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On Ethnographic Authority*

THE 1724 FRONTISPIECE of Father Lafitau's *Moeurs des sauvages amériquains* portrays the ethnographer as a young woman sitting at a writing table amidst artifacts from the New World and from classical Greece and Egypt. The author is accompanied by two cherubs who assist in the task of comparison and by the bearded figure of Time who points toward a tableau representing the ultimate source of the truths issuing from the writer's pen. The image toward which the young woman lifts her gaze is a bank of clouds where Adam, Eve and the serpent appear. Above them stand the redeemed man and woman of the Apocalypse on either side of a radiant triangle bearing the Hebrew script for Yahweh.

The frontispiece for Malinowski's *Argonauts of the Western Pacific* is a photograph with the caption "A Ceremonial Act of the Kula." A shell necklace is being offered to a Trobriand chief who stands at the door of his dwelling. Behind the man presenting the necklace is a row of six bowing youths, one of them sounding a conch. All the figures stand in profile, their attention apparently concentrated on the rite of exchange, a real event of Melanesian life. But on closer inspection one of the bowing Trobrianders may be seen to be looking at the camera.

Lafitau's allegory is the less familiar: his author transcribes rather than originates. Unlike Malinowski's photo, the engraving makes no reference to ethnographic experience—despite Lafitau's five years of research among the Mohawks, research that has earned him a respected place among the fieldworkers of any generation. His account is presented not as the product of first-hand observation but of writing, in a crowded workshop. The frontispiece from *Argonauts*, like all photographs, asserts presence, that of the scene before the lens. But it suggests also another presence—the ethnographer actively composing this fragment of Trobriand reality. Kula exchange, the subject of Malinowski's book, has been made perfectly visible, centered in the perceptual frame. And a participant's glance redirects our attention to the observational standpoint we share, as readers, with the ethnographer and his camera. The predominant mode of modern fieldwork authority is signaled: "You are there, because I was there."

The present essay traces the formation and breakup of this authority in twentieth century social anthropology. It is not a complete account, nor is it based on a fully realized theory of ethnographic interpretation and textuality.¹ Such a theory's contours are problematic, since the activity of cross cultural representation is now more than usually in question. The present predicament is linked to the breakup and redistribution of colonial power in the decades after 1950 and to the echoes of

that process in the radical cultural theories of the 1960s and 1970s. After the Negritude movement's reversal of the European gaze, after anthropology's *crise de conscience* with respect to its liberal status within the imperial order, and now that the West can no longer present itself as the unique purveyor of anthropological knowledge about others, it has become necessary to imagine a world of generalized ethnography. With expanded communication and intercultural influence, people interpret others, and themselves, in a bewildering diversity of idioms—a global condition of what Bakhtin called “heteroglossia.”² This ambiguous, multi-vocal world makes it increasingly hard to conceive of human diversity as inscribed in bounded, independent cultures. Difference is an effect of inventive syncretism. In recent years works like Edward Said's *Orientalism* and Paulin Hountondji's *Sur la “philosophie africaine”* have cast radical doubt on the procedures by which alien human groups can be represented, without proposing systematic, sharply new methods or epistemologies. These studies suggest that while ethnographic writing cannot entirely escape the reductionist use of dichotomies and essences, it can at least struggle self-consciously to avoid portraying abstract, a-historical “others.”³ It is more than ever crucial for different peoples to form complex concrete images of one another, as well as of the relationships of knowledge and power that connect them. But no sovereign scientific method or ethical stance can guarantee the truth of such images. They are constituted—the critique of colonial modes of representation has shown at least this much—in specific historical relations of dominance and dialogue.

The experiments in ethnographic writing surveyed below do not fall into a clear reformist direction or evolution. They are *ad hoc* inventions and cannot be seen in terms of a systematic analysis of post-colonial representation. They are perhaps best understood as components of that “toolkit” of engaged theory recently recommended by Deleuze and Foucault.

The notion of theory as a toolkit means (i) The theory to be constructed is not a system but an instrument, a *logic* of the specificity of power relations and the struggles around them; (ii) That this investigation can only be carried out step by step on the basis of reflection (which will necessarily be historical in some of its aspects) on given situations.⁴

We may contribute to a practical reflection on cross cultural representation by undertaking an inventory of the better, though imperfect, approaches currently at hand. Of these, ethnographic fieldwork remains an unusually sensitive method. Participant observation obliges its practitioners to experience, at a bodily as well as intellectual level, the vicissitudes of translation. It requires arduous language learning, some degree of direct involvement and conversation, and often a derangement of personal and cultural expectations. There is, of course, a myth of fieldwork, and the actual experience, hedged around with contingencies, rarely lives up to the ideal. But as a means for producing knowledge from an intense, intersubjective engagement, the practice of ethnography retains a certain exemplary status. Moreover, if fieldwork has for a time been identified with a uniquely Western discipline and a totaliz-

ing science of “anthropology,” these associations are not necessarily permanent. Current styles of cultural description are historically limited and undergoing important metamorphoses.

The development of ethnographic science cannot ultimately be understood in isolation from more general political-epistemological debates about writing and the representation of otherness. However, in the present discussion I have maintained a focus on professional anthropology and specifically on developments within interpretive ethnography since 1950.⁵ The current crisis—or better, dispersion—of ethnographic authority makes it possible to mark off a rough period, bounded by the years 1900 and 1960, during which a new conception of field research established itself as the norm for European and American anthropology. Intensive fieldwork, pursued by university trained specialists, emerged as a privileged, sanctioned source of data about exotic peoples. It is not a question, here, of the dominance of a single research method. “Intensive” ethnography was variously defined.⁶ Moreover, the hegemony of fieldwork was established earlier and more thoroughly in America and England than it was in France. The early examples of Boas and the Torres Straits Expedition were matched only belatedly by the founding of the Institut d’Ethnologie in 1925 and the much-publicized Mission Dakar-Djibouti of 1932.⁷ Nevertheless, by the mid-1930s one can fairly speak of a developing international consensus: valid anthropological abstractions were to be based, wherever possible, on intensive cultural descriptions by qualified scholars. By the mid-1930s the new style had been made popular, institutionalized, and embodied in specific textual practices.

It has recently become possible to identify and take a certain distance from these conventions.⁸ If ethnography produces cultural interpretations through intense research experiences, how is unruly experience transformed into an authoritative written account? How, precisely, is a garrulous, overdetermined, cross cultural encounter shot through with power relations and personal cross purposes circumscribed as an adequate version of a more-or-less discrete “other world,” composed by an individual author?

In analyzing this complex transformation one must bear in mind the fact that ethnography is from beginning to end enmeshed in writing. This writing includes, minimally, a translation of experience into textual form. The process is complicated by the action of multiple subjectivities and political constraints beyond the control of the writer. In response to these forces ethnographic writing enacts a specific strategy of authority. This has classically involved an unquestioned claim to appear as the purveyor of truth in the text. A complex cultural experience is enunciated by an individual: *We the Tikopia*, by Raymond Firth; *Nous avons mangé la forêt*, by Georges Condominas; *Coming of Age in Samoa*, by Margaret Mead; *The Nuer*, by Evans-Pritchard.

The discussion that follows first locates this authority historically—in the development of a twentieth-century science of participant-observation. It then proceeds to a critique of underlying assumptions and a review of emerging textual practices.

Alternate strategies of ethnographic authority may be seen in recent experiments by ethnographers who self-consciously reject scenes of cultural representation in the style of Malinowski's frontispiece. Different secular versions of Lafitau's crowded scriptorial workshop are emerging. In the new paradigms of authority the writer is no longer fascinated by transcendent figures—by a Hebrew-Christian deity or its twentieth-century replacements, “Man” and “culture.” Nothing remains of the heavenly tableau except the anthropologist's scumbled image in a mirror. And the silence of the ethnographic workshop has been broken—by insistent, heteroglot voices, by the scratching of other pens.⁹

At the close of the nineteenth century nothing guaranteed, *a priori*, the ethnographer's status as the best interpreter of native life—as opposed to the traveller, and especially to the missionary and administrator, some of whom had been in the field far longer and had better research contacts and linguistic skills. The development of the fieldworker's image in America, from Cushing (an oddball) to Margaret Mead (a national figure) is significant. During this period a particular form of authority was created, an authority both scientifically validated and based on a unique personal experience. During the 1920s Malinowski played a central role in establishing credit for the fieldworker, and we should recall in this light his attacks on the competence of competitors in the field. For example, the colonial magistrate Rentoul, who had the temerity to contradict science's findings concerning Trobriand conceptions of paternity, was excommunicated in the pages of *Man* for his unprofessional “police court perspective.” The attack on amateurism in the field was pressed even further by Radcliffe-Brown who, as Ian Langham has shown, came to epitomize the scientific professional, discovering rigorous social laws, etc.¹⁰ What emerged during the first half of the twentieth century, with the success of professional fieldwork, was a new fusion of general theory and empirical research, of cultural analysis with ethnographic description.

The fieldworker-theorist replaced an older partition between the “man on the spot” (in Frazer's words) and the sociologist or anthropologist in the metropole. This division of labor varied in different national traditions. In America, for example, Morgan had personal knowledge of at least some of the cultures that were raw material for his sociological syntheses; and Boas, rather earlier than elsewhere, made intensive fieldwork the *sine qua non* of serious anthropological discourse. But in general, before Malinowski, Radcliffe-Brown, and Mead had successfully established the norm of the university trained scholar testing and deriving theory from first-hand research, a rather different economy of ethnographic knowledge prevailed. For example, Codrington's *The Melanesians* (1891) is a detailed compilation of folklore and custom, drawn from a relatively long term of research as an evangelist and based on intensive collaboration with indigenous translators and informants. The book is not organized around a fieldwork “experience,” and it does not advance a unified interpretative hypothesis, functional, historical or otherwise. It is content

with low level generalizations and the amassing of an eclectic range of information. Codrington is acutely aware of the incompleteness of his knowledge, believing that real understanding of native life begins only after a decade or so of experience and study.¹¹ This understanding of the difficulty of grasping the world of alien peoples—the many years of learning and unlearning needed, the problems of acquiring a thorough linguistic competence—tends to dominate the most serious ethnographic work of Codrington's generation. But such assumptions would soon be challenged by the more confident cultural relativism of the Malinowskian model. The new fieldworkers sharply distinguished themselves from the earlier "men on the spot," the missionary, the administrator, the trader, and the traveller, whose knowledge of indigenous peoples, they argued, was not informed by the best scientific hypotheses or a sufficient neutrality.

