Chapter 8

Interpreting and explaining cultural representations[1]

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A representation sets up a relationship between at least three terms: that which represents, that which is represented and the user of the representation. A fourth term may be added when there is a producer of the representation distinct from its user. A representation may exist inside its user: it is then a *mental representation*, such as a memory, a belief or an intention. The producer and the user of a mental representation are one and the same person. A representation may also exist in the environment of its user, as is the case, for instance, of the text you are presently reading; it is then a *public representation*. Public representations are usually a means of communication between a user and a producer distinct from one another.

A mental representation has, of course, a single user. A public representation may have several. A speech may be addressed to a group of people. A printed text is aimed at a wide audience. Before recent techniques such as printing or magnetic recording made the strict duplication of a public representation possible, oral transmission allowed the production of representations similar to one another: the hearers of a tale may, for instance, become in turn its tellers. It must be stressed, however, that oral transmission is not a reliable means of reproduction; it generates a fuzzy set of representations which are more or less faithful *versions*, rather than exact copies, of one another (cf. Wikan, this volume).

1. This article expands and corrects Sperber (1989).

Consider a social group: a tribe, the inhabitants of a town or the members of an association. Such a group and its common environment are, so to speak, inhabited by a much larger population of representations, mental and public. Each member of the group has, in his or her head, millions of mental representations, some short-lived, others stored in long-term memory and constituting the individual's 'knowledge'. Among these mental representations some – a very small proportion – get communicated, i.e. cause their user to produce in the environment a public representation which in turn causes another individual to construct a mental representation similar in content to the initial one.

Among communicated representations some – a very small proportion again – are communicated repeatedly and may even end up being distributed throughout the group, i.e. have a mental version in most of its members. When we speak of *cultural representations*, we have in mind – or should have in mind – such widely distributed and lasting representations. Cultural representations so understood are a fuzzy sub-set of the set of mental and public representations of a given social group.

Anthropologists have not converged on a common view of cultural representations, a common set of questions about them, or even a common terminology to describe them. Most authors approach the various genres of representations separately and talk of beliefs, norms, techniques, myths, classifications, etc. according to the case. I would like, nevertheless, to reflect on the way anthropologists (and other social scientists) represent and attempt to explain cultural representations in general.

Interpreting cultural representations

Suppose you want to produce a representation of a basket: you may produce an image of the basket, or you may describe it. In other terms, you may either produce an object that resembles the basket, for instance a photograph or a sketch, or else you may produce a statement. The statement in no way resembles the basket, but it says something true about it. (Truth, of course, is a necessary, but not a sufficient condition for a description to be adequate.) It might seem that the situation is the same when what you want to represent happens to be a representation, the

tale of Little Red Riding Hood, for instance. You might record or transcribe the tale (or, rather, a particular version of it), that is, produce an object that resembles the tale in the manner in which a photograph or a sketch resembles a basket. You might also describe the tale by stating, for instance: 'It is a tale found throughout Europe, with one animal and several human characters, etc.'

Yet, there would be something missing in these representations of Little Red Riding Hood: the recording or the transcription in themselves only represent an acoustic form, while the description suggested tells us little more about the *content* of the tale, which, after all, *is* the tale. All you need do, one might argue, is describe the tale in greater detail. You might state for instance: 'Little Red Riding Hood is a tale found throughout Europe, which tells the story of a little girl sent by her mother to take a basket of provisions to her grandmother. On her way, she meets, etc.' You could, of course, in this manner recapture the content of the tale as closely as you would wish, but notice what would be happening then: instead of *describing* the tale, you would be *telling* it all over again. You would be producing an object that represents the tale, not by saying something true about it, but by resembling it: in other words, you would be producing yet another version of the tale.

Let us generalise: in order to represent the content of a representation, we use another representation with similar content. We do not describe the content of a representation, we paraphrase it, we translate it, we summarise it, we expand on it, in a nutshell, we *interpret* it.[2] An *interpretation* is a representation of a representation by virtue of a similarity of content. In this sense, a public representation, the content of which resembles that of the mental representation it serves to communicate, is an interpretation of that mental representation. Conversely, the mental representation resulting from the comprehension of a public representation is an interpretation of it. The process of communication can be factored into two processes of interpretation: one from the mental to the public, the other from the public to the mental.

Interpretations are just as ordinary in our mental life as are descriptions; they are a form of representation produced and

2. On the distinction between interpretation and description, see Sperber (1985a, Ch. 1) and Sperber and Wilson (1986, Ch. 4).

Contextualisation of the said, and of discourse, will not suffice to ensure an adequate rendering of what is being said or understood. As Hobart has observed, contextualising, 'raises the delicate issue of whose criteria of relevance are at stake?' (1986: 8). Think of a contextualised interpretation of the Hindu balian's discourse with the Muslim woman. Would not 'the context' here have to be made up, in large part, of their discrepant religious positions leading us to presuppose, in fact, that here was a blatant confusion of tongues? Certainly, this was the context I brought to bear on the scene, as it was what the Muslim's deliberations beforehand suggested would be the appropriate one. Had I not been unusually close with her, I do not see how this context could have been invalidated, I might even have embedded my (mis)interpretation in thick description to make it emerge as highly plausible.

