Anthropology's World

Life in a Twenty-First Century Discipline

Ulf Hannerz

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Anthropology, Culture and Society

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Acknowledgments

Most of the chapters in this book were not originally written to form an integrated and comprehensive treatise on what anthropology is now, or is not, or could be; in their first versions, they came into being as papers for conferences, or lecture series, or other edited volumes. These versions are identified in the endnotes for the particular chapters. As they appear here, however, the chapters have been extensively revised in order to fit better together, and to avoid repetition. Most of them have also been extended.

Let me take the opportunity here, however, to thank the members of those audiences who listened to early versions, and offered useful and generous comments: in Bayreuth, Berkeley, Budapest, Durham, Frankfurt am Main, Istanbul, Ljubljana, Poznan and Vienna.

Furthermore, I have been discussing the matters dealt with in this book, over the years, with a great many friends and colleagues, in many places. It would be impossible to remember all these conversations, and very difficult to draw a line around some smaller number of those conversation partners to be identified by name. It is at least a bit more manageable to thank collectively those circles of colleagues whose friendship, hospitality and intellectual company I have enjoyed in places to which I have had the pleasure of longer-standing links: in Stockholm, Vienna, Tokyo, Oslo, Tel Aviv and Jerusalem, New York and Oxford.

Finally, one close colleague deserves special mention, so let me do something probably seldom done in acknowledgments—I will quote myself: "In particular I am indebted to my wife and colleague, Helena Wulff Hannerz … for her constant thoughtfulness, support, and willingness to listen, read, respond and ask for more" (Hannerz 1992a: ix). True some twenty years ago, and still true.

Ulf Hannerz
Stockholm, February 2010

Series Preface

Anthropology is a discipline based upon in-depth ethnographic works that deal with wider theoretical issues in the context of particular, local conditions—to paraphrase an important volume from the series: *large issues* explored in *small places*. This series has a particular mission: to publish work that moves away from an old-style descriptive ethnography that is strongly area-studies oriented, and offer genuine theoretical arguments that are of interest to a much wider readership, but which are nevertheless located and grounded in solid ethnographic research. If anthropology is to argue itself a place in the contemporary intellectual world, then it must surely be through such research.

We start from the question: "What can this ethnographic material tell us about the bigger theoretical issues that concern the social sciences?" rather than "What can these theoretical ideas tell us about the ethnographic context?" Put this way round, such work becomes *about* large issues, *set in* a (relatively) small place, rather than detailed description of a small place for its own sake. As Clifford Geertz once said, "Anthropologists don't study villages; they study *in* villages."

By place, we mean not only geographical locale, but also other types of "place"—within political, economic, religious or other social systems. We therefore publish work based on ethnography within political and religious movements, occupational or class groups, among youth, development agencies, and nationalist movements; but also work that is more thematically based—on kinship, landscape, the state, violence, corruption, the self. The series publishes four kinds of volume: ethnographic monographs; comparative texts; edited collections; and shorter, polemical essays.

We publish work from all traditions of anthropology, and all parts of the world, which combines theoretical debate with empirical evidence to demonstrate anthropology's unique position in contemporary scholarship and the contemporary world.

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Professor Vered Amit
Dr Jon P. Mitchell

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Introduction: In the World, and a World in Itself

Anthropology's World, as in the title of this book, can mean at least two things. On the one hand it is anthropology as a social world in itself—the community of a discipline, with its internal social relationships, its ideas and practices. On the other hand, anthropology's world is the wider outside world to which the discipline must relate in various ways. For anthropology, which more than any other discipline may have a constant ambition to be global in its scope, this involves humanity everywhere, and the attempt to understand its variety of ways of life and thought and its conditions of existence. It is a world anthropologists are inclined to think of as made up of a multitude of "fields": research sites, actual or potential. In a more close-up sense, however, that outside world also includes people and structures which demand attention on a more everyday, often practical level: wider academic environments, student populations, local or national publics, the media. In both these senses—or perhaps I should say all these senses—the world of anthropology keeps changing.

This book is about some aspects of contemporary life in this world. Anthropology is now a global discipline both through engaging in research everywhere (at least in principle) and in having local practitioners everywhere. Yet within that worldwide community there are variations in scholarly interests and in working circumstances. In what follows I will draw continuously on my own experiences, taking my own path through anthropology's world. My enduring perspective is from one corner of Europe, but looking out. Over the years, I have developed close ties with anthropologists in this part of the world, but I have also had, and continue to have, some of my own formative experiences in American anthropology. When I have a chance (and such opportunities have included various stays and visits in Africa, Asia, Latin America and Australia), I cultivate contacts with colleagues elsewhere in the world as well.

The subtitle of this book specifies that its focus is on what the discipline is or can be now: in that twenty-first century we have

already moved some distance into. Generally around us, these are times of many surprises: on the front tables in bookstores there are bestsellers with titles like *The Black Swan*, or *The Age of the Unthinkable*. What will happen next in anthropology, or to anthropology, is not easy to forecast. But some questions will be there, to be definitively answered or (more likely) to be debated again and again, perhaps wherever there are anthropologists. What, in these times, is anthropology for? What is its place in the world? How do we go about our work? Who should work where? How do we want to be understood, and how do we not want to be seen? For whom do we write, and whom should we read?

I will try and confront here some of the challenges that anthropologists face today and will face in the future. But I am inclined to take the long view towards them—in large part that of a twentieth-century anthropologist. It is almost 50 years since I began as an undergraduate student of anthropology (strictly speaking, in something then still named "general and comparative ethnography"). Some of the changes in the discipline and in the world since then have been fairly quiet and gradual, others more turbulent. I would hope that my sense of the present may in some ways be sharpened by a sense of the past. Moreover, as I will argue particularly in Chapter 7, that past can also be explored as a resource for the continued renewal of the anthropological imagination.

The period when I began in anthropology was, for one thing, still one of decolonization: Asia had mostly come through it, Africa was still in it. That dramatic historical process had much to do with my choice of direction, and no doubt many in my generation of anthropologists shared this interest. Yet if it was decolonization and newly independent countries that attracted us to the discipline, the growth of anthropology had until then clearly had its links to colonialism itself. Scholarship had developed most strongly in those European countries which were also major colonial powers, and in those other countries where European settlers had established their domination over indigenous populations. For a period in the latter half of the twentieth century, consequently, new cohorts of anthropologists and anthropology students were preoccupied with figuring out—revealing, debating, pinpointing—the nature of this factor in the relatively recent history of the field.[1]

After a decade or two, that issue had more or less sedimented as a part of the discipline's past. It had become more a topic of intellectual curiosity, less a moral burden or a topic of conflict between academic generations.[2] The decolonization of anthropology

itself, however, had some enduring consequences. One of them was that it was no longer intellectually, morally or politically defensible to have a separate discipline for those parts of humanity which were "non-western"—sharing only the characteristic that they were exotic to the Occidentals from whom they were thus separated (although through colonialism they had also been linked to, and mostly dominated by, these Occidentals). It is from this point on that anthropology has moved towards being more explicit and consistent in identifying itself as a discipline concerned with all of humanity.

But that concern also meant that it became legitimate, perhaps even necessary, to engage as well in what became known somewhat loosely as "anthropology at home."[3] For a variety of reasons, this has become a rather large proportion of anthropology as currently practised, although it works out in different ways in different places, depending on a number of conditions. It cannot now be taken for granted that the anthropologist, in the field-working, ethnographic phase of his or her work, is an expatriate. This fact has implications for the world of anthropology both in its internal relationships and in the interfaces between the discipline and its surroundings, inside academia as well as with the wider society.

It has also been in the times after colonialism that anthropology has really developed as a worldwide community of practitioners, becoming more or less well-represented in countries that never had colonies (or only briefly, or on a very limited scale), and in those that were themselves once colonies. In that way anthropology has diversified, and the question is reasonably raised to what extent we should now speak of anthropologies in the plural form—national and regional varieties, shaped by differing histories, circumstances and interests. I have something to say about this in Chapter 2, where I also comment on the relationships between such anthropologies. It is also one aspect of this global spread of the discipline that it is now conducted (thought, spoken, taught, written) in more languages, for different purposes. That has consequences for its internal cohesion, as well as for its relationship to its local and national environments. I turn to these matters in Chapter 6.

Anyhow, for a discipline self-consciously defining itself as global in scope, a more recent development has also had implications for how research fields are defined, and where they are found. The term "globalization" really worked its way into everyday language only towards the end of the twentieth century. In reaction to its becoming a buzzword, it has been pointed out often enough that the realities of global interconnectedness have been around much longer, although

they may not have been equally acutely experienced by everybody: to take one example, the West Africans transported away across the Atlantic a few hundred years ago in the slave trade, for deployment on the plantations of the New World, were certainly being forcibly "globalized," and colonialism was itself one form of globalization. Yet the rapid spread of the new label reflected a new intensity in such interconnectedness, new forms, and not least a new diversity of forms. An integrated world economy with built-in inequalities, new material consumption patterns, media with new capacities to carry a great variety of cultural forms efficiently across great distances, transnational labor migration, refugee streams, diasporas, long-distance tourism, a plethora of international organizations and transnational movements, international crime and terror syndicates—all of these are conspicuous ingredients of the emergent global ecumene.

If the dominant mode of work in anthropology during much of the twentieth century, not least in field research, had involved a standard operating procedure of focusing on bounded local units (always, to a degree, an analytical fiction), the varieties of globalization and transnational connections posed new challenges to the discipline, which thus switched to seeing the older types of units as more open, and at the same time increasingly took on other units with not-so-local characteristics. Perhaps, after all, such steps came fairly readily to anthropologists, once they moved away from more distinctly local circumscriptions of their fields: they had never been as committed to the nation-state as the unit of societal analysis— what has been termed "methodological nationalism"—as some other scholarly disciplines have tended to be.[4] Rather, they followed their topics wherever they would take them in the global terrain, allowing ethnography to show the ways the world comes together.

This, of course, is not to say that the entire discipline has now turned to the study of globalization. I became involved quite early in anthropology's global and transnational turn; although, not much later, I suggested that the time would come soon enough when global connectedness would itself hardly be so much of a research focus, but would be largely assimilated within the background understanding of much ethnographic work.[5] Yet this connectedness has also added a range of new research topics to all those fields of study which were already established, and which mostly continue to be cultivated. The new topics and experiences have also played a part in provoking some reconsideration of key concepts, and of methodology. What should we mean, for example, by "culture"—

and what should we ensure that we are not taken to mean? (See Chapter 3.) And again, what is now a "field," and what is field work? (See Chapter 4).