Before the emergence of professional ethnography, writers like McLennan, Lubbock, and Tylor had attempted to control the quality of the reports on which their anthropological syntheses were based. They did this by means of the guidelines of *Notes and Queries* and, in Tylor's case, by cultivating long-term working relations with sophisticated researchers in the field like the missionary Lorimer Fison. After 1883, as newly appointed Reader in Anthropology at Oxford, Tylor worked to encourage the systematic gathering of ethnographic data by qualified professionals. The United States Bureau of Ethnology, already committed to the undertaking, provided a model. Tylor was active in founding a Committee on the North-Western Tribes of Canada. The Committee's first agent in the field was the nineteen-year veteran missionary among the Ojibwa, E. F. Wilson. He was replaced, before long, by Franz Boas, a physicist in the process of turning to professional ethnography. George Stocking has persuasively argued that the replacement of Wilson by Boas "marks the beginning of an important phase in the development of British ethnographic method: the collection of data by academically trained natural scientists defining themselves as anthropologists, and involved also in the formulation and evaluation of anthropological theory." With Boas' early survey work and the emergence in the 1890s of other natural-scientist fieldworkers like A. C. Haddon and Baldwin Spencer, the move toward professional ethnography was underway. The Torres Straits Expedition of 1899 may be seen as a culmination of the work of this "intermediate generation," as Stocking calls them. The new style of research was clearly different from that of missionaries and other amateurs in the field, and part of a general trend, since Tylor, "to draw more closely together the empirical and theoretical components of anthropological inquiry."¹²

However, the establishment of intensive participant-observation as a professional norm would have to await the Malinowskian cohort. The "intermediate generation" of ethnographers did not, typically, live in a single locale for a year or more, mastering the vernacular and undergoing a personal learning experience comparable to an initiation. They did not speak as cultural insiders, but retained the natural scientist's documentary, observational stance. The principal exception, before the

third decade of the century, Frank Hamilton Cushing, remained an isolated instance. As Curtis Hinsley has suggested, Cushing's long first-hand study of the Zunis, his quasi-absorption into their way of life, "raised awkward problems of verification and accountability. . . . A community of scientific anthropology on the model of other sciences required a common language of discourse, channels of regular communication, and at least minimal consensus on judging method."¹³ Cushing's intuitive, excessively personal understanding of the Zuni could not confer scientific authority.

Schematically put, before the late nineteenth century the ethnographer and the anthropologist, the describer/translator of custom and the builder of general theories about humanity, were distinct. (A clear sense of the *tension* between ethnography and anthropology is important in correctly perceiving the recent, and perhaps temporary, conflation of the two projects.) Malinowski gives us the image of the new "anthropologist"—squatting by the campfire, looking, listening and questioning, recording, and interpreting Trobriand life. The literary charter of this new authority is the first chapter of *Argonauts*, with its prominently displayed photographs of the ethnographer's tent pitched among Kiriwinian dwellings. The sharpest methodological justification for the new mode is to be found in Radcliffe-Brown's *Andaman Islanders*. The two books were published within a year of each other. And although their authors developed quite different fieldwork styles and visions of cultural science, both early texts provide explicit arguments for the special authority of the ethnographer-anthropologist.

Malinowski, as his notes for the crucial Introduction to *Argonauts* show, was greatly concerned with the rhetorical problem of convincing his readers that the facts he was putting before them were objectively acquired, not subjective creations.¹⁴ Moreover, he was fully aware that "In Ethnography, the distance is often enormous between the brute material of information—as it is presented to the student in his own observations, in native statement, in the kaleidoscope of tribal life—and the final authoritative presentation of the results."¹⁵ Stocking has nicely analyzed the various literary artifices of *Argonauts* (its engaging narrative constructs, use of active voice in the "ethnographic present," illusive dramatizations of the author's participation in scenes of Trobriand life) techniques Malinowski used so that "his own experience of the natives' experience (might) become the reader's experience as well."¹⁶ The problems of verification and accountability that had relegated Cushing to the professional margin were very much on Malinowski's mind. This anxiety is reflected in the mass of data contained in *Argonauts*, its sixty-six photographic plates, the now rather curious "Chronological list of Kula Events Witnessed by the Writer," the constant alternation between impersonal description of typical behavior and statements on the order of "I witnessed . . .," and "Our party, sailing from the North. . . ."

Argonauts is a complex narrative, simultaneously of Trobriand life and of ethnographic fieldwork. It is archetypical of the generation of ethnographies that suc-

cessfully established participant-observation's scientific validity. The story of research built into *Argonauts*, into Mead's popular work on Samoa, into *We the Tikopia*, became an implicit narrative underlying all professional reports on exotic worlds. If subsequent ethnographies did not need to include developed fieldwork accounts, it was because such accounts were assumed, once a statement was made on the order of, for example, Godfrey Lienhardt's single sentence at the beginning of *Divinity and Experience*: "This book is based upon two years' work among the Dinka, spread over the period 1947–1950."¹⁷

In the 1920s, the new fieldworker-theorist brought to completion a powerful new scientific and literary genre, the ethnography, a synthetic cultural description based on participant-observation.¹⁸ The new style of representation depended on institutional and methodological innovations circumventing the obstacles to rapid knowledge of other cultures that had preoccupied the best representatives of Codrington's generation. These may be briefly summarized.

First, the persona of the fieldworker was validated, both publicly and professionally. In the popular domain, visible figures like Malinowski, Mead, and Griaule communicated a vision of ethnography as both scientifically demanding and heroic. The professional ethnographer was trained in the latest analytic techniques and modes of scientific explanation. This conferred an advantage over amateurs in the field: the professional could claim to get to the heart of a culture more quickly, grasping its essential institutions and structures. A prescribed attitude of cultural relativism distinguished the fieldworker from missionaries, administrators, and others whose view of natives was, presumably, less dispassionate, who were preoccupied with the problems of government, or conversion. In addition to scientific sophistication and relativist sympathy, a variety of normative standards for the new form of research emerged: the fieldworker was to live in the native village, use the vernacular, stay a sufficient (but seldom specified) length of time, investigate certain classic subjects, and so on.

Second: it was tacitly agreed that the new-style ethnographer, whose sojourn in the field seldom exceeded two years, and more frequently was much less, could efficiently "use" native languages without "mastering" them. In a significant article of 1939 Margaret Mead argued that the ethnographer following the Malinowskian prescription to avoid interpreters and to conduct research in the vernacular did not, in fact, need to attain "virtuosity" in native tongues, but could "use" the vernacular to ask questions, maintain rapport, and generally get along in the culture while obtaining good research results in particular areas of concentration.¹⁹ This, in effect, justified her own practice, which featured relatively short stays and a focus on specific domains, like childhood, or "personality." These foci would function as "types" for a cultural synthesis. But her attitude toward language "use" was broadly characteristic of an ethnographic generation that could, for example, credit an authoritative study called *The Nuer*, that was based on only eleven months of difficult research.

Mead's article provoked a sharp response from Robert Lowie, writing from the older Boasian tradition, more philological in its orientation.²⁰ But his was a rear-guard action; the point had been generally established that valid research could, in practice, be accomplished on the basis of a one or two-year familiarity with a foreign vernacular (even though, as Lowie suggested, no one would credit a translation of Proust that was based on an equivalent knowledge of French).

Third: the new ethnography was marked by an increased emphasis on the power of observation. Culture was construed as an ensemble of characteristic behaviors, ceremonies and gestures, susceptible to recording and explanation by a trained onlooker. Mead pressed this point furthest (indeed, her own powers of visual analysis were extraordinary). As a general trend the participant-*observer* emerged as a research norm. Of course, successful fieldwork mobilized the fullest possible range of interactions, but a distinct primacy was accorded to the visual: interpretation was tied to description. After Malinowski, a general suspicion of "privileged informants" reflected this systematic preference for the (methodical) observations of the ethnographer over the (interested) interpretations of indigenous authorities.

Fourth: certain powerful theoretical abstractions promised to help academic ethnographers "get to the heart" of a culture more rapidly than someone undertaking, for example, a thorough inventory of customs and beliefs. Without spending years getting to know natives, their complex languages and habits, in intimate detail, the researcher could go after selected data that would yield a central armature of structure of the cultural whole. Rivers' "genealogical method," followed by Radcliffe-Brown's model of "social structure," provided this sort of shortcut. One could, it seemed, elicit kin terms without a deep understanding of local vernacular, and the range of necessary contextual knowledge was conveniently limited.

Fifth: since culture, seen as a complex whole, was always too much to master in a short research span, the new ethnographer tended to focus thematically on particular institutions. The aim was not to contribute to a complete inventory or description of custom, but rather to get at the whole through one or more of its parts. We have noted the privilege given, for a time, to social structure. An individual life-cycle, a ritual complex like the Kula ring or the Naven ceremony could also serve, as could categories of behavior like "economics," "politics," and the like. In the predominantly synecdochic rhetorical stance of the new ethnography, parts were assumed to be microcosms or analogies of wholes. This setting of institutional foregrounds against cultural backgrounds in the portrayal of a coherent world lent itself to realist literary conventions.

