Ethnography as Commentary

Fabian, Johannes

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Introduction:  
Closing House—A Late Ethnography

A Time and a Place: Events and Documents

In the fall of 1974 I was getting ready to leave Lubumbashi. I had lived there for two and a half years, a stay interrupted only by a few short trips to the United States and Europe. This was my second visit to the mining region of Shaba/Katanga in the southeast of what was then Zaire and is now again the Democratic Republic of the Congo (the first one, in 1966–67, had been my initiatory dissertation fieldwork). I had arrived in the region in 1972 with a project of research titled “Language and Labor among Swahili-Speaking Mine Workers in Katanga.” Fieldwork was carried out first in Kolwezi, where most of the mines and treatment plants were located, and later in Lubumbashi, the capital, in two smaller establishments that made neo-African furniture and other craft items for sale mostly to expatriates.
Going on the mass of documentation assembled, mainly in the form of notes and sound recordings made at the workplaces and in the homes of workers, the project was certainly productive. Yet, as the material piled up, it dawned on me that my agenda had been conceived too narrowly as a sociolinguistic and lexical-semantic study. Workers’ language, I began to see, could not be isolated from speech and communicative practices that had developed in this urban-industrial region and that, through Swahili as the linguistic medium, had created a lively, multifaceted popular culture. At about that time—the period of funded research was coming to an end—my stay mutated into a stint of teaching and administration at the local campus of the National University, allowing me to bridge a difficult period between losing one job and finding another in the United States.

Work at the university ruled out sustained and focused research. Yet some of the contacts and discoveries made during this period would later become more important than the sociolinguistic project I had started out with. During free time at night or on weekends I kept in touch with members of the religious movement I had studied in the sixties, and I could easily have been drawn once again into their circles of thought and instruction. But I also took the habit of going out for a drink and chat in one of the cités, the townships set up for Africans in colonial times. I had become acquainted with the Mufwankolo troupe of actors and enjoyed their company. I also met and had conversations with many of the popular genre painters of Shaba/Katanga. There was so much to be curious about.

During the final months before departure, at a time when it was clear that I would soon leave the university and the country, two chance contacts led me to take up, or rather stumble onto, ethnographic inquiries I had not planned and carried out without the kind of mandate or the obligations that come with being supported by a research grant. One of them was meeting the painter and historian Tshibumba Kanda Matulu. One late afternoon I was driving home from the university when I saw him walking on a street, carrying several paintings. I stopped to take a look at his work and we began to talk, first on the roadside, later at our house. Our conversation encouraged him to take up a project that had been on his mind for some time: a painted and narrated history of Zaire that has by now become a monument to his genius and to the urban popular culture that produced him. It
is fair to say that Tshibumba not only represented history, he made it (Fabian 1996).

On September 30, one week before the first of four sessions with Tshibumba was recorded on October 6, a similarly intensive exchange had taken place in our house on Mpolo Avenue with Kahenga Mukonkwa Michel, by trade a provider of health and protection. It was not the first time we had met. An acquaintance from the Lubumbashi art scene, the painter Mwenze Kibwanga, had recommended Kahenga to us when my then-spouse told him about aches and pains which did not respond to conventional treatment. She became Kahenga’s patient during the weeks that followed while I went about my daily work and faced my daily worries, among them the break-ins and thefts all expatriates had come to fear. Either the subject had come up by chance or I brought it up because I wanted to try out a local solution to my problems with safety; Kahenga offered his services and I became his client. He performed a ritual that I was allowed to watch, and when he returned a few days later with some herbal medicine he accepted my invitation to talk about the ritual and about his work in general. That conversation was recorded and then filed away among notes and documents I found interesting but had no concrete plans for.