The introductory course through which I passed into anthropology's internal world was offered in a minuscule unit which had only very recently been constituted as a university department—it actually functioned as part of a much older ethnographic museum. The course had drawn a mere handful of students, perhaps a dozen. By now, in any more sizeable university, such a small number would very likely be considered a disaster. Over the last fifty years or so, the discipline has grown, I would say enormously, in terms of its number of practitioners and students, and also in terms of the institutional structures they inhabit. While it has been argued, as above, that anthropology was a child of colonialism, in terms of its population size, it really grew up in the postcolonial era.

Academia, in its varied shapes, at present makes up a large part of anthropology's world (in most places, I am sure, much larger than museums, which had a proportionately greater part in the discipline's earlier history; these seem to have become less places of work, and more objects of study). If there are thus now many more people teaching and learning anthropology, it is likely to have something to do with the way the central concerns of anthropology match changes in their world. More of these people seem to sense that this is a discipline which speaks to their personal experiences: one where they may expand on these experiences, organize them, and even put them to use. In my own introductory course those many years ago, probably all the students in the class were ethnic Swedes like me, mostly of similar background and experience (a large part perhaps even stereotypically blond and blue-eyed). Certainly that continues to be true in some places: students are embedded in everyday milieux of mostly cultural sameness, and meet the facts and stories of anthropology, the message of diversity, with a fresh sense of wonder. But in other places, many students now receive more of their own impressions from encounters with the foreign, whether in their own neighborhoods or from backpacking around the world. Some of them, too, will have their very own roots in the distant places we lecture about and make them read about, and their own views of them.

So classroom encounters may show us how some facets of contemporary global interconnectedness impinge on the way we do anthropology, think anthropology, talk anthropology—even when globalization is not itself our intended topic. But it makes its

appearances elsewhere as well, as we now engage in our long or short conversations with people around us. A certain amount of cultural relativism may long have been a part of the anthropological message (at least as a critique of simple-minded ethnocentrism). That may have come more easily, perhaps too easily, when other cultures were mostly somewhere else. Does it make a difference that, for many people, some of those controversial ideas and practices of Others are now in evidence among neighbors and work mates, in their children's classrooms, even among new members of their families? And generally—in the flow of information or disinformation about other parts of the world and their inhabitants, through news media, entertainment channels, and political rhetoric—how should anthropology be heard in the crowd? What can be its part in the public division of communicative labor? Chapter 5 takes up some of these questions, examining how varieties of anthropological research and reporting can contribute to greater transparency in a world combining interconnectedness and diversity.

Getting out of the classroom, on my memory trip, come along for a moment to the office as well. The department office in the 1960s was fairly low-tech: there were typewriters, carbon copies, and rather untidy mimeograph machines. A bit later on, photocopiers and fax machines already made a difference. To the field you perhaps carried your portable typewriter, and a likewise supposedly portable tape recorder which was in truth quite unwieldy. What certainly makes the practice of anthropology in the twenty-first century different from what it was during most of the twentieth, in a development which also has its obvious connections to globalization, is the arrival of the Internet, and everything that goes with it. Anthropology's world, in both the senses identified above, is now also a cyberworld. This has become quite central. It entails changes in social and cultural life generally, and consequently in our field studies of that life, and it can even provoke debates about what should count as field studies. Taking the more internalist view of the discipline's own smaller world, the ubiquitous presence of that screen penetrates our everyday activities—our reading, writing, publishing, teaching, and chances of collaboration.[6] I will touch on this in several chapters, though we can be reasonably sure that some of its possibilities have not yet been explored, or fully exploited—including, perhaps, some new ways of spending time less well.

But back to the classroom, and the growing student numbers of the later decades of the twentieth century. While the expansion of anthropology in the universities of the world sounds like a success

story, and the attractions of anthropology itself surely had a large part in this growth, we cannot disregard the fact that it also reflected the overall expansion of higher education in this period. In much of the world there are now more colleges and universities, they have become larger, and it follows that there are both more students and more teaching jobs. Yet academia also has its problems, mostly not peculiar to anthropology. It is quite widely recognized that in much of Europe, and in many other parts of the world as well, the increase in resources for teaching and research has lagged behind the growing student numbers, particularly in the wide field of social sciences and humanities where anthropology usually finds itself. In many places and too many fields of study, too many students (I am thinking particularly of undergraduates) get too little teaching, hang around for too long, sometimes drift away without the degrees or other qualifications they were supposed to get, and finally head off towards what would appear to be an uncertain future in occupational life. These are not the circumstances in which it is always possible to carry out either teaching or learning in the way one might have liked; even so, the challenge is there to ask what kind of curriculum, and what sort of pedagogy, would best serve the purpose of introducing newcomers precisely to anthropology's world.[7]

While we may have been inclined to see some rather irresponsible politics, often at a national level, at the roots of some of the difficulties of the academic teaching industry, we have more recently also seen the political reactions to them, forming in combination with wider conjunctures. I tend to be wary of terms that come into fashion for which the border between analytical scrutiny and political cliché threatens to become blurred. Yet it seems undeniable that, in the last couple of decades or so, we have seen the emergence of a major, more or less worldwide set of ideas and practices which I would describe as a neoliberal culture complex, and which also—I believe especially since the turn of the millennium—has tended to affect lives and institutions in academia. It is obviously a central assumption of neoliberalism that "the market" generally offers a superior model for organizing activities and social relationships. Yet, in Europe at least, where universities tend to be in one way or another closely tied to the state apparatus, this stream of thought is conspicuously present in the reshaping of state management. Some of its manifestations actually seem less in evidence in North American universities, which are more pluralistic and under rather less centralized control; although since the late twentieth century, they and their professoriate have come under more pointed ideological attack, largely as part of

the "culture wars." So here as well one finds critical or pessimistic pronouncements suggesting the demise of the university in its more scholarship-centered form.[8]

A number of recurrent keywords—accountability, transparency, privatization, quality control, branding, auditing, excellence, ranking—signal the presence of the neoliberal culture complex. When it makes its way across continents, like other such complexes in history it takes somewhat different shapes in different settings, as it interacts with what was already in place. The complex may acquire national characteristics, and in academia its encounters with different disciplines work out in varied ways. It seems to have merged most effectively with the natural sciences, medicine and technology, partly because their products tend to be those of greater interest in the marketplace, but probably also because there are other intellectual, organizational and procedural affinities. By contrast, when the neoliberal complex meets the humanities and at least some of the social sciences, the frictions tend to be greater. There seems often to be little insight within higher-level political decision-making into the varied modes of knowledge production in different scholarly fields, and little curiosity about the unanticipated consequences of decisions. The recently popular practice of concentrating research funding into large lump sums in the hope of an instant creation of "centers of excellence," for example, probably fits better with the research practices of some disciplines than others. Yet decisions on such matters seem not always to be preceded by much careful analysis. Generally, the politicians of neoliberal academia would not appear to attach any particular importance to the reproduction of disciplines, or the survival of departments.

Perhaps it will eventually—I would hope sooner rather than later—be understood that universities cannot be run quite like businesses, that their multifaceted cultural roles demand some particular care, and that different disciplines may work according to different logics.

This is not to say that all changes are to be resisted. Who can be against accountability, transparency or quality control as a matter of principle? It is true, too, that the anthropological understanding of human ways of life has in no small part been a study of varieties of environmental adaptation; and at this point we may want to give some thought to how our own community may best not only resist the neoliberal culture complex, or argue for changes in it, but also make such strategic adaptations to it as best serve our long-term interests. Perhaps we may even occasionally find that

the environment involves not only constraints, but also some emergent opportunities.

Personally I have also spent a fair amount of time, on and off over several decades, as a ground-level academic administrator, chairing my department (and also a couple of years running a small institute of advanced study). Academic organizations have their peculiarities, but in some ways a department head is indeed much like a small business owner: trying to make ends meet, keeping employees reasonably happy, attracting a flow of customers, and turning out a reasonably satisfactory line of products. That role is not always easy to combine with that of a scholar (although one had better try), but it may breed a certain sensitivity to what goes on at the interfaces between a discipline and at least some segments of the external environment. For one thing, especially in a period when that environment seems more turbulent than usual, one may worry about how that discipline which is one's business presents itself, and how it is understood by a wider public. That kind of concern provides a point of departure for Chapter 3.

Then again, as I pointed out above, this book deals only with some aspects of contemporary life in anthropology's world; it makes no claim to a complete overview. As may already be clear, it largely stays away from the particularities of the "-isms"—the sort of things anthropologists usually think of as theory and theoretical debate. The focus is on more general, and probably more durable, principles and practices in anthropological work. Some issues that could have been raised have also been left out because I know less about them, have never thought much about them, feel less strongly about them, or have already dealt with them elsewhere. A few of those areas which the book mostly does not deal with, however, I at least want to identify.

One involves a major change in academic anthropology in the second half of the twentieth century. As late as the 1960s, there were remarkably few writings in anthropology focusing on gender or on women's lives—despite the fact that this tended to ignore half of humanity, and despite the early presence of a number of quite prominent women anthropologists. As it happened, one of my first published articles in anthropology was on a gender topic.[9] Since then I have not added much to the body of writings on such topics myself, but probably there are now about as many women as men among professional anthropologists (if not more), and gender issues are continuously dealt with along varying lines, theoretically and ethno-graphically, just about everywhere in anthropology's world. You will

no longer find one of the flagship journals of the discipline named simply *Man* (as it was until 1995). (English, which I usually think of as a language rich in nuance and distinctions, is remarkable in not having a simple word for "human being" without that ambiguous gender bias.)

By now, my perspective might even be a bit contrarian. Quite often these days, in many places, the students in undergraduate anthropology classes include strikingly large proportions of women. Assuming that this is not in some country which has recently lost a great many young men in war, or which keeps them incarcerated in jail instead of sending them to school, they must be out there somewhere, exercising some choice of their own. So why are they not coming, in the same numbers as the women of their cohorts, to introductory anthropology? Again, this discipline is about all of humanity, there is room in it for very varied personal interests, and it is probably widely agreed that it benefits from a diversity of interacting perspectives. So I see no intrinsic reason why it should tend to become more a part of general education for one gender than for another. In certain places, a significant challenge to anthropology teaching may now be to find a way to reach these young men—without losing the women students it has gained.

I should also note here that an increasing number of anthropologists now find their working opportunities outside academia—in government, in business, in other organizations. In some parts of the world, not least those with weak and erratically functioning universities, NGOs have offered desirable alternative employment, with implications for the shape of anthropological practice. In large part, the growth of such a professional anthropology is surely itself an outcome of the expansiveness of universities, and I do not see any reason to regret that some considerable proportion of the community extends outside the campus environment. A discipline that merely reproduces itself as an inward-turning ivory-tower specialism does not seem like an entirely attractive and easily defensible prospect. While I will not focus so much on these other parts of anthropology's world in what follows either, I see it as an important challenge for the discipline to keep its borders open, and the conversations going, between what remains on campus and what has ventured outside it.

