

Culture

Our Authorial Authority

Robert Paine



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Résumé de l'article

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*Il s'agit donc d'un commentaire sur le genre de questions posées notamment dans *Writing Culture* (éd. Clifford et Marcus, 1986) et, en particulier, sur quelques changements dans la présentation du soi anthropologique.*

This article moves from Sir James Frazer, through successive generations (British for the most part) of social anthropologists, and onto the post-modern present-day of James Clifford and Marilyn Strathern, among others, and to the place that an advocacy anthropology may have in it.

It enquires, in each case, about (i) the nature of the 'authority' which the anthropologist appropriates and (ii) strategies used to persuade and/or assuage readers of anthropology (among whom are included its subjects).

It is, then, a commentary on the kind of issues brought to us most notably in *Writing Culture* (eds. Clifford and Marcus, 1986) and, in particular, on some changes in the presentation of the anthropological 'self.'

*"To let otherness speak, we have to stage it."
(Hastrup, 1986:13).*

For me, this touches the essence of the current swirl of debate about our authorial authority. *To stage it*—how exquisitely ambiguous! Is it, then, our duty to 'arrange' what Otherness says, so that context appears by our authorial wisdom, i.e. privilege? Or is it our duty to let Otherness speak for itself (himself, herself)—to give it the stage?

Some brief introductory remarks are necessary.

What follows is essentially a transcript of an oral presentation to colleagues.¹ Interested in how anthropology has presented itself to its readers, I incorporated an iconographic medium (using slides): photographs or other illustration from anthropological classics, or books that are on their way to being so (Figs. 1-16). It is worthwhile noticing which of these are (a) frontispiece, (b) cover/dust jacket illustration, or (c) merely a photograph somewhere inside the book.

The questions I try to address are these:

- what different authorial authorities have we appropriated to ourselves?
- what are our arguments among ourselves about them?
- how are our authorial modes accepted by others?

Of course others have looked at these questions, and quite recently too, but I do not necessarily refer to



Fig. 1 Frontispiece *The Golden Bough* by Sir James Frazer. MacMillan & Co. 1st ed. 1880, abridged 1922.

them as my oral presentation was a personal view of things. Nonetheless, I cannot forbear mentioning James Clifford's seminal "On Ethnographic Authority" (1988 [1983]). He asks (1988: 25): "If ethnography produces cultural interpretations through intense research experiences, how is unruly experience transformed into an authoritative written account?" There we have much of the nub of the matter.

Consider, though, for a moment, Clifford as the worried anthropologist of his own time—and that could be your time, fretting over a situation in which "an intersubjective ground for objective forms of knowledge, is precisely what is missing or problematic for an ethnographer entering an alien culture" (1988: 35). Meyer Fortes turned that on its head: "'You know,' he said, 'your greatest strength is the difference between you and them'" (Drucker-Brown 1989:379). Somewhere in the movement between Meyer Fortes and James Clifford—with many way-stops—lies the story of our changing authorial authority.

I begin—as is the fashion—with the veritable Sir James Frazer. One could have guessed what the frontispiece of *The Golden Bough* (Fig. 1) would have been like? Even so, Sir James's life overlapped those of Engels, the Webbs, and Havelock Ellis. Seemingly, the question the frontispiece poses for its Victorian readers is something like—'what will emerge from the mist: savage or civilized man?'

The Golden Bough, of course, grew in the library, not out in the field. Indeed, Frazer (according to Leach, 1966) believed "first-hand experience of primitive peoples is a discomfort which the more intelligent anthropologist can well afford to do without" (1966:560). And much of the reason is that "savages . . . have the simple-minded ignorance of children which is sharply contrasted with the sophisticated highly-trained mind of the rational European" (1966 : 563).

Frazerian anthropology, then, is evolutionist, devoted to the detection of savagery that once underlay our now civilized society (Strathern, 1987:256). There is simple ethnocentrism of the white supremacist variety. For the post-Darwinian, Victorian reading public Frazer's message was comforting: not only was it more of men than of apes, but it justified 'the white man's burden.' It was widely read.

Although there is the implication of human 'progress' throughout the Frazerian *oeuvre*, there is no consideration of 'change.'

