CURRENT ANTHROPOLOGY Volume 31, Number 3, June 1990

Is Anthropology Art or Science?[1]

by Michael Carrithers

Anthropological knowledge has been thought to lack the absolute certainty attributed, wrongly, to natural-scientific knowledge. One consequence of this comparison has been a view of ethnography as unreliable and ethnographers as writers of fiction. But, in the first place, the standard of natural-scientific knowledge against which ethnography is compared is wrongly conceived. The measure of natural-scientific knowledge is not absolute certainty but its usefulness within specific human practices. Second, just as natural-scientific knowledge is founded on intersubjective pattern recognition, so is ethnographic knowledge, though in the latter case the patterns are patterns of human action and interaction. All humans are capable, for example, of grasping a closely knit series of interactions in a narrative sequence. The anthropological knowledge erected on such evidence may therefore be regarded not as absolute but as reliable within recognizable limits. The ultimate standard against which ethnography must be judged is the practical knowledge of persons acting in a social setting.

MICHAEL CARRITHERS is Reader in the Department of Anthropology of the University of Durham (Durham DH1 3HN, England). Born in 1945, he was educated at Wesleyan University (B.A., 1967; M.A., 1971) and at Oxford University (D.Phil., 1978). He has lectured at the London School of Economics and at Oxford and has done fieldwork in Sri Lanka and India. His research interests are human sociality, narrative and figurative thought in action, and South Asia. Among his publications are *The Forest Monks of Sri Lanka: An Anthropological and Historical Study* (Delhi: Oxford University Press, 1983), *The Buddha* (Oxford: Oxford University Press, 1983), "An Alternative Social History of the Self," in *The Category of the Person*, edited by M. Carrithers, S. Collins, and S. Lukes (Cambridge: Cambridge University Press, 1985), and "Why Humans Have Cultures" (*Man*, in press). The present paper was submitted in final form 6 XI 89.

Though the question may seem dated, it is still—to use Taylor's (1985) imagery—part of the great conversation of our civilization: Can knowledge of the human world be erected upon the sort of apparently firm foundation that scientific knowledge of the natural world enjoys?

Just now in our sociocultural anthropologists' corner the discussion has grown lively, and for the moment the answer seems to be a thoroughly Romantic one: whatever anthropology is, it is not a science, and the knowledge anthropologists create is in no sense scientific. Anthropological knowledge is interpretive and hermeneutic rather than positive, tentative rather than conclusive, relative to time, place, and author rather than universal.

This answer has an august pedigree in the phenomenological and Romantic tradition and has been mediated to the social sciences in general by such writers as Dilthey, Weber, Schutz, Ricoeur, and Gadamer. The advantage of this view is that we can conceive our knowledge of other humans to be especially rich because we, like our subjects, are humans "suspended in webs of significance" rather than unthinking minerals. But, on the other hand, if all we know is others' attitudes and beliefs, and if all we can use to understand their attitudes and beliefs is our attitudes and beliefs, then such knowledge may be as insubstantial as it is rich.

Two recent writers, Geertz (1988) and Clifford (1988), have embraced this possibility and pursued it much farther. They conceive that anthropologists are first and foremost writers, and "writers" they understand on the model of writers of fiction. What anthropologists do is create for themselves writerly personae with more or less authority, and that authority derives from the text itself and its style of presentation. Anthropologists even produce new genres (Clifford). And in consequence the reliability of the knowledge anthropologists pretend to make is of far less interest than the inventiveness (Clifford) or the persuasiveness (Geertz) of their texts.

This line has encountered serious and, as far as I am concerned here, decisive opposition (Spencer 1989; Roth 1989; Carrithers 1988, n.d. *b*). Nevertheless, Geertz and Clifford reveal where the problem lies. Geertz (1988:10) remarks on "the oddity of constructing texts ostensibly scientific out of experiences broadly biographical." For North Atlantic societies scientific knowledge is the very type of knowledge and by definition impersonal, yet anthropologists' knowledge is based ultimately on personal experience. How is this possible? Clifford refers to the "dialogic" nature of anthropological knowledge—its essentially interpersonal and intersubjective character. Once again: how can knowledge, which we represent to ourselves as being impersonal and objective, be founded on matter so subjective and mutable as interpersonal relations? The difficulty is epistemological—what is the character of anthropological evidence if it is not scientific?—but it is also social and political. How are we to represent anthropology as a serious activity to ourselves and to those with whom we are engaged if it is so nebulous?

1. I thank the members of the Department of Social Anthropology in Edinburgh, who invited me to write this as a Munroe Lecture, and David Riches and Bob Layton, who encouraged me to publish it.

The Problem

I suggest that a different and more credible answer can be given to these questions. First, we must look at what anthropologists present as evidence. Second, we must look closely at the benchmark of scientific knowledge against which ethnographic knowledge has been so often measured.

One writer who has already made some headway in this enterprise is Sperber (1985), and I will begin with the same sample of anthropological evidence as he does, drawn from Evans-Pritchard's *Nuer Religion* (1956:222):

> I was present when a Nuer was defending himself against silent disapproval on the part of his family and kinsmen of his frequent sacrifices. He had been given to understand that it was felt that he was destroying the herd from inordinate love of meat. He said that this was not true. . . . It was all very well for his family to say that he had destroyed the herd, but he had killed the cattle for their sakes. It was "*kokene yiekien ke yang,*" "the ransom of their lives with cattle." He repeated this phrase many times as one by one he recounted cases of serious sickness in his family and described the ox he had sacrificed on each occasion to placate the spirit *deng.*

Sperber argues (pp. 14–15) that

> this is about as raw a factual account as you will ever find in most ethnographic works. Yet not a single statement in it expresses a plain observation. "Silent disapproval" cannot be observed but only surmised. Similarly, that a man "had been given to understand that it was felt that . . ." is an inference from a variety of often ambivalent and complex behaviors. These inferences are likely to have been made not directly by the ethnographer, but by his informants. The resulting description is actually what the ethnographer selected from what he understood of what his informants told him of what they understood.

It is difficult to do justice in a short space to Sperber's subtle argument, but his basic orientation to science and to anthropological evidence is as follows: We can realistically hope for a "factual account," a "plain observation," or a "description" from anthropology, but not from an anthropology that conceives itself as based in ethnography. The real anthropology would be rather like cognitive psychology, and for Sperber cognitive psychology falls unambiguously into the category of "science." Ethnography, in contrast, is an interpretive discipline that aims at understanding (read *Verstehen*), while anthropology would aim at scientific explanation (read *Erklären*). The interpretations of ethnography could become the scientific material for anthropology only if accompanied by "an appropriate descriptive comment that clarifies their empirical import" (1985:32). In the present practice of ethnography, however, ethnographic evidence as presented is not factual, is not plain observation, is not description. It is interpretation, and its empirical import is undetermined: we cannot tell unambiguously what the object of what statements is or who their author might be. Nor could the bulk of ethnography ever aspire to truly scientific status.

Thus, though his ambitions could hardly be farther from those of Geertz and Clifford, Sperber shares with them both the assumption that anthropology must be located with respect to science and a particular understanding of science, of the interpretive nature of ethnography, and of the polar opposition between them. Moreover, he shows in clear and therefore disputable detail just how he would handle an actual piece of ethnographic evidence. I will argue that the notion of scientific knowledge underlying these remarks is erroneous, that the implied opposition between "plain observation" and Evans-Pritchard's interpretation is a false one, that we can therefore take him to have "observed" something like "silent disapproval," that the "inference" could have been made directly by the ethnographer, and that such evidently interpretive statements can easily be given useful empirical import.

The Bugbear, Science

One part of the problem lies in the received and abbreviated version of science that so deeply influences many social-scientific writers. I want first to suggest that a more realistic grasp of science as a human practice would inoculate anthropologists against the need to caricature our own activity by way of contrast. For a view of scientific practice I rely chiefly on what might be called the "modified sociological realism" of Ziman (1978), Hacking (1982, 1983), Taylor (1982), and Harre (1986). Much of this view can be traced to Polanyi (1958).

The central intuition of these writers is that science is a human activity and as such is not so alienated from the world of human practice as to produce an absolute truth, absolute facts, or an absolute confidence in itself. Their theory of truth is not one of correspondence—facts simply match the way the world is—but rather a pragmatic one that considers the measure of truth to be in its use. It is in fact a false dichotomy between knowledge and activity that has created the spectre of unconditional and disembodied knowledge. As Hacking (1983:131) puts it, "The harm comes from a single-minded obsession with representation and thinking and theory, at the expense of intervention and action and experiment."

Indeed, Hacking regards activities such as calculating, modelling, structuring, theorizing, speculating, and approximating as only part of what scientists do. They also measure, scrutinize, notice, manipulate, mix, build, calibrate, make machines work (and, I may add, consult, argue, lecture, publish, and do many other constitutively social things as well). Scientists do of course make representations—for example, a table, a graph, a diagram, a set of equations, a verbal description, a model—but we are to think of these not as being true but only as being more or less useful. "When there is a final truth of the matter, then what we say is brief, and it is either true or false. It is not a matter of representation. When, as in physics, we provide representations of the world, there is no final truth of the matter" (p. 145). This contrasts with our usual view of science, which Hacking characterizes thus: "When science became the orthodoxy of the modern world we were able, for a while, to have the fantasy that there is one truth at which we aim. That is [what we took to be] the correct representation of the world" (p. 144).

A necessary part of modified sociological realism is that there be different representations of some subject, representations that may compete but that may also be just alternatives, each offering some advantages in manipulating the matter at hand. It is, moreover, a conception of science that is comfortable with a broader and historically informed view of scientific change and mutability. For my purposes the effect of Hacking's argument is to remove the sense of metaphysical absoluteness that we unthinkingly attach to science through the attribution of "truth" to scientific judgments. And that is the sense in which Hacking's view is "modified," namely, that he cedes usefulness and effectiveness to scientific representations without making them the touchstone of truth.

Finally, modified realism is sociological in that it recognizes that the sorts of knowledge thus produced are produced by people configured in relation to each other and flowing within a much larger stream of human events. As Ziman (1978:125–26) puts it, "the cognitive contents of science depend for their form and integrity on the manner by which this social institution shapes and governs its members." Science has a social as well as an intellectual history, for new notions of evidence and argumentation may arise, old ones may perish, and the explanation for such events cannot be limited to the impersonal success of their results. No knowledge is knowledge *simpliciter*, but rather all knowledge is relative to a community of knowers. We need not think of science as transcending the human world; rather, it is embedded within the human world as one of the sorts of things that we do—or have done, for a little while, in some places.

Now, if this general view of science is accepted, then I think its implications for the writings of Geertz, Clifford, and Sperber and for the absolute realist view that they implicitly espouse are very important. On their absolutist view scientific evidence and argument transcend the sociality and historicity of our merely human world, and measured against that standard ethnography cannot but seem insecure and trifling. Yet we see that scientific practices do not transcend our human world: they are human activities as well, part of human history and part of what humans do to, and with, each other as well as to the natural world. In that perspective science is more provincial, less universal, and less powerful than we might have thought. Thus one pole in the opposition between interpretive and scientific knowledge is removed, and we are liberated from the compulsion to compare anthropological knowledge with an impossibly rigorous standard. Only such a compulsion could have called forth the otherwise unaccountable reaction of treating ethnography as fiction.

The liberation can be carried farther as well. As Hacking (1982), Taylor (1982), Ziman (1978), and Roth (1987) argue, this modified realism also entails that there are distinct modes of reasoning and different forms of evidence appropriate to different disciplines, to different kinds of representations, interventions, and manipulations. Such differences support the rejection of a unitary scientific touchstone of truth, but some particular rigour, some particular canon of evidence is still appropriate to each discipline. Indeed, we can broaden our perspective, for on this account there is no reason to dwell solely upon the natural sciences. Even in the social sciences we may still be concerned with "how properly to warrant claims from within a chosen perspective" (Roth 1989*a*:561). I am concerned with how claims are warranted in ethnography.