A Project Emerges:
Ethnography and the Virtual Archive

Almost exactly thirty years went by before I listened to that memorable conversation again. I transcribed and translated the recording and deposited the text in a virtual archive where it can now be looked at by everyone who has access to the Internet (http://www2.fmg.uva.nl/lpca). This book will be “based on” that text, as we say, a convenient expression that allows us to make empirical claims—that what we report is supported by a document of research (data, if you wish)—without having to specify how we get from an “ethnographic text” to a text we then call “an ethnography.” There was a time when it was easier to get away with claims concerning empirical validity and simply go about the business of “writing up” at home what we had noted down and recorded in the field. That time is long gone; when we present knowledge now we are expected to lay open, account for, the processes by
which observations and experiences become documents on which we may base the interpretations and insights we call ethnography.

This critical task is not made easier when the interval between the events that were documented and work on these documents has become long enough to accommodate a generation. Such a distance makes questioning the givenness of data even more urgent than it would have been if research and writing had taken place during the more usual period of a few years. When so much time has elapsed, problems with assuming that what we write about is simply there are complicated by a heightened awareness of a then. While this does not mean that short-term and long-term recourse to data differ in kind—every note, document, recording, every “fact” is then, a thing of the past—it may well be that an undertaking such as the one I am embarking on in this book changes the nature of ethnography. What we do becomes historiography as well as autobiography, by necessity rather than as a stylistic choice. There is no alternative to telling a story, or stories, when we go about re-presenting knowledge gained and recorded in the past; it is impossible to make that past present without recourse to the ethnographer’s personal memory or memories.

To realize this predicament of a “late ethnography” and to spell out some of the epistemological conditions of such an enterprise is one thing; to conclude that all the ethnographer can do is tell self-centered stories (because this is all that is left for postcolonial, postmodern, or post-theoretical anthropology) is another. In ways that are not quite clear to us, a decidedly modern technical development, the Internet-connected personal computer, may invigorate our commitment to ethnography. Ethnographic texts can now be consigned to a virtual archive where they are accessible to the writer of ethnographies as well as to his or her readers because, beyond being accessible (which physical archives may also be), texts in a virtual archive take on a kind of presence that even the most skilled ethnographer could not achieve as long as possibilities to refer to, cite, or display documents were severely limited by the conventions of publishing prior to the Internet (or to CDs that could be distributed with the printed book).

I have begun to think about the consequences that virtual presence of texts may have for our writing and will take this up later when I discuss the question of genre; here I limit myself to a point that is relevant to this
introduction: We have become used to opposing virtual to real and tend to forget that “virtual” originally connotes effectiveness, strength rather than weakness. Consigning documents to a virtual archive makes them more real, not in any ontological sense but in terms of their “practicality,” that is, as regards their potential to mediate between the events that were noted or recorded and our efforts to represent the knowledge gained as adequately as possible. This—a heightened presence calling for acknowledgment and response—has been what made me embark on this book. To make it clear, my point of departure has not been to start with a subject matter, a topic, much less a theoretical question and then seek evidence or support in a document. I began with the transcription and translation of a recorded conversation and, repeating somehow what happened when I recorded the conversation with Kahenga, I had at first no other plans than to deposit the texts in Archives of Popular Swahili. Only when I had almost completed the task of producing the texts—for reasons that will be discussed later on, this took several months of hard work—the idea of a book emerged.

Crossing Borders: Ethnography as Transgression

Recalling, as I do now, the story of the project as a series of almost inadvertent steps, I realize, suggests a procedure that may appear to violate principles of scientific inquiry (or the image we have of it). But then, such transgressive behavior echoes the crossing of borders that constituted the object of this study. Not counting linguistic and social barriers that had to be crossed earlier, at least three kinds of transgression led up to this project: The first one occurred when the ethnographer became a patient and client, something that in my mind (and in my memory) was not the same as moving from observation to “participant observation.” There was no “method” involved, no attempt to change roles for ethnographic purposes. Crossing that border implied, second, a transgression of sorts when we left the confines of Western biomedicine to seek treatment by an herbalist and, third, it meant stepping outside the boundaries of rational conduct when I became the client of a practitioner of magic or, perhaps, sorcery, to use labels that are as handy as they are inadequate.