Its authorial authority? Classical erudition—for which the engraving of its frontispiece is a warranty and sufficient advertisement for the book.

But both the warranty and the advertisement of anthropology were to change—without Frazer, but well within his lifetime, as is seen in Fig. 2.



Fig. 2 Frontispiece *The Andaman Islanders* by A.R. Radcliffe-Brown. Cambridge University Press. Field research 1906-08; first edition 1922.

The evocation is *remoteness*—as if forever in that quiet lagoon. Though based on fieldwork, this book is still ‘Frazerian’ in serious respects: the savage is *the Other*; he belongs to a distant but fading world that we may romanticize even though certain calamitous changes have torn the garment of savagery. And *that* is part of the “difficulties” of the research endeavour, says Radcliffe-Brown himself, for it is devoted to the earlier, traditional, pristine culture (1948:22, 72n). Another difficulty is that the natives have “no words in their language to denote any but the simplest relationships” (1948: 82n)—however, the reverse may be nearer the truth: that the ethnographer was only able to converse in the simplest terms in the natives’ language.

Authorial authority? Key informants on topics selected by the ethnographer. And if Fig. 2 is how the ethnographer wished to portray “his” native, Fig. 3 gives us an idea of how he wished us to view him — an evocation, this time, of rank married to civilized, urbane sageness.



Fig. 3 Frontispiece *Social Structure, Studies Presented to A.R. Radcliffe-Brown*, Edited by Meyer Fortes, Russell & Russell, Inc., 1963.

But, of course, another book appeared in 1922 (Fig. 4).



Fig. 4 Frontispiece *Argonauts of the Western Pacific* by Bronislaw Malinowski. Routledge and Kegan Paul.

The product of fieldwork of “a quite unprecedented intensity” (Leach, 1966: 565), the *Argonauts*—its Frazer-like title notwithstanding—reverses the Frazerian quest for “the detection of savagery:” Malinowski pursues the detection of civilization under savagery (Strathern, 1987: 256). He argued: other peoples’ lives (e.g. the Trobrianders’) may be based on “entirely different assumptions from our

own;" and that their behaviour "makes sense" in the light of their assumptions (Leach, 1966: 565). In short, relativism in anthropological interpretation. But we know now that this was not to be, and still is not, at all a straightforward matter. Radcliffe-Brown put his finger on an inherent problem of cognitive presentation. "There is the danger," he wrote in *The Andaman Islanders*, "that the ethnologist may interpret the beliefs of a native people not by reference to their mental life but by reference to his own" (1922:ix).

Metaphor being an important instrument of anthropological interpretation, we must be clear in our own minds why we choose the metaphors we do, and what the possible epistemological implications are. For instance, what happens when we look at that frontispiece to the *Argonauts* and we see (and tell our students to so see) 'precedence at court'? or 'trade delegation'? There will be more to say about ethnocentricism/relativism.

The authorial authority of the *Argonauts*? "Participant observation" (though Malinowski didn't call it that: Drucker-Brown 1985) and these "savages" certainly *did* have "words in their language" for the most complicated of relationships.

If we allow 1922 as the year of the birth of modern anthropology (in its "British school" at least—Kuper, 1973:9), the matter of authorial authority was *not* settled then, nor is it today. "Participation observation" may be our article of faith, our favoured professional trademark, but it conceals more than it reveals about anthropological praxis and shortcomings therein. This becomes increasingly clear as we proceed with our account. Among the 'greats' in the next generation of anthropologists, there was Evans-Pritchard whose name is, I suppose, synonymous with *The Nuer* (Fig. 5).

As we read it as students (in the '50s), I think we saw Aristotelian structure overlaying Nuer thought: not that that worried us unduly. In fact, I recall being much impressed. But of course his fieldwork among the Nuer has been placed in doubt quite recently (Rosaldo, 1986). It therefore gladdened me to read Mary Douglas' description of "the programme" Evans-Pritchard wrote for himself:

[He] would abhor speculative abstractions. There would be evidence for everything he reported. The facts would not be selected by subjective bias. They would not be selected from hundreds of societies. The evidence would not be hearsay; it would not be elicited by interrogation; it would not be purely verbal (1980: 36; cf. Evans-Pritchard 1951:77-80).