The reference to 'context' can lend a false sense of security, whereas invoking it explains nothing. I sense a tendency in our field, to call on context as if to underscore the authority of one's account. 'Context' parades like 'I was there' (Clifford 1988) to lend credibility to one's account. Also, for this reason, does Rosaldo's story stand out as exceptional. It exposes the arbitrariness of context. His reinterpretation called for a new context, but it was one which was there – in Ilongot's conceptual universe – all the time. Such reappraisals are sorely lacking in anthropology (but see Colson 1984).

Context is 'just an analytical convenience . . . but there is a danger of it being seen as somehow substantive' (Hobart 1985b: 34). Another problem, as Sperber and Wilson point out, is that 'context' tends to be invoked as if it were given beforehand, whereas in real life it is continually shifting and changing since 'each new utterance . . . requires a rather different context' (1986: 137, 16; see also Sperber, this volume). Their theory of relevance is a major step in helping us understand how context is selected in actual everyday communication.

I suggest what may be needed is not so much 'contextualisation' as finding ways of transcending what we conventionally have conceived of as 'the context'. Thick description is not an answer, for it leaves unexplained *what* is to be thickly described (cf. also Rosaldo 1989: 94ff). We need to find ways to tune in to what people are telling us and why they do, rather than *how* or *the* way they are telling it. Delaney (1988: 293)

speaks of the need to refine one's sensibilities; to know how to listen. We need to listen in such a way that we heed the *effect* people are trying to make and the *relevance* of their worlds in terms of how they are positioned and where they want to go, rather than the message that their word might seem to create.[12]

Does this not come closer to what we all do in our daily lives when understanding is of the essence, and we can ill afford to go wrong? It would entail another kind of reading of anthropological texts: again a going beyond – in a manner I myself have failed to do in my readings, and critiques, of Bateson, Belo, Mead and Geertz – and for which my understanding, and eventual representation, of Bali may have suffered (Wikan 1987, 1990). I now propose to follow Davidson on an experimental tour which exoticises the anthropologist's familiar round. With his theory, what light might be shed on the fieldwork encounter?

Passing theories

Now think that our task was to meet another person, or other persons, from a different culture – how could we proceed? Davidson suggests we would need to try to develop a vocabulary which would fit the task at hand. Most probably we could use 'mumbles, stumbles, malapropisms, metaphors, tics', etc. (Rorty 1989: 14) to avoid being taken by surprise: and we would resort to a set of guesses about what the other person would do under the circumstances. Most probably, so would she in response to us. Davidson refers to such guesswork, not-to-be-taken-by-surprise, as a '"passing theory" about the noises and inscriptions presently being produced by a fellow human' (Davidson 1986, quoted in Rorty 1989: 14). This is part of a larger passing theory about this person's total behaviour. Such a theory is 'passing' in the sense that it must constantly be corrected to allow for mumbles, stumbles, occasional egregious stupidity, strokes of genius, and the like:

12. An implication of what I am saying is that I consider the use of tape recorder and often also notebook as problematic in the field, and best to be avoided except for certain clearly defined purposes. Reliance on such devices seems to reinforce, and express, our over-reliance on words and the *exact* utterance.

understood by everyone. To express oneself or to understand other people's expressions is, implicitly, an act of interpretation. Moreover, we are all producing explicit interpretations when answering questions such as: What did he say? What does she think? What do they want? In order to answer such questions, we represent the content of utterances, thoughts or intentions by means of utterances of similar content.

Needless to say, the anthropological study of cultural representations cannot ignore their contents. As a result, and whether this pleases us or not, the task of anthropology is for a large part interpretive. However, precisely because interpretation is based on a quite ordinary ability rather than on a sophisticated professional skill, most anthropologists have produced interpretations just as Molière's Monsieur Jourdain produced prose: without being aware of doing so, or at least without reflecting much on it.

As long as interpretation is about individual words or thoughts, the degree of freedom that the interpreter grants herself is often manifest and unproblematic. You inform me, for instance, in one sentence and with a sneer, of what the Prime Minister said in his press conference; I have no trouble understanding that while the gist may be the Prime Minister's, the conciseness and the irony are yours. Similarly, ordinary anthropological renderings *of individual words and thoughts* are, often enough, easy to understand and to accept (as long as not too much depends on exact formulation, and, even in the latter case, a carefully glossed translation may still do).

In anthropology, however, what gets interpreted is often a *collective representation* attributed to a whole social group ('The So-and-So believe that...'), and which need never be entertained, let alone expressed, by any one individual member of that group. There is neither a clear commonsense understanding of what such collective representation might be, nor a straightforward way of assessing their faithfulness. The lack of a clear methodology makes it difficult to evaluate and therefore to exploit these interpretations. Nevertheless, they are given an important role in anthropological accounts and, as we shall see, are even sometimes offered as ultimate explanations.