Of course, there would be no document of what happened nor a story to
tell if these acts of transgression had been completely unselfconscious and free of the duplicity without which ethnographic research would be impossible—a duplicity that makes us cross borders but not without establishing a record that lets us return to our professional roles and habits. When I speak of the ethnographer’s duplicity, this is not to be taken as confessional breast-beating. I want duplicity understood in a sense that Nietzsche, in his essay on truth and lying, called *aussermoralisch*—neither immoral nor amoral but extramoral (1976). Put positively, duplicity is involved in performing, playing roles, which we do when we work “as” ethnographers; in that sense it is an epistemological concept helping us to understand how knowledge is produced. Not being forthright at all times because, without a certain shiftiness, a stranger in a society might not be able to move at all; putting on an act, say, by trying to communicate and interact above one’s linguistic and cultural means and competence; assuming multiple roles and guises as serious researcher, disinterested observer or hanger-on, affable conversationalist, convivial companion or friend—to the moralist all this must have something unsavory or outright repulsive. Yet, I would contend, ethnography, much like living our ordinary lives, could not be done in any other way.

So, how straightforward was I when I asked Kahenga to “close” our house in Lubumbashi? As best as I can remember I wanted to have protection. There certainly was no intention on my part to trick Kahenga into giving me an “ethnographic” performance (see also Fabian 1990b). When the day of the house-closing came I did not really know what to expect and had made no preparations for documenting the event. I was curious, of course, and professional habit, or compulsion, made me ask Kahenga whether I could watch him at work. Without hesitating for a moment he agreed and the ritual took its course. What I saw was fascinating. When it was over and Kahenga had left I was unable to resist the temptation to take a few notes on what I had observed—and that, at the latest, was the moment when I crossed the border between client and ethnographer. Or did that happen when I asked him whether we could meet again and talk about the things I had seen and heard? I don’t remember; all I can recall is that I wanted to record our conversation.

In fact, as I am writing this I realize just how precarious memories can be because the text, the protocol of our exchange, will show that borders were
crossed even earlier during the days when Kahenga did his work as a healer. He had allowed his patient to accompany him on a search for herbal medicines and to take photographs, and note down the names, of specimens he collected. A botanist at the university examined the photographs and came up with a list of tentative identifications. Specimens and photographs were before us when we talked. They played an important part in structuring our conversation and the text that is before us now.

Perhaps I should leave it at that and get on with formulating thoughts, insights, and findings which, as I announced earlier, will be based on that text, except that I anticipate a question I should answer, at least briefly, right away: What does “based on” mean? In a general way we use this phrase to indicate (show or claim) that the knowledge we present is grounded on evidence. But what does it mean specifically when the evidence is a text? Or worse, from the point of view of the “empirically” minded, when there is seemingly “nothing but” a text—no statistical data generated by surveys or standardized questionnaires, no compilation of facts found in archives or in “the literature”? Does that not make my project vulnerable to accusations of “textual fundamentalism”? This introduction, a circuitous story of how the text was produced, should have shown the contrary. Our document is not a foundation; it is not a ground on which conclusions may rest, if only for the simple reason that the text does not rest or just sit there. Its current state of fixation, I hope to have shown, was but a phase in a series of events. The text is not a depository of facts but a mediator. Its presence makes it a pièce de résistance on the road from past experience to future representation.

At this point, the reader hopefully has an idea of the long prehistory of this book; he or she may still wonder what exactly it is about and how I plan to present material and findings. As is customary I should now provide a concise statement of the subject matter, followed by a sketch of the way in which the presentation will be structured in the chapters that follow. Much as I want to respect custom, I find myself compelled to commit yet another transgression by leaving this task in suspense, for reasons I must discuss now.
Introduction

**Ethnography and Form: The Question of Genre**

Somewhere along the path I have described so far I decided to conduct this project as an experiment in breaking up conventions of ethnography that enjoin us to be clear about what we want to do before we start and to do one thing at a time. The working title of the project was *Closing House: A Late Ethnography*. It gave an indication of what I had in mind: putting an event, the ritual performed by Kahenga, at the center of attention. At the same time the pun on “closing shop” was intended: I undertake this project at a time when I begin to look for some closure in my professional work. Circumstances may see to it that this book will be my last and it is difficult to accept this without at least trying to take stock and coming to some kind of conclusion about matters that have occupied me in my work. How can I do this without making the ethnography of a rite of protection a mere pretext for an intellectual memoir? Either one may be of interest but can they be fitted into a coherent form?