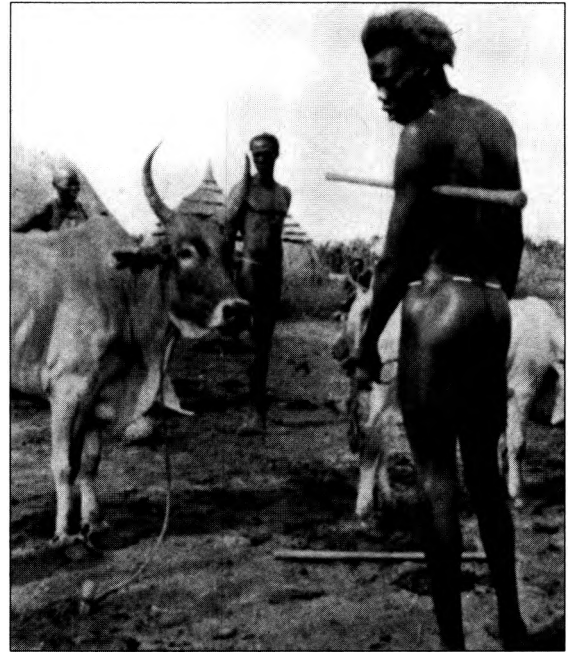


Fig. 5 Frontispiece *The Nuer* by E.E. Evans-Pritchard. Oxford University Press. Field research 1 year between 1930-36; first edition 1940.

But what dates this programme is its confident dismissal of "subjective bias."

With more than the English Channel separating him from Evans-Pritchard, has been Lévi-Strauss (Fig. 6).



Fig. 6 In *La Pensée Sauvage* by Claude Lévi-Strauss. University of Chicago Press. First edition 1962; English trans. 1966.

A recent interviewer raised the objection: “you have read many books but done little fieldwork” (Eribon, 1988). Lévi-Strauss answers (in part):

I realized . . . that for 20 or 30 years material had been accumulating in considerable proportions, but in such disorder that it was not clear how to make sense of them and use them (1988: 3).

My intention here is not to suggest a ‘regression’ to Frazer, for that is not the case. (Though I do find the absence of Lévi-Strauss, in Strathern’s 1987 piece on Frazer, strange.) What interests me is both the contrast and the similarity, between Evans-Pritchard and Lévi-Strauss, concerning authorial authority. In the place of Aristotle, Lévi-Strauss—with a different intellectual mandate to Evans-Pritchard—builds much on Jakobsen’s phonemic analyses in a quest to map the passage from nature to culture or from animality to humanity (not simply from savage to civilized). But note that while both adopt a relativist position vis-à-vis the validity of other cultures—the keys, in the case of both men, to understanding these Others are from a European key chain.

But already with this generation of anthropologists, “ethnocentrism” is being handled differently from the way it was in the Frazerian period. Then, ethnocentrism was European, and something that arose naturally—that is, by evolution—from the course of human history. Now, building upon the Malinowskian approach, ethnocentrism is used heuristically, as a tactic: the reader is first assured that his commitment to *his own* cultural world was legitimate, *then* he is asked to grant as much to the Other culture (Strathern, 1987: 260). Evans-Pritchard’s *Witchcraft, Oracles and Magic among the Azande* of 1937 is the outstanding example.

Beginning in the 60s, concern with authorial authority takes a new turn. Evans-Pritchard’s “programme,” while insisting on “participant observation” as the *sine qua non* of fieldwork, took all possible distance from “subjective bias”—*or so it was thought* but, in truth, there was little chance of knowing, precisely because the matter of ethnographic self-consciousness was suppressed.

The nearest we get to it is, perhaps, Evans-Pritchard’s “Neurosis” pun (1940:13)! All the time, of course, there had been *The Diary* (Malinowski, 1967) (how many others besides Malinowski’s, one wonders)—but under lock and key. And when Laura Bohannan published her “subjective account” (in 1954), it was fictionalized and appeared under a nom-de-plume.