Sixth: the wholes thus represented tended to be synchronic, products of short-term research activity. The intensive fieldworker could plausibly sketch the contours of an "ethnographic present"—the cycle of a year, a ritual series, patterns of typical behavior. To introduce long-term historical inquiry would have impossibly complicated the task of the new-style fieldwork. Thus, when Malinowski and Radcliffe-

Brown established their critique of the “conjectural history” of the diffusionists it was all too easy to exclude diachronic processes as objects of fieldwork, with consequences that have by now been sufficiently denounced.

These innovations served to validate an efficient ethnography based on scientific participant-observation. Their combined effect may be seen in what may well be the *tour de force* of the new ethnography, Evans-Pritchard's *The Nuer*, published in 1940. Based on eleven months of research conducted—as the book's remarkable introduction tells us—in almost impossible conditions, Evans-Pritchard nonetheless was able to compose a classic. He arrived in Nuerland on the heels of a punitive military expedition and at the urgent request of the government of the Anglo-Egyptian Sudan. He was the object of constant and intense suspicion. Only in the final few months could he converse at all effectively with informants who, he tells us, were skilled at evading his questions. In the circumstances his monograph is a kind of miracle.

While advancing limited claims and making no secret of the restraints on his research, Evans-Pritchard manages to present his study as a demonstration of the effectiveness of theory. He focuses on Nuer political and social “structure,” analyzed as an abstract set of relations between territorial segments, lineages, age-sets, and other more fluid groups. This analytically derived ensemble is portrayed against an “ecological” backdrop composed of migratory patterns, relationships with cattle, notions of time and space. Evans-Pritchard sharply distinguishes his method from what he calls “haphazard” (Malinowskian) documentation. *The Nuer* is not an extensive compendium of observations and vernacular texts in the style of Malinowski's *Argonauts* and *Coral Gardens*. Evans-Pritchard argues rigorously that “facts can only be selected and arranged in the light of theory.” The frank abstraction of a political-social structure offers the necessary framework. If I am accused of describing facts as exemplifications of my theory, he then goes on to note, I have been understood.²¹

In *The Nuer*, Evans-Pritchard makes strong claims for the power of scientific abstraction to focus research and arrange complex data. The book often presents itself as an argument, rather than a description. But not consistently: its theoretical argument is surrounded by skillfully observed and narrated evocations and interpretations of Nuer life. These passages function rhetorically as more than simple “exemplifications,” for they effectively implicate readers in the complex subjectivity of participant-observation. This may be seen in a characteristic paragraph which progresses through a series of discontinuous discursive positions:

It is difficult to find an English word that adequately describes the social position of *diel* in a tribe. We have called them aristocrats, but do not wish to imply that Nuer regard them as of superior rank, for, as we have emphatically declared, the idea of a man lording it over others is repugnant to them. On the whole—we will qualify the statement later—the *diel* have prestige rather than rank and influence rather than power. If you are a *dil* of the tribe in which

you live you are more than a simple tribesman. You are one of the owners of the country, its village sites, its pastures, its fishing pools and wells. Other people live there by virtue of marriage into your clan, adoption into your lineage, or of some other social tie. You are a leader of the tribe and the spear-name of your clan is invoked when the tribe goes to war. Whenever there is a *dil* in the village, the village clusters around him as a herd of cattle clusters around its bull.²²

The first three sentences are presented as an argument about translation, but in passing they attribute to “Nuer” a stable set of attitudes. (I will have more to say later about this style of attribution.) Next, in the four sentences beginning “If you are a *dil* . . .”, the second-person construction brings together reader and native in a textual participation. The final sentence, offered as a direct description of a typical event (which the reader now assimilates from the standpoint of a participant-observer) evokes the scene by means of Nuer cattle metaphors. In the paragraph’s eight sentences an argument about translation passes through a fiction of participation to a metaphorical fusion of external and indigenous cultural descriptions. The subjective joining of abstract analysis and concrete experience is accomplished.

Evans-Pritchard would later move away from the theoretical position of *The Nuer*, rejecting its advocacy of “social structure” as a privileged framework. Indeed, each of the fieldwork “shortcuts” enumerated above was, and remains, contested. Yet by their deployment in different combinations, the authority of the academic field-worker-theorist was established in the years between 1920 and 1950. This peculiar amalgam of intense personal experience and scientific analysis (understood in this period as *both* “rite of passage” and “laboratory”) emerged as a method: participant-observation. Though variously understood, and now disputed in many quarters, this method remains the chief distinguishing feature of professional anthropology. Its complex subjectivity is routinely reproduced in the writing and reading of ethnographies.

“Participant-observation” serves as shorthand for a continuous tacking between the “inside” and “outside” of events: on the one hand grasping the sense of specific occurrences and gestures empathetically, on the other stepping back to situate these meanings in wider contexts. Particular events thus acquire deeper or more general significance, structural rules, and so forth. Understood literally, participant-observation is a paradoxical, misleading formula. But it may be taken seriously if reformulated in hermeneutic terms as a dialectic of experience and interpretation. This is how the method’s most persuasive recent defenders have restated it, in the tradition that leads from Dilthey, via Weber, to “symbols and meanings anthropologists” like Geertz. Experience and interpretation have, however, been accorded different emphases when presented as claims to authority. In recent years, there has been a marked shift of emphasis from the former to the latter. This section and the one that follows will explore the rather different claims of experience and interpretation as well as their evolving interrelation.

The growing prestige of the fieldworker-theorist downplayed (without eliminating) a number of processes and mediators that had figured more prominently in previous methods. We have seen how language mastery was defined as a level of use adequate for amassing a discrete body of data in a limited period of time. The tasks of textual transcription and translation along with the crucial dialogical role of interpreters and “privileged informants” were relegated to a secondary, sometimes even despised, status. Fieldwork was now centered on the *experience* of the participant-observing scholar. A sharp image, or narrative, made its appearance—that of an outsider entering a culture, undergoing a kind of initiation leading to “rapport” (minimally, acceptance and empathy, but usually implying something akin to friendship). Out of this experience emerged, in unspecified ways, a representational text authored by the participant-observer. As we shall see, this version of textual production obscures as much as it reveals. But it is worth taking seriously its principal assumption, that the experience of the researcher can serve as a unifying source of authority in the field.

Experiential authority is based on a “feel” for the foreign context, a kind of accumulated savvy and sense of the style of a people or place. Such an appeal is frequently explicit in the texts of the early professional participant-observers. Margaret Mead’s claim to grasp the underlying principle or ethos of a culture through a heightened sensitivity to form, tone, gesture, and behavioral styles, or Malinowski’s stress on his life *in* the village and the comprehension derived from the “imponderabilia” of daily existence, are prominent cases in point. Many ethnographies, Colin Turnbull’s *The Forest People* for example, are still cast in the experiential mode, asserting, prior to any specific research hypothesis or method, the “I was there” of the ethnographer as insider and participant.

Of course, it is difficult to say very much about experience. Like “intuition” one has it or not, and its invocation often smacks of mystification. Nevertheless one should resist the temptation to translate all meaningful experience into interpretation. If the two are reciprocally related, they are not identical. It makes sense here to hold them apart, if only because appeals to experience often act as validations for ethnographic authority. The most serious argument for the role of experience in the historical and cultural sciences is contained in the general notion of *Verstehen*.²³ In Dilthey’s influential view, understanding others arises initially from the sheer fact of coexistence in a shared world. But this experiential world, an intersubjective ground for objective forms of knowledge, is precisely what is missing or problematic for an ethnographer entering an alien culture. Thus during the early months in the field (and indeed throughout the research) what is going on is language-learning in the broadest sense. Dilthey’s “common sphere” must be established and re-established, building up a shared experiential world in relation to which all “facts,” “texts,” “events,” and their interpretations will be constructed. This process of living one’s way into an alien expressive universe is, in his scheme, always subjective in nature. But it quickly becomes dependent on what he calls “permanently fixed expressions,”

stable forms to which understanding can return. The exegesis of these fixed forms provides the content of all systematic historical-cultural knowledge. Thus experience, for Dilthey, is closely linked to interpretation (and he is among the first modern theorists to compare the understanding of cultural forms to the reading of "texts"). But this sort of reading or exegesis cannot occur without an intense, personal participation, an active at-homeness in a common universe.²⁴

Following Dilthey, ethnographic "experience" can be seen as the building-up of a common, meaningful world, drawing on intuitive styles of feeling, perception, and guesswork. This activity makes use of clues, traces, gestures, and scraps of sense prior to the development of developed, stable interpretations. Such piecemeal forms of experience may be classified as esthetic and/or divinatory. There is space here for only a few words about such styles of comprehension as they relate to ethnography. An evocation of an esthetic mode is conveniently provided by A. L. Kroeber's 1931 review of Mead's *Growing up in New Guinea*.

First of all, it is clear that she possesses to an outstanding degree the faculties of swiftly apperceiving the principal currents of a culture as they impinge on individuals, and of delineating these with compact pen-pictures of astonishing sharpness. The result is a representation of quite extraordinary vividness and semblance to life. Obviously, a gift of intellectualized but strong sensationalism underlies this capacity; also, obviously, a high order of intuitiveness, in the sense of the ability to complete a convincing picture from clues, for clues is all that some of her data can be, with only six months to learn a language and enter the inwards of a whole culture, besides specializing on child behavior. At any rate, the picture, so far as it goes, is wholly convincing to the reviewer, who unreservedly admires the sureness of insight and efficiency of stroke of the depiction.²⁵

A different formulation is provided by Maurice Leenhardt in *Do Kamo: Person and Myth in the Melanesian World*, a book which, in its sometimes cryptic mode of exposition, requires of its readers just the sort of esthetic, gestaltist perception at which both Mead and Leenhardt excelled. Leenhardt's endorsement of this approach is significant since, given his extremely long field experience and profound cultivation of a Melanesian language, his "method" cannot be seen as a rationalization for short-term ethnography.