Anthropology and readers of anthropological writing have by now become used to ethnography with a heavy dose of autobiography. That is not really my problem; I never thought it desirable or even possible to keep the author out of accounts based on research requiring his or her active presence. What preoccupies me is to find out what it means to write, as indicated in the provisional subtitle, a “late ethnography,” one that is belated, more than thirty years after the event, late in the ethnographer’s life, and above all late in an historical constellation—something we are trying to grasp with our theorizing about the postmodern, postcolonial condition. Is it not, to put this bluntly, simply too late to be writing ethnography?

True, we all but abandoned the once canonical form of the monograph when we realized how compromised it was by its roots in the imperial gaze of a science of mankind conceived as a branch of natural history. For a while, this made us agonize about, or, as some would prefer to put it, experiment with, genres such as personal or historical narrative, dialogue, poetry, and essay. Yet we still designate as “ethnographies” most dissertations and publications that are not textbooks or purely theoretical treatises. Whatever the outcome of these developments will be, it is difficult to imagine that, given
its growing popularity outside anthropology, the term “ethnography” will be abandoned in the near future (Fabian and de Rooij, in press). In the meantime, any contribution to anthropology will also have to be a contribution to the debate about genre, that is, about the quest for legitimate forms of presenting knowledge of cultures and societies.

Such a conclusion, I think, is inevitable. We are bound to worry about genre, no matter how tired we may be of literary introspection and how much we sympathize with the colleague who exclaimed “genre be damned” (Webster 1986; I think this was before the phrase became a fad). On the other hand, the irreversible turn of attention to literary form is no substitute for continuing to worry about the production, not just the presentation, of knowledge. In fact, I would argue that the only excuse for leading anthropology up the path of literary theory is epistemological; attention to genre ultimately means attending to generation, that is, to the making of knowledge.

Commentary as a Genre

What could be the genre commensurate to the task I set myself in this book? I must admit that it has been tempting to present what I have to say as a collage. Parts written in genres such as dialogue, personal and historical narrative, and interpretive essay could be assembled to form an evocative and coherent picture. In the end I could not envisage such a project. As I understand it, collage is more than juxtaposition of elements. At any rate, when applied to writing, collage is a metaphor and it is successful, rather than confusing, only as a poetic creation. Anthropology has a few masters of poetic collage; I am not one of them. And as to collage in its more literal sense, I am not about to embark on a pictorial mode of representation (ethnography as exhibit or tableau vivant) after all the criticism I have heaped on “visualism” in anthropological discourse (Fabian 2001a).

So, for better or worse, the question of genre must be faced and this book will be an occasion further to develop a position I began to formulate some years ago (Fabian 2002b): “Commentary,” I predicted, is likely to emerge as a genre of ethnography. The point of departure of the argument, introduced
above, was the new kind of presence ethnographic texts take when they are deposited in publicly (or at least widely) accessible virtual archives. As briefly as possible I should now spell out what this view entails.

First, to qualify as a genre, commentary must be more than just a gloss on a brief excerpt from a source, or an annotation to, say, a diagram or an illustration. As the form of a piece of ethnography, be it an article or a book, commentary requires the co-presence of a substantial text and the interpretive, analytic, or historical writing based on that text.

Second, commentary as a genre not only determines literary form, it defines a practice of writing. Commentary is made “practically” possible by the virtual presence of text(s) and it can be realized, practiced, within a community of writers and readers who have access to the Internet. While this may be considered limiting (despite all the talk about globality, access to the Internet remains limited), disadvantages are outweighed by the advantages of freeing ethnographic writing from many constraints inherent in print publication, foremost among them the injunction to keep presentations of ethnographic text (especially in the original language) to a minimum of quotes.