Here is an illustration. The scene, reported by the French anthropologist Patrick Menget, takes place among the Txikao of Brazil:

> At the end of a rainy afternoon, Opote came back home carrying a fine *matrinchao* fish he had caught in his nets. He put it down without a word next to Tubia, one of the four family heads of his house. Tubia cleaned it and put it on to smoke. Until the fall of night he ate it, by himself, in small mouthfuls, under the interested eyes of the other inhabitants of the house. No one else touched the *matrinchao*, nor showed any desire to have some of it. Yet the hunger was universal, and the flesh of the *matrinchao* is among the most highly praised. (Menget 1982: 193)

So far, this is essentially an ordinary description: every sentence in it expresses a proposition presented by the anthropologist as true. The situation described is, however, quite puzzling: 'Why', asks Menget, 'this general abstention?' And he goes on to answer:

> The fisherman, Opote, possessor of fishing magic, could not consume his catch without the risk of damaging this magic. The other family heads avoided the flesh of the *matrinchao* for fear of endangering the health and the lives of their young children, or their own health. Since their wives were nursing, they had to abstain for the same reason. The children, finally, would have absorbed the particularly dangerous spirit of this species. (ibid.: 193)

This time the anthropologist – who does not himself believe in magic or spirits – is not presenting as true that Opote was running the risk of damaging his magic, or that the children would have absorbed a particularly dangerous spirit. He is presenting these statements as similar in content to the beliefs motivating the abstinence of Opote's people. These are interpretations. Such interpretations of individual thoughts are neither harder to comprehend, nor more suspicious than the interpretations we all use all the time to talk of each other.

However, the anthropologist's ultimate goal is not to report particular events. Menget's aim, for instance, in reporting the anecdote of Opote's *matrinchao*, was to illustrate some hypotheses on the 'couvade', first among the Txikao themselves, then among the South American Indians, and ultimately on the couvade in general.[3] Menget proposes a subtle analysis of the relevant Txikao views on life and its transmission and concludes:

3. 'Couvade', it will be remembered, refers in anthropological literature to a set of precautions a man is expected to take during and just after the birth of a child of his, precautions similar to those imposed on the mother of the child for more obvious reasons. For a discussion of the couvade, see Rivière (1974).

> everything happens as if two antagonistic principles ruled over the life processes. . . . A strong principle, tied to blood, to fat, rich meats and fermentation results from the constant somatic transformation of weaker substances, water, milk, sperm, white flours, lean meats. But inversely, the human body, in rhythms that vary with age, sex, and condition, anabolizes the strong substances and neutralizes their danger.
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> . . . In the couvade, the whole set of occupational, alimentary, and sexual taboos comes down in the end to avoiding either an excess of strong substances . . . or a loss of the weak somatized substances. . . . The creation of a new human being activates the whole universal process of transformation of substance, but also the separation of a part of the somatized substance of the parents and the initiation of an individual cycle. (ibid.: 202–3)

Again, the ethnographer is interpreting: he does not himself, for instance, believe or intend to assert that 'the human body anabolizes strong substances' the non-assimilation of which 'leads to swelling diseases'. He is offering such formulations as similar in content to cultural representations underlying the Txikao couvade practices.

However, while it is easy enough to imagine Opote thinking or saying, in roughly similar terms, that he could not consume his catch without damaging his magic, it is hard to conceive of Txikao thoughts or utterances involving notions of, say, the 'somatic transformation of weak substances', or the 'anabolization of strong substances'. The resemblance of content between the interpretation and the representations interpreted is manifestly weaker here than in ordinary interpretations of individual thoughts or utterances, and the degree of resemblance is hard or even impossible to evaluate. (What is at stake is not the work of an individual anthropologist: on the contrary, I have chosen to discuss Menget's essay because I see it as a good example of today's best ethnography. At stake here are the limits inherent in the interpretive approach to cultural representations.)

An ethnographer is faced at first with a great diversity of behaviour which she progressively manages to understand by discerning underlying intentions, that is by becoming able to conceptualise this behaviour as actions. She becomes adept, in particular, at discerning the intentions governing speech acts, in other terms, at comprehending what her interlocutors mean (cf. Edelman, this volume). Intentions thus understood still call for further and deeper understanding. Let us accept that 'the family

heads avoided the flesh of the *matrinchao* for fear of endangering the health and the lives of their young children, or their own health', but how are such means supposed to serve such ends? A deeper understanding of intentions involves grasping how they could be rational, or in other word, seeing how they might follow from underlying desires and beliefs. If, for the Txikao, the flesh of the *matrinchao* is 'strong' and hazardous for one's health, if father and child are of one and the same substance, a substance which, contrary to appearances, does not divide into two independent beings until some time after birth, then we begin to grasp how the behaviour of Opote's people might be rational. To grasp it further, we should try to establish the rationality of the underlying beliefs themselves, that is, not just their mutual consistency, but also their compatibility with Txikao's experience.

In our everyday striving to understand others we make do with partial and speculative interpretations (the more different from us the others, the more speculative the interpretations). For all their incompleteness and uncertainty, these interpretations help us – us as individuals, us as peoples – live with one another. Anthropologists have contributed to a better understanding and thus a greater tolerance, of culturally different others. To do so, they haven't relied on scientific theories or rigorous methods, which are not part of the anthropologist's standard tool-kit. Given the cultural distance, the comprehension goals of anthropologists are particularly ambitious and arduous. Still, the form of comprehension involved is quite ordinary: anthropologists interpret behaviour, verbal behaviour in particular, by attributing beliefs, desires and intentions to individual or collective actors, in a manner that makes this behaviour appear rational.