In reality, our contact with another is not accomplished through analysis. Rather, we apprehend him in his entirety. From the outset, we can sketch our view of him using an outline or symbolic detail which contains a whole in itself and evokes the true form of his being. This latter is what escapes us if we approach our fellow creature using only the categories of our intellect.²⁶

Another way of taking experience seriously as a source of ethnographic knowledge is provided by Carlo Ginzburg's recent investigations into the complex tradition of divination.²⁷ His research ranges from early hunters' interpretations of animal tracks, to Mesopotamian forms of prediction, the deciphering of symptoms in Hippocratic medicine, to the focus on details in detecting art forgeries, to Freud, Sherlock Holmes, and Proust. These styles of noncstatic divination apprehend spe-

cific, circumstantial relations of meaning, and are based on guesses, on the reading of apparently disparate clues and “chance” occurrences. Ginzburg proposes his model of “conjectural knowledge” as a disciplined, nongeneralizing mode of comprehension that is of central, though unrecognized, importance for the cultural sciences. It may be added to a rather meager stock of resources for understanding rigorously how one feels one’s way into an unfamiliar ethnographic situation.

Precisely because it is hard to pin down, “experience” has served as an effective guarantee of ethnographic authority. There is, of course, a telling ambiguity in the term. Experience evokes a participatory presence, a sensitive contact with the world to be understood, a rapport with its people, a concreteness of perception. And experience suggests also a cumulative, deepening knowledge (“ . . . her ten years’ experience of New Guinea”). The senses work together to authorize an ethnographer’s real, but ineffable, feel or flair for his or her people. But it is worth noticing that this “world,” when conceived as an experiential creation, is subjective, not dialogical or intersubjective. The ethnographer *accumulates* personal knowledge of the field. (The possessive form, “my people,” has until recently been familiarly used in anthropological circles; but the phrase in effect signifies “my experience.”)

It is understandable, given their vagueness, that experiential criteria of authority—unexamined beliefs in the “method” of participant-observation, in the power of rapport, empathy, and so on—have come under criticism by hermeneutically sophisticated anthropologists. In recent years the second moment in the dialectic of experience and interpretation has received increasing attention and elaboration.²⁸ Interpretation, based on a philological model of textual “reading,” has emerged as a sophisticated alternative to the now apparently naive claims for experiential authority. Interpretive anthropology demystifies much of what had previously passed unexamined in the construction of ethnographic narratives, types, observations, and descriptions. It contributes to an increasing visibility of the creative (and in a broad sense, poetic) processes by which “cultural” objects are invented and treated as meaningful.

What is involved in looking at culture as an assemblage of texts to be interpreted? A classic account has been provided by Paul Ricoeur, notably in his 1971 essay, “The Model of the Text: Meaningful Action Considered as a Text.”²⁹ Clifford Geertz, in a number of stimulating and subtle discussions has adapted Ricoeur’s theory to anthropological fieldwork.³⁰ “Textualization” is understood as a prerequisite to interpretation, the constitution of Dilthey’s “fixed expressions.” It is the process through which unwritten behavior, speech, beliefs, oral tradition or ritual, come to be marked as a corpus, a potentially meaningful ensemble separated out from an immediate discursive or performative situation. In the moment of textualization this meaningful corpus assumes a more or less stable relation to a context, and we are familiar with the end result of this process in much of what counts as

ethnographic thick description. For example, we say that a certain institution or segment of behavior is typical of, or a communicative element within, a surrounding culture. (Geertz's famous cockfight becomes an intensely significant locus of Balinese culture.) Fields of synecdoches are created in which parts are related to wholes—and by which the whole, what we often call culture, is constituted.

Ricoeur does not actually privilege part-whole relations and the specific sorts of analogies that constitute functionalist or realist representations. He merely posits a necessary relation between text and "world." A world cannot be apprehended directly; it is always inferred on the basis of its parts, and the parts must be conceptually and perceptually cut out of the flux of experience. Thus, textualization generates sense through a circular movement which isolates and then contextualizes a fact or event in its englobing reality. A familiar mode of authority is generated which claims to represent discrete, meaningful worlds. Ethnography is the interpretation of cultures.

A second key step in Ricoeur's analysis is his account of the process by which "discourse" becomes text. Discourse, in Benveniste's classic discussion, is a mode of communication where the presence of the speaking subject and of the immediate situation of communication are intrinsic.³¹ Discourse is marked by pronouns (pronounced or implied) "I" and "You," and by deictic indicators, "this," "that," "now," and so on, which signal the present instance of discourse rather than something beyond it. Discourse does not transcend the specific occasion in which a subject appropriates the resources of language in order to communicate dialogically. Ricoeur argues that discourse cannot be interpreted in the open-ended, potentially public way that a text is "read." To understand discourse you "had to have been there," in the presence of the discoursing subject. For discourse to become text it must become "autonomous," in Ricoeur's terms, separated from a specific utterance and authorial intention. Interpretation is not interlocution. It does not depend on being in the presence of a speaker.

The relevance of this distinction for ethnography is perhaps too obvious. The ethnographer always ultimately departs, taking away texts for later interpretation. (And among those "texts" taken away we can include memories—events patterned, simplified, stripped of immediate context in order to be interpreted in later reconstruction and portrayal.) The text, unlike discourse, can travel. If much ethnographic writing is produced in the field, actual composition of an ethnography is done elsewhere. Data constituted in discursive, dialogical conditions are appropriated only in textualized form. Research events and encounters become field notes. Experiences become narratives, meaningful occurrences, or examples.

This translation of the research experience into a textual corpus separate from its discursive occasions of production has important consequences for ethnographic authority. The data thus reformulated need no longer be understood as the communication of specific persons. An informant's explanation or description of custom

need not be cast in a form that includes the message “so and so said this.” A textualized ritual or event is no longer closely linked to the production of that event by specific actors. Instead, these texts become evidences of an englobing context, a “cultural” reality. Moreover, as specific authors and actors are severed from their productions, a generalized “author” must be invented to account for the world or context within which the texts are fictionally relocated. This generalized author goes under a variety of names: the native point of view, “the Trobrianders,” “the Nuer,” “the Dogon,” as these and similar phrases appear in ethnographies. “The Balinese” function as author of Geertz’s textualized cockfight.

The ethnographer thus enjoys a special relationship with a cultural origin, or “absolute subject.”³² It is tempting to compare the ethnographer with the literary interpreter (and this comparison is increasingly commonplace)—but more specifically with the traditional critic, who sees the task at hand as locating the unruly meanings of a text in a single, coherent intention. By representing the Nuer, the Trobrianders, or the Balinese as whole subjects, sources of a meaningful intention, the ethnographer transforms the research situation’s ambiguities and diversities of meaning into an integrated portrait. But it is important to notice what has dropped out of sight. The research process is separated from the texts it generates and from the fictive world they are made to call up. The actuality of discursive situations and individual interlocutors is filtered out. But informants—along with field notes—are crucial intermediaries, typically excluded from authoritative ethnographies. The dialogical, situational aspects of ethnographic interpretation tend to be banished from the final representative text. Not entirely banished, of course; there exist approved *topoi* for the portrayal of the research process.

We are increasingly familiar with the separate fieldwork account (a sub-genre which still tends to be classified as subjective, “soft,” or unscientific). But even within classic ethnographies, more or less stereotypic “fables of rapport” narrate the attainment of full participant-observer status. These fables may be told elaborately or in passing, naively or ironically. They normally portray the ethnographer’s early ignorance, misunderstandings, lack of contact, frequently a sort of childlike status within the culture. In the *Bildungsgeschichte* of the ethnography these states of innocence or confusion are replaced by adult, confident, disabused knowledge. We may cite again Geertz’s cockfight, where an early alienation from the Balinese, a confused, “non-person” status, is transformed by the appealing fable of the police raid with its show of complicity.³³ The anecdote establishes a presumption of connectedness which permits the writer to function in his subsequent analyses as an omnipresent, knowledgeable exegete and spokesman. This interpreter situates the ritual sport as a text in a contextual world and brilliantly “reads” its cultural meanings. Geertz’s abrupt disappearance into his rapport—the quasi-invisibility of participant-observation—is paradigmatic. Here he makes use of an established convention for staging the attainment of ethnographic authority. As a result, we are seldom made aware of the fact that an essential part of the cockfight’s construction as a text

is dialogical, talking face-to-face with particular Balinese rather than reading culture “over the[ir] shoulders.”³⁴

Interpretive anthropology, by viewing cultures as assemblages of texts, loosely and sometimes contradictorily united, and by highlighting the inventive poesis at work in all collective representations, has contributed significantly to the defamiliarization of ethnographic authority. But in its mainstream realist strands it does not escape the general strictures of those critics of “colonial” representation who, since 1950, have rejected discourses that portray the cultural realities of other peoples without placing their own reality in jeopardy. In Leiris’s early critiques, by way of Maquet, Asad and many others, the unreciprocal quality of ethnographic interpretation has been called to account.³⁵ Henceforth, neither the experience nor the interpretive activity of the scientific researcher can be considered innocent. It becomes necessary to conceive ethnography, not as the experience and interpretation of a circumscribed “other” reality, but rather as a constructive negotiation involving at least two, and usually more, conscious, politically significant subjects. Paradigms of experience and interpretation are yielding to paradigms of discourse, of dialogue and polyphony. The remaining sections of my essay will survey these emergent modes of authority.