The world of anthropology inhabited in these pages, I should likewise acknowledge, may seem a bit limited to some readers: I am largely concerned with the one-field discipline of social and cultural anthropology, rather than that of the "four-field approach"

prevalent in much of North American anthropology. It is a difference I am recurrently confronted with, for one thing, when I arrive at the immigration desk of an American international airport; as I am identified as an anthropologist, the officer in charge starts joking about bones and potsherds. As I understand it, the gathering of cultural anthropology (sometimes labeled ethnology) with archaeology, linguistics and biological anthropology under a single disciplinary roof was largely a historical product of the early focus of American anthropologists, more or less until World War II, on North American Indians, which included an inclination to gather all knowledge about them in a single academic space. Meanwhile, the horrendous memory of some of their continent's twentieth-century history (in which one version of physical anthropology did indeed play a part) may have made European anthropologists particularly averse to blurring the boundary between what is, or what is alleged to be, biology and what is not, and therefore to one seemingly threatening implication of the four-field combination. I realize that this contrast between European and North American maps of anthropology has not been, and is not now, entirely stable—where it has been institutionalized, the "four-field approach" is at present under debate, and is at times more celebrated as a principle than it is actually maintained as a strong scholarly practice. Meanwhile, there are signs that something resembling it may be growing in certain corners of European anthropology.[10]

Then again, other kinds of disciplinary boundaries and border zones can also complicate the place of anthropology in the academic landscape. In the late decades of the twentieth century—and mostly, I believe, in the Anglophone parts of the world (especially Great Britain)—the rise of "cultural studies," perhaps as much a movement as a discipline, caused some consternation and irritation both in anthropology and in other established fields.[11] One could argue that it found its intellectual niche because some of these fields had for too long disregarded a number of increasingly significant phenomena and issues: popular culture, the media, class, youth, gender, transcontinental migration and minorities. On the whole, it may seem by now, cultural studies was ultimately more successful as a concept for marketing books and journals, and in launching a handful of successful scholarly careers, and less so in institutionalizing an autonomous existence within academic structures—even the University of Birmingham, England, where it all began, turned out not to provide a secure base. I will refer to it again in passing, but

I would assume that anthropology by now has learned something from its rise: one should not ignore emergent social and cultural phenomena that are near at hand.

Meanwhile, other countries and regions may define disciplines in yet other ways. That can also add to the diversity of anthropology's world, as will already be evident in the next chapter.

2
Editing Anthropology:
Two Experiences in Space and Time

Writing—writing anthropology, writing culture—has drawn a fair amount of attention and comment among anthropologists for some time now.[1] While otherwise the travails and the heroism of field work have been the obvious focus, it is true that only through writing (or some comparable communicative technology) does the anthropologist contribute to a shared body of scholarship and knowledge. Until, or unless, he or she "writes up," the field worker is not really an ethnographer, only someone indulging in a kind of deep tourism. In writing, the anthropologist takes on a complicated task, trying to do justice to the people written about, and attempting at the same time to reach out effectively to one audience or other. At both ends, anthropological writing thus tends to be a social engagement—on closer inspection, very likely a complex one, shaped by the past, and with implications for the future. And at the same time writing itself is in large part an individual, even solitary activity, possibly with its own crises. No wonder that the complexities of writing—the purposes, the risks, the successes, the failures, the sheer styles and techniques—can become a preoccupation once we start thinking about them. I will come back to that—in particular the matrix of those social engagements—in another chapter.

Much anthropological writing is indeed closely linked to field work; the term "ethnography" to a degree conflates them. But now and then, in some other chunk of time, the anthropologist may engage with writing in another way: editing. This—in the sense of putting together the work of some number of other people—is a second-order endeavor dependent on writing; it is desk work rather than field work, many anthropologists may do little of it or none at all, and it can seem like rather humdrum activity. Yet there are things to be learned from editing, as a concrete intellectual and organizational exercise. Leaving journal editing aside (an undertaking with characteristics of its own), whether you are dealing with a book or a thematic issue of a periodical, you have to come up with a basic idea, and polish it, delimit it and make it presentable, and you have

to identify your desired contributors (and also those who, having acquired notoriety for not delivering, should be avoided), recruit them, and then manage them. There is a division of knowledge to consider here, as well as perhaps a complementarity of perspectives. And since these chosen collaborators may then still surprise you, in terms of both content and style, you may have to come back, perhaps again and again, and perhaps creatively, to matters of coherence and standards, as well as to delicate issues (not least toward the end) of timetable and deadlines—perhaps even rejections. After all this, you may just possibly wonder if you have not spent as much time and effort editing that volume as you would have had you withdrawn into your chamber and written an entire book yourself.

But there is a very strong likelihood that this edited work is one that you simply could not have done on your own. Moreover, edited volumes, while not always warmly welcomed by publishers, have after all had a considerable impact on anthropology, from *African Political Systems* to (indeed) *Writing Culture* and beyond. So we will very probably be attracted to their possibilities again and again.

In this chapter I will look at two editing experiences of mine. One involved a particular journal issue, on the theme of national anthropologies. The other, a much larger-scale enterprise, had me in the role of anthropology editor for a major encyclopedia. Together, I believe, these engagements can offer certain concrete insights into some characteristics of anthropology's world—not least its ordering in space, across boundaries, and in time. That is why I have placed this chapter early in the book.

THE SHAPING OF NATIONAL ANTHROPOLOGIES

Some time in the early 1970s, a manuscript came into my hands, by Maurice Freedman, then professor of social anthropology at Oxford. It was a review of social and cultural anthropology, commissioned by UNESCO; for one reason or other, it only appeared in its final published form after the author's death (Freedman 1979). The fairly slim volume was remarkably well-informed and even-handed. But what stuck in my mind, in particular, was Freedman's (1979: 14) point that "it could be argued as an absorbing paradox that the internationalism and transcultural nature of anthropology lie precisely in its plurality of national viewpoints."

Around then, I had been doing a bit of comparative anthropology-watching. I had seen a fair amount of the American and British varieties of anthropology, as well as a little of some others,

and I had been observing at close hand the commitments and peculiarities of Swedish anthropology in its first decade or so of serious growth; so on that basis I thought Freedman's suggestion might deserve some more attention. I suggested to a colleague with interests in intellectual history and the sociology of knowledge, Tomas Gerholm, that we should try and put together a journal issue on "The shaping of national anthropologies." It turned out to be a somewhat complicated enterprise, but materialized in the journal *Ethnos* a few years later (Gerholm and Hannerz 1982).[2] Mostly we were able to recruit contributors within our network of friends and acquaintances, or by way of them.[3] Apart from our joint introduction, there were articles on anthropology in India, Sudan, and Brazil. Given that the Iron Curtain was still in place, and our personal contacts with colleagues behind it more or less nonexistent, we had some trouble finding someone from there to add a piece to our collection; but after consulting with one or two Central European anthropologists in exile, we made our way to two Polish anthropologists who agreed to take on the task. We then had had in mind getting a contribution on Canadian anthropology—but as it turned out, the sensitive politics of scholarship at the time made it necessary to get two—one on Anglophone anthropology in Canada, and one on Quebec. I contributed an article on Swedish anthropology, and then, as an afterword, we included "a view from the center" by George Stocking, of the University of Chicago, and the leading historian of anthropology. As can be seen, apart from that by Stocking, the articles were all about national anthropologies that were, at least at the time, less central in the internationally constituted discipline. The authors were also all local, "natives," residents, rather than expatriates or members of the national diaspora.

In our introduction to the issue, Gerholm and I identified a number of factors that could play a part in pulling anthropology in different directions in different national contexts. We did not intend to celebrate such differences, but to analyze how they could come about. As far as more internal factors were concerned, we pointed, somewhat skeptically, to the possibility of general intellectual styles in national cultures that could penetrate the respective anthropologies. We noted some of the social conditions that could generate diversity or maintain uniformity within national academic structures: local contexts, department structures, dominant funding bodies, the exchange of examiners and seminar speakers. It was clear to us that disciplinary boundaries are not drawn in the same way everywhere, and that national university degree structures may influence which

subjects go with each other, and to what extent contacts with other disciplines become possible or attractive. It was also apparent that, at least historically, national anthropologies often had distinctive tendencies in social recruitment: the discipline could have attracted prosperous cosmopolitans, or members of ethnic minorities, with possible implications for scholarly perspectives. We could also see that some national anthropological communities had a more conspicuous representation of some generations than others, and that this could leave its mark on the distribution of intellectual, and ideological, commitments.

We did, however, focus much of our attention on external influences—on the way those national anthropologies we were dealing with interacted with "international anthropology." In the metaphorical language we used, we identified "a prosperous mainland of British, American and French anthropologies, and outside it an archipelago of large and small islands—some of them connected to the mainland by sturdy bridges or frequent ferry traffic, others rather isolated." On the whole, the people on the islands did not take much notice of each other; but if an Indian and a Scandinavian anthropologist were to meet, they might try to place one another by way of common acquaintances in Chicago or Cambridge, "rather like Australian Aborigines identifying each other by searching for kinship links when they meet as strangers."

What shaped this geography of center-periphery relationships? In our introduction we lined up a number of factors, large and small, beginning with the fact that Britain and France had had their empires, and the United States its "internal colonialism" vis-à-vis Native Americans, which had given them a head start in developing anthropologies, and that these were also large countries with strong and comparatively affluent academic structures generally. And we emphasized that their languages reached out widely beyond national boundaries. We also pointed to a whole range of other interrelated realities or possibilities—international cultural policies involving exchange programs and subsidized textbook editions; scholars from centers visiting peripheries as teachers, and scholars from peripheries coming to centers as students; or the personal interest that a field worker from the center might occasionally have taken in helping the local scholars of the periphery. Other contributors to the issue took up the international center-periphery influences in their own ways. The Indian contributor raised very large questions about the transplantation of intellectual traditions from one civilizational context to another. The Sudanese case involved a transition from a

colonial anthropology dominated by expatriates to a postcolonial anthropology shaped by the preoccupations of local scholars. The post–World War II penetration of a new ideological hegemony had to be treated somewhat circumspectly in what was still Communist Poland, although openings in other directions were also noted. But then, rather less delicately, the writer on anthropology in Anglophone Canada declared, already in his title (drawing on a more general formulation by the eloquent Canadian prime minister, Pierre Trudeau), that a major fact of life in his anthropology was that it was "in bed with the elephant." Canadian anthropology had a hard time maintaining any kind of autonomy, as the influence of the United States was just about everywhere.