One might assume that the best interpretation is the most faithful one, i.e. the one whose content most resembles that of the interpreted representation. On reflection, things are not that simple. If her aim were just to maximise faithfulness, the anthropologist should only publish translations of actually uttered words. However, most utterances heard by the anthropologist make sense only in the very specific context in which they were produced; they rely on shared cultural representations which they do not express directly.

The anthropologist must, for her own sake to begin with, go beyond mere translation: only then can she hope to understand

what she hears, and thus be genuinely able to translate it. She must speculate, synthesise, reconceptualise. The interpretations that the anthropologist constructs in her own mind or in her notebooks are too complex and detailed to be of interest to her future readers, and moreover they tend to be formulated in an idiosyncratic jargon where native terms, technical terms used in an *ad hoc* way, and personal metaphors mix freely. Later, writing for readers who will spend a few hours on a study to which she devoted years, the anthropologist must synthesise her own syntheses, retranslate her own jargon, and, unavoidably, depart even more from the details conveyed by her hosts. In order to be more relevant, the anthropologist must be less faithful.

Moreover, similarity of content varies with the point of view and the context. To say, for instance, that for the Txikao, the human body 'anabolizes strong substances' is suggestive and not misleading in the context of Menget's discussion: in that context, the notion of anabolisation is taken quite metaphorically. In other words, the resemblance between the chemical notion of anabolisation and the Txikao notion it interprets is seen as pertinent but quite restricted. On the other hand, the same interpretive statement would be misleading in the context of a comparative study of cultural views of the chemistry of digestion, where consideration of relevance would lead one to take the notion of anabolisation much more literally.

The intuitive and context-dependent character of interpretation does not mean that all interpretations are equally good or bad, but it does mean that our criteria of evaluation are themselves partly intuitive and of limited intersubjective validity. Some imaginable interpretations would be, by all reckonings, quite bad (e.g. that the true content of the Holy Trinity dogma is a recipe for chocolate mousse). But it may happen that significantly different interpretations of the same representation all seem plausible. The data interpreted by Menget in an 'intellectualist' manner (i.e. as involved in an attempt at explaining the world) might, for instance, be approached with equal subtlety in a psychoanalytic vein. Presented with both types of interpretations, readers would, no doubt, choose according to their theoretical preferences. Moreover, in doing so, they would act rationally. Here, however, is the rub: if it is rational to prefer one particular interpretation to another on the basis of prior theoretical preferences, then it is

hard or impossible to validate or invalidate a general theory on the basis of particular interpretations.

Interpretation allows a form of understanding that we cannot do without in everyday life, the understanding of representations, mental and public, and therefore the understanding of people. In the scientific study of representations, interpretation is just as indispensable a tool as it is in everyday life. But can we use as a scientific tool an intuitive and partly subjective form of understanding?

No evidence is absolutely reliable, and, arguably, no evidence is theory-independent. However, the basic requirement for the scientific use of any evidence is not that it should be absolutely reliable and theory-independent, but only that it should be more reliable than the theories that it serves to confirm or disconfirm and therefore independent of these particular theories (or of any equally or more controversial theory).

Some interpretations are more reliable than others and more intersubjectively acceptable. If these interpretations somehow depend on 'theories' of human comprehension, these are tacit theories that human beings in general and anthropologists in particular are not even aware of and therefore not intending to challenge. Thus, we would all, I guess, trust Menget and accept his claim that Opote could not consume his catch without the risk of damaging his fishing magic as, at the very least, a reasonably approximate interpretation of part of what Opote himself or others around him might have said. That is, we would trust Menget's ability to understand and sometimes to anticipate what individual Txikaos may have said to him on specific occasions, just as we would trust ourselves if we had been in Menget's place, having learnt the language, spent the time among the Txikao, etc. Fairly literal and flat interpretations of particular utterances and ordinary intentions made by individuals competent in the language and familiar with the people are not totally reliable or theory-independent, but they are often uncontroversial.

Commonsensical interpretations of particular utterances and of other normally intelligible individual behaviours are reliable enough to be used, with methodological caution, as basic evidence for anthropological theorising. That is, these interpretations are significantly more reliable than the theories we might want to test with their help. On the other hand, more

speculative forms of interpretation, such as interpretations of beliefs the believers themselves are incapable of articulating, or interpretations of collective mentalities, whatever their attractions and merits, will not do as evidence.

The question then is: can anthropological theorising rely only on the first, more reliable but also more modest, kind of interpretation? The answer depends on the kind of theorising one wants.

Explaining cultural representations

'To explain' may be taken in two senses. In a first sense, to explain a cultural representation, for instance a sacred text, is to make it intelligible, i.e. to interpret it. The previous section dealt with such interpretive explanations. In another sense, to explain a cultural representation is to show how it results from relatively general mechanisms at work in a given specific situation (cf. Pálsson, this volume). In this second sense, the only one to be considered in this section, the explanation of cultural representations has an essential theoretical aspect: the identification of the general mechanisms at work. This theoretical objective is not a concern of most anthropologists, whose main focus is ethnography, and is pursued in a scattered and piecemeal fashion. There is not even a majority view – let alone a general agreement – as to what might be regarded as an adequate explanatory hypothesis in anthropology.