A discursive model of ethnographic practice brings into prominence the intersubjectivity of all speech, along with its immediate performative context. Benveniste’s work on the constitutive role of personal pronouns and deixis highlights just these dimensions. Every use of “I” presupposes a “you,” and every instance of discourse is immediately linked to a specific, shared situation. No discursive meaning, then, without interlocution and context. The relevance of this emphasis for ethnography is evident. Fieldwork is significantly composed of language events; but language, in Bakhtin’s words, “lies on the borderline between oneself and the other. The word in language is half someone else’s.” The Russian critic urges a rethinking of language in terms of specific discursive situations: “There are,” he writes, “no ‘neutral’ words and forms—words and forms that can belong to ‘no one’; language has been completely taken over, shot through with intentions and accents.” The words of ethnographic writing, then, cannot be construed as monological, as the authoritative statement about, or interpretation of, an abstracted, textualized reality. The language of ethnography is shot through with other subjectivities and specific contextual overtones; for all language, in Bakhtin’s view, is “a concrete heteroglot conception of the world.”³⁶

Forms of ethnographic writing which present themselves in a “discursive” mode tend to be concerned with the representation of research contexts and situations of interlocution. Thus a book like Paul Rabinow’s *Reflections on Fieldwork in Morocco* is concerned with the representation of a specific research situation (a series of constraining times and places) and (in somewhat fictionalized form) a sequence of individual interlocutors.³⁷ Indeed, an entire new sub-genre of “fieldwork accounts”

(of which Rabinow's is one of the most trenchant) may be situated within the discursive paradigm of ethnographic writing. Jeanne Favret-Saada's *Les mots, la mort, les sorts* is an insistent, self-conscious experiment with ethnography in a discursive mode.³⁸ She argues that the event of interlocution always assigns to the ethnographer a specific position in a web of intersubjective relations. There is no neutral standpoint in the power-laden field of discursive positionings, in a shifting matrix of relationships, of "I's" and "you's."

A number of recent works have chosen to present the discursive processes of ethnography in the form of a dialogue between two individuals. Lacoste-Dujardin's *Dialogue des femmes en ethnologie* and Shostak's *Nisa: The Life and Words of a !Kung Woman* are noteworthy examples.³⁹ The dialogical mode is advocated with considerable sophistication in two other texts. The first, Kevin Dwyer's theoretical reflections on "The Dialogic of Ethnology" springs from a series of interviews with a key informant and justifies Dwyer's decision to structure his ethnography in the form of a rather literal record of these exchanges.⁴⁰ The second work is Vincent Crapanzano's more complex *Tuhami: Portrait of a Moroccan*, another account of a series of interviews which rejects any sharp separation of an interpreting self from a textualized other.⁴¹ Both Dwyer and Crapanzano locate ethnography in a process of dialogue where interlocutors actively negotiate a shared vision of reality. Crapanzano argues that this mutual construction must be at work in any ethnographic encounter, but that participants tend to assume they have simply acquiesced to the reality of their counterpart. Thus, for example, the ethnographer of the Trobriand Islanders does not openly concoct a version of reality in collaboration with his informants but rather interprets the "Trobriand point of view." Crapanzano and Dwyer offer sophisticated attempts to break with this literary/hermeneutical convention. In the process, the ethnographer's authority as narrator and interpreter is altered. Dwyer proposes a hermeneutics of "vulnerability," stressing the ruptures of fieldwork, the divided position and imperfect control of the ethnographer. Both Crapanzano and Dwyer seek to represent the research experience in ways that tear open the textualized fabric of the other and thus, also, of the interpreting self.⁴² (Here etymologies are evocative: the word text is related, as is well known, to weaving, vulnerability to rending or wounding, in this instance the opening up of a closed authority.)

The model of dialogue brings to prominence precisely those discursive—circumstantial and intersubjective—elements that Ricoeur had to exclude from his model of the text. But if interpretive authority is based on the exclusion of dialogue, the reverse is also true: a purely dialogical authority would repress the inescapable fact of textualization. While ethnographies cast as encounters between two individuals may successfully dramatize the intersubjective, give-and-take of fieldwork and introduce a counterpoint of authoritative voices, they remain *representations* of dialogue. As texts they may not be dialogical in structure. (Although Socrates appears as a decentered participant in his encounters, Plato retains full control of the dialogue.⁴³) This

displacement, but not elimination of monological authority is characteristic of any approach that portrays the ethnographer as a discrete character in the fieldwork narrative. Moreover, there is a frequent tendency in fictions of dialogue for the ethnographer's counterpart to appear as a representative of his or her culture—a type, in the language of traditional realism—through which general social processes are revealed.⁴⁴ Such a portrayal reinstates the synecdochic interpretive authority by which the ethnographer reads text in relation to context, thereby constituting a meaningful “other” world. But if it is difficult for dialogical portrayals to escape typifying procedures, they can, to a significant degree, resist the pull toward authoritative representation of the other. This depends on their ability fictionally to maintain the strangeness of the other voice and to hold in view the specific contingencies of the exchange.

To say that an ethnography is composed of discourses and that its different components are dialogically related, is not to say that its textual form should be that of a literal dialogue. Indeed, as Crapanzano recognizes in *Tuhami*, a third participant, real or imagined, must function as mediator in any encounter between two individuals.⁴⁵ The fictional dialogue is, in fact, a condensation, a simplified representation of complex, multi-vocal processes. An alternative way of representing this discursive complexity is to understand the overall course of the research as an ongoing negotiation. The case of Marcel Griaule and the Dogon is well known and particularly clear-cut. Griaule's account of his instruction in Dogon cosmological wisdom, *Dieu d'Eau (Conversations with Ogotemmêli)*, was an early exercise in dialogical ethnographic narration. But beyond this specific interlocutory occasion, a more complex process was at work. For it is apparent that the content and timing of the Griaule team's long-term research, spanning decades, was closely monitored and significantly shaped by Dogon tribal authorities.⁴⁶ This is no longer news. Many ethnographers have commented on the ways, both subtle and blatant, in which their research was directed or circumscribed by their informants. In his provocative discussion of this issue, Ioan Lewis even calls anthropology a form of “plagiarism.”⁴⁷

The give and take of ethnography is clearly portrayed in a recently published study, noteworthy for its presentation within a single work of both an interpreted other reality and the research process itself: Renato Rosaldo's *Ilongot Headhunting*.⁴⁸ Rosaldo arrives in the Phillipine highlands intent on writing a synchronic study of social structure. But again and again, over his objections, he is forced to listen to endless Ilongot narratives of their local history. Dutifully, dumbly, in a kind of bored trance, he transcribes these stories, filling notebook after notebook with what he considers disposable texts. Only after leaving the field, and after a long process of reinterpretation (a process made manifest in the ethnography) does it become clear that these obscure tales have in fact provided Rosaldo with his final topic, the culturally distinctive Ilongot sense of narrative and history. Rosaldo's ex-

perience of what might be called “directed writing” sharply poses a fundamental question. Who is actually the author of field notes?

The issue is a subtle one, and deserves systematic study. But enough has been said to make the general point, that indigenous control over knowledge gained in the field can be considerable, and even determining. Current ethnographic writing is seeking new ways to adequately represent the authority of informants, and there are few models to look to. But it is worth reconsidering the older textual compilations of Boas, Malinowski, Leenhardt, and others. In these works, the ethnographic genre has not coalesced around the modern interpretational monograph closely identified with a personal fieldwork experience. We can contemplate an ethnographic mode that is not yet authoritative in those specific ways that are now politically and epistemologically in question. These older assemblages include much that is actually or all but written by informants. One thinks of the role of George Hunt in Boas’s ethnography, or of the fifteen “*transcripteurs*” listed in Leenhardt’s *Documents néo-calédoniens*.⁴⁹

Malinowski is a complex transitional case. His ethnographies reflect the incomplete coalescence of the modern monograph. If he was centrally responsible for the welding of theory and description into the authority of the professional fieldworker, Malinowski nonetheless included material that did not directly support his own all-too-clear interpretive slant. In the many dictated myths and spells which fill his books he published much data that he frankly did not understand. The result was an open text subject to multiple reinterpretations. It is worth comparing such older compendia with the recent model ethnography, which cites evidence to support a focused interpretation, and little else. In the modern, authoritative monograph there are, in effect, no strong voices present except that of the writer. But, in *Argonauts* and *Coral Gardens* we read page after page of magical spells, none in any essential sense the ethnographer’s words. These dictated texts, in all but their physical inscription, are written by specific, unnamed Trobrianders. Indeed, any continuous ethnographic exposition routinely folds into itself a diversity of descriptions, transcriptions, and interpretations by a variety of indigenous “authors.” How should these authorial presences be made manifest?

A useful—if extreme—standpoint is provided by Bakhtin’s analysis of the “polyphonic” novel. A fundamental condition of the genre, he argues, is that it represents speaking subjects in a field of multiple discourses. The novel grapples with, and enacts, heteroglossia. For Bakhtin, preoccupied with the representation of non-homogeneous wholes, there are no integrated cultural worlds or languages. All attempts to posit such abstract unities are constructs of monological power. A “culture” is, concretely, an open-ended, creative dialogue of subcultures, of insiders and outsiders, of diverse factions; a “language” is the interplay and struggle of regional dialects, professional jargons, generic commonplaces, the speech of different

age groups, individuals, and so forth. For Bakhtin, the polyphonic novel is not a tour de force of cultural or historical totalization (as realist critics like Lukàcs and Auerbach have argued), but rather a carnivalesque arena of diversity. Bakhtin discovers a utopian textual space where discursive complexity, the dialogical interplay of voices, can be accommodated. In the novels of Dostoyevski or Dickens, he values precisely their resistance to totality, and his ideal novelist is a ventriloquist—in nineteenth-century parlance, a “polyphonist.” “He do the police in different voices,” a listener exclaims admiringly of the boy, Sloppy, who reads publicly from the newspaper in *Our Mutual Friend*. But Dickens, the actor, oral performer, and polyphonist, must be set against Flaubert, the master of authorial control moving godlike among the thoughts and feelings of his characters. Ethnography, like the novel, wrestles with these alternatives. Does the ethnographic writer portray what natives think by means of Flaubertian “free indirect style,” a style that suppresses direct quotation in favor of a controlling discourse always more-or-less that of the author? (In a recent essay Dan Sperber, taking Evans-Pritchard as his example, has convincingly shown that *style indirect* is indeed the preferred mode of ethnographic interpretation.⁵⁰) Or, does the portrayal of other subjectivities require a version that is stylistically less homogeneous, filled with Dickens’ “different voices?”