The concluding comments on "The shaping of national anthropologies" by George Stocking were thoughtful, multifaceted and subtle, as expected; also perhaps a bit bleak. I will only dwell on a couple of his points. He noted that some of the national anthropologies at the periphery were quite small-scale enterprises: "Counted in terms of its faculty, current graduate students and past degree recipients my own department at the University of Chicago is larger than most of the national anthropologies represented in this volume, and it grants less than a twentieth of the total number of doctorates produced annually in the United States." He experimented with a certain typologization of the anthropologies represented in the issue: Poland and Sweden were "secondary metropolitan," Brazil and the two Canadas "white settler," India and Sudan "ex-colonial." Probably more fundamental yet, Stocking began his discussion by pointing to a contrast between anthropologies of empire-building and of nation-building, most obvious in parts of Europe in the separate disciplines often described by the German terms *Völkerkunde* and *Volkskunde*. Historically, the former had been concerned with the distant dark-skinned others of an overseas empire, the latter with those internal peasant others who were, or should be made, part of the nation—symbolically, even placed at its heart. The metropolitan anthropologies, those we had seen as constituting the center of the discipline, had been of the empire-building kind.

Even as it appeared, our journal issue was not entirely unique in focusing attention on the characteristics of national anthropologies, their varied histories, and the divides as well as the connections between them. The Wenner-Gren Foundation had sponsored a conference on national and regional intellectual traditions in the discipline in 1968; it took quite a long time for the resulting, rather quirky volume, *Anthropology: Ancestors and Heirs*, edited

by Stanley Diamond (1980) to appear, and it was by then already significantly out of date. The same foundation also supported another conference about a decade later, and in this case the book, *Indigenous Anthropology in Non-Western Countries*, edited by Hussein Fahim (1982), came out rather quicker, in the same year as our *Ethnos* issue. It was, as the title shows, a volume with a more specific focus, but one that to an extent overlapped with our concern with the national anthropologies of the periphery. Yet our edited issue drew a fair amount of attention in the time that followed. The Indian contribution was reprinted in a New Delhi–based journal of opinion, and I understand that at least parts of the issue were rather widely circulated in photocopy in anthropological circles in Latin America, where the Swedish journal was presumably not easy to come by. Especially the Introduction to the issue and George Stocking's Afterword have been quite frequently cited, the latter no doubt because Stocking also included it in a collection of his own writings (Stocking 2001).

But then the research and writing genre of scrutinizing national and regional anthropologies, and the overall international structure of the discipline, also took off around the turn of the millennium. While, again, the Wenner-Gren Foundation played a certain part in sponsoring a conference on the topic of "world anthropologies" (Ribeiro and Escobar 2006), there were also a great many other collective and individual initiatives.[4] By the time I was invited to contribute an afterword to one of the resulting volumes, a quarter-century after the *Ethnos* issue, I could comment that "the anthropology of anthropology has become a world-wide reflexive effort," and that there was now perhaps hardly a country where anthropology existed in which the history and current characteristics of the discipline had not been chronicled and/or analyzed, briefly or at length (Hannerz 2008a). This by now allows more of a comparative view of variations in the discipline, and also some sense of recent changes.

With regard to linkages across borders, these later publications repeatedly referred to our imagery of mainland and archipelago. On the whole, I would say, its appropriateness was accepted. But then it has seemed that, over the last two or three decades, there has been some reconfiguration of relationships within the international anthropological community. It appears to be a fairly widespread view that those three anthropologies of the center—British, French, and American—are by now less each other's equals than they used to be. If French intellectual life has continued to exercise a strong

influence in anthropology almost everywhere, including in the Anglophone metropoles, it has recently more often done so by way of the work of philosophers or sociologists (Foucault, Ricoeur, Derrida, de Certeau, Bourdieu, Latour) than that of card-carrying anthropologists. Benoît De l'Estoile (2008b), who has thoughtfully sketched one comparative approach toward the internationalization of the discipline, has concluded that "France appears in a situation of a former central anthropology that contended for hegemony, rapidly being provincialized." The relatively few translations into English of writings by more recent French anthropologists get some respectful attention in Anglophone and other countries, but they are not a major inspiration for new work, and do not draw extensive comment or exegesis. And as much work remains untranslated, it matters that the status of French as an academic world language has rather declined, so that these writings are in fact not accessible to large outside audiences. De l'Estoile also points to the importance of language: "most French anthropologists still tend to publish in their own national language as first choice." I will return to the problematic role of language diversity in scholarly life in Chapter 6.

Some would argue that the central position of British anthropology is not quite what it used to be, either. Fredrik Barth (2005), after describing the period 1945–70 as its "golden age," notes that it has since, to a degree, become the victim of its own growth, and of its increasing diversity. There are now many more university departments of anthropology, and many more institutions of higher education where anthropology is being taught, than there used to be; but this has meant a certain loss of cohesion. Changes in academic power relations have also resulted in less of a shared focus, and less of a single voice—although Barth does not use the term, perhaps we may say, less of an accumulating canon. As British anthropologists currently spread around the world as field workers, rather than concentrating as in the past in those areas which belonged to the empire, their ethnographic concerns also become more varied. While in Barth's view much of this could be seen as a success story, it also meant that British anthropology might in these ways now seem less distinctive to the outside scholarly world than it had been. But he also describes the context as "an epoch of marked decline in British universities generally, caused by shrinking economies and stifling regimes of bureaucratic regulation and oversight."[5]

On the whole, in Barth's opinion, this has led to a shift in the balance between British and American anthropologies, with a stronger flow of ideas from the latter into the former, and less of

a counter-flow. Yet he also suggests that these two are now less separate from one another than they once were. There is now more of a shared Anglophone tradition, with elements of the British heritage embedded in the emergent American mainstream.[6]

The volume in which Barth's overview of British anthropology was published was one presenting perspectives toward American, British, French and German (or more exactly German-language) anthropologies, and that fact implies a certain current centrality of the latter as well.[7] True, the volume resulted from the opening of a major new German research institution, the Max Planck Institute for Social Anthropology in Halle, which would presumably have been enough of a reason to consider the past, present and future of the local research tradition; but then the tangible fact of the establishment of this large, high-profile institute could itself be taken as evidence of a change in the anthropological landscape. In our introduction to the *Ethnos* issue, Gerholm and I quoted one sharply critical comment, by a prominent American anthropologist, on "antiquated, provincial and reactionary" German anthropology; that comment was dated 1970. Whether such harsh words were justified at that time or not, some 40 years later it appears very clear that anthropology in the German-speaking countries has many practitioners, is probably as prosperous as anywhere, and is both up-to-date and perhaps powerful enough to define the anthropological agenda to a degree in its own way. If German anthropology a century or so ago was indeed as strong as any, and then for reasons of politics and war went through bad times, these are now over. Yet, rather like their French colleagues, these practitioners are inclined to publish in their own first language, and therefore mostly do not get so much attention outside their language area. That is a matter I will take up again in Chapter 6.

If there have been certain changes in the geography of the anthropological mainland, however, it would also seem that what Gerholm and I referred to as the archipelago, of more peripheral national anthropologies, has some new traffic patterns. Evidently there are, so to speak, more bridges, more frequent ferries, between many of its islands. A "World Anthropologies Network" has come into existence, with its most prominent activists in large part based away from the old centers of the discipline.[8] Even more significantly, there appears to be an increase in regional patterns of interaction, which may include the old centers, but which have also set up more direct linkages within the peripheries (or what at least used to be peripheries—to some national anthropologies that term may no

longer be so applicable). Such growing networks of contacts may be quite noticeable in Latin America, and in East and Southeast Asia. In Europe, the European Association of Social Anthropologists (EASA), founded in 1989, has obviously been important in familiarizing many anthropologists across the continent with one another. For one thing, contacts between what was once east of the Iron Curtain and what was west of it have become a great deal closer. In its first 20 years, EASA held three of its biennial conferences in Prague, Krakow and Ljubljana.[9] The problem Gerholm and I had in recruiting a contributor from Eastern Europe for our *Ethnos* issue would not occur today.

If relationships of dependency between centers and peripheries are no longer so pronounced, that may also be a result of internal change in many of the national anthropologies. With time, the latter may to a degree have matured, and become somewhat more self-sufficient. More than a few of them came into being, or grew from extremely modest bases, in the 1960s and 1970s. Since then, they may have become more institutionalized, with their own organizations and publications, and—not least—several active generations, rather than just the pioneers. To draw on Stocking's comments again, they might well now compete in size, even successfully, with the anthropology department at the University of Chicago. That may also offer some basis for a strengthened local consensus on what are the issues and the priorities—a consensus which need not fully coincide with views at the center.

There may even have been a certain impatience with some of the preoccupations of the centers of the discipline. Here and there in the worldwide anthropological community, for example, the varieties of postcolonial ennui are not, or have not been, perceived as particularly relevant to local experience. If one's own country had no colonies, and if it hardly had any anthropologists in the colonial era, sharing the burden of a colonialist past could seem like a somewhat remote notion; to make later cohorts complicit in this past would take a more complicated and less obviously credible line of reasoning. Since one's country is not a present-day great power either, the potential new misuses of anthropological knowledge in the international arena are some distance away—surely not without interest, but not mixing citizenship and professional identity in quite the same way. If one's country is itself an ex-colony, of course, one may have both strong and mixed feelings about taking up a line of work with a tainted reputation; African colleagues are most likely to have to deal with this.[10] On the other hand, in eastern Europe,

anthropology as an academic category seems more often to have been a part of the post-socialist transition, without the taint of compromised past orthodoxies.[11] One may sense that a little earlier, in certain southern European countries coming out of periods under other kinds of authoritarian regime, anthropology similarly arrived with the favorable connotations of intellectual liberation—not least the pleasurable sensation of new comparisons, and more countries heard from.

Clearly, this touches on Stocking's distinction between two types of anthropology—those of empire-building and nation-building. It matters here that the established centers of the discipline have been of the former kind, with its more or less controversial past. (German anthropology, too, was obviously once of that type, although that was longer ago.) Yet a slightly more nuanced map of the varieties of national anthropology may need to periodize a little, and take in Stocking's other typologizing as well. France and Britain had old-style empires; if American anthropology has been of the empire-building type, focused on those distant dark-skinned overseas others, it has mostly been so in the post-imperial era after World War II. This is when the United States turned more (if not altogether unequivocally) internationalist, when its universities built up area studies programs, when funding for going abroad for research became more readily available—and when, with time, many American anthropologists became more worried about matters such as Project Camelot, anthropologists on the war path in Southeast Asia, and the "Human Terrain System" as a part of Pentagon armor.[12] The term "empire" has indeed come back in recent years, often to refer in one way or other to American world power after the Cold War. Going back to the times before World War II, however, it seems that, with some important exceptions, American anthropology, like the Canadian and Brazilian anthropologies included in our *Ethnos* issue, was rather more a "white settler" anthropology, one primarily engaged (in Stocking's phrasing) in studying groups internal to national society, yet originally encountered as culturally alien others.

