Men

Gender is not for the Zafmaniry the prime identity that it may be in some cultures. First one is a child, an adult, an elder, a parent, etc., and then one is a special kind of child, a special kind of parent, etc., that is female or male. Furthermore, the relative prominence of gender differentiation varies with age. Gender becomes more important at adolescence and then becomes gradually less so.

However, women and men have certain bodily characteristics which mean that they are different. These bodily differences concern the linked activities of sex and the production of children. Because sex and reproduc-
tion require both women and men, these differences are complementary and not a matter of more or less. At the same time, there is a hierarchical aspect to gender. Insofar as they are comparable, women are usually physically weaker and, in a way which appears to the Zafimaniry inevitably linked, probably also mentally inferior to men, although in certain circumstances (which although unusual are not rare) it is possible for women to be intellectually superior and stronger than men. This is so, for example, if a wife fulfills her duties while a husband does not or if the man is weakened by disease or other infirmity or simply if she is big and he is small.

What a Good Marriage Is Like

The model of the good marriage is as central to Zafimaniry conceptualization as that concerning the maturation of people. The core of the model is the image of a complementary, loving, fruitful union of two spouses engaged in joint domestic and agricultural tasks. Fruitfulness manifests itself first of all in the number of children produced and then in the number of children produced by these children, and so on. Again, it is manifested in the success, principally the agricultural success, of the parents in providing for these children, grandchildren, and so on, so that they thrive. A fruitful marriage is one in which the combination of the spouses leads to the growth of a unity which they have created. This means that the children of the marriage are the continuation of this unity, and therefore siblings are part of a single totality since they are the outgrowth of the original unity produced by loving complementarity.

The centrality and character of this image of loving complementarity can be conveyed by a few hints provided by Zafimaniry ethnography. One of these is that parents who arrange that their children should marry keep this a secret from them. It is felt that parents can only point their children in the right direction, and mutual compatibility can only be achieved if it is believed by the parties to be spontaneous. Another hint is the fact that Zafimaniry diviners, in contrast to those in other parts of Madagascar, normally find the cause of sterility to be not a problem the woman has but the lack of compatibility of the pair. Then there is the total absence of public quarreling between spouses in a society where nothing much can be kept private and the fact that people say that if such quarreling became public the marriage would immediately break up.

At the heart of the model is the image of the couple cooperating harmoniously in the performance of single domestic and agricultural activities through different but complementary tasks. Most agricultural tasks are indeed carried out by husband-and-wife pairs working together, and this is often commented upon favourably. People often pointed out to me the emblematic significance of the particularly heavy work involved in carrying
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crops from the field to the house; this is a task that spouses share, although their ways of carrying, one on the head the other on the shoulder, are different.

Marital harmony is the product of the psychological and physiological compatibility of the spouses, but for the Zafimaniry this requires that a balance be established between the two parties. If the family of either the groom or the bride is much weaker, there cannot be a proper marriage, because a proper marriage requires that equal respect be paid to the origin of both spouses. Sexual unions in which this is not the case often occur, but to the Zafimaniry they are not marriages because marriage is marked by mutuality and reciprocal exchange at all levels. Balance is always threatened by the difficult intrusion of an element of imbalance brought about by the superiority of the man over the woman. Much of the Zafimaniry conceptualization of society flows from the attempt to reintroduce in this situation the balance of the good marriage.

A Zafimaniry marriage is the result of the stabilization of what starts as a playful, fleeting relationship between two very young persons who are like that because of their state of maturation. As the children grow up and form couples who begin to have regular sexual relationships, this fact becomes noticed, and the young people are forced to appear before the girl's parents in a simple ritual which is called tapa maso (literally, 'the breaking of the eyes'). This refers to the fact that it is very wrong for people of different generations, especially if they are of different sexes, to have knowledge of each other's sexual activity. The appearance of the young couple together asking for the girl's father's blessing therefore breaks this avoidance—it 'breaks the father's eyes'.

After the tapa maso the couple will most probably stay with the girl's parents until the next ritual, which is called the fanambarana (literally, 'the making clear'). The essential element of this ritual is the groom's fetching of the bride and her trousseau of kitchen furniture to his family's locality. The reason it is the bride who follows the groom and not the other way round is that the man is stronger than the woman. This fact, however, contradicts the image of balance, and so various strategies are adopted to restore it.