Intersubjective Pattern Recognition

I want now to introduce some notions used by Ziman (1978) to characterize many forms of natural science as reliable knowledge. I recognize that there is a danger in this, since when I then apply the ideas to ethnography I might be thought to be asserting that ethnography is like, say, botany, full stop. But what I mean to say is this: there is a general design in the practices developed by North Atlantic societies for the collective creation of knowledge, and there are shared human capacities underlying that creation, and it so happens that the design and one set of capacities have so far been best understood in regard to the natural sciences. (If, of course, some of the confidence that attaches to the word "science" were to rub off on ethnography through rhetorical suggestion, then that might in the present atmosphere be no bad thing.)

Ziman's understanding of scientific knowledge comprises three elements: a community of knowers, that which is perceptually consensible to them, and that on which they reach consensus. For the present I will be concerned with two elements, the community and consensibility.

The community is logically constituted as all those who can in principle perceive and report the same natural phenomena, such as a change in the colour of litmus paper. In this sense all observers are interchangeable, and, as Ziman stresses, interchangeability or equivalence of observers is "the foundation stone of all science" (p. 43). To discern the force of this dictum we have only to ask ourselves how science would differ if only members of the Church of England could observe a change in litmus paper or only registered Democrats could detect neutrinos or only Bantu-speakers could measure crystal growth. Entry to the community of scientific observers is in principle universal, and even though in practice access is limited by many contingencies, this "in-principle" universality guides the self-understanding and routine procedures of science.

The universality of scientific observation is prominent in our received understanding of science; its collective character is less so, but of course the principle of interchangeability would mean little if the observations so made were idiosyncratic or hermetically private. In that sense the whole edifice of science rests upon perceptual consensibility, the ability of people to perceive things in common, to agree upon and to share perceptions. Moreover, Ziman continues, the "very possibility of perceptual consensibility depends upon a very ordinary faculty, shared by all human beings and by many animals. Without conscious effort, we all have remarkable skill at *recognizing patterns.*" This "intersubjective pattern recognition," he says, "strikes deeper at the roots of 'logicality' in science than the positivists seem to realize" (pp. 43–44).

To illustrate his point Ziman presents the following, which he calls a "message," designed to convey the results of a visual inspection to other scientists (p. 44):

> Deciduous shrub, glabrous or nearly so, with weak, trailing sub-glaucous, often purple-tinted stems, either decumbent or forming low bushes 50–100 cm high, or climbing over other shrubs, rarely more erect and reaching 2 m. Prickles hooked, all more or less equal. L'flets 2–3 pairs, 1–3.5 cm, ovate or ovate-elliptic, simply, rarely doubly serrate. . . .

"What is this strange plant?" he asks. Nothing other than a species of rose, the common field rose of Britain. "It does indeed have the characteristics listed above; in the picture [a line drawing of a rose], however, we perceive a pattern which the botanist learns to distinguish like the face of a friend" (pp. 44–45).

In Ziman's account, the picture and its message—what Sperber would call its descriptive comment—are not simply a verbal and a pictorial representation of the same thing, and they are certainly not two versions of a single propositional truth. Rather, the pattern is just the pattern, which is not in that sense propositional at all. On the other hand, the description is used to "refer to other remembered visual patterns. How would one define the adjective 'serrate,' except to say that it was 'like a saw'?" (p. 45) The message helps to place the image in an "archive" of images.

The message performs other functions as well: it may, for example, place in the archive other information about dates or locations or time of day or persons present or other identifying tags. Indeed, the whole archive consists in a lacework of images with their messages: some of the archive might be propositional, but to think of the lacework or any of its individual constituents merely as bearing truth values can hardly do justice to the complexity of its construction and use. This is the sense in which a logical-positivist view of science is plainly inadequate.

Moreover, the messages are intersubjective in that they work to create the consensibility, the shared perception, that allows the image to be used as evidence within a collectivity—perhaps better, a lacework—of persons. Thereafter, even though consensibility is the basis of the enterprise, the conversion of consensible evidence into consensual bodies of reliable knowledge still depends upon a complex and by no means infallible social process.

Finally, the ability to perceive the pattern and the ability to produce the pattern are not the same thing: one may not be able to draw the rose effectively even though one is able to recognize it. Experience intervenes as well at the other end of the process, for one can perceive the finally elicited pattern, read the message, and still not be able to do much with it. A consensible pattern is only one, though an essential, part of the laborious weaving of scientific knowledge.

Ziman's is an intricate argument, but I want to take from it just one question. Is there anything in ethnographic practice that corresponds to intersubjective pattern recognition?

Human Patterns

In making a point very similar to the one I wish to make here, Raymond Firth presents what can be regarded as just such pattern recognition. During his fieldwork among Tikopia he received word that his friend, Pa Rangifuri, son of the local chief, was *teke,* which means "unwilling (to do something)" or "angry" or "objecting (even violently)" (1985:39):

> When we got to his house we found him highly agitated. He and I greeted each other with the usual pressing of noses, as publicly recognized friends, but for him this was an unusually perfunctory gesture, and he paid me little attention. He was uttering brief incoherent statements: "I'm going off to see" . . . "They said their axe should cut first" . . . "But was it for a dirge, no! It was for a dance!" Men were trying to soothe him down by respectful gestures, and to enquire the reason for his agitation. Tears were streaming down his cheeks, his voice was high and broken, his body quivering from time to time.

My experience with colleagues and students has been that they grasp this passage immediately, with no effort. The "message" that Firth offers includes the phrase "highly agitated." One effect of this part of the message is to remind us of other occasions on which we have met such a pattern, through personal experience or through representations of emotion in conversation, literature, film, or even ethnography. Pa Rangifuri's tears, the incoherence of his words, and his general demeanour are distinct, vivid, and discriminable from other patterns such as, say, "riotous jollity." Moreover, the pattern is not merely visual or auditory. Other, constitutively social components—the contrast between Pa Rangifuri's ordinary greeting and this "perfunctory" one, the soothing gestures and enquiries of the other men—also contribute to the passage's consensibility.

An anthropologist might of course ask first for what is culturally specific in such an event. It is likely, for example, that some of Pa Rangifuri's display of emotion was specific to the style and emotional registers of the Tikopia. The high, broken voice and the tears do not, for example, sound very British. It is also likely—and Firth later makes this clear—that the occasion for the emotion was strongly determined by local conceptions of rights and obligations and by the particular circumstances of Pa Rangifuri's life in relation to others, and, indeed, something of this dimension is already inherent in the actions and judgments of those surrounding him. Perhaps the events accompanying Pa Rangifuri's distress followed the cultural grain of Tikopia life in what Schieffelin (1976) has called a "cultural scenario." And it should also be stressed that some literary skill has gone into presenting the pattern.

But nevertheless the pattern in itself is "intelligible"—the word used by Firth—and Anglophone readers do not need the whole social and cultural setting to get the basic idea. Just as Ziman did not require a theory of perception to make his point that visual patterns are consensible, so Pa Rangifuri's demeanour is consensible without our having to embrace any particular view of how this comes about. We need not subscribe, for example, to a theory of "hard-wired" perception of emotion to realize that Pa Rangifuri is upset. We need not know the details of Tikopia folk psychology or of Pa Rangifuri's place in social relations to grasp the fundamentals of the pattern. Indeed, there is something peculiarly pure about our apprehension of Pa Rangifuri's state: along with the other Tikopia then present, we are mystified about the causes of his condition. Yet with them we can perceive that *something* has happened and very roughly identify the character of that something. For us now it would be a matter of leisurely curiosity, though for the Tikopia then it was a matter of pressing urgency, to connect this consensibly recognized pattern with some larger explanation.

I have begun with this truncated example of Firth and Pa Rangifuri not because it is absolutely simple—it is not—but because it is simple relative to the sorts of consensible patterns that ethnographers usually use. Often the reader is asked to compass at a glance a pattern comprehending several individuals at once. Here is a passage from Lewis (1980:50) explaining how the Gnau of Papua New Guinea pass on their ritual knowledge:

> When I questioned people about how they had learned or failed to learn about something, for example, a myth, or genealogies, or the meaning of some ritual action, they sometimes mentioned individuals who told them . . . or they said it was the sort of thing men used to talk about in the evening in the [men's house] when they were lying on their beds before going off to sleep, or on rainy days when they hung around by the fireside. In similar circumstances, although rarely, I have heard men by some chance get round to a myth and tell it, or go into some explanatory point about the meaning of a rite.

The point of Lewis's exposition, in other words, is that the Gnau do not systematically and purposefully go about passing on such knowledge and have no institutions devoted to that end. The evidence for this argument consists in the unplanned and purposeless occasions on which the Gnau do pass on such knowledge. These occasions form a consensible pattern.

The relevant part of the message that goes with the pattern might be something like "sociable purposelessness" or, better, "hanging around." It is true that a rainy afternoon in, say, a college dormitory in Connecticut is in many ways very different from a "similar occasion" in a men's house up the Sepik, but the sense of similarity that gathers these and the other nameless occasions Lewis mentions into one set would be difficult to miss. I suppose that part of the consensibility lies in the contrast with that "social activity" or "doing something together" that is so characteristic of us as a species. It even seems likely that "having a purpose," and therefore its opposite, are fundamental not only to the human but perhaps also to other species. But however that may be, the image that bears Lewis's argument is socially complex, comprehending a number of individuals taken together, and yet immediately graspable.

Some of the most compelling, and yet complex, consensible patterns used by ethnographers are those that add a further level of complexity, namely, temporal change and movement, to a situation shared by a number of people. In the following passage Lienhardt (1961:233) describes such a movement in order to reveal what counts for the Dinka as the most significant part of a sacrifice. He begins by pointing out that during a sacrifice the Dinka invoke divinities over and over again:

> This rhythmical repetition of particular sets of words and ideas, spoken first singly then in unison, gradually has an effect which may be observed by anyone attending a sacrifice and, moreover, comes to be felt by the foreign observer himself. At the beginning of such a ceremony there is usually a lot of chatter and disorder. People come and go, greet each other. . . . It is common for those officiating to try to call people to some order. . . .
>
> As the invocations increase in tempo, however, the little bursts of incisive speech by the invoker and his chorus draw the congregation more and more towards the central action. . . . As the invocations proceed, the repeaters of the invocations work together more smoothly in rhythmical speech, and a collective concentration upon the main theme and purpose of the gathering becomes apparent.
>
> This concentration of attention on a single action ends when the sacrificial victim is thrown and killed. . . .

The assertion towards which Lienhardt leads us is that the killing of the victim is the central act of sacrifice and is so considered by the Dinka. The evidence he adduces for this consists in a pattern which can be grasped hardly less immediately than Lewis's "hanging around," though it is a pattern that develops over a considerable time. Part of the accompanying message might be "collective concentration and release," which suggests a family resemblance to many other such occasions throughout the world. Lienhardt conveys a slightly less abstract message with more art by using such words as "congregation," evocative of assemblies with a quite specific cultural provenance. Other comparisons could usefully be made, and indeed one such comparison appears in the chief identifying tag, "sacrifice." Yet there is no reason to believe either that these messages or any other, either singly or together, should exhaust the possibilities for comparison. The pattern itself is consensible apart from any particular messages that might be associated with it.

The Dinka sacrifice raises a number of issues that were not so easily distinguishable in the earlier examples. In the first place, as Robert Layton has recently reminded me and as I can richly attest, fieldwork usually begins for the ethnographer in a welter of confusion and incomprehension. Even the most elementary matters, such as when a ceremony begins or even if it is going on, are far from obvious. It therefore seems quite conceivable that Lienhardt would not have been able at first to appreciate fully the pattern in what he was watching. The idea of consensibility does not, however, require that patterns be immediately and easily elicited. The only requisite is that once patterns are elicited they be intersubjectively intelligible.

It might also be suggested that Lienhardt's very artfulness militates against reliable consensibility. But once again the notion of consensibility does not preclude care and workmanship in the representation of patterns. A line drawing of a field rose is artfully made, but such craftsmanship, far from being superfluous or deceitful, is an absolute requirement for the archive of botany.