Simplifying greatly (and with apologies for the unfairness that such simplification entails), I will nevertheless distinguish four types of explanation – or purported explanation – in anthropology, three of them widespread: interpretive generalisations, structuralist explanations and functionalist explanations; and a rarer type of explanation a version of which I have been defending for some time: epidemiological models (see Sperber 1985b, 1990).

Interpretive generalisations

Many anthropologists seem to think that a – if not *the* – right way to arrive at theoretical hypotheses consists in taking the interpretation of some particular phenomenon in a given culture

and tentatively generalising it to all phenomena of the same type in all cultures. Thus, on the basis of European examples, the couvade was long considered as a symbolic – more precisely, hyperbolic – expression of the ties of paternity. Mary Douglas for instance suggests: 'The couvading husband is saying, "Look at me, having cramps and contractions even more than she! Doesn't this prove I am the father of her child?" It is a primitive proof of paternity' (Douglas 1975: 65). Claude Lévi-Strauss offers another generalised interpretation, inspired by American Indian examples:

> It would be a mistake to suppose that a man is taking the place of the woman in labor. The husband and wife sometimes have to take the same precautions because they are identified with the child who is subject to great dangers during the first weeks or months of its life. Sometimes, frequently for instance in South America, the husband has to take even greater precautions than his wife because, according to native theories of conception and gestation, it is particularly his person which is identified with that of the child. In neither event does the father play the part of the mother. He plays the part of the child. (Lévi-Strauss 1966: 195)

Patrick Menget, whose essay develops Lévi-Strauss's suggestion, concludes in a more abstract fashion (rendered even more abstract by out-of-context quotation): 'The power of the couvade lies in its articulation of a logic of the natural qualities of the human being and a problematic of succession, and in signifying by its progression and durability the irreversibility of human time' (Menget 1982: 208).

Such anthropological interpretations raise two issues. First, what exactly are these interpretations supposed to represent? Some would say: they represent the general meaning of the institution they interpret. Yet, any bearer of meaning, be it a text, a gesture or a ritual, does not bear meaning in itself, but only *for someone*. For whom, then, does the institution have its alleged meaning? Surely, it must be for the participating people, say for Opote and his fellows. There is every reason to suppose, however, that the participants take a view of their institution that is richer, more varied, and more linked to local considerations than a transcultural interpretation might ever hope to express. At best, therefore, these general interpretations are a kind of decontextualised condensation of very diverse local ideas: a gain in generality means a loss in faithfulness.

The second issue raised by these interpretive generalisations is the following: in what sense do they explain anything? How – and for whom – would the performance of an easy rite by the husband of every about-to-be or new mother serve as a 'proof of paternity?' How would the father's playing 'the part of the child' protect – or even seem to protect – the child from grave dangers? Who would willingly endure great deprivations for the sake of 'signifying the irreversibility of human time?' A meaning is not a cause; the attribution of a meaning is not a causal explanation. (Of course, there are cases where the attribution of a meaning to a behaviour fills a gap in an otherwise satisfactory causal explanation, but not so here.)

Interpretive generalisations do not explain anything and are not, properly speaking, theoretical hypotheses. Interpretive generalisations are patterns that can be selected, rejected and modified at will in order to construct interpretations of local phenomena. As such, and only as such, may they be useful.

Structuralist explanations

Structuralist explanations attempt to show that the extreme diversity of cultural representations results either from variations on a small number of underlying themes, or from various combinations of a finite repertory of elements, or from regular transformations of underlying simple structures.

All varieties of structural analysis start from interpretive generalisations, but then attempt to go beyond them. This rooting of structural analysis in interpretive generalisation is particularly manifest in the work of one of the founders of the genre, Georges Dumézil (e.g. Dumézil 1968). Dumézil tried to show that the myths and rituals of the Indo-Europeans are all variations on the same underlying pattern: an image of social life as constituted of three 'functions': sovereignty, war and production. This tri-functional pattern is, of course, an interpretive generalisation, but Dumézil exploited it in a properly structuralist way. He tried to show how this pattern gave rise to different structural developments, according to the type of cultural phenomena involved (pantheons, myths, epics, rituals, etc.), and according to the particular culture. He did not search for the explanation of this common pattern and varying structural development in interpretation but rather in history, building on the model of historical linguistics.

In Dumézil's style of structural analysis, just as in standard interpretive generalisations, the only relationships among representations held to be relevant are relationships of resemblance: two representations resembling one another can both be interpreted by means of a third representation which abstracts away from their differences. Lévi-Strauss (e.g. Lévi-Strauss 1963, 1973) has widened the field of structural analysis by considering that systematic differences are no less relevant than resemblances.[4] He maintained, for instance, that a myth may derive from another myth not just by imitating it, but also by systematically reversing some of its features: if, say, the hero of the first myth is a giant, the hero of the second myth might be a dwarf, if the one is a killer, the other might be a healer, and so on. Thus a network of correspondences richer than mere resemblance relationships may be discovered among representations: either among representations of the same type, myths for instance, or between different types of representations, myths and rituals, for instance.