First of all, attempts are made during the fanambarana to produce a downright denial that an imbalance exists. The marriage is said to be a swop; everybody repeats the well-worn phrase that the marriage is 'an exchange of a male child for a female child', implying that the parents of the boy gain a daughter and the parents of the girl gain a son—and, indeed, from then on the spouses address their parents-in-law with the same terms as they use for their own parents. Similarly, people say that a marriage is 'the exchange of male orange for a female orange'. This gnomic statement is then explained as meaning that it is an exchange of like for like, since no one can tell the difference between the two sorts of oranges.
Other ways of lessening imbalance concern specific practices. The spouses must spend much time with the girl's parents, and they will seek the blessings of both sides equally for any important task. Their children are considered to be equally the grandchildren of both sides. Imbalance is also temporarily corrected by the fact that the obligations of the couple to the girl's parents have priority over their obligations to the boy's parents because, as they say, 'they are less often with the former'. Most important, the imbalance caused by gender is corrected by the introduction of the concept of the unity of siblings. This comes into play in two ways.

Since siblings are part of a unity, marriage to a particular person implies, to an extent, marriage to his or her siblings, including those of the other gender. The siblings of ego's spouse are also ego's spouses. Thus Zafimaniry men refer to the brothers of their wives and the husbands of their sisters by a term (*tady laby*) which literally means 'male spouse', and Zafimaniry women refer to the sisters of their husbands and the wives of their brothers by a term (*tady uavy*) which literally means 'female spouse'. By stressing these 'marriage' relationships and the reciprocal and equal cooperation which exists between 'female spouses' and especially between 'male spouses', the marriage relationship regains a gender-free balance which the difference between women and men threatens to disrupt.

The unity of siblings is also used to recover the balance essential for the good marriage in an even more radical way. This involves making a specific marriage part of a reciprocal exchange wherein pairs of cross-sex young people classed as siblings by the kinship terminology intermarry simultaneously. Zafimaniry have a strong preference for marriage of a brother and a sister to sister and brother. This creates a pattern very similar to that familiar to anthropologists as cross-cousin marriage, and it has similar sociological effects in that it leads to a loose moiety system, something I discussed in a previous publication on the Zafimaniry (Bloch 1973). That this is so should not, however, make us forget that this moiety system is conceptualized as the result of the need for balance in a good marriage, not in the traditional description by means of rules and terminology which characterized my earlier attempt.

**What Trees and Wood Are Like**

The importance of trees and wood, two words which are translated into Malagasy by the single word *hazo*, is central to the Zafimaniry. As shifting cultivators they depend on burning wood to make fields; because the climate is so cold and damp they must have wood fires continually burning in their houses. Besides this they used to, and to a certain extent still do, make their cloth from the bark of trees, and their houses and most of their utensils are made of wood. It is not surprising, therefore, that all Zafimaniry
possess very extensive scientific knowledge about wood, about the many species of trees, about the many different qualities of the woods they yield, and about the way different woods must be treated to prepare them for the various uses to which they are put.

The most valued trees and also the rarer ones are those which produce the kind of wood used in house building, and these must contain what the Zafimaniry call *teza*. The *teza* of trees is a dark impacted core which is much harder than the outer part, called by a term which normally refers to white of egg. It is often compared to the bones of animals and humans, which can also be called by the same word. A central image of *teza* is that it is what remains after a swidden has been fired, since this hardened core does not normally burn. Indeed, the word *teza* is the root of the verb *mateza*, 'to last or to remain'.

The maturation of trees with *teza* is of the greatest significance to the Zafimaniry. Young trees have no *teza* at all; then, as the tree gets older, stronger, and less bendable, the *teza* starts to develop as a tiny core surrounded by a very extensive 'white of egg'. Gradually, over many years, this proportion will change, so that in very old, very tall trees the *teza* will occupy most of the trunk, leaving a small outer ring of 'white of egg'. Such trees are the greatest and the most useful of all trees, since their *teza* can be used for the building of long-lasting houses and other artefacts which partake of the lasting nature of *teza* because they are made of it.

The presence of *teza* is awe-inspiring; it is a thing worthy of respect. It is the product of a maturation similar to the maturation of human beings in that it implies a straightening and a hardening of an inner core, but the *teza* of trees goes farther in that process than human beings can. Humans soon reach their peak of straightness and hardness and then go into reverse in old age and death; trees with *teza*, in contrast, continue to harden and become more and more lasting. As hard, dried wood, they outlast transitory human beings.

It is this lasting apotheosis of the *teza* of the noblest wood that is celebrated in the Zafimaniry carvings which are famous throughout Madagascar. These are low-relief geometrical patterns which cover the wooden parts of houses. Many writers have attempted with little success to understand what these carvings represent. In fact they represent nothing; they are a celebration of the lasting qualities of the *teza* which they cover.