Moreover, from this it follows that intersubjective patterns need not be conceived as having one correct, canonical form. That there may be other and very different ways of representing some matter—a rose, a sacrifice—does not invalidate the consensibility or the evidential character of a pattern as represented. A new dimension of the matter at hand can be explored by devising a new representation without rendering a former representation erroneous. Indeed, for the same reason the message accompanying the pattern need not be regarded as single, simple, or closed: as we learn more about Dinka or about other ways of life we may want to draw out other entailments of the Dinka sacrifice by using new messages.

Finally, there is no reason to believe that the consensibility of patterns exhibited by ethnographers comprises a lexicon of patterns. Here once again, the analogy with natural-scientific pattern recognition is useful: we do not suppose that we would fail to learn and recognize the forms of even an indefinitely large spectrum of rose species, even if each were only slightly different from the field rose. Whatever it is that allows us to see patterns, it is not a foreordained dictionary of images to which the world conforms. And so, analogously, we need not assume that there is only a limited or specifiable number of patterns to be found in human life (see Hofstadter 1986).

Narrative Patterns

Indeed, at their most complex the patterns displayed as evidence by anthropologists are ones which, while being consensible and intelligible, are nevertheless unique and irreproducible. These are narrative patterns, the unfolding of events even more intricate and elaborate than the conventional Dinka sacrifice as sketched by Lienhardt.

Firth's account of Pa Rangifuri develops into just such a narrative elaboration. Firth was shown by the others how to take Pa Rangifuri by the wrist with the appropriate decorum and lead him back to his father to apologize. He did so, and the story goes on from there (1985:40)

> The background to his [Pa Rangifuri's] outburst then became clear to us [all those present before the chief]. My friend's son had been lost at sea some months before (as I knew) and he had wanted to make preparations for a celebratory mortuary rite. . . . But when he had gone to ask his father for an axe to begin to cut down trees to make barkcloth for the graveclothes the old chief had temporized, and he had thought his father was refusing him, so threw himself out of the house. (As it emerged later, in private, he had put this down to manipulations by his brothers whom he had suspected of wanting a dance festival to precede the mourning ritual, so making their drain on family resources take priority.) His father now explained that he had not refused the request for the axe, that he had had something else on his mind, and that if his son had only waited, permission to go ahead with the funeral preparations would have been given to him. After this, the axe was handed over, and the way to the funeral rites was now open.

Let me first separate the workmanship of representation in this passage from the pattern itself. There are some terms—"celebratory mortuary rite," "barkcloth," "graveclothes," "mourning ritual," "dance festival," "funeral rites"—that must be supposed to indicate Tikopia words, practices, and articles that are not, however, further specified. The craft lies partly in the elegant variation so prized in English expository prose, partly in what is suggested in English by "funeral," "mortuary," and "mourning," but also in a suggested contrast between a "dance festival" and "funeral rites." By this contrast Firth hints that the strong opposition between such activities conveyed in the English words is likewise felt among the Tikopia and was important to Pa Rangifuri. So in that sense we need not know the actual contents of a "dance festival" or "funeral rites," for their significance is provided by the flow of events. An analogous argument could be made for "chief." In the course of the passage we learn that the son "requested" the axe of him, that he had not "refused," and that his "permission" was required. From all this we understand, even if we know very little about Tikopia chiefs, their relevant characteristic in this series of actions and reactions—the ability to give or withhold permission.

It might be asked, what warrants our confidence that these elliptical suggestions are elsewhere elaborated?

Just this: in ethnographies the practice is to connect any one piece of evidence with many others in such a way that the language of representation gains clarity and specificity over a whole ethnography. In the cases of Lewis and Lienhardt above, for example, the evidence presented and the language in which it is couched depend not only on the cited example but on a weaving together of many cognate patterns with their messages. The same is true for Firth: in his writing on Tikopia as a whole there are plentiful illustrations of the detailed specificity of a Tikopia "chief," a Tikopia "dance festival," a Tikopia "funeral." (For the incident of Pa Rangifuri, see esp. Firth 1956:60–74.) Whereas Geertz has argued that we believe in the ethnography because we believe that the ethnographer was there, in fact we believe that the ethnographer was there because of the dense and interwoven specificity of the ethnography (Carrithers 1988).

Still, it is perhaps not immediately clear wherein the pattern consists. The passage is so compact and allusive, full of changes of tense and viewpoint and of reactions by one person to another's attitude, that we might be tempted to call it unspecifiable. But I think we can do better than that.

We can begin by unpacking the passage chronologically. First, the immediate events of the quarrel and reconciliation are set in a larger flow of events, having a scale of months rather than minutes or hours. That flow consists in Pa Rangifuri's loss of his son and consequent grief. Through this we understand something of the more enduring predicament, and therefore disposition, of Pa Rangifuri, and that disposition in turn renders the action in the foreground more easily comprehensible.

Moreover, there is a larger setting yet, one measured in decades and generations, in which the old chief took office, had sons who rivalled each other, and so forth, in the characteristic ways of Tikopia at that time. This larger frame is usually presented by anthropologists in the form of norms or the schemes of social organization, but to do so they almost always rely on material with a great deal of narrative content, such as legends or myths, to connect a narrated past with occurrences of the recent past and present. Even evidence of this scale can assume a narrative pattern.

In the foreground, on the shortest time scale, there is the immediate flow of events in which actions and reactions are closely linked. Pa Rangifuri asked his father for an axe, his father temporized, he reacted badly because of grief and because he suspected his brothers' opposition, his friends and relations calmed him and arranged for an apology, the apology was made, and he was eventually given the axe. This is the pattern, created by actions and immediate reactions, each one leading to the next, flowing together with compelling emotional logic.

Narrativity

The chief requirement I have so far imposed upon such human patterns is that they be intersubjectively recognizable, and I think this passage meets that criterion as well as the earlier ones. Yet it is just too complex for its comprehension to be taken for granted. Visual intersubjective pattern recognition is founded upon a capacity common to all humans, even though the capacity must be activated or supported or formed by training and experience. Is there an analogous capacity for comprehending a flow of human events?

From the answer to this question will arise the answer to a second, equally pressing one: What do we understand when we grasp a pattern such as this? Is it simply a true and accurate record of events, or something else?

I do indeed think that there is a general human capacity for comprehending a flow of events. It could be called the "narrative mode of understanding" (Bruner 1986; see also Ricoeur 1983) or narrativity (Carrithers n.d. *a*, 1989, n.d. *b*). The basic argument is as follows: Humans understand *characters*, which embody the understanding of rights, obligations, expectations, propensities, and intentions in oneself and many different others, and *plots*, which show the consequences and evaluations of a multifarious flow of actions among characters. Narrativity, that is, consists not merely of telling stories but of understanding complex nets of ever-new deeds and changing attitudes. Another way to put this would be to say that human beings perceive any current action within a large temporal envelope, and within that envelope they perceive any given action not only as a response to the immediate circumstances or current imputed mental state of an interlocutor or of oneself but also as part of an unfolding story. (I owe this latter formulation to Paul Harris.) This capacity is most richly attested in human speech and storytelling, but it is not reducible to language or to narrowly linguistic abilities.

I think it essential that character in narrativity be conceived very broadly, since it must comprehend both individuals as having statuses and roles—that is, as standing in a prescribed relation to one another—and individuals as having idiosyncratic histories, propensities, and relations. On the one hand, there must be some room for abstraction, so that people can be understood as acting generally, on a first approximation, with a specific set of obligations and rights, as, for example, a chief or a father acts with obligations and rights toward subordinates or sons. On the other hand, the particularity of one person rather than another, of Pa Rangifuri rather than his father, must also be grasped. We must understand not just the type of the Tikopia chief, for example, but also his individual propensities: mellowness or irascibility, generosity or stinginess, and so forth. Whether or not the Western notion of an individuated personality really grew out of a much earlier sense of people as personae or types as Mauss suggested (see Carrithers, Collins, and Lukes 1985), narrativity must comprehend both of those possibilities and many others as well.

But characters with their relationships are also set in a flow of events, a plot, with its sense of plans, situations, acts, and consequences. Plots embody what a character or characters did to or about or with some other character or characters, for what reasons, and what followed from that. Indeed, characters can no more be understood in isolation from the plots in which they are enmeshed than plots can exist without the characters who populate them. And in particular the characters and plots can ripen over a lifetime, so that, for example, a much earlier, or a still anticipated, transition to being a father or a chief or whatever can be understood as bearing on present action (see Carrithers n.d. *b*). There may be many ways other than by narrative to understand social organization in the abstract: figurative language can be used, or graphic illustrations. But to understand the relation to oneself and to each other of the various characters in their continuing mutual engagements calls for a more powerful capacity, one that can account for events in the distant past and connect them with the present and future.

To comprehend a plot is therefore to have some notion of the temporal dimension of social complexity, and it is the temporal dimension that I take to be crucial. Humans cognize not so much thoughts and situations as the metamorphosis of thoughts and situations in a flow of action. The consequence of this intricate ability to understand people in action is human sociality, a sociality remarkable even among our social primate cousins and one enabling us to fashion and refashion social arrangements of unending complexity, variety, and instrumental effectiveness.

I think it important to stress that the apprehension of others that is predicated in narrativity is not an absolute, impersonal, and unqualified Cartesian knowledge, as though an X-ray of someone's gray cells. It is rather an understanding that arises only within the give-and-take of shared life, and so is qualified by time, place, persons present, and the flow of events and relations within which those persons are immersed. It was not Pa Rangifuri's state of being *teke*, its psychological description and its physiological manifestations, that concerned the Tikopia or constituted their explanation but the significance of that state for what was going on. He was *teke* not in some abstract or absolute sense but relative to the persons involved—his father, his brothers, his dead son—and to the swiftly changing situation in which he and the others present found themselves. In that intersubjective sense the designation *teke* was a seed bearing the potential to grow into an elaborated narrative of persons, relations, and events—a plot with characters—that would satisfy initial puzzlement. Indeed, the only thing that *could* satisfy that puzzlement was a story, one that set Pa Rangifuri's distress in a narrative flow of people acting in respect of each other. Moreover, this story, as Firth presents it, was not one that he or anyone else devised in private but rather arose out of events and utterances occurring before a body of concerned persons. Not just the events themselves but also their unfolding explication and commentary were widely known. There might, of course, have been various interpretations of events at various times during the action and especially afterwards; but in order to act relevantly the participants had to fasten on some minimal shared understanding, an understanding that grew more explicit as the affair moved toward resolution.

Narrativity presupposes, in other words, a thoroughly intersubjective account of emotions, intentions, attitudes, and motives, not a cognitivist or methodological-individualist one. I make this point because so many, including an illustrious company of anthropologists and social theorists (see, e.g., Evans-Pritchard 1951:46; Nadel 1954:108; Lukes 1973:117; Roth 1989*a*; but also see Carrithers 1980, 1990), have treated intentions, emotions, attitudes, and motives as essentially unaccountable or irrelevant. They have perhaps done so in reaction to our academic folk psychology (based on our philosophical folk psychology), which has deemed it reasonable to consider people quite apart from their social setting.

But, as Bennett (1976) has shown, even the simplest and most routine act of conversation entails mutual attributions of attitudes and motives of a powerful and elaborate sort. The only requirement for such "mind-reading" (see Whiten n.d.) is that it work to make conversations possible. Dennett (1987) has shown how pervasive in human life and how serviceable is the "intentional stance," the understanding of events by attributing motives, purposes, and plans to agents. The yardstick against which such attribution is to be judged is not omniscience but relative success. And similarly the attributions of motive and attitude that appear in narration need only be adequate to account usefully for the stream of action and reaction. Indeed, it is difficult to see how such attributions could go beyond what is revealed in the stream of action. We cannot seek an absolutely correct, unequivocal, "scientific" understanding of such mental states apart from interaction, for it is only interaction that gives them sense and makes them available to consensible representation.