Third, these practical advantages are by no means only practical. They also have theoretical significance: the possibility of a form of ethnography that is not predicated on the absence of its object or, to be more precise, on the object being consumed by devices of presentation such as tabulating quantifications, drawing graphics, figures, and diagrams, or simply by a prose that, being either strictly descriptive or predominantly expository, manages to withhold from (some may say spare) the reader the events and documents on which it must nevertheless ground its authority. When I propose commentary as an alternative, this should not be misunderstood as just another version of recent calls (often long on ethics and short on epistemology) for “giving a voice” to our sources. Nor would I want to advocate burdening our readers with a kind of methodologism that preempts critical theoretical reflection by endless accounts of procedure. After all, our task is producing knowledge, and that involves more than either showing how it is done or just writing down what we know.
Commentary, Comments, and Memoranda

Now that the case is made for commentary as a genre of text-centered ethnography, that is, as a form that informs a piece of writing in its entirety, the question remains of how the general idea is to be realized specifically.

A condition of writing in the mode of commentary is, to repeat this, the presence of an ethnographic text. The awkward attribute “ethnographic” is meant to remind ourselves as well as our readers of the peculiar status of texts such as the one that will occupy us in this book. In a narrow understanding of the term, ethnographic texts are not literature. They are neither found nor written as fiction; they don’t come from, and are not meant to contribute to, a canon of readings. Nor are they, again in a narrow understanding, documents of the kind historians find in archives which they themselves did not set up. Ethnographers, one might say, can deal with their texts without being weighed down by a canon or an archive. Just as well, because the demands of the ethnographic text are heavy enough.

To begin with, the presence of a text puts us in a paradoxical situation. As the protocol of a performance or of a communicative exchange, a text’s presence signals the absence of the event it documents; the text may be present, the event is past. Commentary, therefore, is writing in the face of that tension. One may say this is the case with all narrative and even with descriptive writing (as writing, narrative and description are always “after the fact”) but tension is exacerbated, instead of relieved, by the seemingly nonproblematic presence (givenness, availability) of a text. Hence it would be wrong to think of commentary as a relatively relaxed genre, so to speak, free of the constraints imposed by conventional monographic rubrics that must be filled or analytical schemes that must be completed.

Thinking about this last statement set me off on a metaphorical reverie. Let us say that commentary is made up of many comments and that comments relate to commentary like bricks to a house; you build up a commentary with comments. This sounds plausible but there is something fundamentally wrong with the image (fundamentally: more than what is wrong with every image). Brick construction not only requires elements that are fabricated, made, not found, but also that these elements be identical (if not absolutely then at least “for practical purposes,” the ones a builder
has). The first requirement is met by “comments”; they must be made, formulated. The second one, if fulfilled, would mean that every comment would have to come from the same mold and that would mean that writing commentary consists of filling preexistent rubrics and the result would not be an alternative to, but a kind of, monograph.

If the brick doesn’t work as an image, what about the stone and stone-construction? A stone is found (or quarried), not made; it gets its shape from being hewn rather than molded and fired. As to the identity of elements, stone masonry has requirements similar to those of brick construction and would be useless as a metaphor, unless we consider building with fieldstones, in which case the elements may be quite dissimilar (big or small, rough or smooth, almost round or almost square, and so forth). That would come much closer to the idea I have of building commentary with comments if it were not for an inconvenient, unacceptable implication: Fieldstones are found in the field. Ethnographic comments, tempting as it may be to latch onto the “field” (as in “fieldwork”) as another metaphor, are definitely not just found.

So, short of declaring this reflection an idle exercise, what can we take away from it? Thinking of writing commentary as building it up from, or out of, comments may be misleading unless we take metaphorization one step further and postulate that when we write comments we make stones the are shaped like fieldstones. In plain words, we should not feel compelled to write comments that fit a mold or come out as standardized elements; we should pick them up in whatever shape they come and put them together in a structure that holds up.