Patrick Menget follows a Lévi-Straussian line when he attempts to relate the couvade and the prohibition of incest. The couvade, as he interprets it, expresses the progressive separation of the child's substance from that of its parents. Incest prohibition prevents a man and a woman descended from the same parents from re-fusing substances separated by means of the couvade:

> There is both a relationship of continuity between the couvade and the incest prohibition, since the latter keeps separated what the former had separated out of a common substance, and a functional complementarity, insofar as the couvade orders a communication within the social group which allow its diversification, and the incest prohibition establishes its external communication. (Menget 1982: 208)

Such a structural account does not by itself explain the couvade, but if one accepts it, it modifies the explanatory task. The *explanandum* is not anymore just the couvade; it is a complex of representations and practices having to do with the mechanism of biological reproduction (as understood by the

4. For a discussion of Dumézil's approach and a comparison with Lévi-Strauss's, see Smith and Sperber (1971).

Txikaos), a complex the coherence of which the anthropologist has attempted to establish, in spite of its superficial motley appearance.

Structural analysis raises two main problems, one methodological, the other theoretical. The methodological problem is as follows: in order to establish structural relationships among representations, the anthropologist interprets them. It is among the resulting interpretations, rather than among the observable or recordable data, that systematic resemblances and differences may be manifest. However, with a bit of interpretive ingeniousness, any two complex objects can be put into such a structural relationship. One could thus show that, say, *Hamlet* and *Little Red Riding Hood* are in a relationship of 'structural inversion':

Hamlet	*Little Red Riding Hood*
A male hero	A female hero
hostile to his mother	obeying her mother
meets a terrifying supra-human creature	meets a reassuring infra-human creature
who is in fact well-disposed	who is in fact ill-disposed
and who tells him not to waste time	and who tells her to take her time
etc.	etc.

Such pastiches do not, of course, invalidate structural analysis, but they illustrate its limits: the reliability of the analysis cannot be higher than that of the interpretations it employs. And the fact is that structuralists, just as all other anthropologists, practise interpretation essentially guided by their intuitions and without any explicit methodology. Moreover, the interpreter's intuitions are themselves guided by the aims of structural analysis, with an obvious risk of circularity and no obvious safeguards.

The theoretical problem raised by structural analysis is the following: in what way does structural analysis constitute an explanation of cultural phenomena? Some defenders of structuralism see in their approach a mere means of putting order in the data, that is, a means of classifying rather than of explaining. Dumézil combined structural analysis and historical explanation. Lévi-Strauss associates in a more intricate manner structural analysis with an essentially psychological kind of genetic explanation. The structures uncovered through structural analysis are assumed to be the product of a human mind inclined to flesh out abstract structures with concrete experience and to explore possible variations on these structures.

For instance: a given cultural group makes uses of representations of certain animal and vegetable species in order to display in the form of a myth some basic conceptual contrasts: say, between nature and culture, descent and affinity, life and death. A neighbouring group may then transform the myth, by reversing the value of the distinctive features of the characters, thus symbolising, over and above the contents of the myth, the group's difference from the neighbour from whom the myth was actually borrowed. Progressive transformations of the myth from one group to another may render it unrecognisable, but the systematic character of these transformations makes it possible for structural analysis to bring to the fore the underlying common structures, which, ultimately, are supposed to be the structures of the human mind.

However, Lévi-Strauss's references to the human mind do not provide an explanation; at best they suggest where he believes an explanation should be sought. Lévi-Strauss himself has hardly tied his investigations to those of contemporary psychology. The mental mechanisms deemed to generate cultural representations are postulated but not described.

More generally, the theoretical problem raised by structural analysis boils down to this: complex objects, such as cultural phenomena, display all kinds of properties. Most of these properties are epiphenomenal: they result from the fundamental properties of the phenomenon but are not among these fundamental properties. In particular, they play no causal role in the appearance and development of the phenomenon and are not, therefore, explanatory. A structural analysis brings to the

fore some systematic properties of phenomena, but, in itself, it gives no means of distinguishing fundamental properties from epiphenomenal ones. In a nutshell, structural analysis does not explain; at best, it helps to clarify what should be explained.

Functionalist explanations

Showing that a cultural phenomenon has beneficial effects for the social group is a favourite form of 'explanation' in anthropology. Functional analyses differ according to the type of beneficial effects (biological, psychological or sociological) they stress. In the Marxist improved version of functional analysis (see Bloch, 1983, for a review), contrary effects and dysfunctions are taken into account in order to throw light on the dynamics of society.

Functional analyses have been a great source of sociological insight. However, they all fall under two objections, one well-known and having to do with their explanatory power; the other less common and having to do with their use of interpretations.

Might a description of the effects of a cultural phenomenon provide an explanation of this phenomenon? Yes, but with two qualifications: first, the effects of a phenomenon can never explain its appearance; second, in order to explain how the effects of the phenomenon cause it to develop or at least persist, one must establish the existence of some feedback mechanism.