This argument can be very slightly expanded to answer the second question: is this pattern of Pa Rangifuri as recounted by Firth an accurate record of events, or something else? Well, it must be something else, for it is not simply and straightforwardly accurate. Rather, it is a synthesis, an artefact, but one produced under a particular constraint: it had to set out in a perspicuous order those events and attributions adequate to produce an account of what made participants act and what the consequences of those acts were. The criterion for including any detail was just that it contribute to showing how one thing led to another. As a synthesis it is no less "created" than Leinhardt's account of Dinka sacrifice or the drawing of the field rose.

The negative side of such a synthesis is that there is no guarantee whatsoever that all possible relevant details were included or that all relevant viewpoints were considered. Perhaps the old chief had a much deeper plan than anyone realized, or there was conflict over another, unmentioned matter that had been simmering. Perhaps Firth himself was unwittingly the vessel of a pervasive and disruptive colonial influence. There are myriad possibilities, and no account of human events can be wholly proof against such rude surprises.

On the other hand, the synthesis does possess five characteristics that inspire confidence: (1) It accounts for the flow of events. (2) The attributions of attitude and motive are closely and intelligibly tied to people's interactions. (3) The attributions are those disclosed by the participants in the course of events. (4) The action is unequivocally and vividly related to the particular circumstances of life among the Tikopia. (5) The episode as told has robustness and independence from its use by Firth. It could be used by someone else to illustrate fraternal rivalry, generational conflict, an anxiety to pacify chiefs, or the very peculiar position of axes among the Tikopia at the time. In that sense the episode has a distinct character as evidence rather than argument, as an item in the archive rather than the reasoning made from such items, as a foundation rather than the edifice rising above the foundation. For all these reasons we would be justified in accepting and using Firth's account until some startling new datum is revealed.

Evans-Pritchard vs. Sperber

The little story of the Nuer justifying his frequent sacrifices has much the same character as Firth's tale of Pa Rangifuri. It is elliptically told and refers to a flow of events understandable in both a larger and a more immediate frame. It does not suggest a theoretical use in itself but would be amenable to many such uses.

Sperber says that "this is about as raw a factual account as you will ever find in most ethnographic works. Yet not a single statement in it expresses a plain observation." This cannot, however, really be a relevant consideration. First, the ideas of "plain observation" and "raw factual account" are inappropriate both to the natural-scientific model underlying Sperber's criticism and to ethnography. Representing the patterns used as evidence in either case is likely to take a good deal of art and energy, so there is no such thing as a "raw" fact or a "plain" observation. Second, if it be thought that "observation" can be direct, immediate, and achieved without skill or application, that too is false. Evans-Pritchard had not just parachuted in but had already spent time among the Nuer, time that was vital to his perceiving and reporting patterns in Nuer life. And third, the absolute scientific knowledge that would be subserved by "raw facts" or by "plain observations," and against which ethnography fails, simply does not exist.

Sperber's next observation concerns the following statement by Evans-Pritchard: "I was present when a Nuer was defending himself against silent disapproval on the part of his family and kinsmen of his frequent sacrifices." Sperber remarks: " 'Silent disapproval' cannot be observed but only surmised." But, to the contrary, I suggest that "silent disapproval" is just the sort of thing that might be grasped with very little surmise. In the first place, Evans-Pritchard's remark is set in an elliptical but quite unambiguous narrative frame: "he [the Nuer in question] had been given to understand that it was felt that he was destroying the herd from inordinate love of meat. He said that this was not true. . . . It was all very well for his family to say that he had destroyed the herd, but he had killed the cattle for their sakes." Sperber could rightly complain that the who, when, and where of this story are left obscure, but the basic narrative flow is not. For some time the Nuer had been killing cattle from his herd in frequent sacrifices. This had depleted the herd, indeed, severely in the eyes of his "family and kinsmen." They conveyed their objections to him and—now the action switches to the immediate scene witnessed by Evans-Pritchard—met his prolonged protestations with silent disapproval.

Thus *in this setting* the "silent disapproval" is understood not as so many attitudes or mental states of uninterrogated witnesses but as part of a flow of actions and reactions within a group of people, the Nuer and his kinsmen. "Silent disapproval" gains its sense and meaning as a consequence of earlier events—the frequent sacrifices and the kinsmen's reaction to them—and it leads on to further action, the protestations of the man against unspoken but implicit accusation. Provided that Evans-Pritchard was privy to the stream of events in which the silent disapproval was set, he could have used the phrase with confidence not as a description of a mysterious inner state but as an attribution necessary for things to continue as they did.

Was Evans-Pritchard privy to the stream of events? Sperber is sceptical. He writes that the preceding part of the narrative, unravelled from the phrase "he had been given to understand that it was felt that . . . ," is "an inference from a variety of often ambivalent and complex behaviors. . . . likely to have been made not directly by the ethnographer, but by his informants." The material does not permit an unequivocal response to this. Sperber may be right. But there is strong evidence to the contrary, and, moreover, the very fact that Sperber admits Evans-Pritchard's accuracy to be an empirical matter suggests that, if not here, then elsewhere such evidence is admissible.

One circumstance that would incline us strongly to Evans-Pritchard's account would arise if he had actually witnessed an earlier argument between the Nuer and his relatives over sacrifice. There is no way of knowing whether this is so, but Evans-Pritchard has noted more than once that his Nuer research was carried out mostly without the advantage of informants, simply by living among the Nuer. So he might have had that sort of confidence in his own judgment.

But what if he had been merely told of antecedent disagreements? Is it likely that the disagreements were merely surmised by the informant? Perhaps—but if the informant had local knowledge then we would not think it mere "inference" or "surmise" if he or she told Evans-Pritchard that the Nuer and his family quarrelled over frequent sacrifice. It just is the sort of thing that neighbours know as a matter of course. One hears raised voices. Nor would we think such knowledge ambivalent, even though it would certainly be complex.

Moreover, it is possible that the antecedents were revealed by the Nuer himself in the course of expostulation. Perhaps he said, "You always say I sacrifice too much, but I don't!" Indeed, the following indirect speech, "It was all very well for his family to say that he had destroyed the herd," suggests that the clue to the continuing quarrel was contained in the Nuer's speech itself. To expand whatever the Nuer said to "he had been given to understand that . . ." is perhaps inference, but it is hard to see it as invalid or misleading. Indeed, Sperber admits as much when he later writes of this statement that "the clues are clear enough to determine an adequate descriptive comment" (1985:19).

Finally, any one of these more trusting interpretations seems more plausible than the complicated speculation Sperber offers us: "The resulting description is actually what the ethnographer selected from what he understood of what his informants told him of what they understood."

If my interpretation of the passage is accepted, then the implication is, I think, that we must attribute to Evans-Pritchard a kind of practical knowledge of events. Such knowledge is of course neither complete nor abstract, but it has at least one desirable characteristic: it arises out of the stream of events that alone can make the details intelligible. This does not amount to treating Evans-Pritchard as a Nuer or to saying that he understands all dimensions of Nuer life or even to saying that he could hold his own in argument with a Nuer. Rather it is to accept that, in the setting of this particular case, he possesses enough competence to make his way sensibly.

I suggest, therefore, that the measure of such knowledge is not narrowly epistemological but pragmatic: could one act appropriately in its light? Or—since the knowledge is sometimes discovered in retrospect or in a failure to act properly—could one have acted appropriately had one only known? In the case of Evans-Pritchard and the protesting Nuer, we take that limited competence already to have been achieved. Elsewhere, for example, when Lienhardt remarks that the effect of Dinka invocation "comes to be felt by the foreign observer himself," a process of achieving the knowledge by oneself is fleetingly revealed. In other cases, as when Firth is shown just how to lead Pa Rangifuri by the wrist to apologize, the knowledge is explicitly taught. And in some very marked cases, such as that of Briggs (1970), the evidence arises not from a finished competence but from a very protracted and painful course of learning. It might be thought that anthropologists' inexperience invalidates their evidence, but, on the contrary, it is often from our very lack of expertise—and its correction—that the most persuasive testimony originates.

From Consensibility to Consensus

I began with an apparent paradox, namely, the problem of constructing public and reliable knowledge out of material that seems irreducibly personal and autobiographical. But once we understand ethnography as an activity the paradox resolves itself. There is one sort of knowledge, that worn by, for example, the Nuer themselves as accountable agents in their society. Such knowledge is personal in the important sense that it is knowledge of persons exercised by persons in respect of each other. Some part of the knowledge must be distinctly cultural and general, but this distinguishable generalizing power must be knitted together with actual persons and actual circumstances. Each person's knowledge is thereby verified or corrected in public, though the public is not a college of scholars but the school of hard knocks. The ethnographer engages with this expertise, not perfectly perhaps but partly out of the will to do so and partly out of sheer necessity, and from the encounter he or she elicits consensible patterns.

The object of such engagement is the creation of a second sort of knowledge, one founded upon the Nuer knowledge of persons by persons but validated among a much wider and more diffuse community, including the readers of this journal and the world of anthropology and, nowadays, often the informants themselves. For this community the knowledge is transformed from knowing *how* to knowing *that*, from a performer's to a critic's consciousness. Indeed, it is just the transformation of social knowledge into declarative knowledge that gives ethnography its distinctive values and character as a discipline. We place requirements upon the new knowledge that are quite foreign to its original matrix: it must fit into a more abstracted view of human societies, and it must be corrigible or falsifiable. Moreover, some anthropologists mix this knowledge with a literary skill whose effect, as I have presented it here, is not to mystify but to clarify. Yet did the anthropological knowledge thus created not retain its animating spirit in the Nuer's personal knowledge of each other it would be not knowledge but fancy. A finished ethnography encompasses much more than consensible patterns, but consensible patterns are as necessary to the ethnography as pages are to a book.

A complete portrait of anthropology as a discipline would demand much more than I have offered here. We should have to understand the other kinds of evidence ethnographers use, the way in which they weave that evidence together, and the process of forming consensus or of differing with and correcting each other. And, of course, we should also have some notion of the knowing community. These are philosophical but also empirical and social-historical questions that have not yet been fully answered. Here I have only tried to suggest that ethnography begins in the study of the variety of human sociality by means of human sociality itself. We may ask of that study not certainty but reliability.

Comments

ANDREW BARRY
Department of Human Sciences, Brunel University, Uxbridge, Middlesex UB8 3PH, England. 11 I 90

There was a period when it was commonplace for social scientists to legitimise (or criticise) their practices by comparing them to those of the natural sciences. Fortunately, those days are largely over. In any case, as Carrithers notes, the natural sciences themselves are not now thought to be nearly so "scientific" as they once were. For Carrithers, the "new" philosophy and sociology of science appear to let anthropology off the hook. No longer is there any need to worry about the scientific status of anthropological knowledge when the status of natural science itself is problematic. Instead, the anthropologist can be concerned with the specificity of anthropological discourse: its attention to the detail of fieldwork, its "particular rigour" and its "particular canon of evidence." In this way, according to Carrithers, anthropology can avoid the excessive demands both of rationalists who would wish anthropology to be a science in the traditional sense and of postmodernists who would seek to translate anthropology into a form of literary experimentation.

It may be, however, that the arguments of the philosophy and sociology of science are less compatible with his position than Carrithers supposes, for in providing a critique of traditional accounts of scientific method and forms of representation sociologists have necessarily drawn attention to the diversity of the practices conventionally understood as scientific, as well as the significant differences in the ways in which "scientific" discourses constitute their relations to their objects (Rabinow 1986:236–41). Furthermore, the idea that there exists any necessary distinction between the discourses of the natural sciences and political and literary discourses has been increasingly challenged. Indeed, the natural sciences can usefully be understood as forms of political discourse (Latour 1983, Shapin and Schaffer 1985).

In contrast to these arguments, Carrithers conceives of anthropology as a peculiarly undifferentiated discipline with its own quite distinctive interests and values. In general terms, he distinguishes anthropology by its concern with the identification of "consensible patterns" through the acquisition of a "practical knowledge of events." However, the very possibility of such a general characterisation is problematic in the light of significant differences between the ways in which anthropologists themselves have construed their activities. For example, it is possible for ethnography to construct quite varied relations between its authors, its readers, and its objects (Strathern 1987). Moreover, any general characterisation of anthropology is likely to obscure rather than reveal the relations between specific anthropological texts and broader political and "scientific" discourses. If anthropology does indeed have any distinctive characteristics, then these should be demonstrated, not assumed.