What kind of building will be constructed in such a manner? If commentary is realized by writing comments on a text and if we keep in mind what was said about presence and absence, present and past, it follows that such comments may be thought of as memoranda: observations, statements, explanations, references written down as reminders, anything between what the text reminds us of and what we think should be remembered when we read a text. Remembering and memories are crucial to ethnography, as I have argued with regard to recognition of alterity as anthropology’s central theme (Fabian 1999), in a reflection on remembering in ethnographic practice (Fabian 2007: chap. 10), and will show in detail later on when I confront
the conversation with Kahenga. If memory is as important as I think it is in this undertaking, would “memoir” not be a label more appropriate than “commentary,” especially in a project that includes elements of autobiography? There are reasons to reject that option. First, this account will not live up to the expectations raised by calling it a memoir because it will be narrative only in parts and, at any rate, it does not aim to tell a single, coherent story. Second, in another (French) connotation, memoir may designate a thesis or dissertation and that is not what I envisage either.

For better or worse, I made a commitment to commentary as the genre of this book—after trying it out in two earlier, shorter pieces of writing (Fabian 2003 and http://www2.fmg.uva.nl/lpca/jlpca/vol1/fabian.html)—only to run into problems that began with deciding on a table of contents. What should be the major divisions, what their order or sequence? I toyed with three or four outlines and began to have serious doubts until I realized that my troubles were caused by a failure to distinguish between commentary as a practice of writing and commentary as a form of representation. I was reminded of the debate concerning dialogue as ethnographic practice and dialogue as a literary form. From the fact that much of our research consists of dialogues with our “informants” it does not follow that dialogue is the most, let alone the only, appropriate genre of ethnographic writing. Similarly, adopting commentary as a practice of dealing with texts does not mean that the presentation of knowledge gained must conform to a genre that exists in our literary canon. All that is required is that the final form of writing a commentary must be such that it can be traced back to commentary as a practice. Even monographs that followed rules of data collection and respected rubrics of presentation that had become established in our discipline came in many different kinds; this will be the case, even more so, with commentaries. Their tasks may ultimately be determined by anthropology’s current agenda but they must meet the exigencies of specific texts.

A Text Never Comes Alone: Con-texts

If relying for evidence almost exclusively on texts makes the ethnographer suspect of textual fundamentalism (see above), working with just one text should be even more problematic. It is time, therefore, to spell out what text-
centered approaches assume about texts and, perhaps more importantly, what they don’t assume. From the “empirical overkill” of which Franz Boas, one of the founding fathers of anthropology, was accused because of his massive text collecting, through the making of vast bodies of texts (“myths”) the grist for structuralist analytical mills, to text(s) as a key metaphor for culture (the latter accompanied by the disappearance of ethnographic texts from writing in the “interpretive” mode), anthropologists have tried out almost every conceivable approach without being able to establish theoretical consensus or at least widely shared habits and conventions in dealing with texts. Such a state of anarchy may be deplored; for me it has made working with ethnographic documents an undiminished pleasure. Of course, it also forces me to rethink theory and method every time I approach a text. Specific issues will be addressed as they come up later on; here I want to make only a few general statements regarding my present views.

Text-centered ethnography has been for me the practical consequence of taking a language-centered approach, which I adopted as the result of a conception of ethnographic inquiry as a (predominantly) communicative undertaking. In much of cultural anthropology knowledge production is interactive. As language and speaking mediate between the ethnographer and the people he studies, texts mediate between communicative events (experience) and the representation of knowledge.

In other words, when ethnography is called text-centered this regards above all its epistemological foundation; it does not mean that collecting texts is the principal purpose of research. A text, to become “collectible,” must be relevant to a project that is always wider than what a specific text may document. This also applies to the document selected for interpretation in this book. It is not “unique,” no matter how singular its history may appear. Perhaps not when it took place but certainly afterward, the conversation with Kahenga became part of a corpus of texts, a term that may have a technical meaning in literary theory but never loses completely its metaphorical connotations. Like a body, a corpus has size, volume, weight, articulation of parts and members; as long as it is alive it grows and changes.