Let us suppose that a given cultural institution, for instance the couvade, has beneficial effects for the groups that have adopted it. For this to help explain the presence of some form of couvade in so many cultures, it should be shown that these beneficial effects significantly increase the chances of survival of the cultural groups that are, so to speak, 'carriers' of the institution. The onus of the proof would be, of course, on the defenders of such a functional explanation.

In practice, most functionalists are content to show, often with great ingeniousness, that the institutions they study have some beneficial effects. The existence of an explanatory feedback mechanism is hardly ever discussed, let alone established. Imagine, for instance, a functionalist, taking as her starting point an interpretation of the couvade similar to that proposed by Mary Douglas. She could easily enough argue that the couvade strengthens family ties, in particular the ones between the father

and his children, and therefore enhances social cohesion. But how would she go from there to an explanatory feedback mechanism? Moreover, it would not be too hard to establish that many institutions, including the couvade, have harmful effects: food deprivation, such as that suffered by Opote and his fellows, may in some cases be quite harmful.

Most cultural institutions do not have effects, on the chances of survival of the groups involved, of a character and magnitude such as to explain their own survival. In other words, for most institutions a description of their functional powers is not explanatory. Even where such a description does provide some explanatory insight, it does so in a very limited manner: the feedback mechanism neither explains the introduction of cultural forms through borrowing or invention nor the transformation of existing cultural forms.

Another weakness of the functionalist approach is that it fails to provide any specific principle for the identification of types of cultural phenomena. Rather, it relies uncritically for that task on an interpretive approach.[5]

What is it, for instance, that is supposed to make different local practices tokens of the same general type, say the 'couvade', a type which the anthropologist must then try to describe and explain? The identification of types is never itself based on function alone: for instance, no one would argue that all the sundry practices that have the 'function' of strengthening father-children ties should be seen as constituting a distinct and homogeneous anthropological type. The identification of types is not behavioural: some behaviour may count as couvade in one society and not in another. In fact, whatever its function, whatever its behavioural features, a practice is categorised as an instance of couvade in accordance with the native point of view. However, native points of views are local, and quite diverse even within the same culture. So, in the end, the identification of a cultural type is based on the synthetic anthropological interpretation of a motley of local interpretations.

Thus 'couvade' is defined by means of an interpretive generalisation: local practices that *can be interpreted as* ritual precautions to be taken by a prospective or new father are classified as cases of couvade. As I argued before, the price for

5. The weaknesses of functionalist typologies have been discussed by Leach (1961) and, more thoroughly, by Needham (1971, 1972). I have argued that these unprincipled and fuzzy typologies are based on interpretive rather than descriptive criteria; see Sperber (1985a, 1986).

such an interpretive usage is a heavy loss of faithfulness: the conception of a ritual, that of an appropriate precaution, what it means for a practice to be imposed on someone, who is considered a father, etc. varies from culture to culture. At the level of generality adopted by anthropologists in their 'theoretical' work, these local conceptions could be interpreted indefinitely in many ways. One interpretation is retained by the anthropological tradition; local variations and other interpretive possibilities are ignored.

Is the loss of faithfulness with respect to local representations compensated for by a gain in relevance? More specifically, are the types defined by means of such interpretive generalisations useful types for scientific work? I see no reason to believe that they are. Why should one expect all tokens of an interpretively defined type to fall under a common and specific functional explanation - or, for that matter, under any common and specific causal explanation? The point is not particular to the couvade; it holds for all cases of interpretively defined institutions, that is for all the types of institutions defined in anthropology: from a causal explanatory point of view, anthropological typologies, being based on interpretive considerations, are quite arbitrary.

Epidemiological models

We call 'cultural', I suggested, those representations that are widely and durably distributed in a social group. If so, then there is no boundary, no threshold between cultural representations and individual ones. Representations are more or less widely and durably distributed and hence more or less cultural. In such conditions, to explain the cultural character of some representations amounts to answering the following question: why are these representations more 'contagious' than others, more successful in a given human population? And in order to answer such a question, the distribution of all representations must be considered.

The causal explanation of cultural facts amounts, therefore, to a kind of *epidemiology of representations*. Comparing cultural transmission and contagion is hardly new. The comparison can be found, for instance, in the work of the French sociologist Gabriel Tarde, or in that of the diffusionists at the beginning of the twentieth century. It has recently been revived by biologists such as Cavalli-Sforza (Cavalli-Sforza and Feldman 1981) or

Dawkins (1976). The epidemiological metaphor has often been grounded in rather superficial resemblances as, say, between fast moving, short-lived epidemics on the one hand, and rumours or fashions on the other, or between slow moving, long-lasting endemics on the one hand, and traditions on the other. An application of epidemiological models to the transmission of cultural representations based on such superficial similarities both misses essential differences and deeper resemblances.

The transmission of infectious diseases is characterised by processes of duplication of the infectious agent. Mutations are relatively rare. In contrast, the transmission of representations is characterised by processes of transformation. In verbal communication, for instance, addressees construct their own, more or less faithful interpretation of the speaker's meaning and go on to correct or adjust the information received in light of their previous beliefs.[6] Duplication of thought through communication or imitation, if it ever occurs, is better seen as a limiting case of zero-degree transformation. This makes an epidemiology of representations, unlike that of infectious diseases, first and foremost a study of the transformation of representations in the process of transmission.