Carrithers's paper begins by posing the question whether anthropology is an art or a science. Yet, as he might agree, it is probably more helpful to ask instead whether questions raised in the study of art or science might provide insights for an understanding of anthropology. In recent years, for example, ethnography has been described by a number of writers as a form of fiction. Thus, a whole arsenal of questions developed within literary theory can be applied to the ethnographic text, and the possibility of alternative literary forms can be explored. However, while an emphasis upon the idea of ethnography as fiction may lead to a concern with the character of the text and the possible significance of alternative textual forms, this should not be at the expense of an attention to the possible relations between the politics and practices of anthropology and those of the natural sciences. It may, indeed, be more helpful to conceive of anthropology not as fiction but as science, not in order to provide the subject with a dubious (and unnecessary) legitimation but in order to compare the forms of persuasion deployed in anthropology with those of the natural sciences. Paradoxically, now that the natural sciences are beginning to lose their earlier authority as forms of knowledge, it may be possible and significant to reexamine their connections with anthropology.

IVAN BRADY
Department of Anthropology, State University of New York College, Oswego, N.Y. 13126, U.S.A. 9 I 90

Through sharpened logic and a proposed vocabulary for identifying and theoretically manipulating patterns of evidence, Carrithers seeks a more deterministic (certain and reliable) ethnography; he also seeks to destroy what he considers a straw man that some postmodern critics have set up as "science." Both efforts are worthwhile. So is his emphasis on pragmatic theories of truth, his identification of science as a human activity situated in the world of human practice and perceptual consensibility, and his inference that claims to absolute or complete truth are dogma. His timing is also right—the criteria for reaching analytic agreement in answering questions and making successful arguments in the social sciences and the humanities are themselves less agreed upon than ever before (see Rorty 1979, 1989; Bernstein 1983; Roth 1989). But the article falters in the midst of its strengths.

The title raises the spectre of Lord Snow's famous Rede lecture and a long-standing conversation in anthropology. The articulation of art and science in any context is not easily untangled, and the either/or distinction cannot be sustained by the conventional divisions between value and fact, the true and the beautiful, the real and the fantastic, the subjective and the objective, or the apparently intuitive and the formally inductive (see Kuhn 1977, Barthes 1982, Bruner 1986, Brady 1990). Anthropology shows a tension between the extremes, to be sure, but it is mutually constructed in its opposition—a "moiety" effect (see Boon 1982, 1984) that ultimately makes it indivisible as an "artful science." Carrithers doesn't deal with the problem on this level, although the title suggests that he might have.

His position on Geertz and Clifford is similarly vague and undefended. The relative and culturally situated science that he espouses is much closer to "interpretive" anthropology than he says. His criticism applies better to Sperber, especially where it rises against shallow caricatures of science.

Rather than finding ethnography wanting in relation to something else held to be science, some aesthetes, humanists, and textualists argue that scientific knowledge itself is inessential, that it can be transcended by "a point of view other than, somehow higher than, that of science," and that human thought should not culminate in the application of scientific methods (Rorty 1981:155). It is also true that the positivistic model of science criticized in this discourse is sometimes a straw target that, as Eagleton (1983:144) notes, "does not exhaust the term"; declaring "that there are no absolute grounds for the use of such words as truth, certainty, reality and so on is not to say that these words lack meaning or are ineffectual." Carrithers is right to pursue the issue; he just may be looking in the wrong places.

Theory tailored to the open-ended patterns of social life is bound to be ambiguous. It is loaded with potential for intellectual terrorism in interpretation and for its virtual opposite, a hovering over the facts forever in "analytic neutral" (Marien 1988), the adoption of an invulnerable and mischievous but empty critical position (Eagleton 1983:144–45). But, in defense of Carrithers, staying *that* empty of determinism is largely a self-inflicted handicap that underestimates both the closure in patterns that can be discovered in particular resolutions of culture, no matter how transient they are in the long run, and the potential for defensible conclusions in other kinds of interpretations, including poetic and psychological ones (see also Lukes 1982).

Finally, I see the discussion of locating objectivity in mutable subjective relations more as a problem of misleading premises and incomplete information—a talk trick—than as a genuine paradox. It is resolved in part through Carrither's analysis of narrative processes and ethnography as a social activity. It only remains obvious if one believes that perfectly impersonal or objective knowledge is possible—and that is a positivistic conceit Carrithers apparently does not share. He seems to agree that impersonal knowledge is not dragged cleanly out of a "mirror-of-nature" mind (Rorty 1979) by Archimedean method, that scientific writers, like all others, are situated culturally, that scientific knowledge, like all knowledge, is *personal* (see Polanyi 1958, Grene 1969, Schwartz 1974). One can discover in this nexus that all knowledge is ultimately carved out of a tacit and ambiguous pool of perceptions and that it is subject to sharing and conversion in various forms. Concentration (a comparative dialogue with self) and agreeable conversation about it with others can make it precise. It becomes "impersonal" or "objective" only through related cultural laundering. There is neither magic nor hamstringing paradox in that, just a relative bias neither more nor less encumbering than the change to any other rigorous cultural-provenience or language game. Getting to know *how* and getting to know *that* in ethnography are equally bound by these principles. Carrithers wants to know *how* this might be true, confronts it in a practical framework, and (fuzzy opposition and talk tricks notwithstanding) moves the conversation ahead one respectable step.

CLIFFORD GEERTZ
Institute for Advanced Study, School of Social Science, Princeton, N.J. 08540, U.S.A. 12 XII 89

I do not wish to comment on the substance of Carrithers's paper, which strikes me as distracted and banal by turns. I only wish to have it on record that I do not hold the views he attributes to me. I do not believe that anthropology is not or cannot be a science, that ethnographies are novels, poems, dreams, or visions, that the reliability of anthropological knowledge is of secondary interest, or that the value of anthropological works inheres solely in their persuasiveness. On the second page of *Works and Lives,* in a passage invoking ladies sawed in half, I explicitly, and as I thought forcefully, both denied that I held such views and predicted that I would be accused by the easily frightened of holding them.

I do, indeed—doesn't Carrithers?—think that rhetorical effectiveness has something to do with who gets believed and who doesn't and that it matters a bit who says what, where, when, and to what purpose. But the notion that I have an "absolute realist" conception of science is a sheer fantasy. (I have never written at any length on the nature of science, but if I did it would look more like Thomas Kuhn's work than anything else; it would not look, as much as I admire him, like Dan Sperber's.) So, too, is the notion that I differ from the views of Taylor, Hacking, Polanyi, or Roth that science is a human, thus social and cultural, activity, that it does not involve the search for absolute truth, that the specific form it takes varies from field to field, even from problem to problem, that it involves more than thinking and theorizing, and that representations are one thing and what they purport to be representations of, like Carrithers's of me, quite another.

As I have spent much of my career vigorously opposing the idea that "there is one truth at which we aim . . . [one] correct representation of the world" (or, I might add, any one correct way of representing it), inside "science" or out, and that there is some red line to be drawn across thought polarizing "insubstantial art" and "firm science," it is more than a little dismaying now to be represented as defending it. Perhaps a more interesting question, after all, than why so many anthropologists can't write is why so many can't read. Or won't.

ROGER M. KEESING
Institute of Advanced Studies, Australian National University, Canberra, A.C.T. 2601, Australia. 9 I 90

Carrithers's paper makes some useful points, although I am less persuaded than he is about the cross-cultural transparency and translatability of patterns.

I agree with him that characterizations of science in interpretive anthropology have often caricatured the knowledge of the "hard" sciences as harder and more abstract than it is. His insistence, following Ziman, Hacking, and others, that science is socially and historically constituted is useful. (However, he ignores another voice in the polyphony of postmodernism that sees scientific knowledge as radically problematic and socially and culturally constructed. Some postmodernist anthropologists, citing Rorty and his ilk, go much farther in this direction than Carrithers does, rather than idealizing an objective natural science to which ethnography is radically contrasted.)

I take for granted that some forms of ethnographic knowledge are more "objective" than others. If an ethnographer writes that in the 28 households surveyed, the number of pigs per household was 5.7, I will suspend skepticism and assume that she got things right (although, knowing how few of the Kwaio pigs I counted I actually saw at the time, I probably should be more skeptical; I also remember asking Himalayan villagers how many sheep and how many goats they had: we finally got down to the limiting case of one sheep-and-goat). If the ethnographer tells me that 32% of the married couples in the village are living uxorilocally, I am inclined to accept the fact that they were carefully counted (but will suspect that, for reasons noted long ago by Goodenough, the classification scheme is inappropriate to the choices villagers make). If the ethnographer tells me that the villagers have no word corresponding to "angry" and therefore don't get angry with one another, I will surmise that the ethnographer arrived in the field with a distorted view of language and a muddled view of emotions. I always assume that the ethnographer is part of the picture, and my understandings of the Nuer and the Tikopia will always include Evans-Pritchard in a pith helmet and Firth in khaki shorts, trying to make sense of it all.

That brings me to Carrithers's point about the interpretability of behavioral sequences and patterns across cultures and hence the reasons we should have faith in well-constructed and plausible fieldwork narratives. I certainly agree that narrative conventions provide means of giving plausibility and coherence to our ethnographic accounts and evoking the co-participation and faith of readers in our interpretations. But I worry about whether our narrative powers really reflect the depth of our understanding of the cultural scenes in which we participate and our empathy in comprehending local nuances of meaning and emotion. Geertz's accounts of Bali have a magical power to convince the reader; less wondrously constructed and artfully crafted ethnographic narratives usually are much less persuasive, though they may rest on years of fieldwork, near-native fluency in a local language (on the possibilities of mistranslation and false exoticization of cultural texts, see Keesing 1989), and mountains of case histories and observational data. Could there even be an inverse correlation between the solidity of our ethnographic knowledge and our ability to convey interpretations persuasively? I worry that the narrative devices and pattern perceptions in which Carrithers places his faith may operate more powerfully if we have seen the ceremony only once and don't understand the local language very well than if we have seen it a thousand times and can tell when the officiant stammers or slips in a pun *sotto voce.*

Carrithers's argument also seems to me to overlook a problem noted long ago by Li (1937) and a related one noted less long ago by Rosaldo (1980). Li pointed to a contradiction in the process of pattern recognition: whereas an American seeing A, B, and C will assume that this implies D and E (because that is a pattern that fits together in the conceptual and experiential universe Americans live in), a Chinese will assume that A, B, and C implies F and G (and not D and E). His case in point had to do with Zuni marriage, which looked very different through his Chinese eyes than it had to American anthropological observers. That is, the missing pieces of a pattern—and we never see "all" of it—are inevitably supplied by the observer, and these may be deeply problematic (even if we accept the recognizability of A, B, and C, which raises a host of other issues). Rosaldo argued (1980:21–24) that we anthropologists are given to assuming that the most obvious, everyday, and familiar aspects of other peoples' lives and talk can be taken for granted as unproblematic—our challenge being to translate across cultures what strikes our eyes as most exotic and unfamiliar against this background of common humanness. She warns that the sense of the familiar in everyday talk, experience, and life-routines may be radically misleading. Our intuitions about cultural sameness, in other words, may give us impressions of solidity where cultural quicksand lies below.

My argument is not for infinite and radical cultural diversity and untranslatability: I have recently suggested (n.d.) that "if radical alterity did not exist, it would be anthropology's project to invent it." Yet I think that the path to cultural translation is more difficult and treacherous—and less easily crossed by rhetorical persuasiveness—than Carrithers seems to believe.

PAUL A. ROTH
Department of Philosophy, University of Missouri–St. Louis, St. Louis, Mo. 63121, U.S.A. 27 XII 89

The title question is evidence that the "unity of method" thesis is not yet put to rest. Doubt regarding the scientific status of ethnography appears to stem more from comparisons between it and some antiquated ideal of science than from specific issues arising from failures of the ethnographic enterprise. The time for believing that "science" means conformity to specific formal rules is long past. Much recent work supports Carrithers's shift in emphasis to anthropology as a kind of practice (Rouse 1987, Fuller 1988).