Some use of indirect style is inevitable, unless the novel or ethnography be composed entirely of quotations, which is theoretically possible but seldom attempted.⁵¹ In practice, however, the ethnography and the novel have recourse to indirect style at different levels of abstraction. We need not ask how Flaubert knows what Emma Bovary is thinking, but the ability of the fieldworker to inhabit indigenous minds is always in doubt: indeed this is a permanent, unresolved problem of ethnographic method. Ethnographers have generally refrained from ascribing beliefs, feelings, and thoughts to individuals. They have not, however, hesitated to ascribe subjective states to a culture. Sperber’s analysis reveals how phrases such as “The Nuer think . . .” or “The Nuer sense of time . . .” are fundamentally different from quotations or translations of indigenous discourse. Such statements are “without any specified speaker,” and are literally equivocal, combining in an unspecified way the ethnographer’s affirmations with that of an informant or informants.⁵² Ethnographies abound in unattributed sentences like “The spirits return to the village at night,” descriptions of beliefs in which the writer assumes, in effect, the voice of culture.

At this “cultural” level, ethnographers aspire to a Flaubertian omniscience that moves freely throughout a world of indigenous subjects. But beneath the surface their texts are more unruly and discordant. Victor Turner’s work provides a telling case in point, worth investigating more closely as an example of the interplay of monophonic and polyphonic exposition. Turner’s ethnographies offer superbly complex portrayals of Ndembu ritual symbols and beliefs; and he has provided, too, an unusually explicit glimpse behind the scenes. In the midst of the essays collected in *The Forest of Symbols*, his third book on the Ndembu, Turner offers a portrait of his

best informant, "Muchona the Hornet, Interpreter of Religion."⁵³ Muchona, a ritual healer, and Turner are drawn together by their shared interest in traditional symbols, etymologies, and esoteric meanings. They are both "intellectuals," passionate interpreters of the nuances and depths of custom; both are uprooted scholars sharing "the quenchless thirst for objective knowledge." Turner compares Muchona to a university don; his account of their collaboration includes more than passing hints of a strong psychological doubling, linking ethnographer and informant.

But there is a third present in their dialogue, Windson Kashinakaji, a Ndembu senior teacher at the local mission school. He brought Muchona and Turner together, and shares their passion for the interpretation of customary religion. Through his Biblical education he "acquired a flair for elucidating knotty questions." Newly skeptical of Christian dogma and missionary privileges, he is looking sympathetically at pagan religion. Kashinakaji, Turner tells us, "spanned the cultural distance between Muchona and myself, transforming the little doctor's technical jargon and salty village argot into a prose I could better grasp." The three intellectuals soon "settled down into a sort of daily seminar on religion." Turner's accounts of this seminar are stylized: "eight months of exhilarating quickfire talk among the three of us, mainly about Ndembu ritual." They reveal an extraordinary ethnographic "colloquy." But significantly, Turner does not make this three-way collaboration the crux of his essay. Rather he focusses on Muchona, thus transforming triologue into dialogue and flattening a complex productive relation into the "portrait" of an "informant." (This reduction was in some degree required by the format of the book in which the essay first appeared, Joseph Casagrande's important collection of "Twenty Portraits of Anthropological Informants," *In the Company of Man*.⁵⁴)

Turner's published works vary considerably in their discursive structure. Some are largely composed of direct quotations; in at least one essay Muchona is identified as the principal source of the overall interpretation; elsewhere he is invoked anonymously, for example as "a male ritual specialist."⁵⁵ Windson Kashinakaji is usually identified as an assistant and translator rather than as a source of interpretations. Overall, Turner's ethnographies are unusually polyphonic, openly built up from quotations. ("According to an adept . . .", or, "One informant guesses . . .") He does not, however, do the Ndembu in different voices, and we hear little "salty village argot." All the voices of the field have been smoothed into the expository prose of more-or-less interchangeable "informants." The staging of indigenous speech in an ethnography, the degree of translation and familiarization necessary, are complicated practical and rhetorical problems.⁵⁶ But Turner's works, by giving visible place to indigenous interpretations of custom, expose concretely these issues of textual dialogism and polyphony.

The inclusion of Turner's portrait of Muchona in *The Forest of Symbols* (1967) may be seen as a sign of the times. The Casagrande collection (1961) in which it originally appeared had the effect of segregating the crucial issue of relations be-

tween ethnographers and their indigenous collaborators. Discussion of these issues still had no place *within* scientific ethnographies. But Casagrande's collection shook the post-Malinowski professional taboo on "privileged informants." Raymond Firth on Pa Fenuatara, Robert Lowie on Jim Carpenter—a long list of distinguished anthropologists described the indigenous "ethnographers" with whom they shared, to some degree, a distanced, analytic, even ironic view of custom. These individuals became valued informants because they understood, often with real subtlety, what an *ethnographic* attitude toward culture entailed. In Lowie's quotation of his Crow interpreter (and fellow "philologist") Jim Carpenter, one senses a shared outlook: "When you listen to the old men telling about their visions, you've just *got* to believe them."⁵⁷ And there is considerably more than a wink and a nod in a story recounted by Firth about his best Tikopian friend and informant.⁵⁸

On another occasion talk turned to the nets set for salmon trout in the lake. The nets were becoming black, possibly with some organic growth, and tended to rot easily. Pa Fenuatara then told a story to the crowd assembled in the house about how, out on the lake with his nets one time, he felt a spirit going among the net and making it soft. When he held the net up he found it slimy. The spirit had been at work. I asked him then if this was a traditional piece of knowledge that spirits were responsible for the deterioration of the nets. He answered, "No, my own thought." Then he added with a laugh, "My own piece of traditional knowledge."⁵⁵

The full methodological impact of Casagrande's collection remains latent, especially the significance of its accounts for the dialogical production of ethnographic texts and interpretations. This significance is obscured by a tendency to cast it as a universalizing, humanist document revealing "a hall of mirrors . . . in full variety the endless reflected image of man."⁵⁹ However, in the light of the present crisis in ethnographic authority, these revealing portraits spill into the oeuvres of their authors, altering the way they can be read. If ethnography is part of what Roy Wagner calls "the invention of culture," its activity is plural and beyond the control of any individual.⁶⁰

One increasingly common way to manifest the collaborative production of ethnographic knowledge is to quote regularly and at length from informants. (A striking recent example is June Nash, *We Eat the Mines, the Mines Eat Us*.⁶¹) But such a tactic only begins to break up monophonic authority. Quotations are always staged by the quoter, and tend to serve merely as examples, or confirming testimonies. Looking beyond quotation, one might imagine a more radical polyphony that would "do the natives and the ethnographer in different voices." But this, too, would only displace ethnographic authority, still confirming the final, virtuoso orchestration by a single author of all the discourses in his or her text. And in this sense Bakhtin's polyphony, too narrowly identified with the novel, is a domesticated heteroglossia. Ethnographic discourses are not, in any event, the speeches of invented characters. Informants are specific individuals with real proper names—names to be cited, in altered form when tact requires. Informants' intentions are overdetermined, their

words politically and metaphorically complex. Ethnography is invaded by heteroglossia. If accorded an autonomous textual space, transcribed at sufficient length, indigenous statements make sense on terms different from those of the arranging ethnographer.

This suggests an alternate textual strategy, a utopia of plural authorship that accords to collaborators, not merely the status of independent enunciators, but that of writers. As a form of authority it must still be considered utopian for two reasons. First, the few recent experiments with multiply-authored works appear to require, as an instigating force, the research interest of an ethnographer, who in the end assumes an executive, editorial position. The authoritative stance of “giving voice” to the other is not fully transcended. Second, the very idea of plural authorship challenges a deep Western identification of any text’s order with the intention of a single author. If this identification was less strong when Lafitau wrote his *Moeurs des sauvages américains*, and if recent criticism has thrown it into question, it is still a potent constraint on ethnographic writing. Nonetheless, there are signs of movement in this domain, and we may anticipate a gradual increase in experiments with multiple authorship. Anthropologists will increasingly have to share their texts, and sometimes their title pages, with those indigenous collaborators for whom the term “informants” is no longer adequate, if it ever was.

Ralph Bulmer and Ian Majnep’s *Birds of My Kalam Country* is an important prototype.⁶² (Separate typefaces distinguish the juxtaposed contributions of ethnographer and New Guinean, collaborators for more than a decade.) Even more significant is the collectively produced study, *Piman Shamanism and Staying Sickness (Ka:cim Mumkidag)* which lists on its title page, without distinction (though not, it may be noted, in alphabetical order): Donald M. Bahr, anthropologist; Juan Gregorio, shaman; David I. Lopez, interpreter; Albert Alvarez, editor. Three of the four are Papago Indians, and the book is consciously designed “to transfer to a shaman as many as possible of the functions normally associated with authorship. These include the selection of an expository style, the duty to make interpretations and explanations, and the right to judge which things are important and which are not.”⁶³ Bahr, the initiator and organizer of the project, opts to share out authority as much as possible. Gregorio, the shaman, appears as the principal source of the “theory of disease” which is transcribed and translated, at two separate levels, by Lopez and Alvarez. Gregorio’s vernacular texts include compressed, often gnomic, explanations, which are themselves interpreted and contextualized by Bahr’s separate commentary. The book is unusual in its textual enactment of the interpretation of interpretations.

In *Piman Shamanism* the transition from individual enunciations to cultural generalizations is always visible in the separation of Gregorio’s and Bahr’s voices. The authority of Lopez, less visible, is akin to that of Windson Kashinakaji in Turner’s work. His bilingual fluency guides Bahr through the subtleties of Gregorio’s language, thus permitting the shaman “to speak at length on theoretical top-

ics.” Neither Lopez nor Alvarez appear as specific voices in the text, and their contribution to the ethnography remains largely invisible to all but qualified Papagos, able to gauge the accuracy of the translated texts and the vernacular nuance of Bahr’s interpretations. Alvarez’s authority inheres in the fact that *Piman Shamanism* is a book directed at separate audiences. For most readers focussing on the translations and explanations the texts printed in Piman will be of little or no interest. However, the linguist Alvarez corrected the transcriptions and translations with an eye to their use in language teaching, using an orthography he had developed for that purpose. Thus the book contributes to Papagos’ literary invention of their culture. This different reading, built into *Piman Shamanism*, is of more than local significance.