**What Houses Are Like**

As is the case for people, marriages, and trees, the best way of understanding the Zafimaniry mental model of the house is to see it as a process of maturation. Indeed, the model of the maturation of the house is intimately tied to the model of the maturation of a marriage.
The first sign of appearance of a Zafimaniry house is usually the beginning of a flimsy building to the south of a young man's parents' house. Since a son owes his parents respect, this position is chosen because the south is an inferior direction to the north. The building will be very flimsy; apart from the four corner posts, it will be made of flexible woven bamboo and mats. It will most probably not possess the two focal features of the Zafimaniry house—the central house post, made of the teza of the hardest wood known to the Zafimaniry, and the hearth. These two crucial features are added only when the young man feels that his marriage is sufficiently well advanced to move his wife and children into the house. Then, with the permission of his father, he will erect the central house post and build the hearth. These will become the twin foci of the house. The central house post will be associated with the man for the rest of his life, and his normal place will be leaning against it. The hearth is little more than three stones which support a cooking pot and under which a fire can be lit. However, it will be furnished with the pots and cooking utensils which the bride brings with her after the fanambarana ceremony, and after that it will become permanently associated with the woman of the marriage. Before the house can be fully lived in, that is, before eating, cooking, or having sexual intercourse in it is allowed, a ritual of inauguration will be held wherein the elders of the families of both spouses will bless the house, especially the hearth and the central house post.

This ceremonial opening is, however, only a stage in a very long process during which the house will become harder and more permanent. What this means is that the soft and perishable parts of the house will gradually be replaced by the massive teza of great trees shaped so as to slot into each other. These are called the 'bones of the house', and they make the house extraordinarily lasting. The process of strengthening and beautifying the house is very long drawn out. It takes a long time for the house to become completely wooden, and before this some wood may already have had to be replaced. As the house is becoming more wooden, the wood itself will be gradually carved to 'celebrate' (to give vonahitra to) the teza of the posts, of the planks, and of the whole house itself. This hardening and beautifying is carried out at first by the husband working in marital cooperation with his 'male spouses', especially his wife's brothers. Then, as the spouses grow older, the task will be taken over by sons and daughters' husbands, then by grandsons and granddaughters' husbands, and so on. Thus the house which began with the marriage of two people will grow and become beautiful together with the fruitful balanced compatibility which they achieve. If the marriage continues to be fruitful, that is, if further descendants are born to it, this process of house growth will continue long after their death.

This is possible partly because of the unity of the group descended from a marriage and partly because of a symbolic substitution made conceivable.
by the association of people and trees via the notion of *teza*. After the death of the couple the man will come to be represented by the central house post and the woman by the hearth and especially the furniture for it which she brought at her marriage. These remaining (*maatza*) objects become relics representing the original couple, and they are addressed as such and offerings are made to them by the descendants, especially on the occasions when they gather in the house, by then referred to as *تناو ماسنَا* (‘holy house’). These meetings will principally be to make requests for blessings from the house/ancestors and to settle disputes among descendants. A successful marriage therefore becomes an ever harder and more beautiful house which never stops growing as descendants in both male and female lines continue to increase. For these people it remains their ‘house’, though what this means is that it is a place of cult for them.

There is, however, a further aspect to all this. The couple in the house will itself have been the balanced product of two different houses: that of the parents of the bride and that of the parents of the groom. This fact is well recognized by the Zafimaniry. Thus young spouses with children, normally living in a house which has only just begun to harden with bones, are also children of two other couples. In fact, both the woman and the man are children of two couples, because, as we have seen, the marriage has made them the children of each other’s parents. This dual filiation is demonstrated by the fact that the young couple and their children spend much time in the houses of their two sets of parents and will go and seek blessings from both sets whenever an important decision has to be made. In fact, of course, they will in theory also do this in the four sets of grandparents’ houses, and so on, except that for these more remote ascendants it will not be the actual couples who are visited and asked for blessings but the paired house posts and hearths of holy houses.

It might seem as if everybody would belong to a near-infinity of ascendant houses, but the reality is usually much simpler. The reason is that the strong preference for marriages which overcome the imbalance caused by differences in gender (such as marriages between two pairs of cross-sex siblings) means that most marriages tend to occur between pairs of localities or moieties originating from two holy houses. The repetition of marriages means that most couples need only be concerned with two holy houses in which their respective groups originated as well as the two houses where their parents live. Intermediary houses tend to be forgotten.