This debate—that has taken several turns since, and is still very much with us—may have been launched through the influence of Erving Goffman, with Gerald Berreman, in his 1962 monograph, *Behind Many Masks*, as a principal conduit into anthro-

pology. There is an interest in loose, everyday “encounters” (Goffman’s term) as well as in the ceremonial, ritualistic, “structurally” significant; hence tentative, limited beginnings towards ‘reparation’ of social anthropology bringing us closer to *looking at ourselves as an Other*. A key figure here is, surely, Julian Pitt-Rivers, beginning with his Spanish monograph (1954) which preceded Campbell’s classic (Fig. 7) by a full decade.



Fig. 7 Frontispiece *Honour, Family and Patronage* by J.K. Campbell. Oxford University Press. Field research 1954-55; first edition 1964.

And these matters drew the anthropologist towards a new responsibility of accountability concerning himself/herself with his informants. (I suppose Casagrande: *In The Company of Man*, 1960, is a milestone here.) In the phrases of that time, we were asked to consider how the ethnographer and his informants each engage in “games” of “impression management” (Nash and Wintrob 1972:528 [Fig. 8]).



Fig. 8 *In Reflections on Fieldwork in Morocco* by Paul Rabinow. University of California Press. First edition 1977.

However, less attention was paid to what this said about the very basis of anthropological authorial authority—and of course *that* issue is exploding all around us at the present time. I, for one, do not believe it is a storm in a teacup. From the beginning of modern anthropology there has been if not a contradiction, certainly a tension between the “objective” or “scientific” *authorial authority* and the “subjective” or personal *authority of the anthropologist*—but it had been left unspoken, largely. For all its suggestion of the accidental and its take-it-on-good-faith plea, the *privileged* personal testimony of the anthropologist has been—in line with what I said about participant observation—our distinguishing (and we like to think, distinguished) trademark. This authority says, “I was there (not you)” (Rabinow 1986:244). Yet you will search *The Nuer* and the *Argonauts* in vain for a photo of the author in the field (ditto Firth and the Tikopia, Fortune and the Dobu, etc.). All the more remarkable, then, has been our deep attachment to that form of authorial authority. An attachment that is seen, or rather heard in talk—talk that could have been lifted from memoirs of colonial District Officers—of “my people.” The dissonance between this and a tentative trend towards the ‘repatriation’ of anthropology needs no elaborating.

The irony of the situation was not completely lost on us graduate students. As much is suggested by the “We, the Tikopia and I, Schapera” joke: an L.S.E. joke (Kuper, 1973: 154) that I heard in Oxford. Diffusion. But Oxford had its own joke-story. Briefly: an immaculately, Saville Row-dressed black man comes to Oxford. He is a Nuer, coming to see “E-P.” He’s furious. Oh God! we thought, has E-P got it all wrong about lineages, or is it segmentary opposition? But the visitor demanded to know “why do you say *there are no trees* in my land?” What are we to make of this? First, it is fiction *and* it is true that it was told. And as a ‘text’ of its time? Is it not amazing how impressed anthropologists were (are?) by the move (putative at that) from nakedness to clothed? (From the ‘raw’ to the ‘cooked’?) More significant still, I suggest it was a story to confer *absolution* on us for our “‘my’ people”-ness. It did so through making the issue wholly frivolous.

Clifford (1988:40), I suppose, would see more than absolution here and more than ‘mere’ anecdote—the ethnographer’s anecdote, he says, is there to establish “a presumption of connectedness, which permits the writer to function in his subsequent analyses as an omnipresent, knowledgeable exegete and spokesman.” He is probably right, I think that our joke—which soon became an anecdote—*did*, in its own way, serve to make “E-P,” in our eyes, still

more of the exegete of all things Nilotic; and furthermore, I think we felt enhanced as (yet to be) ethnographers.

Eventually, it was the opening to the reading public of other sources of authorial authority beyond that of anthropology that broke our compact of silence on our authority. The intellectual readership began to ‘relativize’ the professional authority of anthropologists to alternative ways of knowledge (Marcus and Fischer, 1986:37); and books like Edward Said’s *Orientalism* (1978), which he wrote as an attack on Europocentrism and which castigated Western authors, generally, for holding their ‘Oriental’ subjects passive, helped serve to demystify the authority of the anthropologist. Nearer home for many of us, native minority groups used anthropology as a whipping-boy in their defiant anger against Euro-domination. We may have written about what the ‘natives’ were doing as “symbolic opposition”—but that’s quite beside the point in the present context: Aboriginal America (to name one continent) was questioning anthropology’s authorial authority.