The most important aspect of his essay, however, is its treatment of narrative as a form of explanation. The details of such analyses ought to allay concerns about how, for example, anthropology and history provide objective explanations. Allowing that narratives explain also renders pointless, I suggest, the putative distinction between explanation and understanding.

Before addressing the substance of Carrithers's proposal, I have three quibbles to mention:

1. Geertz is not guilty, as I believe Clifford is, of con-

flating issues of authorial voice and textual authority. As I argue elsewhere (Roth 1989*b*), Geertz's work exemplifies the virtues central to narrative explanations. Geertz demonstrates how authorial self-presentation shapes a work without assuming that such insights account for their authoritative status.

2. To argue, as I do, that it is hopeless and pointless to seek a systematic account of the intentional idiom is far from deeming it irrelevant. Like Daniel Dennett, I would urge an instrumental or pragmatic interpretation of this way of talking; Dennett's position, moreover, is quite unlike that of Jonathan Bennett, who takes intentions as a basis for an analysis of communication. My disdain of reification of this idiom is not tantamount, however, to demanding its exclusion from explanations of social behavior.

3. The work in narrativity that needs to be confronted and overcome (N.B.: *not* rejected) is that of Hayden White (1973, 1987). White, Louis Mink, and others, reacting against positivism, emphasized the parallels between a historian's craft and a novelist's; the current problem, I would argue, is to attend more to the characteristics of narratives *qua* explanations (Roth 1988, 1989*b*; see Megill 1989 for a review of some of the key problems in this area).

Perhaps because of his acquaintance with Ricoeur's work, Carrithers emphasizes narrativity as temporal synthesis. Consequently, he wonders what characteristics such a synthesis should possess in order to "inspire confidence." However, the five attributes he then enumerates illuminate no *logical* or even structural aspects of the required synthesis. It is simply no help to be told that narrativity consists in "understanding complex nets of ever-new deeds and changing attitudes." The term "understanding" is one that has created problems in the first place. Carrithers's five attributes presuppose but do not clarify a demand for understanding. Even worse, they ignore the whole complex of issues regarding reading ourselves into others about which Winch, Geertz, and Clifford have rightly cautioned.

What Carrithers neglects are the social and integrative demands that candidate narrative explanations must satisfy. On the social side, two factors stand out. What counts as an explanation is time-bound and audience-dependent; this is what makes an explanation social. It is time-bound not in the Kantian sense suggested by talk of temporal synthesis but, rather, in a Kuhnian sense, in terms of having to rely on an available stock of currently acceptable forms. This also defines the respect in which explanations are audience-dependent: it is other inquirers, broadly or narrowly defined, who constitute the jury. The status of works may fall or rise following shifts in academic fashion and changes in audience, but such is the fate of all formulations of scientific knowledge.

Anthropological knowledge (indeed, any putative knowledge) is also to be judged in regard to how it *integrates* with what else passes as knowledge. The works of, for example, Carlos Castañeda fail to satisfy this constraint. This is not to insist that others must be judged according to whether or not their actions conform to some mythic standard of scientific rationality; I have inveighed elsewhere against that view (Roth 1987, esp. chaps. 4, 5, and 9). Rather, whatever account we take to be explanatory must at least be consistent with what we take to be correct in other fields of inquiry.

These constraints are very general, and much more needs to be specified in order to provide an account of how narratives explain. But they point to the social embeddedness of our explanatory practices and to the various factors, within fields and across disciplines, that narrative explanations must accommodate.

ROBERT A. RUBINSTEIN
%Ford Foundation, Cairo Office, 320 E. 43rd St., New York, N.Y. 10017, U.S.A. 30 XII 89

Carrithers's article is one of a number of recent works that signal a refreshing return in discussions of epistemological issues in anthropology to a focus on our discipline as a collective enterprise rather than on the idiosyncratic interpretation of our professional and personal anthropological experiences. Especially congenial are his attempts to dissolve apparent paradoxes in ethnography by treating it "as an activity" and his grounding of anthropological interpretations in human cognition. I find this general orientation promising (Rubinstein, Laughlin, and McManus 1984) but disagree with some of Carrithers's specific applications of it.

I agree that it is incorrect to attempt to legitimate anthropological understanding by showing that it conforms to an erroneous model of science through imitating "scientific method" or by seeking refuge in a strictly literary-interpretive understanding of our enterprise (Rubinstein 1984:48). Yet after rejecting the comparison of anthropology to the "received model of science" Carrithers seeks to legitimate his analysis by appeal to alternative models of the physical sciences. He then mistakenly grounds his argument on an "in-principle" scientific community ("The community is logically constituted as all those who can in principle perceive and report . . ."). Appealing to such "in-principle" understandings inevitably leads to misrepresentations of the scientific enterprise (Rubinstein 1984, 1988; Straight 1976), especially in analyzing epistemological problems in anthropology. Pursuing Carrithers's example, it is not simply a matter of observing a change in the color of litmus paper but one of understanding the theoretical significance of that change and putting it to use for some purpose. It is the purposefulness of scientific cognition that connects it to everyday cognition. It is in the context of purposeful action that we can determine if a scientific representation is "more or less useful," the criterion that Carrithers correctly substitutes for truth or falsity for evaluating scientific claims. Since all scientific knowledge is relative to a community of practitioners, it is important that we refer to a real, socially organized, not simply logically constituted, community.

Carrithers treats the dynamics of anthropological understanding as a form of pattern recognition. Examining an extract from Firth's account of Tikopia, he argues that it is interpretable on the basis of the commonsense understanding of Anglophone readers: "Yet with them we can perceive that *something* has happened and very roughly identify the character of that something." His explication of ethnographic understanding fails because it assimilates complex ethnographic description to a limited, culture-bound model of an idealized anthropological community. The reference community Carrithers employs is not coextensive with the community of professional anthropologists, or even of some subgroup of it; rather, it is introspectively constructed. His explication thus shares the weaknesses of analyses of the scientific process based solely on introspection: it is incomplete and distorting (Rubinstein et al. 1984:88; Piaget 1973:12).

Further, this explication refers anthropological understanding to a view of pattern recognition—the processes of which are more complex and active than Carrithers indicates (Neisser 1976, Laughlin et al. 1986)—that is insufficient for the understanding of scientific cognition.

Scientific cognition proceeds through the construction of *problems*, an activity that involves more than pattern recognition and depends upon the dynamics of particular professional communities (Hufford 1982; Rubinstein et al. 1984:61–84; Rubinstein 1989). The ability to construct problems from the analysis of experience develops through practice (Schön 1983, Argyris 1980) and depends upon a variety of nonlogical factors, including historic circumstance, the training of intuition, the social organization of a scientific community, and the purpose of the investigation. The same subject can, as Carrithers notes, be constructed as a number of different problems, "each offering some advantage in manipulating the matter at hand" (see also Rubinstein and Laughlin 1977:478; Rubinstein and Pinxten 1984; Rubinstein and Perloff 1986). This requires that anthropological understandings incorporate the *rule of minimal inclusion:* an adequate account of behavior must include "any and all levels of systemic organization efficiently present in the interaction between the system operating and the environment of that system" (Rubinstein et al. 1984:93).

Science, including anthropology, directs attention to different levels of organization for different purposes. This requires the recognition that the understanding it gives is incomplete and unstable except insofar as it occurs in a particular investigative context (Rubinstein 1984:173–78; Hawking 1988:12–14). Evaluating knowledge claims then requires a metric other than truth or falsity. Carrithers proposes the concept of "more or less useful" but does not expand on it. For anthropology, such evaluations may be made on the basis of an amplified concept of isomorphism, as applied to the fit between scientific, and other, understanding and the phenomena of concern (Rubinstein et al. 1984:21–35).

As White (1938) pointed out, science is preeminently a way of dealing with experience. Carrithers's article will be more rather than less useful if it marks a return in anthropology to the empirical study of science as human activity.

ELVI WHITTAKER
Department of Anthropology and Sociology, University of British Columbia, Vancouver, B.C., Canada V6T 2B2. 4 I 90

Encountering a title which queries whether anthropology is art or science forces all those who have lived through the post-positivist discourse to flex their muscles to do battle yet one more time. I expected to confront one of two positions: more science-bashing, statements about the inadequacies of the scientistic paradigm in dealing with the anthropological agenda, and pointed arguments about the naiveté of the scientific epistemology or yet another diatribe against the indulgences of interpretation, hermeneutics, or postmodernism—a call to cease, desist, and "get on with the job." These positions are, however, not resurrected here. Instead Carrithers offers us a "modified realism," a neopositivism with which I have no quarrel. Now that the issue of anthropology and science has lain dormant for about a decade, its time has come again in the history of intellectual politics.

The position that Carrithers takes reflects the lively practices of the philosophers of science, who for decades have been chipping away at the unquestioned claim that science has asserted for itself as the moral guardian not only of universal rationality but of quality, propriety, and rigour. As one of these philosophers, Einstein himself proposed that physics was merely a "creation of the human mind" of "freely invented ideas and concepts" aimed at forming "a picture of reality" and establishing "its connection with the wide world of sense impressions" (Einstein and Infeld 1938:310). Since then innumerable others have pointed out the contingent nature of scientific knowledge, implying that it is politically determined and interactionally instituted and directed and, given enough revealed contingencies, can be, like art or music, an interpretive and representational practice. In short, it can be merely another discourse about the world.

One of my regrets is that, despite the direction given here and there to an anthropology of knowledge (Crick 1982), no significant development in this area has emerged. There seems to be some advantage to using "knowledge" as the organizing theme of a critical discourse. Although rigorous deconstructionists would see it merely as another essentialism, such an approach would encompass the notion that knowledge is nothing more complicated than the stories we tell ourselves about the world as well as the philosophical and analytical issues about causation, essence, reality, and so on. Awaiting consideration are matters such as the anthropology of description and the anthropology of observation. Now that practices like ethnography have become "narrative description," we need to examine statistics as numerical description, theorizing as abstract description, and so on. We need to ask: to what conditions must cultures and persons conform in order to be observable or describable?

In this era of deconstruction one can only feel sympathy for Carrithers's quest for a public face for ethnography ("How are we to represent anthropology as a serious activity . . . if it is so nebulous?") and a transcendent universality such as he finds in the human experience of recognition of plots and characters. The agenda that he has set himself has points of similarity with ethnomethodological notions such as that of practical knowledge and its pragmatic outcome ("could one act appropriately?"). His "archive of images" is reminiscent of the notions of indexicality promoted by ethnomethodology in the 1970s. While he has dealt with intersubjectivity and consensibility in much the same way as Gouldner and others in connection with the notion of objectivity, will he have a different answer for the intertextuality promoted by the post-modernism of the 1980s? Finally, one can only be grateful that he has evaded the platitudes about revealing the ethnographer's prejudices, apparently taking a cue from Gadamer and treating them as consensibilities revealed and thereby put at risk.

As a work in neo-positivism or "modified realism," Carrithers's essay has still to withstand the deconstruction of realism, essentialism, and other forms of reification left on our doorstep by Derrida and other post-modernist philosophers. I look forward to seeing his development of these issues.

Reply

MICHAEL CARRITHERS
Durham, England. 6 II 90

Whittaker's intriguing question "To what conditions must cultures and persons conform in order to be observable or describable?" points beyond a concern with method towards the perennially unconsummated enterprise of constructing human nature. By responding to the question I hope to reveal something more of the premises that underlie my article and begin replying to the comments on it.

The perspective of the article could be called *mutualist* (Still, Costall, and Good n.d.), a tag which in this setting points to a loose collection of viewpoints sharing the insight that human life is constituted in interaction and intersubjectivity. Some mutualist notions might be the following: Human infants already evidence sociality by taking turns. Meaning in speech is achieved by mutual attributions of intentions. A sense of oneself is achieved only through others. Symbols have significance in their use by people as instruments to influence, foster, or exploit each other. Computers cannot be intelligent because they cannot be interlocutors. Knowledge and understanding arise out of mutual engagement. These and many other mutualist views are consistent with an especially thorough commitment to sociological apperception: they suggest that humans are distinguished by the depth and complexity of their sociality.