The latter, growth and change, certainly fit the corpus of documents I produced (more often than found) in the course of my work as an ethnogra-
pher. What in the beginning (during dissertation research in the sixties) may have looked like data needed to carry out the proposed study of a religious movement; what I went after when I later worked on language and labor in the seventies; and what I came upon during shorter visits in the eighties—all this turned out to belong to a “body,” something I eventually grasped theoretically with the help of a concept of African popular culture (Fabian 1998). That culture—minimally understood as contemporary practices of survival—was the context in which Kahenga worked, and this makes the document of our exchange a co-text with many others.

If what I said about the new kind of presence texts acquire on the Internet is true, then the virtual archive should also influence our ways of dealing with a corpus of texts, and this should in turn affect the writing of commentary. For instance, specific issues (semantic, topical) that come up in the commentary on one text can now easily and quickly be searched for in other documents placed in the same archive (in Archives of Popular Swahili some are already cross-referenced; many more links could be established). Conversely, given their connections to one and the same easily accessible corpus, the text-centered ethnographies on popular religion, theater, and historiography which preceded the project introduced here will hopefully gain coherence and grow into a body of writing that has a chance of staying alive when current topical and theoretical interests fade.

What exactly will the commentary on my conversation with Kahenga recorded thirty years ago add to the ethnography of popular culture in Zaire/Congo? An easy answer would be to say that it opens the domain of popular medicine. My dealings with Kahenga might then become a case study in medical anthropology. I have no objection to such a reading as long as it is understood that an interest in medical anthropology was not what brought about the events and their records that will occupy us in this book. At any rate, it would be somewhat anachronistic to project back into the early seventies the image of a subdiscipline that thrives today but was then, as “ethnomedicine,” at its beginnings. These qualifications are important because they let me comment on a conversation about medicine without an obligation to relate my findings to state-of-the-art medical anthropology.
Introduction

Appropriation / Expropriation: The Ethics of Commentary

I have always thought that to practice anthropology reflexively and critically is all that is needed to make our work legitimate. We must be alert to the quandaries we face as members of institutions and as citizens or residents of countries that pursue political and economic interests, more often than not to the detriment of the societies we study. As a scientific discipline, anthropology should be governed by rules and habits of “disciplined inquiry” (not to be equated with “methods”) but I don’t believe that anthropology should be subjected to a code of ethics any more than mathematics or philology. Probably this sounds hopelessly out of tune with developments during the last thirty years that have made “ethics” a central concern. It means that I cling to a notion of my field as an (academically supported) intellectual endeavor rather than a profession whose relations with clients and sponsors require rules similar to those that are devised to guide the conduct of lawyers and physicians. I dread the possibility that, being caught between human-subject regulations and cultural-property exactions, it may become impossible for us to conduct ethnographic inquiries in a manner and for purposes—communicative ethnography producing shared knowledge—that made anthropology a distinctive form of inquiry.

This being said, I want to address, however briefly, questions of legitimacy encountered by text-centered ethnography of the kind I will pursue here. Concerning those that regard legalities of proprietorship and copyright to texts deposited in a virtual archive I refer to a statement on our website. As long as it is not clear what international law will eventually impose on us, we act on the assumption that documents deposited in a (virtual) archive are publicly and globally accessible. We also assume that, with exceptions (we have not made any so far), accessibility means freedom to quote, copy, or work on the texts.

Another issue I want to dispose of right away is usually referred to as the “protection of informants,” a requirement allegedly met by withholding the names of our interlocutors or using pseudonyms. In the past, I have dealt with this by simply not publishing anything that, to the best of my knowledge, could be damaging or dangerous to those with whom I worked. Very
little ever was; a more difficult problem has been to distinguish between
damaging and critical statements. I never subscribed to the precept that
ethnographers should be neutral toward (the representatives of) cultures
they study, if only because I cannot imagine “neutral” communication other
than interaction in a clinical setting, which is not my idea of research. If
there is a rule about withholding names it should be this: In our accounts
ethnographers and interlocutors are both agents; why should the author be
named and others remain anonymous?