Parenthetically, let me raise one historical issue in this context. In the introductory chapter, I suggested that the "four-field approach" of American anthropology may have been a result of its early concentration on North American Indians, with a logic of collecting all knowledge about them under one academic roof. There may have been other reasons as well—lingering evolutionary notions, or sheer Boasian idiosyncrasy institutionalized—but I wonder if settler anthropologies elsewhere have shown a similar

tendency? Afrikaans-language anthropology in apartheid-era South Africa—*Volkekunde*, more clearly a white-settler discipline than the parallel English-language anthropology—also leaned toward the ethnology–linguistics–prehistory–physical anthropology packaging of classical American anthropology.[13] In Russia, before Sovietization absorbed anthropology into dialectical materialism and universal history, the expansion of national society into Siberia, with its indigenous populations, had shaped a discipline "based on the idea of combining ethnography, archeology, and physical anthropology, and these disciplines set the necessary terms for ethnologists to know ethnic languages" (Kuznetsov 2008: 26).[14] Even in India, where several of those who have become prominent as anthropologists abroad identify themselves as sociologists at home, the rationale for maintaining an institutionally separate anthropology seems often to have been that it primarily studies those alien "tribal" populations in all their characteristics—customs as well as skull measurements—rather than the Indian mainstream, with its own historical notions of settler origins.

It is evident that the settler anthropologies have not really been what Stocking described as nation-building anthropologies, insofar as they studied groups that were understood as more fundamentally "aliens within." In contrast, the original nation-building anthropologies in the European tradition—those he identified with the German term *Volkskunde*, mostly nineteenth-century creations—were precisely concerned with assimilating local traditions into something national. One might wish to argue that particularly some of the ex-colonial anthropologies have been turning themselves into nation-building anthropologies, as after the achievement of national independence, in the latter half of the twentieth century, their practitioners have been dealing in their research with groups and communities made up of people who are basically compatriots, and the research in question has aimed at building (not least developing) the nation.

This could in itself be a tendency which entails a loosening of connections to the international centers of the discipline. The Cameroonian anthropologist Paul Nchoji Nkwi (2006: 157) has a vivid formulation of the dilemma:

> African anthropologists were trapped in a terrible "catch-22": the more they practiced anthropology by the standards of the former colonial powers, the more their governments regarded them as worthless, or worse; and the more they worked to develop an

anthropology that served the needs of the state, the more their knowledge production was dismissed in European and North American centers of anthropology.

Does this mean that these relatively new national anthropologies of Africa, Asia or Oceania are turning into *Volkskunde*? Not, as far as I can see, in the sense of forming much in the way of direct links with the old—mostly eastern, central and northern European—discipline which still goes under some version of that label, or with its immediate descendants now existing under other labels. That discipline, which still seems not to have an adequate English-language name ("folklore," sometimes used as a translation, points too narrowly in the direction of oral traditions, folktales, riddles, and so on), has its own distinctive identity in a way that still seems poorly understood, from a distance, in those western European or North American countries where it was never fully academically established.[15] It is a discipline that has come through another problematic past: in the early, and sometimes even the latter, parts of the twentieth century, *Volkskunde*, with its nationalist origins, could become the hostage sometimes of fascism, sometimes of communism; and even where it did not, its antiquarian tendencies lost much of their appeal. Since then, it has been reinventing itself, in somewhat varying directions. In any case, the new, more or less nation-building anthropologies of the global South have been constructed rather more as offshoots of the originally empire-building anthropologies of the metropoles.

Meanwhile, since Gerholm and I were putting together our *Ethnos* issue, and since Stocking wrote his response to it, another development has become more obvious in anthropology in many countries in the global North as well, gaining recognition not least by having a name: there is now a quite high-profile "anthropology at home." On the European scene, its arrival may have been definitively marked by a conference volume in the well-known, largely British monograph series of the Association of Social Anthropologists (Jackson 1987), although one could note that several years earlier, across the Atlantic, there had been a somewhat comparable book under the title *Anthropologists at Home in North America* (Messerschmidt 1981). Mostly this was not a matter of nation-building in the strict, intentional, and ideological sense, as early *Volkskunde* had been. There may have been an element of that in France, with some identifiable top-down political promotion of research to accumulate the national memories of the future.[16] But,

rather more, in most places "anthropology at home" was simply social and cultural anthropology without so much long-distance travel—which in some countries meant that it found itself in the same terrain as a post-*Volkskunde* which was no longer so interested in nation-building either.

And if, by the late twentieth and early twenty-first centuries, "anthropology at home" was not necessarily nation-building, anthropology abroad was, in most instances, hardly empire-building in any precise sense. Barth, we have seen, notes that British anthropologists have been spreading the geographical range of their field interests. The same is true of French anthropologists—and of course, some of the best-known of them, even when there still was an empire, took their research elsewhere: Lévi-Strauss to Brazil, Dumont to India. In those national anthropologies that never had any links to empire (not any to speak of, at least, as in the Scandinavian cases), and yet valued having at least some field experience further away, scholars could seek out sites just about anywhere, in no very predictable pattern. As compared to the past, the result was a rather more complicated map of criss-crossing, rather than compartmentalizing, connections between homes and fields.

Yet here and there past ties turn out to linger, in ways which may be surprising or not so surprising. As we sample the growing body of writings on national anthropologies, we will find a Brazilian colleague reporting that anthropologists from her country, as they go abroad, may head for other Lusophone lands: Mozambique, Cape Verde Islands, East Timor, once upon a time also colonized by Portugal (Peirano 2008: 193). And while the Ottoman empire seems never in its days to have created an anthropology of its own, a present-day Turkish anthropologist notes that her compatriots and colleagues now go to the Balkans, and to Azerbaijan, Uzbekistan and Turkmenistan (Tandogan 2008: 104–5)—countries to which Turkey also has old cultural and political ties. Perhaps we might call these, not empire-building, but rather empire-tracing anthropologies?

ANTHROPOLOGY FROM A TO Z

Now over to my other major editing experience—that involving an encyclopedia. It has been said about university departments of anthropology that they come in two basic varieties: department stores and boutiques. The former try to cover all parts of the discipline, thematically, regionally, and methodologically (and may make an effort to recruit staff to handle such breadth). Boutique

departments aim at pushing one conception of the discipline, one trend in scholarly work, perhaps in a rather avantgarde mode. Taking the contrast to the field of publishing, one may sense that there are also journals that are more like department stores and others that are more like boutiques. Encyclopedias, however, are necessarily most like department stores: places where you go expecting to find at least a little bit of everything.

Social science, and its various disciplines, are now covered in a wide range of such works, single- or multi-volume. Yet a few enterprises may have come to stand out as particularly ambitious in scope, and as historical landmarks. In the early 1930s, over a period of several years, an American publisher issued the *Encyclopedia of the Social Sciences* in 15 volumes, and, more or less as its replacement, the *International Encyclopedia of the Social Sciences* appeared in 17 volumes in 1968. Among anthropologists, the first of these became renowned for entries such as that on "Culture" by Bronislaw Malinowski, and that on "Fashion" by Edward Sapir. The 1968 encyclopedia also had its share of contributions by leaders in the field, ranging from Raymond Firth and Meyer Fortes to Mary Douglas on the British side, and from Margaret Mead and Clifford Geertz to Leslie White and Marvin Harris on the American side.

But there were some 30 years between these two works, and as the end of the twentieth century approached, it again seemed that it was time for another comparably large-scale attempt to present the current state of social science knowledge. A major international encyclopedia publisher took on the project; if the first of its predecessors had been an *Encyclopedia* and the second an *International Encyclopedia*, this new third member of the line of descent was an *International Encyclopedia of the Social and Behavioral Sciences*, since by this time the representatives of some branches of psychology, in particular, felt that what they were doing was certainly science, but not necessarily so social. Like the previous two, this enterprise started from scratch, not as a mere updating of its predecessor: the planning was new, the editing was new, the writing was new. Several years went into this, so the encyclopedia appeared, in 26 volumes, in 2001.

"International" was again in the title, and the publisher made it clear that it wanted not just an international readership and coverage, but a work that was international in its scholarly production as well. It recruited two prominent senior academics—the American sociologist Neil J. Smelser and the German psychologist Paul Baltes—as overall editors, not only to stand as guarantors of quality,

but also to keep a wide recruitment of contributors on the agenda. I assume that I was recruited to be the section editor for anthropology partly because I was reasonably well-placed to help in this effort: I had recently served as chair of EASA and generally had a long-established and fairly well-developed network of European contacts, and for a European I had probably had more to do with American anthropology than most. Apart from that, on the basis of varied reading and travel I had at least a general idea of who was where in the world, and what various dispersed colleagues knew about.

An encyclopedia, not least one of this scope, is an intriguing exercise in the architecture of knowledge (which in the end is more or less concealed by its conventional alphabetical ordering). Altogether, we were 39 section editors, a number suggesting that there were not just a number of editors each responsible for a separate discipline and working, as it were, side by side.[17] The two main editors had arrived at a design where there were also a number of cross-cutting categories. Some of these were overarching concerns—such as the history of the social and behavioral sciences, or research ethics. Others were themes of transdisciplinary interest, which had sometimes emerged as foci of both scholarship and public concern more recently: for example, gender, the environment, and science and technology studies. Yet another set comprised what were seen as more applied fields, such as organizational studies, urban studies, and media studies. These cross-cutting fields also had editors of their own.

Early in the work, the main editors and the section editors met for a few intensive days of planning and coordination at the Center for Advanced Study in the Behavioral Sciences, on a hilltop overlooking the campus of Stanford University, in Palo Alto, California. One of the main editors, Neil Smelser, was at the time the director of the Center; for me it recalled a year I had spent there earlier, in the mid 1980s. That had been a very good year—time off from teaching and administration, to read, think and write. When I had lifted my face to look out through the window, the San Francisco Bay had been in front of me (and by the time I returned some 15 years later, I noticed there was more smog over the hills on the other side). There had been the good company of close to 50 other visiting research fellows, with interesting conversations over lunch, usually on the sunny terrace, and shared social activities. But then the Center also made it clear, in various ways, that the fellows were expected to accomplish something during the year. For one thing, we were given lists of the scholars who had been in our particular rooms in previous years.

The roster for my room included Max Gluckman and Adamson Hoebel, author of one of the first textbooks in anthropology that I had encountered, and like Gluckman known not least for his classic work in the anthropology of law. But the room had also had occupants from other disciplines. One of them had been the economist George Shultz, the rather unsmiling elderly gentleman who by the time I got to it was President Reagan's secretary of state.