But we are now ourselves pulling our ‘house’ apart. Here one could stray far and wide in comment, but I will try to follow the thread of the argument thus far. I see the question of relativism problematized as a painful question of sociology of knowledge. We appear to be disingenuous if not dishonest about it. For if the metaphysical ‘leap’ to understanding other cultures is so great, as we love to say, how come that most of us—us *average* anthropologists—are convinced—when addressing our readers, though they may not share our opinion—that we *made* the ‘long jump’ and landed on our feet?

The best evidence against relativism is . . . the very activity of anthropologists, while the best evidence for relativism [is] in the writings of anthropologists (Sperber, 1982:181—& cited by Geertz, 1984:274).

On my reading of Strathern, she develops the problem in respect to its *two* coordinates: first, between the anthropologist and the Other culture, and second, between the anthropologist and his/her own readership. On the first, she asks:

how are we “to create an awareness of different social worlds when all at one’s disposal is terms which belong to one’s own” (1987: 256).

On the second—bearing in mind how readers have their own experiences through which they ‘read’ what you wrote—she observes:

We typically think of anthropologists as creating devices by which to understand what other

people think or believe. Simultaneously, of course, they are engaged in constructing devices by which to affect what their audience [readership] thinks and believes" (1987: 256).

This leads Strathern to her focal point: our monographs have "literary strategies" (1987: 257) in order "to convey alien ideas across cultures" (1987: 265) (cf. Geertz's *Works and Lives*, 1988). Hence her notion of *persuasive fiction*—not as a failing or corruption of anthropological writing but as a necessary part of its make-up.

Then there is the paradox. Even though "the very use of the 'fiction' conveys a self-conscious playfulness" (1987: 265), the author has been absent from so many anthropological monographs. Strathern (following Clifford 1988) speaks of "the author absent because the fieldworker is the authority of the text" (1987: 264). In other words, the anthropologist, hidden behind his alter ego, the fieldworker, has been leaving himself out of his own account of the relationship he has forged between his readers and the Other culture. We may disagree about the facts of the case, but the point is serious. For that relationship rests upon the one between the anthropologist and the Other culture, and by not telling how he or she shifted back and forth between cultures while in the field, the anthropologist forfeits the best chance of bringing readers into the *trans*-cultural world of *his* (or *her*) creation.

I want to take the point about persuasive fiction a little further. I just said that the anthropologist *forges* a relationship between his readers and the Other culture; surely this verb—"to forge" with its double entendre—takes us to the ambiguous nature of anthropological authorial authority? Anthropology forges contacts between cultures alien to each other—yes, but inevitably, there'll be an element of the fake in this forging. Or, as Kirsten Hastrup said, "to let otherness speak, we have to *stage* it"! (1986: 13).² She also sees the truth that we seek in our accounts as arising out of a "creative relationship" that "cannot be constructed by the anthropologist in the first person" (1986: 10) but only in concert with, first, those of the alien culture, and secondly, with the readers.

The dressed-up slogans of this post-modern orientation—or shall we say the slogans of these persuasive fictions of post-modernism in anthropology—are already familiar: double hermeneutics, dialogical replacing analogical anthropology (Tedlock, 1983, ch. 16), and so forth. More plainly, the

time-honoured claim of "I was there" is challenged: the fieldworker certainly, but the author, was he ever "there" or simply in his study?



Fig. 9 Dustjacket. *Man and Culture: An Evaluation of the Work of Malinowski*. Edited by Raymond Firth. Routledge & Kegan Paul. Fourth impression (hardback) 1963.

You *don't* find the kind of photo shown in Fig. 9 in the *Argonauts* of early editions—it (and other photos now unearthed) appears as "a sign of our times, not his" (Clifford, 1986a :2). All there was, was a photo (Fig. 10) with the caption "The Ethnographer's Tent on the Beach of Nu'agasi."