In this perspective Whittaker's use of observability and describability seems subtly misplaced, since such notions do not give any place to reciprocal interaction in fieldwork. I suggest rather that we would want to ask, "To what conditions must people confronted with mutual unfamiliarity conform in order to achieve mutual intelligibility?" This is a more general question, no doubt, but it is one that stresses that the activity of ethnographic fieldwork is only *one* example of *one* form of extended human interaction. We know that such interaction occurs quite apart from the practice of ethnography—that, for example, traders, war brides, *Gastarbeiter*, missionaries, and foreign students manage to get along. The general explanation would have to account for two extremes: assimilation, on the one hand, and minimum competence, on the other. If we could account for such cases we could also give an account of the sheer possibility of fieldwork—an account that we do not yet possess—and throw some light on how extended interaction works in less testing circumstances, between people who share a first language and a great deal of experience.

I will indicate where we might look for an answer to Whittaker's (rephrased) question by turning to Roth's demand for a closer account of narrativity. He remarks that "it is simply no help to be told that narrativity consists in 'understanding complex nets of ever-new deeds and changing attitudes,' " so I will try to expand a bit. I should first say that by "narrativity" I do not mean something that is a property of a text or discourse. I mean rather a capacity that distinguishes humans from other social primates. Correspondingly, the answer I will give is one that relates not directly to ethnography but to people in social life in general. Ethnography and allied fields of learning are a special case of a more general phenomenon.

The sentence that captures the nature of narrativity follows the one that Roth criticizes. It says that "human beings perceive any current action within a large temporal envelope, and within that envelope they perceive any given action not only as a response to the immediate circumstances or current imputed mental state of an interlocutor or of oneself but also as part of an unfolding story." In the first instance this assertion is directed to psychologists, who in the recent past have given a great deal of attention to the way in which people understand each other's beliefs, desires, and intentions (for sources see Whiten n.d.). From an anthropologist's perspective the psychologists' experiments and the explanations that accompany them are socially and temporally oversimplified (Carrithers n.d.*b*). Moreover, insofar as actual human life is both socially and temporally complex, the temporal and social dimensions go hand in hand. I might put it this way: to act reasonably a person must often track many people whose multifarious relations with each other are both created by and understood in terms of preceding events and relationships.

On reflection I think it was probably too simple to say merely that any given action is perceived "as part of an unfolding story." I do assume that, as Liam Hudson put it, "Asleep and awake it is just the same: we are telling ourselves stories all the time" (1985:85). But this process of cognition and mutual informing need not always be wholly self-consistent and continuous. For each one of us there are many stories or—to convey a sense of the episodic and ephemeral nature of much of our experience—many broken pieces of stories tumbling over one another. For long stretches of experience there could conceivably be no need to knit these together into something larger. But sometimes an especially puzzling or discordant event requires elucidation, and when that happens—as it did for Pa Rangifuri, his father, Firth, and all the others involved—people set out to explain themselves and each other to themselves and each other with a will. The social work of story begins in earnest.

I think it important to have as full a sense as possible of the work that story might and might not do. We might expect, for example, that some narrative elements, such as that Pa Rangifuri was *teke*, would be so firmly anchored in shared experience and public recognition as to be incontrovertible. But this need not mean that there has to be a single, mutually agreed-upon, canonical version of all events at which all participants arrive at some point. We would expect events and relationships in the past to be brought to bear on present events, but there would be no assurance that everyone would agree on which events and relationships were relevant. We would expect all stories to be told from a viewpoint, but not all accounts would be equally interested or biassed. And, finally, we would expect stories or bits of stories to become the object of further stories; and indeed we would expect some recountings to be decisive acts in themselves, just as the mutual telling of their differing accounts helped to constitute the reconciliation between Pa Rangifuri and his father. English-speakers would say that the two of them "came to an understanding," even though much was left tactfully unsaid.

Narrative understanding has three fundamental characteristics. First, it shows how intentions and feelings result in actions. Second, it shows how actions and happenings result in intentions and feelings or in changed intentions and feelings. And third, it can aggregate such causal connections into larger patterns such that persons are understood as having dispositions, events are understood to be part of a course of causally linked events, and relationships are understood to be entailed by dispositions and events. Or, to put it another way, the core of narrativity lies in its relating of our mental life to what happens to us. Bruner writes that narrative understanding "deals in human or human-like intention and action and the vicissitudes and consequences that mark their course" (1986:13).

I think that the concept of intention here must be understood to stand in for a whole series of related concepts as well, such as plans, aspirations, dreams, fears, hopes, and so forth. If we take it that human life is largely about our intentions in this wider sense and what happens to them, then it is no wonder that we are so richly equipped with narrativity. Indeed, the example that Roth elsewhere (1989) gives of a narrative explanation, namely, Geertz on the Balinese cockfight, is a narrative explanation precisely because it shows the cockfight to be a matter of the cocks' owners' intentions (aspirations, fears) and the fate of those intentions. Again, consider the fate of intentions just in this one brief part of Pa Rangifuri's story. Pa Rangifuri intended (hoped, yearned) to give his son a proper funeral as soon as possible, and so he asked his father for permission. His father intended (wished, planned) to avoid deciding between his sons and between alternative uses of scarce resources, so he temporized. Pa Rangifuri found his intentions thwarted, so he reacted. His father found his intentions thwarted as well—and so events carried on. Firth tells a better version, but this re-telling has the virtue of revealing some of the workings of narrativity.

From this it seems to follow that the discernment of human patterns I identified earlier, those of the Gnau hanging around and of the Dinka sacrificing, would also have to be counted as narrative understanding. That is so because the sociological and evolutionary significance of narrativity lies in its capacity to integrate the many distinct occurrences and partly formed data that constitute social life into a larger comprehension and a larger competence. Hence for anthropologists the stress would lie on a "top-down" view, on explaining the finer scale of experience by the larger (while cognitive or individual psychologists might take a "bottom-up" view). And so we would want to accept even the broken bits of story as essentially, constitutively narrative in nature.

Against this background I can return very briefly and very tentatively to the question of how mutual unfamiliarity might turn into mutual intelligibility. I suppose that we might start to answer the question by thinking of the primal ethnographic scene: two strangers, utterly different in experience, appearance, and language, sharing no knowledge that each other's sort exists, meet on, say, a jungle path. They gaze at each other in wild surmise, surmise that has much of imagination in it and much of conventional expectation as well. They attribute intentions to each other and they act (even silence is an act, even involuntary movement a gesture); they react and attribute further intentions, and so it goes. They soon share a past, so they share material for agreed-upon or disputed narratives. They begin, in short, to concoct stories about each other.

It might appear that Keesing is less hopeful than I about the outcome of such a primeval scene. He remarks that "the path to cultural translation is more difficult and treacherous . . . than Carrithers seems to believe," but I wonder whether we share enough information yet to know whether we agree or disagree. I did not directly address the issue of cultural translation or, to put it a different way, the issue of how ethnographers weave evidence together into elaborated arguments. I stressed almost completely the issue of evidence alone.

To show how ethnographers develop arguments using evidence would require analyzing a relatively lengthy passage. One of the clearest samples I know can be found in five pages of Schieffelin (1976:46–52). He begins by setting out the assertion that, for the Kaluli of Papua

New Guinea, "food is important because it is a vehicle of social relationship." The argument then ranges between more specific but still abstracted assertions that expand on this opening statement (e.g., "the giving and sharing of food . . . communicates sentiment; it conveys affection, familiarity, and good will") and anecdotes and habitual actions that illustrate the assertions. As the argument develops the abstracted assertions are connected to one another just as each of them is connected in turn with some anecdote or customary piece of behaviour. If, as I have suggested, the anecdotal slices of evidence are best understood as patterns, then the passage—and indeed the ethnography as a whole, and with it the cultural translation—is best understood as a *pattern of patterns.* It is a second-order pattern whose larger design is fashioned of abstracted assertions and whose finer detail comprises the anecdotes. The design as a whole has a great deal in common with Ziman's "archive" or "lacework" of patterns with their "messages." I take it that Keesing's remarks, and his examples, refer chiefly to flaws in the larger design, and if so we might perhaps agree on what strictures should apply to the making of such designs.

We might, however, still disagree because we have different views about the sources of understanding and misunderstanding and of the balance of the two. Keesing quotes Rosaldo's observation that an ethnographer can fail by taking assumptions to be shared that are not shared; but I suggest that an ethnographer can also fail by looking for the exotic where none exists. Keesing worries that brief acquaintance with a ceremony might yield a clearer pattern than extended acquaintance; but it is quite possible, and not only in ethnography, that brief acquaintance could be a reliable guide. Moreover, an ethnographer might find in an informant greater consonance of interests, viewpoints, life projects, or even techniques than in some colleagues.

Though each of Keesing's strictures is reasonable in itself, together they point to a view of culture that is founded in cultural differences, and indeed the very notion of cultural translation enshrines that view, along with the view that the set of differences that we describe as "cultural" is especially productive of misunderstanding. But I would suggest, first, that there are other differences, such as differences of interests, that are at least as productive of misunderstanding; and second, that there are commonalities that often go unremarked by ethnographers but allow for a fruitful connection among informant, ethnographer, and reader. A candidate for such a commonality might be the assumption of intended efficacy of communication (Brown and Levinson 1987). And the greatest commonality might be the shared disposition to understand people and events in a narrative way.

So the ethnographic sensibility that might grow from a mutualist perspective would be a bit less insistent on cultural differences and perhaps—though I do not know Keesing's view on this—more perceptive of change. This mutualist view is captured neatly by Brady when he writes of "the open-ended patterns of social life" but also of "the closure in patterns that can be discovered in particular resolutions of culture, no matter how transient they are in the long run."

It would seem therefore to follow—as Rubinstein and Barry say forcefully and others imply—that anthropology too would be open-ended, not a monolithic enterprise or constituted by a single sensibility. Perhaps even the image of anthropology as a conversation does not quite reach: it is more like one long argument—and even that is not enough, since anthropologists often argue but not about the same things. Maybe the only common thread joining anthropologists is an only partly overlapping series of different lists of significant interlocutors, lists that would include many, both within and without the learned professions, who are not anthropologists.

But I still think I can save some of the sense of collective purpose and of morale that informed my article. I argued in a largely ahistorical style, and the comments are made in that spirit. If, however, we were to ask a historical question, one concerning not so much *anthropology* in general but rather the character of *ethnography,* then we could discern more uniformity than the ahistorical view might suggest. The raw material for such a view of ethnography would consist in the ethnographic projects that have been undertaken and their social setting. They would be seen to be constituted—and increasingly constituted as ethnography has continued so far—in an insistence on fieldwork, on the value of interaction with those ethnographized, and on the nature of evidence as "illustration" rather than "documentation" (to use Evans-Pritchard's [1940:261] distinction). This core of practices has sometimes assorted ill with other practices that we have from time to time associated with anthropology (see Strathern 1987), but it does have integrity as a discernibly distinct and still living collective enterprise.

Ethnographers are conscious, for example, of the quality of other ethnographers' work and try to equal or surpass it. The institutions of doctoral supervision and examination enshrine this value in social practice, as do the processes of review in publication. There is a shared sense, and a shared experience, that any one whole project of ethnography is an effortful undertaking made up of many challenges and, if successful, of many achievements. Some of the achievements, such as writing, overlap in character and even to an extent in style with those of other disciplines, but the enterprise as a whole has its own complex character, its social and cultural historical basis, and its own peculiar and changing aspirations.

Writing elsewhere (Carrithers 1988) of Geertz's *Works and Lives,* I have said that the book is not largely about ethnography. I have also said that to the extent that it does concern ethnography it misrepresents it, as it also misrepresents the nature of writing in ethnography. I have suggested and briefly sketched how one might begin to represent ethnography, and writing in ethnography, more faithfully. The representation of ethnography is important because on the answer depends how many resources, how much credence, and how much spirit are spent on it.