So now I was back at the Center, in the company of the other editors. We all brought our preliminary lists of suggested entries in our sections, and possible authors for each entry. Compiling these lists was itself a challenge; it was too easy to forget something obvious. (As I remember it, the distinguished political scientist who was the section editor for his discipline had a first version of his list on which the concept of "state" did not appear—he was quickly reminded of that.) But then the design of the encyclopedia was such that it was not obvious at first which entries belonged to whom. Some theoretical concepts, or research concerns, were common to disciplines. In other cases, the cross-cutting categories, such as gender, obviously necessitated some deliberations at the intersections. Inclined toward collaboration rather than competition, we could usually quickly come to an agreement among the editors directly involved about who would be responsible for what entry; usually we tried to identify the author we would want for it as well. And then there were times when the main editors of the encyclopedia would diplomatically propose that there could be multiple entries for some topics, to make room for particular disciplinary perspectives or other scholarly divides. (Consequently I was allowed my own entry, for example, for the anthropology of the state.) So after series of hectic mini-meetings, mostly in twosomes or threesomes, we were able get together for a final lunch in a beach restaurant on the shore of the Pacific Ocean. The rest of our contacts, over the following couple of years, would mostly be by email.

Again, this was to be an altogether new work, not an updating of its nearest predecessor. Certainly this appealed to me as a challenge—it is one thing to sit around among one's nearest colleagues and complain about the deficiencies in coverage of one dictionary, handbook or encyclopedia or other; it is another to face the moment of truth, and fit a discipline into a set number of entries, and of words. The fact that the encyclopedia was scheduled for publication more or less at the turn of the millennium also made the enterprise seem significant in a particular way. What was anthropology at the beginning of the twenty-first century?

In this instance, I had a budget of 200 entries, and of 600,000 words. An encyclopedia is something other than a dictionary; each entry is supposed to be an authoritative extended treatment of its subject, marked by in-depth knowledge, perhaps even original scholarship.According to the editorial guidelines, we could allot anything between 2,000 and 5,000 words to an entry. While I was the editor for anthropology, in practice this meant sociocultural rather than four-field anthropology. Linguistics and archeology had their own editors, and as "evolutionary sciences" made up a cross-cutting category, with an anthropologist as one of the co-editors, biological anthropology (along with some aspects of cultural evolution) was largely covered there. Moreover, following agreements with other section editors, a number of basically anthropological articles, with anthropologist authors, found room in other section budgets, so that the total number of anthropology entries was somewhat higher than that in my entry budget.

Naturally enough, we had been instructed as section editors to make quality our highest priority, and to seek out contributors with the strongest possible qualifications to write their entries. While the main editors limited the assignment of entries to at most two per author, I took it further, wanting nobody to write more than a single entry within the anthropology budget. In the end I myself became the only exception to this, as I had decided from the beginning to contribute the entry for "Anthropology," to make it congruent with the overall coverage of the discipline in the encyclopedia, and then took on one more entry (on "Center-periphery relationships") after the originally intended contributor withdrew. This tough line on the distribution of authorship was intended to make sure that each author would take this one assignment as seriously as possible, rather than as a routine it might just become if someone had half a dozen entries to write. I also tried to invite authors who had not very recently written some similar overview article for another publication, as I did want entries that were not copies or repetitions of existing statements. Furthermore, if an entry involved some ongoing scholarly controversy, I avoided contributors who would be likely to treat the topic in too partisan a manner.

Such considerations apart, there were obviously some other factors to keep in mind in assigning entries. To repeat, the publishers as well as the main editors wanted to involve contributors from all parts of the world in the production of this international encyclopedia. This returns us to matters I dealt with above, with

respect to national anthropologies and their interrelationships. How did the encyclopedia reflect the global landscape of scholarship?

To begin with, one might consider the selection of editors. The national identifications here all refer to academic affiliations, not to origin or citizenship. We have seen that the two main editors were an American and a German. That may already have affected the recruitment of section editors to some degree: among them there were 25 Americans and seven Germans. Only one other country—Italy—had as many as two section editors, and one of these two was actually of German nationality; but, on the other hand, among those counted as Germans here, there was one Norwegian. There was one section editor from Great Britain, and one from France. A co-editor for one section was at an Australian university, but no other editor was based outside North America or western Europe. All in all, hardly a strikingly broad international distribution.

Yet these editors probably did then make an effort to spread their recruitment of entry authors more widely. The encyclopedia as a whole ended up with 58 percent North American authors (including, of course, a number of Canadians), 35 percent Europeans, and 7 percent from other parts of the world. I am obviously most familiar with what happened within anthropology.

The coverage of the discipline in the *International Encyclopedia of the Social Sciences* of 1968 had been strikingly American- and British-dominated. Louis Dumont had contributed an entry on marriage alliance; a couple of other Europeans had minor historically oriented contributions; and there were two entries by internationally prominent Indian anthropologists. Otherwise, I believe, everything came out of the United Kingdom or North America. Some thirty years later, that dominance was still clear enough. I had a little over 50 percent North Americans (that is, a slightly smaller proportion than the entire encyclopedia), and the North American and British contributions together made up some two-thirds of the anthropological entries, but with the rest the spread was reasonably good. While 51 countries were represented among the contributors to the encyclopedia as a whole, the anthropological entry authors alone were from 24 countries. The way they were distributed, however, perhaps deserves some comment. First of all, there was strikingly little interest among the French colleagues first invited to contribute. I suspect that a main concrete reason for this was that many or all of them had already written something for a French anthropological encyclopedia that had just been published, and did not want to engage in this kind of writing so soon again.

Language was probably also an issue. The encyclopedia publisher had let it be known that some modest funding could be made available for translations, but quite likely this was fundamentally understood as an Anglophone enterprise, not a primary concern of those normally writing in other languages. In the end, as the one remaining French contributor-to-be could not meet the deadline, I found myself without any French contributors.

In general, the language issue did not otherwise seem to raise much of a problem. The most striking contrast with the limited French interest was presented by Israeli colleagues, who without exception accepted invitations. But the Israeli academic community is, in general, probably more internationally inclined than most, with a bias toward writing in English. It is, I understand, an old joke in Jerusalem and Tel Aviv that Moses himself would not have got a tenured job at an Israeli university, as he only published in Hebrew.

Anthropologists from Asia, Africa, Latin America, the Caribbean and Australia made up some 12 percent of the entry authors. In a discipline like anthropology, so self-consciously global, one could perhaps have hoped for more, but again the figure may reflect the relative strengths within world academia. Institutions outside the more affluent countries often provide rather limited resources for wide-ranging scholarship. I remember one or two prospective contributors who declined on the grounds that their local libraries were insufficient for writing such overviews as encyclopedia articles. It was significant, too, that some of the authors I had invited from less well-represented countries—India, Egypt, and Jamaica—left these countries during the period when the encyclopedia project was under way. And they all moved to the United States.

Not unexpectedly, the main editors had also requested that, in recruiting authors, the section editors should keep in mind another distributional factor: gender. By the late twentieth century, it was obvious enough that one should aim at as even a distribution between women and men as possible. The end result for the encyclopedia as a whole was 79 percent men, 21 percent women. Among the anthropology authors, the result was just slightly less uneven: 73 percent men, 27 percent women.

I was a bit disappointed at this, as there are, and have long been, many women anthropologists. Yet one factor underlying this result may have been that women anthropologists in the recent period have been somewhat unevenly distributed over the anthropological research field itself—many have been active chiefly in the anthropology of gender, and related areas. So, while there might

be numerous women colleagues as author candidates for writing entries on such topics, there would be fewer for other topics. And if the top woman candidate for a particular entry then declined the invitation, the next candidate in line might well be a man. Moreover, of course, some of the more or less anthropological topics in gender research had ended up with the section editor for gender—who I suspect had rather few men on her list of contributors.

A similar mechanism probably accounted for the large number of authors from the United States. There are a great many American anthropologists, and they are quite well-distributed over research fields within the discipline. If I had come up with a candidate from somewhere else for a particular entry, and for one reason or other did not succeed in recruiting this person, there was thus a good chance that the next person in line was someone from the United States. I should hurry to insert here that, as far as the likely quality of the entry was concerned, I could hardly ever see it as particularly disastrous if I did not get the first candidate on my list: there were often many almost equally desirable alternatives. But in terms of meeting the main editors' hopes for a wide international circle of contributors, the loss of first choices could make some difference.

I cannot remember that the main editors ever expressed a principled point of view on the age of contributors, but I gave it some passing thought. It may well be that the recognized scholars who can be relied upon to make an authoritative statement on a topic are often not all that young. In fact there were some individuals— but I think only one of them in anthropology—who contributed to both the 1968 and 2001 encyclopedias. Yet, if there had been a generation between these two works, and also a generation between the first and the second encyclopedias of this kind, one should perhaps expect that, if there were ever to be another one, it might appear some time in the 2030s. Preferably, then, the articles in our 2001 encyclopedia were to have a certain long-term relevance and credibility—and it might be good if, by the time the next encyclopedia appeared, some of our contributors were still alive and well, and active scholars. This entered my mind at the Palo Alto meeting of section editors, when I saw that the preliminary list of contributors to another section, put together by an editor who was himself indeed a veteran, included mostly people of his own generation. As for the anthropology list, I think there was a good spread of ages among contributors. And I noted later that some of the old-timers were among the quickest to get their manuscripts to me—so apparently they had left the habit of procrastination behind.

More important than who wrote the entries, however, was surely what the entries were about. Early in the editing process, selecting 200 authors for the same number of topics gave me a sense of having the opportunity to handpick my own favorite tutors for private lessons—and some of the pleasures were a little unexpected. As a city person who had had urban life, media and globalization among my research interests, but was to a degree an anthropological generalist, I could still find the incoming manuscripts on topics like "land tenure" and "irrigation societies" intriguing reading. But soon enough, of course, the lessons would no longer be so private. Who were actually the intended readers of this encyclopedia?