Fig. 10 Inside *Argonauts of the Western Pacific* by Bronislaw Malinowski. Routledge & Kegan Paul.

Nor is a photo such as Fig. 11 (half a century after Malinowski was in the Trobriands) assurance enough that the author, as well as the fieldworker, "was there." It is no warranty of dialogical anthro-

pology. But things seemed to have changed since Radcliffe Brown's day (Figs. 2 & 3)



Fig. 11 Inside *Ritual and Knowledge among the Baktaman of New Guinea* by Fredrik Barth. Norwegian University Press. Field research 1968; first edition 1975.

After demolition, reconstruction—or after *deconstruction*, reconstruction. A word or two, then, on changes this may be bringing to our authorial authority. The code word is *self-reflexivity*, isn't it? Our authorial authority depends on our attending to the relationship between what the ethnographer knows and how he came to know it. There are several points about this worth singling out:

1. vis-à-vis the culture under investigation. We must be clear about “the kind of self-reflections we demand of our subjects”—of our informants especially (Caplan, 1988:9).
2. vis-à-vis oneself in that other culture. “It is by observing his own self perform that the fieldworker ‘discovers’ the culture he studies” (Cohen, 1987:207).
3. vis-à-vis our own culture. Perhaps the most important anthropological application of “deconstruction” is *defamiliarization*: to help us to really know what we think we know, we must put our own taken-for-granted categories under the anthropological microscope (Marcus and Fischer, 1986: 149). (Note how this is a reversal of the earlier acknowledgement of our own ethnocentrism as a strategy to invite people to grant as much to—and with it, respect for—Other cultures.)
4. tying it all together. Following Ricoeur, by looking at Others we learn how to look at, and

recognize, ourselves. But here it is essential to recognize that the problem of ‘otherness’ is not simply between such general entities as ‘culture’ or ‘society’ but one of *person* too (Cohen, 1989:37)—as any consideration of role and performance quickly shows. Most remarkable are the layers of ‘otherness’ within the self of the anthropologist. First, he (or she) must ‘participate’ as though belonging, yet remain detached, as ‘observer.’ Then later there is the ‘otherness’ of the author vis-à-vis these selves of the fieldworker.

The question “*where* is the author?” now becomes “*who* is the author (Fig. 12)?”

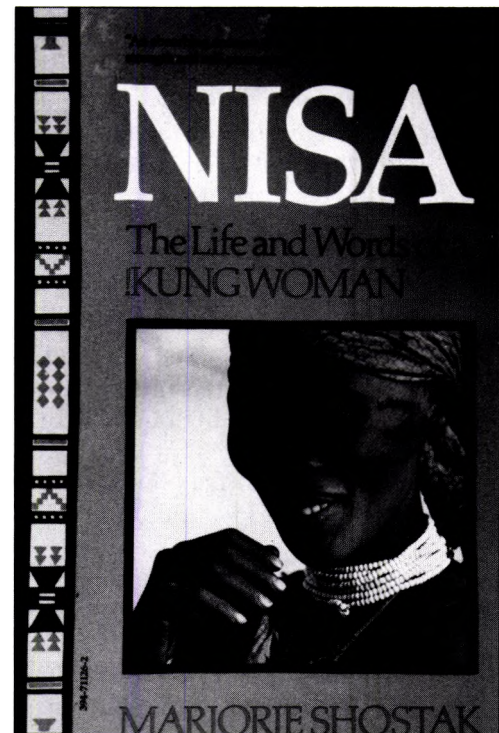


Fig. 12 Cover *Nisa* by Marjorie Shostak. Random House. Field research 1969-71; first edition 1981.

Is the author Shostak, an American caught up in feminist questions of the 60s, editing a Bushman woman? Or is it Nisa?

[Nisa] is not an informant speaking . . . as if to everyone and no one . . . Nisa speaks, throughout, not as a neutral witness but as a person giving specific kinds of advice to someone of a particular age with manifest questions and desires (Clifford, 1986b : 107).

What of the two kinds of authorial authority—‘objective’ and insider? Are they being blurred or just blended or actually collapsed? With intent or by accident?