Another question that has become inescapable in recent years is: Should
our interlocutors be listed as coauthors of our writings? My view of the
matter is that, yes, contributions should be acknowledged and, if possible,
documented but, no, those who worked with us should not be burdened
with the responsibility of authorship in a more narrow sense. Did I consult
Kahenga about these matters? I did not, though I tried to contact him
recently, simply to find out how he is and to inform him of my project. So
far, my attempts have not been successful and I don’t expect this will
change. I fear we must assume, as we do about Tshibumba, that he may no
longer be alive.

Finally, there is “appropriation,” a matter that is often discussed as a
problem of ethics (or politics) although it regards, in my view, above all
questions of theory and epistemology. Does ethnography as a kind of
knowledge, does commentary as a specific form of representing knowledge,
“appropriate” its contents? And if so, why did appropriation—making some-
thing one’s own—acquire such an unsavory taste when it is discussed in
anthropology? Allegations of appropriation come easy to those who are
prepared to reduce anthropology to its colonial-imperial history or to the
services it may render to postcolonial interests. While these charges are
transparent and may fit transparent crimes of cultural robbery, they depend
for effect on the conceptual fog from which they emerge.

A measure of clarity can be achieved when we keep in mind that “prop-
erty,” the concept that underlies appropriation, is an equivocal, shifty
beast. In talk about ideas or about culture and its creations, property/
appropriation is capable of changing, within the space of one argument,
from its literal meaning to analogy, to metaphor, or rhetorical trope and
back. Is it at all possible to speak of ethnography/anthropology as a form of
appropriating other cultures in a way that illuminates our work rather than just denouncing it globally?

Answering this troubling question may have to begin by asking a counter-question: Can the stuff we “make our own” when we learn about and understand, say, the imaginary characters of a story, the prescribed actions of a ritual performance, the movements or rhythms of a dance, and so forth be considered objects in an ontological sense? Are they things? Are not even manifest objects—we call them ethnographic or art objects—collected not just as things but as artifacts, culturally or aesthetically speaking? Between cultural diffusionism throwing hoes, myths, and kinship terms into one and the same bag of objects for the purpose of mapping the distribution of things cultural in space, and sociological functionalism-cum-positivism enjoining us to study social relations like things, comme des choses, as Durkheim put it, anthropology went through a long history of producing allegedly certain knowledge about objects whose status remained uncertain. Yet, without agreement about what can be considered an object (ontologically, to begin with, but also intellectually and legally), let alone about what kind of object can become property (and whose property it would then be), talk about ethnography as appropriation has no distinct referent.

Matters are made worse when, for ideological rather than logical reasons, appropriation is equated with expropriation. Under such an indictment ethnographers could not make anything their own without taking it away from the people they study. Which brings us back to the “ethics” of my project in this book. I would have had to be a villain or a fool to have undertaken the labors it took to transcribe, translate, annotate, and comment on our text if I had thought my efforts to convey an understanding of what happened when Kahenga closed our house left him or his culture dispossessed. I felt enriched by our conversation and I like to think the feeling was mutual. Kahenga was under no obligation to talk to me after he performed, for a fee, his services as a ritual specialist. Perhaps he was just being polite when he agreed to meet afterward; quite likely he had expectations similar to mine about discussing the nature of his work. At any rate, like all my other interlocutors, he was not offered and did not ask for payment for the conversation we had.
What We Talked About:
An Overview and Guide to the Text

This attempt to trace my project back to its remote beginnings and to place it in a context of current concerns with the nature and legitimacy of ethnography may have taxed the reader’s patience. That Kahenga and I conversed; why, when, and where our meeting took place; what remained as a document and what I intend to do with it—all this had to be brought up, at least summarily, so as to stake out the arena in which the text will be confronted. It is now time to take a first glance at the content of our exchange and at the succession of topics we covered.

In the outline that follows, numbers refer to paragraphs of both the Swahili transcript and the translation as they appear on the web site.

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