A work of this scope, inevitably highly priced, could not be expected to find its way to many private shelves, nor to many general libraries either. No doubt the publisher mostly had research libraries in mind, such as those at universities. Consequently I was acquiring articles which would primarily be read by researchers, academic teachers, and advanced students. The entries should offer an overview of some phenomenon or region in the world, or critical scrutiny of some concept or theoretical orientation; and with that as a point of departure, and with the aid of the concluding bibliography, the reader should be able to move on. This could be a gateway to a research field, or an aid to a teacher preparing a new course. This is approximately what I suggested to the authors of entries. It was also an implication that the titles of articles—the entry terms—should be user-friendly and familiar to the likely readers, rather than sophisticated abstract terms, already exclusive insider knowledge that they had never come across before, and would therefore be unlikely to look for. Such concepts could better be introduced later on in the texts. For such reasons, when someone advised that I should have an article titled "Chiliasm," rather than "Cargo cults," I did not agree. On the other hand, I did not insist on any sort of neutral, bland, anonymizing style of writing that would entirely remove any quality of personal authorship. The two predecessors of this encyclopedia, after all, had included articles that became classics in their own right, and I hoped this would be an attractive prospect for contributors this time as well.[18]

Whereas I had a budget of 200 entries to manage, the number of anthropology articles in the 1968 encyclopedia had been about 130.[19] The entire new encyclopedia was certainly much larger than its nearest predecessor, reflecting, as the main editors saw it, the rapid growth of knowledge in recent decades. I intentionally put together my own tentative list of entries before I checked what had

been included in the earlier work, in order to make a fresh start. When I did go back to it to see what was there, I actually found that there were few entry terms that had a notably antiquated ring, so although I might not have made precisely the same choices, I doubt that many of the topics were entirely dropped. On the other hand, the increase in the number of entries by some 50 percent made room for a number of new interests, and also some finer distinctions. Feeling strongly about the anthropological task of offering knowledge about all parts of the world, for example, I expanded the regional coverage to some 20 articles—thus, instead of a single entry for sub-Saharan Africa, one each for West, East, Central and Southern. While not disregarding histories and traditions, I also preferred area entries which took in the present—so the article on West Africa could also say something about current generational conflicts, and the part of very young soldiers in the wars over diamonds and timber; and the article on China could at least raise the question about the implications of the recent one-child family policy.

But a fair amount of the added space was given to newer concerns in the discipline—the keywords of recent anthropology. One entry title from the 1968 encyclopedia which a few decades later had a somewhat quaint ring was "Acculturation"; if anything, that was rather more of a 1930s concept (about which a little more is said in Chapter 7). One might say, however, that the vocabulary for what the term referred to—the interconnectedness of cultures and the conditions and processes it involved—had actually mushroomed. There were now entries for "Globalization," "Modernity," "Hybridity," "Creolization," "Colonialism," "Postcoloniality," "Diaspora," "Refugees," "Multiculturalism," "Cosmopolitanism." The expansion of anthropological interests was also indicated by entries such as "Visual anthropology" and "Popular culture" on the one hand, and "Human rights" and "Genocide" on the other. "Common sense," "Nostalgia," and "Authenticity" suggested shifting sensibilities; "Identity," "Personhood," "Sociality," and "Body" were other articles that could reveal the ongoing growth of research; and certainly there were "Reflexivity" and "Cultural critique."

Nonetheless, there were also "Ancestors," "Witchcraft," "Genealogy"; and there were entries for "Taboo," "Totemism," "Kula ring," "Potlatch," "Clan," and (to repeat) "Cargo Cult." That is to say, I was concerned that the coverage of anthropology in the encyclopedia should also to a degree mirror the past of the discipline, and the passage of the past into the present. Some involved

classic topics, but around which there was still new research going on. "Primitive society" and "National character" were now no longer so much articles about particular really existing societies or psychologies, but rather about topics in intellectual history. "Taboo," "Totemism," and perhaps "Potlatch" were notions which had long ago entered the general imagination, but in view of their somewhat exotic origins it might still be the obligation of anthropologists to discuss what they stood for; something similar could be said about "Cannibalism." Some terms, such as "Clan" and "Mafia," had come into extended, metaphorical use in ordinary language, yet there might be encyclopedia users looking for a more precise understanding of what they stood for, technically or historically.

ANTHROPOLOGY IN TIME, ACROSS BORDERS (AND IN THE ZOO)

What editing anthropology made me think about here, then, was anthropology in time. If the main editors referred to the growth of knowledge in the disciplines concerned as a major reason why this encyclopedia had to be much larger than its predecessors, it meant that there was a cumulative process involved—current knowledge did not merely replace older knowledge. Whether or not they are aware of it, or particularly interested in it, anthropologists carry a heritage of bits and pieces of knowledge about which they may not always be able to claim much individual expertise, but which is still there in their collective imagination. This heritage is more meaningful to them than to non-anthropologists, or at least probably meaningful in other ways, and possibly persists as a potential—as resources that can be retrieved, or as unfinished business that one may again find intriguing, and challenging.

But it is also a heritage that may not be entirely unknown to outsiders. No doubt some of it is, but variously well-informed lay people (or for that matter people in neighboring fields) can have more-or-less firm ideas of what anthropologists do or have done, or know about. These ideas are not necessarily very up to date. All the same, such beliefs are also in their own way part of anthropology's world.

Looking at a number of those conspicuously current anthropological entries in the *International Encyclopedia of the Social and Behavioral Sciences* of 2001, however—globalization, identity, modernity, human rights, popular culture, diaspora, genocide—we can also see that their topics are not ours alone. That seems to mark a change from what anthropology was in the preceding

encyclopedias: in one way or other, there is now a greater openness to interdisciplinarity, or transdisciplinarity. Within the scholarly world, we must be prepared, and probably are prepared, to speak and listen across borders.

Then there are those other borders, which my colleague and I focused on in editing the journal issue on national anthropologies, and which to a degree I returned to in considering the recruitment of authors for the encyclopedia. What about that "plurality of national viewpoints" which Maurice Freedman had referred to in his overview? With regard to the content of the encyclopedia, I should say, I mostly did not give much attention to national differences within the discipline. If the encyclopedia aimed at being "international," this did not seem to amount to adding up, and weighting, some number of separate national anthropologies. There does, after all, seem to be a very considerable overall intellectual coherence to the discipline as it extends throughout the world. In large part, we care to know, and to argue, about the same things.

Yet there are variations, differences in strength and in working conditions and styles, between the many contemporary milieux of anthropology. The fact that a great many of the contributors to the encyclopedia were from one country, and varying numbers from other countries and continents, certainly reflects such circumstances. Those decades ago, one Canadian author in our issue on national anthropologies remarked, on the neighborly presence of American anthropology, that it was like being "in bed with the elephant." Given that no other national anthropology now really seems to be quite as strong, it may at this point be an experience more generally shared. It is difficult to pretend that this big animal is not there.

The situation is obviously similar in many disciplines—the overview article on "Political science" in the encyclopedia that I have been reflecting on here cites one source as estimating that "about 85 percent of all the political science being studied worldwide is located in the USA"—and some two-thirds of the Nobel Prize winners in economics in the last 40 years have been American.[20] No doubt tensions and conflicts may arise out of this situation, but perhaps it is better to understand that the elephant cannot help being an elephant, and that it makes sense for it, in its habitat, to behave in elephantine ways.[21] A well-trained elephant, too, can do a lot of good, such as in carrying heavy loads. For those of us, on the other hand, who find that our particular conditions

of life in anthropology's world—not in Chicago, then, but in Lyon, Stockholm, Yaounde, Brighton or Osaka—are somewhat different, it may be wise to give some thought to what other adaptations we might sometimes want to make. To borrow from the old Greek thinker Archilochus (by way of Isaiah Berlin, 1978) another classic set of metaphors from the intellectual zoo: as foxes (who know many things), or as hedgehogs (knowing one big thing).[22]

3
Diversity Is Our Business

Almost since the beginnings of anthropology as an organized endeavor, its practitioners—some of them at least—seem to have had a morbid tendency to dwell on the likelihood of its impending demise. In *Argonauts of the Western Pacific* (1922), perhaps the earliest field-based ethnography still reasonably widely read, Bronislaw Malinowski started his foreword by proposing that his discipline was "in the sadly ludicrous, not to say tragic, position, that at the very moment when it begins to put its workshop in order, to forge its proper tools, to start ready for work on its appointed task, the material of its study melts away with hopeless rapidity." In the 1960s, as he had just moved on to a chair in sociology, Peter Worsley (1966) warned in an oft-cited conference paper with the title "The End of Anthropology?" that the discipline might disappear, or survive only as a particular form of history, if it continued specializing in isolated "primitive societies."[1] Not so many years later, Rodney Needham (1970), a very different British professor, could foresee that aspects of anthropology would be assimilated into other disciplines, so that future anthropologists might be orientalists, art historians, depth psychologists, political scientists, or whatever—in each case bringing with them an ethnographic knowledge of other cultures. More recently, George Marcus, beginning to respond to the question of whether the discipline might be falling apart, suggested that "anthropology is not on the verge of disintegration. Institutional inertia alone will keep it going for some time" (Rabinow, Marcus, Faubion and Rees 2008: 45). That is hardly a very comforting answer—these days, inertia seems like a rather less reliable feature of academic organization than it may have been in the past. (As Marcus continued his argument, he made it clear that he did not see it as a complete answer either.) And in 2009, the theme of the annual meeting of the American Anthropological Association was "The End/s of Anthropology"—allowing for alternatives, but perhaps still not entirely reassuring.

This chapter, too, first took shape as part of an international lecture series with the overall title "The End of Anthropology?"[2]

While it will tackle it in its own way, by trying to deal at some length with one more specific kind of threat to the discipline, I will address that question briefly in a more general way first—noting that it reminded me of a very well-known and controversial journal article published about twenty years ago: the American political thinker Francis Fukuyama's "The End of History?" (1989). Fukuyama was of course not concerned with the possible demise of the academic discipline of history, but rather envisioned the end of large-scale world-historical process, with the end of the Cold War, the decline of state socialism, and the absolutely final triumph of liberal democracy. Although it appeared in a somewhat obscure publication, the article nonetheless came to draw a great deal of worldwide attention, yet when Fukuyama (1995) reviewed its reception some years after its publication, it turned out he was not altogether pleased. He complained that he had very often been misunderstood. He could list Margaret Thatcher, Mikhail Gorbachev, the first President Bush, and Hosni Mubarak among the people who, noting in their speeches that history still went on, had rejected what they had thought was his thesis. But probably they had not read the article—and perhaps their speech-writers had not either. Moreover, Fukuyama also found that many of his commentators had failed to note that his original article title had ended with a question-mark.

This kind of formulation, then, is a bit risky. Some people might mistake a rhetorical question, or a titillating courtship with imagined danger, for a prognosis or a statement of fact, and in these times of information or disinformation overload, they may not stay around, with undiluted attention, to hear the elaborated and possibly obscure answer. In fact, all they remember could be that first catchy string of words.

I see a number of reasons why the answer to the question "The end of anthropology?" should be "No." As I noted in the Introduction, the number of practitioners and students of anthropology has grown greatly over the past half-century; as I will point out in the next chapter, the scope of the discipline has kept widening. As far as intellectual vitality is concerned, anyone who wanders through the book exhibits at major national or international anthropology meetings, or just skims through the catalogs of the relevant publishing houses, can hardly fail to marvel at the many books continuously produced within the discipline. The number of journals devoted wholly or in large part to anthropology seems also to have grown substantially in the latter decades of the twentieth century. (In 1970 there were as yet no *American Ethnologist*, *Cultural*

Anthropology, *Social Anthropology*, *Anthropology Today*, *Critique of Anthropology*, *Anthropological Theory*, *Focaal, Ethnography*, *Identities* or *Public Culture*.) This seems not to be a good time to request that the last anthropologist to leave the building should please turn off the light.