Certainly the nature of the 'text' has changed: it is both more and less than autobiography; there is an essential 'agreement' between speaker and recorder who are, at the same time, also the researched and the researcher, respectively—a crucial point respecting authorial authority?

This makes the cover and frontispiece of *Writing Culture* all the more interesting (Fig. 13)

The question of authorship has changed again, no longer is it either "where is the author?" or "who is the author?"—we see the answers to those; it is: *what is he writing?* An ethnographic text? his diary? a poem? (Clifford, 1986: 1).

Then there is the irony: What adorns the prominent post-modern anthropology text, given to wage warfare against "the constructed, artificial nature of cultural accounts" (Clifford, 1986a: 2)? Answer: the post-modern ethnographer himself with his back to the native, his head buried in his writing while in the field. It's a zany 'as if' admixture of Malinowski and Frazer.

So in this respect it is the flip side of *Nisa*. Yet what Clifford said (above) of *Nisa*, cannot that also be said of Tylor? He sits there "not as a neutral witness but as a person giving specific kinds of advice to [some people] with manifest questions and desires."

But the cover is honest—if unintentionally so. Have we not all written-up our notes on "Them" or written a letter "home," as "They" squat in the shadows? (Fig. 14) However, its promotion as anthropological praxis is what worries.

It is at this juncture that we may want to remind ourselves of what we took to be *the mandate* of anthropology. I find it appropriate to cite Lévi-Strauss (1966:123):

Our science arrived at maturity the day that Western man began to see that he would never understand himself as long as there was a single race or people on the surface of the earth that he treated as an object.

I want to close by briefly reviewing *advocacy* as an issue of authorial authority for anthropologists—and my juxtaposing remarks on post-modernism

and advocacy is deliberate. Post-modernism can bring much good to anthropology, however all that would be outweighed should its concern with epistemology and hermeneutics exclude a concern with ideology and politics. The two should wax complementally (Scholte 1971; Keesing 1987; Caplan 1988; Myers 1988): "I must know on whose behalf and to what end I write" (Strathern, 1987: 269).

With advocacy, the issue can be starkly presented. I draw upon comments of colleagues from two recent colloquia. The anthropologist finds him- or herself in court as an expert witness: "[And]

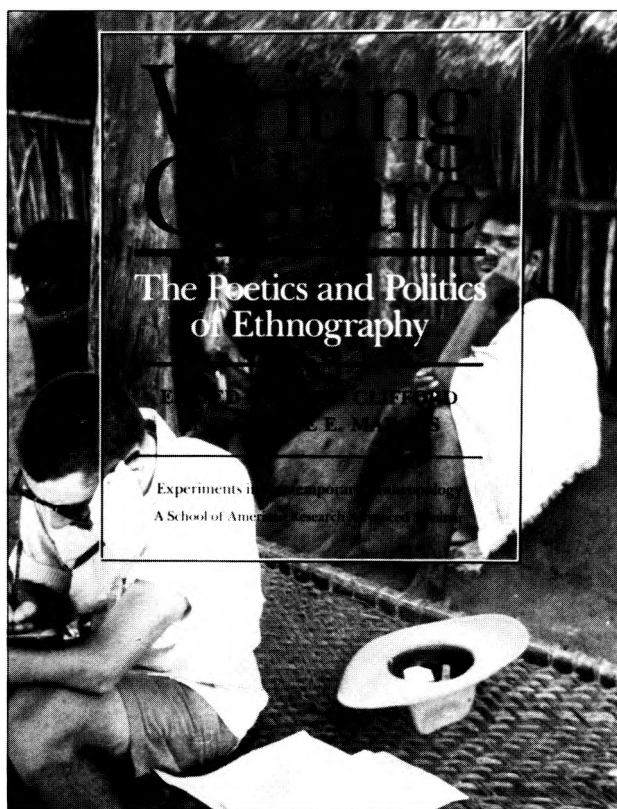


Fig. 13 Cover *Writing Culture* edited by James Clifford and George E. Marcus. University of California Press. First edition 1986.