References Cited

ARGYRIS, C. 1980. *Inner contradictions of rigorous research.* New York: Academic Press. [RAR]

BARTHES, R. 1982. "Inaugural lecture, Collège de France," in *A Barthes reader.* Edited by S. Sontag, pp. 457–78. New York: Hill and Wang. [IB]

BENNETT, J. 1976. *Linguistic behaviour.* Cambridge: Cambridge University Press.

BERNSTEIN, R. J. 1983. *Beyond objectivism and relativism: Science, hermeneutics, and praxis.* Philadelphia: University of Pennsylvania Press. [IB]

BOON, J. A. 1982. *Other tribes, other scribes: Symbolic anthropology in the comparative study of cultures, histories, religions, and texts.* New York: Cambridge University Press. [IB]

———. 1984. Structuralism routinized, structuralism fractured. *American Ethnologist* 11:807–12. [IB]

BRADY, I. Editor. 1990. *Anthropological poetics.* Savage, Md.: Rowman and Littlefield. In press. [IB]

BRIGGS, J. 1970. *Never in anger: Portrait of an Eskimo family.* Cambridge: Harvard University Press.

BROWN, P., AND S. LEVINSON. 1987. *Politeness: Some universals in language usage.* Cambridge: Cambridge University Press.

BRUNER, J. 1986. *Actual minds, possible worlds.* London: Harvard University Press.

CARRITHERS, M. 1980. Ritual and emotion. *Journal of the Anthropological Society of Oxford* 11:172–80.

———. 1988. The anthropologist as author: Geertz's *Works and Lives. Anthropology Today* 4(4):19–22.

———. 1989. "Sociality, not aggression, is the key human trait," in *Peace in society.* Edited by S. Howell and R. Willis. London: Tavistock.

———. 1990. On ethnography without tears. CURRENT ANTHROPOLOGY 31:53–55.

———. n.d.*a.* Why humans have cultures. *Man.* In press.

———. n.d.*b.* "Narrativity: Mindreading and making society," in *Natural theories of mind.* Edited by A. Whiten. Oxford: Blackwell. In press.

CARRITHERS, M., S. COLLINS, AND S. LUKES. Editors. 1985. *The category of the person: Anthropology, philosophy, history.* Cambridge: Cambridge University Press.

CLIFFORD, J. 1988. *The predicament of culture.* Cambridge: Harvard University Press.

CRICK, MALCOLM R. 1982. Anthropology of knowledge. *Annual Review of Anthropology* 11: 287–314. [EW]

DENNETT, D. 1987. *The intentional stance.* London: M.I.T. Press.

EAGLETON, T. 1983. *Literary theory: An introduction.* Minneapolis: University of Minnesota Press. [IB]

EINSTEIN, ALBERT, AND LEOPOLD INFELD. 1938. *The evolution of physics.* New York: Simon and Schuster. [EW]

EVANS-PRITCHARD, E. E. 1940. *The Nuer: A description of the modes of livelihood and political institutions of a Nilotic people.* Oxford: Oxford University Press.

———. 1951. *Social anthropology.* London: Faber and Faber.

———. 1956. *Nuer religion.* Oxford: Clarendon Press.

FIRTH, R. 1956. 2d edition. *Elements of social organization.* London: Watts.

———. 1985. "Degrees of intelligibility," in *Reason and morality.* Edited by J. Overing. London: Tavistock Publications.

FULLER, STEVE. 1988. *Social epistemology.* Bloomington: Indiana University Press. [PAR]

GEERTZ, C. 1988. *Works and lives: The anthropologist as author.* Stanford: Stanford University Press.

GRENE, M. Editor. 1969. *Knowing and being: Essays by Michael Polanyi.* Chicago: University of Chicago Press. [IB]

HACKING, I. 1982. "Language, truth, and reason," in *Rationality and relativism.* Edited by M. Hollis and S. Lukes. Oxford: Basil Blackwell.

———. 1983. *Representing and intervening: Introductory topics in the philosophy of natural science.* Cambridge: Cambridge University Press.

HARRE, R. 1986. *Varieties of realism: A rationale for the natural sciences.* Oxford: Basil Blackwell.

HAWKING, STEPHEN W. 1988. *A brief history of time: From the big bang to black holes.* New York: Bantam Books. [RAR]

HOFSTADTER, 1986. *Metamagical themas: Questing for the essence of mind and pattern.* Harmondsworth: Penguin.

HUDSON, LIAM. 1985. Viewpoint. *Times Literary Supplement,* January 25, p. 85.

HUFFORD, DAVID. 1982. *The terror that comes in the night: An experience-centered study of supernatural assault traditions.* Philadelphia: University of Pennsylvania Press. [RAR]

KEESING, R. M. 1989. Exotic readings of cultural texts. CURRENT ANTHROPOLOGY 30:459–77.

———. n.d. "Theories of culture revisited," in *Assessing developments in anthropology.* Edited by R. Borofsky. New York: McGraw-Hill.

KUHN, T. S. 1977. "Comment on the relations of science and art," in *The essential tension,* by T. S. Kuhn, pp. 340–51. Chicago: University of Chicago Press. [IB]

LATOUR, BRUNO. 1983. "Give me a laboratory and I will raise the world," in *Science observed: Perspectives in the social study of science.* Edited by K. Knorr-Cetina and M. Mulkay, pp. 141–70. London: Sage. [AB]

LAUGHLIN, C. D., JOHN MCMANUS, ROBERT A. RUBINSTEIN, AND JON SHEARER. 1986. The ritual transformation of experience. *Studies in Symbolic Interaction* 7(A):107–36. [RAR]

LEWIS, G. 1980. *Day of shining red: An essay on understanding ritual.* Cambridge: Cambridge University Press.

LI AN-CHE. 1937. Zuni: Some observations and queries. *American Anthropologist* 39:62–76. [RMK]

LIENHARDT, G. 1961. *Divinity and experience: The religion of the Dinka.* Oxford: Clarendon Press.

LUKES, S. 1973. *Individualism.* Oxford: Basil Blackwell.

———. 1982. "Relativism in its place," in *Rationality and relativism.* Edited by M. Hollis and S. Lukes, pp. 261–305. Cambridge: M.I.T. Press. [IB]

MARIEN, M. W. 1988. Hover culture: Shifting into neutral. *Christian Science Monitor,* August 5.

MEGILL, ALLAN. 1989. Recounting the past: "Description," explanation, and narrative in historiography. *American Historical Review* 94:627–53. [PAR]

MINK, LOUIS O. 1987. *Historical understanding.* Ithaca: Cornell University Press. [PAR]

NADEL, S. F. 1954. *Nupe religion.* London: Routledge and Kegan Paul.

NEISSER, URLIC. 1976. *Cognition and reality: Principles and implications of cognitive psychology.* San Francisco: Freeman. [RAR]

PIAGET, JEAN. 1973. *Main trends in psychology.* London: Allen and Unwin. [RAR]

POLANYI, M. 1958. *Personal knowledge.* London: Routledge and Kegan Paul.

RABINOW, PAUL. 1986. "Representations are social facts: Modernity and post-modernity in anthropology," in *Writing culture.* Edited by J. Clifford and G. E. Marcus, pp. 194–233. Berkeley: University of California Press. [AB]

RICOEUR, P. 1983. *Time and narrative.* Chicago: University of Chicago Press.

RORTY, R. 1979. *Philosophy and the mirror of nature.* Princeton: Princeton University Press. [IB]

———. 1981. Nineteenth-century idealism and twentieth-century textualism. *The Monist* 64:155–74. [IB]

———. 1989. *Contingency, irony, and solidarity.* New York: Cambridge University Press. [IB]

ROSALDO, M. Z. 1980. *Knowledge and passion: Ilongot notions of self and social life.* Cambridge: Cambridge University Press. [RMK]

ROTH, P. 1987. *Meaning and method in the social sciences: A case for methodological pluralism.* Ithaca: Cornell University Press.

———. 1988. Narrative explanation: The case of history. *History and Theory* 27:1–13. [PAR]

———. 1989*a.* Ethnography without tears. CURRENT ANTHROPOLOGY 39:555–69.

———. 1989*b*. How narratives explain. *Social Research* 56:449–78. [PAR]

ROUSE, JOSEPH. *Knowledge and power: Towards a political philosophy of science.* Ithaca: Cornell University Press. [PAR]

RUBINSTEIN, ROBERT A. 1984. Epidemiology and anthropology: Notes on science and scientism. *Communication and Cognition* 17:163–85. [RAR]

———. 1988. "Anthropology and international security," in *The social dynamics of peace and conflict: Culture in international security.* Edited by Robert A. Rubinstein and Mary LeCron Foster, pp. 17–34. Boulder: Westview Press. [RAR]

———. 1989. Culture, international affairs, and multilateral peacekeeping: Confusing process and pattern. *Cultural Dynamics* 2(1):41–61. [RAR]

RUBINSTEIN, ROBERT A., AND CHARLES D. LAUGHLIN. 1977. Bridging levels of systemic organization. CURRENT ANTHROPOLOGY 18:459–63, 475–81. [RAR]

RUBINSTEIN, ROBERT A., CHARLES D. LAUGHLIN, AND JOHN MCMANUS. 1984. *Science as cognitive process: Toward an empirical philosophy of science.* Philadelphia: University of Pennsylvania Press. [RAR]

RUBINSTEIN, ROBERT A., AND JANET D. PERLOFF. 1986. "Identifying psychosocial disorders in children: On integrating epidemiological and anthropological understandings," in *Anthropology and epidemiology.* Edited by Craig R. Janes, Sandra Gifford, and Ron Stall, pp. 303–32. Boston: Reidel. [RAR]

RUBINSTEIN, ROBERT A., AND RIK PINXTEN. Editors. 1984. *Epistemology and process: Anthropological views.* Ghent: Communication and Cognition Books. [RAR]

SCHIEFFELIN, F. 1976. *The sorrow of the lonely and the burning of the dancers.* New York: St. Martin's Press.

SCHÖN, D. 1983. *The reflective practitioner.* New York: Basic Books. [RAR]

SCHWARTZ, F. Editor. 1974. *Scientific thought and social reality: Essays by Michael Polanyi.* New York: International Universities Press. [IB]

SHAPIN, STEVEN, AND SIMON SCHAFFER. 1985. *Leviathan and the air-pump: Hobbes, Boyle, and the experimental life.* Princeton: Princeton University Press. [AB]

SPENCER, J. 1989. Anthropology as a kind of writing. *Man* 24: 145–64.

SPERBER, D. 1985. *On anthropological knowledge: Three essays.* Cambridge: Cambridge University Press.

STILL, A., A. COSTALL, AND J. GOOD. Editors. n.d. *Mutualism: A new approach to psychology and the human sciences.* London: Harvester Wheatsheaf.

STRAIGHT, H. STEPHEN. 1976. Comprehension versus production in linguistic theory. *Foundations of Language* 14:525–40. [RAR]

STRATHERN, MARILYN. 1987. Out of context: The persuasive fictions of anthropology. CURRENT ANTHROPOLOGY 28:251–81. [AB]

TAYLOR, C. 1982. "Rationality," in *Rationality and relativism.* Edited by M. Hollis and S. Lukes. Oxford: Basil Blackwell.

———. 1985. "The person," in *The category of the person.* Edited by M. Carrithers, S. Collins, and S. Lukes. Cambridge: Cambridge University Press.

WHITE, HAYDEN. 1973. *Metahistory.* Baltimore: Johns Hopkins University Press. [PAR]

———. *The content of the form: Narrative discourse and historical representation.* Baltimore: Johns Hopkins University Press. [PAR]

WHITE, LESLIE A. 1938. Science is *sciencing. Philosophy of Science* 5:369–89. [RAR]

WHITEN, A. Editor. n.d. *Natural theories of mind: Evolution, development, and simulation of everyday mindreading.* Oxford: Blackwell.

ZIMAN, J. 1978. *Reliable knowledge: An exploration of the grounds for belief in science.* Cambridge: Cambridge University Press.