It is intrinsic to the breakup of monological authority that ethnographies no longer address a single general type of reader. The multiplication of possible readings reflects the fact that self-conscious “ethnographic” consciousness can no longer be seen as the monopoly of certain Western cultures and social classes. Even in ethnographies lacking vernacular texts, indigenous readers will decode differently the textualized interpretations and lore. Polyphonic works are particularly open to readings not specifically intended. Trobriand readers may find Malinowski’s interpretations tiresome but his examples and extensive transcriptions still evocative. And Ndembu will not gloss as quickly as European readers over the different voices embedded in Turner’s works.

Recent literary theory suggests that the ability of a text to make sense in a coherent way depends less on the willed intentions of an originating author than on the creative activity of a reader. In Barthes’ words, if a text is “a tissue of quotations drawn from innumerable centers of culture,” then “a text’s unity lies not in its origin but in its destination.”⁶⁴ The writing of ethnography, an unruly, multisubjective activity, is given coherence in particular acts of reading. But there is always a variety of possible readings (beyond merely individual appropriations), readings beyond the control of any single authority. One may approach a classic ethnography seeking simply to grasp the meanings that the researcher derives from represented cultural facts. But, as we have suggested, one may also read against the grain of the text’s dominant voice, seeking out other, half-hidden authorities, reinterpreting the descriptions, texts and quotations gathered together by the writer. With the recent questioning of colonial styles of representation, with the expansion of literacy and ethnographic consciousness, new possibilities for reading (and thus for writing) cultural descriptions are emerging.⁶⁵

The textual embodiment of authority is a recurring problem for recent experiments in ethnography.⁶⁶ An older, realist mode—figured in the frontispiece to *Argonauts of the Western Pacific* and based on the construction of a cultural *tableau vivant* designed to be seen from a single vantage point, that of the writer and reader—can now be identified as only one possible paradigm for authority. Political and epistemological assumptions are built into this and other styles, assumptions the

ethnographic writer can no longer afford to ignore. The modes of authority reviewed in this essay—experiential, interpretive, dialogical, polyphonic—are available to all writers of ethnographic texts, Western and non-Western. None is obsolete, none pure: there is room for invention within each paradigm. For example, interpretation—as conceived by Gadamer—can aspire to a radical dialogism. We have seen, too, how new approaches tend to rediscover discarded practices. Polyphonic authority looks with renewed sympathy to compendia of vernacular texts—expository forms distinct from the focused monograph tied to participant-observation. And now that naive claims to the authority of experience have been subjected to hermeneutic suspicion, we may anticipate a renewed attention to the subtle interplay of personal and disciplinary components in ethnographic research.

Experiential, interpretive, dialogical, and polyphonic processes are at work, discordantly, in any ethnography. But coherent presentation presupposes a controlling mode of authority. I have argued that this imposition of coherence on an unruly textual process is now, inescapably, a matter of strategic choice. I have tried to distinguish important styles of authority as they have become visible in recent decades. If ethnographic writing is alive, as I believe it is, it is struggling within and against these possibilities.

Notes

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1. Only English, American, and French examples are discussed. If it is likely that the modes of authority analyzed here are able widely to be generalized, no attempt has been made to extend them to other national traditions. It is assumed, also, in the antipositivist tradition of Dilthey, that ethnography is a process of interpretation, not of explanation. Modes of authority based on natural-scientific epistemologies are not discussed. In its focus on participant-observation as an intersubjective process at the heart of twentieth-century ethnography, the essay scants a number of contributing sources of authority: for example, the weight of accumulated “archival” knowledge about particular groups, of a cross cultural comparative perspective, and of statistical survey work.
2. See M. Bakhtin, “Discourse in the Novel” (1935), in Michael Holquist, ed., *The Dialogic Imagination: Four Essays by M. M. Bakhtin* (Austin and London, 1981), pp. 259–442. “Heteroglossia” assumes that “languages do not *exclude* each other, but rather intersect with each other in many different ways (the Ukrainian language, the language of the epic poem, of early Symbolism, of the student, of a particular generation of children, of the run-of-the-mill intellectual, of the Nietzschean and so on). It might even seem that the very word ‘language’ loses all meaning in this process—for apparently there is no single plane on which all these ‘languages’ might be juxtaposed to one another.” What is

said of languages applies equally to “cultures” and “subcultures.” See also V. N. Voloshinov (Bakhtin?), *Marxism and the Philosophy of Language* (New York and London, 1973), esp. Chaps. 1–3; and Tzvetan Todorov, *Mikhail Bakhtine: le principe dialogique* (Paris, 1981), pp. 88–93.

3. Edward Said, *Orientalism* (New York, 1978); Paulin Hountondji, *Sur la “philosophie africaine”* (Yaounde Cameroon, 1980); for more on this ambiguous predicament, J. Clifford, review of Said, *History and Theory*, 19:2 (1980), 204–23.
4. Michel Foucault, *Power/Knowledge* (New York, 1980), p. 145; see also “Intellectuals and Power: A Conversation between Michel Foucault and Gilles Deleuze,” in Foucault, *Language, Counter-Memory, Practice* (Ithaca, N.Y., 1977), pp. 208–209. A recent unpublished essay by Edward Said, “The Text’s Slow Politics and the Prompt Language of Criticism,” has sharpened my conception of a historically contingent, engaged theory.
5. I have not attempted to survey new styles of ethnographic writing that may be originating from outside the West. As Said, Hountondji, and others have shown, a considerable work of ideological “clearing,” an oppositional, critical work remains, and it is to this that non-Western intellectuals have been devoting a great part of their energies. My essay remains inside, but at the experimental boundaries of, a realist cultural science elaborated in the Occident. It does not consider, as areas of innovation, the “para-ethnographic” genres of oral history, the non-fiction novel, the “new journalism,” travel literature, and the documentary film.
6. Compare, for example, Marcel Griaule’s idea of team research (with repeated visits to the field) and Malinowski’s extended solo sojourn: Griaule, *Méthode de l’ethnographie* (Paris, 1957); Malinowski, *Argonauts of the Western Pacific* (London, 1922), Chapter 1.
7. Victor Karady, “Le problème de la légitimité dans l’organisation historique de l’ethnologie française,” *Revue française de sociologie*, 23:1 (1982), 17–36; George Stocking, “The Ethnographer’s Magic: the Development of Fieldwork in British Anthropology from Tylor to Malinowski,” *History of Anthropology* 1 (1983), forthcoming.
8. In the present crisis of authority, ethnography has emerged as a subject of historical scrutiny. For new critical approaches see: François Hartog, *Le miroir d’Hérodote: essai sur la représentation de l’autre* (Paris, 1980); K.O.L. Burridge, *Encountering Aborigines* (New York, 1973), Chapter 1; Michèle Duchet, *Anthropologie et Histoire au siècle des lumières* (Paris, 1971); James Boon, “Comparative De-enlightenment: Paradox and Limits in the History of Ethnology,” *Daedalus*, Spring 1980, 73–90; Michel de Certeau, “Writing vs. Time: History and Anthropology in the Works of Lafitau,” *Yale French Studies*, 59 (1980), 37–64; Edward Said, *Orientalism*; George Stocking, ed., “Observers Observed: Essays on Ethnographic Fieldwork,” *History of Anthropology* 1 (1983), Madison, Wisconsin; forthcoming.
9. On the suppression of dialogue in Lafitau’s frontispiece, and the constitution of a textualized, ahistorical, and visually oriented “anthropology,” see Michel de Certeau’s detailed analysis in “Writing vs. Time.”
10. B. Malinowski, “Pigs, Papuans and Police Court Perspective,” *Man*, 32 (1932), 33–38; Alex Rentoul, “Physiological paternity and the Trobrianders,” and “Papuans, Professors and Platitudes,” *Man* 31 (1931), 153–154, and 32 (1932), 274–276. Ian Langham, *The Building of British Social Anthropology* (Dordrecht, London, 1981), Chapter VII.
11. R. H. Codrington, *The Melanesians* (1891), Dover reprint (New York, 1972), pp. vi–vii.
12. George Stocking, “The Ethnographer’s Magic.”
13. Curtis Hinsley, “Ethnographic Charisma and Scientific Routine: Cushing and Fewkes in the American Southwest,” *History of Anthropology*, 1 (1983), forthcoming.
14. Stocking, “The Ethnographer’s Magic.”