In spite of the differences between the transmission of diseases and that of representations, the epidemiological metaphor has other, appropriate and important implications. Epidemiology is not an independent science or the studying of an autonomous level of reality. Epidemiologists study the distribution of diseases which themselves are studied by pathologists. The distribution of diseases cannot be explained without taking into consideration the way in which they affect individual organisms, that is without having recourse to pathology, and, more generally, to the biology of individual organisms. Conversely, epidemiology is a major source of assumptions and evidence for pathology.

What pathology is to the epidemiology of diseases, cognitive psychology is to an epidemiology of representations. A causal explanation of cultural representations in the form of an epidemiological model should therefore stand in a relationship of partial overlap and of mutual relevance with cognitive

6. As discussed in detail in Sperber and Wilson (1986).

psychology. This is no reduction of the cultural to the psychological: cultural facts properly so-called, the facts anthropologists should try to explain, are not individual representations, but distributions of representations. A distribution of psychological phenomena is itself not a psychological but an ecological phenomenon.

From an epidemiological perspective, a cultural phenomenon such as Little Red Riding Hood is not an abstract tale hovering about in the abstract context of European culture; it is a causal chaining of public narratives having given rise to the construction of mental stories, themselves having given rise to further public narratives, and this millions of times. To explain Little Red Riding Hood as a cultural phenomenon is to identify the factor that made possible this chaining of communications and the resilience of the communicated contents. One of the factors involved in this case (as in the case of all spontaneously transmitted oral narratives) is the fact, experimentally ascertainable, that this story is very easily memorised.

In this perspective, the couvade among the Txikao is not an immaterial institution; it is a causal chain of individual thoughts and behaviour. To explain the phenomenon is not to assign it some abstract meaning, but, again, to identify the mechanisms and factors maintaining this causal chain. No doubt, some of these factors are psychological, such as the Txikaos' views on life and its transmission discussed by Menget; other factors are ecological and include perinatal morbidity and mortality which, at every birth, reactualise the means of avoiding these risks that the Txikaos believe they have.

The epidemiological approach renders manageable the methodological problem raised by the fact that our access to the content of representations is unavoidably interpretive. In this approach, the methodological problem of ethnography is not to devise some special hermeneutics giving us access to representations belonging to a culture, yet uninstantiated in the individual heads or the physical environment of its members. The methodological problem is merely to render more reliable our ordinary ability to understand what people like you, Opote or me say and think. This is so because, in an epidemiological explanation, the explanatory mechanisms are individual mental mechanisms and inter-individual mechanisms of communication; the representations to be taken into account are

those which are constructed and transformed at this low level by these micro-mechanisms. In other words, the relevant representations are at the same concrete level as those that daily social intercourse causes us to interpret.

Another methodological advantage of the epidemiological approach is that it provides a principled way to identify the types of cultural things for which a more general explanation is to be sought. The proper objects for anthropological theorising are types of causal chains of the kind I have described. These types of causal chains are to be individuated in terms of features that play a causal role in their emergence and maintenance. These features may be ecological or psychological: for instance, the lability of oral texts as opposed to the stability of written ones is a key ecological factor in explaining their respective distributions; the high memorability of narratives as opposed to the low memorability of descriptions is a key psychological factor. The two factors just mentioned interact in an obvious way and justify considering oral narratives as a proper anthropological type.

The psychological features pertinent to determining types of cultural things may well include content features. Of course, content features can be characterised only interpretively. To say that various representations share a content feature amounts to saying that they can all be interpreted, at a given level and from a given point of view, by means of a common interpretation. Still, that property of common interpretability, with all its vagueness, may suffice, if not to describe, then at least to pick out a class of phenomena all affected by some identical causal factors. For instance, the very notion of a genealogy, as a type of cultural representation, is interpretively defined and, as a result, quite vague: for what counts as a genealogical relationship in one society may not do so in another, and even the very idea of a genealogical relationship has many very different versions. Still, it is quite plausible that genealogies, in all their versions, are locally relevant, and hence culturally successful, for partly universal reasons.

In an epidemiological perspective, I suggest, the explanation of a cultural fact, that is, of a distribution of representations, is to be sought not in a global macro-mechanism, but in the combined effect of countless micro-mechanisms. What are the factors that lead an individual to express a mental representation in the form

of a public representation? What mental representations are the addressees of the public representation likely to construct? What transformation of content is this process likely to bring about? What factors and what conditions render probable the repeated communication of some representations? What properties, either general or contextual, does a representation need in order to maintain a relatively stable content in spite of such repeated communications?

These and other questions raised by an epidemiological approach are difficult, but at least anthropologists share many of them with cognitive psychologists; a relationship of mutual relevance between the two disciplines may emerge and help. In order to answer these questions, as with all anthropological questions, interpretations must be used as evidence. But at least, the interpretations required in this approach are of a kind with those we use all the time in our daily interactions. This does not make these interpretations unproblematic, but we should recognise their value as evidence – actually we already do recognise the evidential value of such interpretations in matters much dearer to us than mere scientific theorising.