Fig. 14 Cora DuBois, Alor, Netherlands East India (Indonesia) 1939, Peabody Museum, Harvard University; photograph by unknown.

what if you were to stand up in court and say 'I know' and the lawyer asks: 'how do you know?'" (Wallman, 1985:13-15). To be an advocate, warns another colleague, "is to face the charge of fundamental bias in the presentation of facts" (Harries-Jones, 1985:1). "Our main concern," says yet another "is to get the principle of self-definition accepted. It's not up to me to say who is a Shavante and who isn't" (Maybury-Lewis, 1985:146).

In sum: much turns on how we use our authorial authority as anthropologists to buttress *credibility for the authorial authority of the Native Other* (Fig. 15).

The pitfalls are various—I call attention to just those that immediately come to mind. First, *hubris*—Marvin Harris (himself associated with a good share of hubris) puts it well:

It is a dangerous step, too often taken, to move from being aware that one knows more about a people's culture than anyone else to believing and advocating that one also knows what is best for the people (n.d.).

This may happen through a 'missionary' attitude or through 'chauvinistic' embracement of the Native culture. It is all too easy for us to create some scheme (on behalf of the Native Other) that cannot exist without us. We build, so often, 'models of' native life in an *as if* native World, instead of 'models for' Native life in a world that is both non-Native and Native.

Another pitfall is not of the anthropologist's making—and we have probably all run into it. The Native culture or community is factionalized and each faction has its leader—some of whom will excel

as ethnopoliticians, and each of them, let us say, has a particular delineation of the question "who is a Shavante and who isn't." How, then, is the anthropologist to make the case for native authorial authority? This seems to me to be likely more undermining than arguing (through legal procedure) with another anthropologist, each on behalf of a native claim (e.g. Hopi versus Navaho).

But to speak of factions, while true, perhaps leads us away from a more important point: the pitfall (which *is* of the anthropologist's making) of the culture-as-text-position that disregards the probability that "different [people] read the text of their own culture differently" (Pinxten, 1987:172).

Beyond that we should think of cultures, not a culture. Of Others, not *the* Other. I think this applies in general terms, not just specifically to advocacy research, though there it may be more obvious: the 'cultures' of different 'factions,' and these each in their relation to each of the 'cultures' of the dominant society—trader, missionary, policeman, welfare officer, anthropologist, etc.



Fig. 15 Cover *The Politics of Indianness*. Edited by Adrian Tanner. Institute of Social and Economic Research, Memorial University of Newfoundland. Field research 1970s; first edition 1983.

Why do I drag in this point? Because I believe that if anthropology is to have authorial authority in the future, it must have this *interlocutionary* component as well. I see this as meaning another change—amounting even to a reversal—in the praxis of participant observation. Rather than we anthropologists participating in *their* activities (and how far do most of us ever do that, really?) and observing them through our participation, we should acknowledge our place(s) as bystander (stranger/guest/friend/spy/perhaps confidante) and draw the most out of it, namely, by *watching their participant observation in*

*their own culture*³—and thus in the ‘making of’ (i.e. changing of) it.

Because we are ‘re-presenting’ this activity of theirs, we don’t suppose that we are bearers of *the* truth or *the* reality. But the task that we do usefully fulfill, in this way, is precisely that of Them-Us interlocation, both in respect of Them and Uss within the society itself, and between ‘it’ and institutions and persons external to it.

But it won’t be easy:



Fig. 16 Gahan Wilson *The New Yorker* ©1987

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Acknowledgements

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Notes

1. At the Anthropology and Sociology Departmental Seminar, University of British Columbia. My thanks to faculty and graduate students for their lively participation. Principally on account of constraints of space, I have kept to my own text here and not attempted to incorporate controversies of the discussion that followed.

An earlier version was presented to the Institute of Social and Economic Research (Memorial University) 1988 seminar series "Social Scientists and the Real World: Do We Make a Difference?"

2. Hastrup herself puts the stress on "*we*," I hear it on "*stage*."

3. Their participant observation will be *differentiated* reflecting, besides factional commitments, any number of other social/cultural divisions; sometimes, indeed, "the culture is to a great extent in the custody of men trained in erudition and dialectic and is continually set forth by them for the instruction of the majority," as Gregory Bateson reminded us (cited in Keesing, 1987:164).

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