15. Malinowski, *Argonauts of the Western Pacific* (1922), pp. 3–4.
16. Stocking, “The Ethnographer’s Magic.” See also Harry Payne, “Malinowski’s Style,” *Proceedings of the American Philosophical Society*, forthcoming.
17. G. Lienhardt, *Divinity and Experience: the Religion of the Dinka* (Oxford, 1961), p. vii.
18. I am indebted to two important unpublished papers by Robert Thornton of the University of Capetown: “The Rise of Ethnography in South Africa: 1860–1920,” and “The Rise of the Ethnographic Monograph in Eastern and Southern Africa.”
19. Margaret Mead, “Native languages as Field-Work Tools,” *American Anthropologist*, 41:2 (1939), 189–205.
20. Robert Lowie, “Native Languages as Ethnographic Tools,” *American Anthropologist*, 42: 1 (1940), 81–89.
21. E. Evans-Pritchard, *The Nuer* (New York, Oxford, 1969), p. 261.
22. *Ibid.*, p. 215.
23. The concept is sometimes too readily associated with intuition or empathy, but as a description of ethnographic knowledge, *Verstehen* properly involves a critique of empathetic experience. The exact meaning of the term is a matter of debate among Dilthey scholars. See Rudolf Makkreel, *Dilthey: Philosopher of the Human Sciences* (Princeton, 1975), pp. 6–7, and *passim*.
24. This bare summary is drawn from H. P. Rickman, ed., *W Dilthey: Selected Writings* (Cambridge, 1976), pp. 168–245. “The Construction of the Historical World in the Human Studies,” Vol. VII of the *Gesammelte Schriften* (Leipzig, 1914).
25. *American Anthropologist*, 33 (1931), p. 248.
26. Maurice Leenhardt, *Do Kamo* (Chicago, 1979), p. 2; see J. Clifford, *Person and Myth: Maurice Leenhardt in the Melanesian World* (Berkeley, 1982).
27. Carlo Ginzburg, “Morelli, Freud and Sherlock Holmes: Clues and Scientific Method,” *History Workshop*, 9 (Spring 1980), 5, 36.
28. For example: Clifford Geertz, *The Interpretation of Cultures* (New York, 1973); “From the Native’s Point of View: on the Nature of Anthropological Understanding,” in K. Basso and H. Selby, eds., *Meaning in Anthropology* (Albuquerque, 1976), pp. 221–38; Paul Rabinow and William Sullivan, eds., *Interpretive Social Science* (Berkeley, 1979); Irene and Thomas Winner, “The Semiotics of Cultural Texts,” *Semiotica*, 18:2 (1976), 101–56; Dan Sperber, “L’Interprétation en Anthropologie,” *L’Homme*, XXI:I (1981), 69–92.
29. *Social Research*, 38 (1971), 529–562.
30. See especially, “Thick Description: Toward an Interpretive Theory of Culture,” Chapter 1 of *The Interpretation of Cultures*.
31. Emile Benveniste, “The Nature of Pronouns,” and “Subjectivity in Language,” *Problems in General Linguistics* (Coral Gables, Fla., 1971), pp. 217–30.
32. Françoise Michel-Jones, *Retour au Dogon: Figure du double et ambivalence* (Paris, 1978), p. 14.
33. Clifford Geertz, “Deep Play: Notes on the Balinese Cockfight,” *Daedalus*, 101:1 (1972), reprinted in *The Interpretation of Cultures*, pp. 412–53.
34. *Ibid.*, p. 452.
35. Michel Leiris, “L’ethnologue devant le colonialisme,” *Les Temps Modernes*, 58 (1950); reprinted in Leiris, *Brisées* (Paris, Mercure de France, 1966), pp. 125–45; Jacques Maquet, “Objectivity in Anthropology,” *Current Anthropology* 5 (1964), 47–55; Talal Asad, ed., *Anthropology and the Colonial Encounter* (London, 1973).
36. M. Bakhtin, “Discourse in the Novel,” p. 293.

37. P. Rabinow, *Reflections on Fieldwork in Morocco* (Berkeley, 1977).
38. Paris, 1977; translated as *Deadly Words* (Cambridge, England, 1980). See especially Chapter 2. Her experience has been rewritten at another fictional level in J. Favret-Saada and Josée Contreras, *Corps pour corps: Enquête sur la sorcellerie dans le Bocage* (Paris, 1981).
39. Camille Lacoste-Dujardin, *Dialogue des femmes en ethnologie* (Paris, 1977); Marjorie Shostak, *Nisa: The Life and Words of a !Kung Woman* (Cambridge, Mass., 1981).
40. Kevin Dwyer, "On the Dialogic of Field Work," and "The Dialogic of Ethnology," *Dialectical Anthropology*, 2:2 (1977), 143–151, and 4:3 (1979), 205–24.
41. Vincent Crapanzano, *Tuhami: Portrait of a Moroccan* (Chicago, 1980); and "On the Writing of Ethnography," *Dialectical Anthropology*, 2:1 (1977), 69–73.
42. It would be wrong to gloss over the differences between Dwyer's and Crapanzano's theoretical positions. Dwyer, following Lukàcs, translates dialogic into Marxian/Hegelian dialectic, thus holding out the possibility of a restoration of the human subject, a kind of completion in and through the other. Crapanzano refuses any anchor in an englobing theory, his only authority being that of the dialogue's writer, an authority undermined by an inconclusive narrative of encounter, rupture, and confusion. (It is worth noting that dialogic, as used by Bakhtin, is not reducible to dialectic.)
43. On this see Stephen Tylor's "Words for Deeds and the Doctrine of the Secret World: Testimony to a Chance Encounter Somewhere in the Indian Jungle," forthcoming in *Proceedings of the Chicago Linguistic Society*.
44. On realist "types," see G. Lukàcs, *Studies in European Realism, passim*. The tendency to transform an individual into a cultural enunciator may be observed in M. Griaule's *Dieu d'Eau* (Paris, 1948). It occurs ambivalently in Shostak's *Nisa*. (For a discussion of this ambivalence and of the book's resulting discursive complexity see my review, *London Times Literary Supplement*, Sept. 17, 1982, 994–95.)
45. Crapanzano, *Tuhami*, pp. 147–151.
46. James Clifford, "Power and Dialogue in Ethnography: Marcel Griaule's Initiation," *History of Anthropology 1*, forthcoming.
47. I. Lewis, *The Anthropologist's Muse* (London, 1973).
48. R. Rosaldo, *Ilongot Headhunting 1883–1974: A Study in Society and History* (Stanford, 1980).
49. Paris, 1932; for a study of this mode of textual production see J. Clifford, "Fieldwork, Reciprocity, and the Making of Ethnographic Texts," *Man* 15 (1980), 518–32. See also, in this context, B. Fontana, Introduction to Frank Russell, *The Pima Indians* (Tucson, 1975), on the book's hidden co-author, the Papago Indian José Lewis; M. Leiris, "Avant-propos," *La langue secrète des Dogons de Sanga* (Paris, 1948), pp. ix–xxv, discusses collaboration as co-authorship, as does I. M. Lewis, in *The Anthropologist's Muse*. For a forward-looking defense of Boas' emphasis on vernacular texts and his collaboration with Hunt, see Irving Goldman, "Boas on the Kwakiutl: the Ethnographic Tradition," in S. Diamond, ed., *Theory and Practice: Essays presented to Gene Weltfish* (The Hague, 1980), pp. 334–36.
50. D. Sperber, "L'Interprétation en Anthropologie," *L'Homme*, XXI:I (1981), esp. pp. 76–79.
51. Such a project is announced by Evans-Pritchard in his introduction to *Man and Woman among the Azande* (London, 1974), a late work which may be seen as a reaction against the closed, analytic nature of his own earlier ethnographies. His acknowledged inspiration is Malinowski.

52. Sperber, "L'Interprétation en Anthropologie," p. 78.
53. V. Turner, *The Forest of Symbols: Aspects of Ndembu Ritual* (Ithaca, N.Y., and London, 1967), pp. 131–50.
54. New York, 1960, pp. 333–356. For a "group dynamics" approach to ethnography, see T. Yannopoulos and Denis Martin, "De la question au dialogue: à propos des enquêtes en Afrique noire," *Cahiers d'études africaines*, 71 (1978), 421–42. For an ethnography explicitly based on native "seminars" see N. Blurton Jones and M. Konner, "Kung Knowledge of Animal Behavior," in R. Lee and I. De Vore, eds., *Kalahari Hunter-Gatherers* (Cambridge, 1976), pp. 325–48.
55. V. Turner, *Revelation and Divination in Ndembu Ritual* (Ithaca, 1975), pp. 40–42, 87, 154–56, 244; *Forest of Symbols*, p. 21.
56. Favret-Saada's use of dialect and italic type in *Les Mots, la mort, les sorts* is one solution among many to a problem that has long preoccupied realist novelists.
57. J. Casagrande, ed., *In the Company of Man*, p. 428.
58. *Ibid.*, pp. 17–18. 59. *Ibid.*, p. xiii.
60. Roy Wagner, *The Invention of Culture* (Chicago, 1980).
61. New York, 1980. 62. Auckland, London, 1977.
63. Bahr et al., *Piman Shamanism* (Tucson, 1974), p. 7.
64. R. Barthes, *Image, Music, Text* (New York, 1977), pp. 146, 148.
65. An extremely suggestive model of polyphonic exposition is offered by the projected four-volume edition of the ethnographic texts written, provoked, and transcribed between 1896 and 1914 by James Walker, on the Pine Ridge Sioux Reservation. Two titles have appeared so far: James Walker, *Lakota Belief and Ritual*, Raymond DeMallie and Elaine Jahner, eds., and *Lakota Society*, Raymond DeMallie, ed. (Lincoln, Nebraska, and London, 1980, 1982). These engrossing volumes in effect re-open the textual homogeneity of Walker's classic monograph of 1917, *The Sun Dance*, a summary of the individual statements here published in translation. These statements, by more than thirty named "authorities," complement and transcend Walker's synthesis. A long section of *Lakota Belief and Ritual* was written by Thomas Tyon, Walker's interpreter. And the collection's fourth volume will be a translation of the writings of George Sword, an Oglala warrior and judge encouraged by Walker to record and interpret the traditional way of life. The first two volumes present the unpublished texts of knowledgeable Lakota and Walker's own descriptions in identical formats. Ethnography appears as a process of collective production. It is essential to note that the Colorado Historical Society's decision to publish these texts was provoked by increasing requests from the Oglala Community at Pine Ridge for copies of Walker's materials to use in Oglala history classes.
66. For a very useful and complete survey of recent experimental ethnographies see George Marcus and Dick Cushman, "Ethnographies as Texts," *Annual Review of Anthropology*, 11 (1982), pp. 25–69; Steven Webster, "Dialogue and Fiction in Ethnography," *Dialectical Anthropology* 7:2 (1982); and Hussein Fahim, ed., *Indigenous Anthropology in Non-Western Countries* (Durham, North Carolina, 1982).