The Conceptualization of Society

We have looked at five mental models of the sort which, according to connectionist theory, we would expect to be at the basis of people’s conceptualizations of society. From my experience of life with the Zafimaniry these
What Goes Without Saying

... seem to me the central notions which organize these matters for them. To the Zafimaniry, and perhaps to us, these seem very 'obvious', very 'well-founded' observations of how things are. Yet we have seen how, when they are put together, they produce a distinctive conceptualization of society. Had we not proceeded in the way we did here—starting from these apparently 'obvious' understandings of 'how things are'—we would have ended with a description of Zafimaniry society which would have fallen foul of all the problems of misrepresentation I described in the introduction to this paper. This description would probably have been very similar to the account I gave in my first article on the Zafimaniry (Bloch 1975), and the reader who is in any doubt about the difference in the way ethnography is handled here from classical models should refer to that earlier attempt. The structure of the argument would be very familiar, and therefore acceptable, to other anthropologists, but it would suffer from the same problems of representing Zafimaniry ideas of society as if they took a logic-sentential form which in fact they do not take. Most probably, such an account would be totally foreign to the Zafimaniry, whereas I am encouraged by the fact that they find the account presented here to be not 'alien' but so obvious that they think it pointless.

What, then, are the implications of going about things in this way? First, this account is much more likely to be compatible with theories which describe the mental/neural processes of storage and retrieval that people use in everyday life than would be the case for an account based on logic-sentential models. The Zafimaniry 'know' perfectly well that this is how people are and how they mature; they 'know' that trees grow like this and develop teza; it is 'obvious' that men and women are physiologically different, but it is equally 'obvious' that girls and boys are equally the children of their parents; 'clearly' a strong, hard, decorated house is the house of a couple whose children and other descendants are many and successful; it 'goes without saying' that a good marriage involves balance, cooperation, and mutuality. Of course, all this obviousness is ultimately misleading; anthropologists know, usually because they originate from another culture, what the informant does not know—that the 'obvious facts' are, partially at least, the product of specific and in the short term arbitrary historical processes. This, however, is not a reason for giving a false account of how people conceptualize their society.

In fact, there are other advantages in such an approach which can only be suggested here. The anchoring of conceptualization in the material—the body, houses, wood, styles of speaking—and in practices—cooking, cultivating, eating together—means that the cultural process cannot be separated from the wider processes of ecological, biological, and geographical transformation of which human society is a small part (a point made powerfully by Descola in this volume). Culture is not merely an interpretation superimposed on these material facts but integrated with them. When we
are talking of mental processes, as we must when we are talking of conceptualization, we are talking of the interaction of one biological process with other biological and physical processes. Finally, seeing the conceptualization of society as flowing from mental models which are in great part conceptualizations of material things and practices suggests something about the way living in a society is learnt. It is not principally learnt by absorbing verbal rules and lexicographic definitions; rather it is learnt as one learns as a baby to negotiate the material aspect of one’s house, as one follows other children in looking for berries in the forest, as one watches the stiff gait of one’s grandfather, as one enjoys the pleasure of working harmoniously with a spouse, as one cooks with the implements of the hearth, as one sees one’s grandfather lean against the central post, as one cuts through a massive tree trunk, and as one sees the beauty of the house of a fruitful marriage.7

Notes

1. I carried out two prolonged periods of fieldwork among the Zafimaniry, for six months in 1971 and then for another six months in 1988–9. I was familiar with their culture and language before I started, having previously carried out fieldwork for several years among the closely related Merina. Previous accounts of the Zafimaniry include Vérin (1964), Coulaud (1973), Bloch (1975), and Raminosoa (1971–2). The research on which this paper is based was funded by a generous grant from the Spencer Foundation of Chicago. I am grateful for comments on earlier seminar presentations from members of the anthropology departments of the University of Bergen and the London School of Economics. Above all I am grateful for suggestions and help in preparing the manuscript from Fenella Cannell and J. Parry.

2. The term ‘mental model’ is Johnson-Laird’s (1983). What I am referring to is similar to the ‘cultural model’ used by some anthropologists, but this term usually implies a reliance on language which I am trying to avoid (Holland and Quinn 1987).

3. The fact that these models are anchored in practice and material experience means that even to a non-Zafimaniry they may appear ‘obvious’. This is significant, since such an approach rules out many of the wider claims of certain forms of cultural relativism.

4. I am ignoring variation within Zafimaniry culture in this account because when dealing with the fundamental models with which I am concerned I do not believe that there is much variation. This occurs at more superficial levels.

5. It is very interesting to compare this with the account of American marriage given by Quinn (1987). In many ways Quinn is aiming for the same kind of data as I am, but in contrast she depends almost exclusively on linguistic information.

6. This jump from people to things involves cognitive processes very different to those which can be discussed here (see Bloch 1991).

7. The parallel with Bourdieu’s (1973) discussion of the Berber house will be clear, but differences arise from a completely different view of the nature of mental processes; Bourdieu uses precisely the logic-sentential notion of thought which I am criticizing here.
References


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 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). 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-Strauss 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). 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 the-
ory, 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 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 oper-
ates 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 expiatory 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). 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 ethnoscientific, 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 an 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 suggest 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 common-sense 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.

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 relation-
ship, 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-
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Usage 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