How are we doing with regard to our scholarly interests? In his major overview of contemporary anthropology, Michael Herzfeld (2001a: 19) concludes that "it is abundantly clear that the vast increase in available topics, scale of perception, and sheer complexity of subject-matter do not seem to be compelling the discipline to early retirement." Probably most of us simply want to get on with our work, which by now does not appear to be inevitably shaped by any of the more dramatic theoretical divides or confrontations of the later decades of the past century. In a wide-ranging survey, aptly titled "Anthropology in the Middle," Bruce Knauft (2006) has argued that recent thought within the discipline has tended to move away from grand theory, into a fertile middle ground where new connections cross-cut such divides as those between global, regional and local scales, between structures and events, between ethnography and history, between objectivism and experimental genres of writing, and between theory and practical concerns. In a post-paradigmatic period, anthropologists tend to be reasonably comfortable with, and stimulated by, bricolages allowing them to combine different intellectual strands in new ways, and take them to new materials. And Knauft sees such tendencies as characteristic not only of anthropology's present, but also of its continued renewal and future promise. While his survey focused on American sociocultural anthropology, I am inclined to believe that we can discern the same tendencies elsewhere in the anthropological world. No real state of crisis here either, then.

More dramatically, one might imagine that the end of anthropology could come about as part of a more general dissolution of the entities called disciplines—a very large-scale change in the scholarly landscape. We do now encounter sophisticated and interesting analyses of changes in the mode of production of knowledge, toward more interdisciplinary and transdisciplinary styles of organization.[3] As I noted in the previous chapter, reflecting on some of the present encyclopedia entries for anthropology, when one focuses on many current issues, tendencies and phenomena, disciplinary boundaries tend to blur.

Now it is not that, during my years as an inhabitant of that landscape, mostly in my European corner of it, I have always been

entirely pleased with its existing shape. In the humanities and
social sciences, it may rather often have been an obstacle to vitality
and creativity that disciplinary boundaries have been too sharply
demarcated, intellectually and organizationally. Yet I do not think
the best solution is to abolish disciplines, as bodies of knowledge
and as intellectual communities; in these times, I would be concerned
that arguments for the decline of disciplines might become clichés
that are made to serve as opportune alibis for politicians and
administrators to do away with the autonomy of those clusters of
intellectual activity that seem least profitable. Occasionally I hear
colleagues declaring that they would not be so concerned if the
discipline of anthropology disappeared from the organizational
chart of universities, as long as the *ideas* of anthropology continued
to be propagated there, someplace, in some form. That may sound
admirably broadminded—and rather in line with Rodney Needham's
prognosis—but it still leaves me worried. As a matter of academic
realpolitik, I believe the survival and continued development of
this kind of cluster of ideas and practices are best served by their
having their own institutional power base. Meanwhile, in that
same period when American universities tend to be overwhelm-
ingly dominant in the global ranking lists of academic excellence,
one might keep in mind that universities in the United States have
mostly not been inclined to close down disciplinary departments
in favor of alternative modes of organization. As I understand it,
they continue to be much more likely to support *both* disciplinary
departments and various cross-cutting formats for interdisciplin-
ary encounters. So if these institutions are to stand as models for a
successful, intellectually productive organization of academic life
everywhere, disciplines do not seem likely to go away soon.

   After such mostly optimistic remarks about the prospects for
anthropology, however, let me point to some circumstances that I
find rather more disturbing. If what I have said mostly relates to a
fairly healthy situation within the discipline in its internal activities,
I think we ought to be more concerned with the present relationship
of anthropology to the surrounding society and its public life.

   João de Pina-Cabral (2006), the Portuguese anthropologist,
prominent on the European anthropological scene, has written about
the problem at hand in the *Journal of the Royal Anthropological
Institute*. The incident that provoked his statement was the danger,
at one time imminent though ultimately failing to materialize, that
anthropology in France, one of the old heartlands of the discipline,
would be downgraded to the status of a subdiscipline of history in

the national structure of research funding. Pina-Cabral's conclusion was that the public image of the discipline was seriously out of date, and did not serve it well. As I dwell on this issue here, I will perhaps at times be in danger of stating the obvious; but then it happens that I find what anthropologists say on such matters a little thoughtless, and at worst counterproductive. So possibly the obvious sometimes needs both stating and restating.

PITH HELMETS AND GOLDEN FLEECE AWARDS

It seems long ago that a well-known cultural critic would celebrate an anthropologist as a hero, the way Susan Sontag did with Claude Lévi-Strauss in her book of essays, *Against Interpretation* (1966). We now usually draw less honorable mention. An example I wish I did not have: one of the critical moments, more precisely low points, of Barack Obama's campaign in the presidential primaries in the spring of 2008 was when he spoke—he thought privately— to a gathering of San Francisco Bay Area supporters about how small-town people in Middle America had grown "bitter" over lost jobs, which made them "cling to guns or religion or antipathy toward people who aren't like them." The statement was reported, and commented on widely, and was for a while seen as a threat to his candidacy. Commenting self-critically later on, Obama described this as his "biggest boneheaded move," and told his *New York Times* interviewer that it had sounded as if he was "talking to a bunch of wine-sipping San Francisco liberals with an anthropological view toward white working-class voters." What he really meant, he told the interviewer, was that

> these voters have a right to be frustrated because they've been ignored … in fact, if you've grown up and your dad went out and took you hunting, and that is part of your self-identity and provides you a sense of continuity and stability that is unavailable in your economic life, then that's going to be pretty important, and rightfully so. And if you're watching your community lose population and collapse but your church is still strong and the life of the community is centered around that, well then, you know, we'd better pay attention to that (Bai 2008).

Here, it seems to me, the candidate Obama assigns to anthropology the stereotypically distant view, lacking in empathy—and then proceeds to sketch, as its opposite, precisely the sort of close, con-

textualizing understanding which we as anthropologists are much more likely to claim for ourselves.[4] And from this particular source, we may find this stereotype so much more surprising because we might have thought this candidate should have got his anthropology right—more about that later. In any case, this is an instance of a recurrent phenomenon we might term anthropology-bashing.

It comes in various forms, not altogether unrelated. Indeed, there are times when the anthropologist is portrayed as reasonably likeable, but somewhat unreliable and unpredictable, more trickster than hero. But then, as we have seen, in another mode the anthropologist is seen as someone distant, coldhearted—and therefore unable really to understand the sentiments of the people under study, to grasp what makes them move. Very occasionally this professional stranger is seen instead as somebody with a special, dispassionate ability to discern what is hidden to everyone else—but this alternative seems to show up less frequently. Perhaps at worst, the coldhearted anthropologist is portrayed as someone who uses his skills to manipulate situations in ways that are detrimental to the human beings about whom he has built up an expertise.[5] If the main thrust of the imagery of coldheartedness is precisely one of suggesting a faulty emotional makeup, however, it often goes with an implication that the anthropologist is a bumbling, incompetent observer who does not get even obvious realities right—not only less skilled in understanding than the natives who are at home in the place (that may often be fair comment), but sometimes even less perceptive than any untrained amateur just in from the cold, or only just told about the facts on the ground. This scholar, then, probably hurt his head when he fell into the field from the top of his ivory tower.

For another variety of anthropology-bashing, my first example is from my own scholarly neighborhood. In 1990, a Swedish anthropologist submitted a research proposal for an ethnographic study of the growing Spanish professional middle class. She had in fact lived an extended period of her life in Madrid, had then written her doctoral dissertation on life in a working-class area of another Spanish city, and now wanted to extend her research into a stratum she felt was important in understanding the Europeanization of Spain after the isolation of the Franco era. From an anthropological point of view, it seemed like a worthy project for which the applicant was eminently well qualified. But in her proposed budget, she unwisely included the expected cost of some clothing that she

felt she would need to go about her participant observation in some rather clothes-conscious milieux.

I happened to be on the review committee of the funding body, a state research council. It was a committee made up of representatives of several social science disciplines, who agreed that it was a strong proposal but that the budget item for clothing had to go—in principle, members of some occupational groupings might make legitimate demands that they should not have to pay out of their own pockets for special attire (uniforms, lab coats or whatever), but ethnographers conducting participant observation were not such a group. So we struck that item from the budget, before we recommended the project for support.

The proposal then reached the research council's ethics committee, which seems to have read the original proposal but to have ignored the statement of our review committee. In its protocol, it thus made a strong statement against the clothing item of the budget, as if it were still on the table. By then it was summer, and since the research council protocols are public documents, a freelance journalist looking around for stories found this one. This example of the scandalous misuse of public funds then appeared on the front page of a tabloid evening newspaper (a failing one, probably desperate for scoops); and—partly, I believe, because the silly season had arrived, when temporary summer journalist hirees and substitute editors struggle with the relative dearth of newsworthy events—the story circulated for a month or so between national and local newspapers and on to radio news programs and talk shows. By then it had become known as "the Spanish luxury project." There were irate letters to the editors as well, all based on the assumption that the story was true. I did write an article for one national newspaper to explain that, basically, it was not; and I had some correspondence with a radio news editor debating standards of news reporting. But by early August that year, I was ready to feel that at least one good thing about Saddam Hussein's invasion of Kuwait was that, even in Sweden, it gave the media something more real to deal with. Later in the autumn, however, I heard the story again, at a conference in Italy, from someone who had seen it in a British paper, and who related it to me as if it were true (entirely unaware that I had indeed heard it before, and would not be amused). Unfortunately, the spread of these kinds of stories seems to know no borders.

One might have thought that the scholarly community would have been better served by a less cowboyish ethics committee that had asked questions first and shot later (if at all)—who oversees the

ethics of the ethics committee? I understand some of its members were a trifle embarrassed afterwards, attempting to fix the blame somewhere else in the group. But my point is that the case is not so unique. Anthropology is an easy target for a kind of populism proclaiming that research in and about far-away places is useless, and that money devoted to it is therefore not well-spent. Probably many of us have our own examples of such incidents, from different corners of anthropology's world, but the most widely known may be the US Senator William Proxmire's Golden Fleece Awards, announced regularly over an extended period in the late twentieth century. Senator Proxmire, in many ways apparently an honorable man making his satirical awards to publicize the striking waste of taxpayers' money, may often have had a good point; one of his more celebrated prizes was to the Department of the Army for a study on how to buy a bottle of Worcestershire sauce. But in a significant number of instances the recipients were prominent anthropologists.

Another kind of anthropology-bashing in public discourse may often be gentler. This involves the position of the discipline in time, describing it in one way or other as an anachronism, an activity out of the past. Thus, when an electronic journal named *Inside Higher Ed*, devoted to news of higher education, reported on what had been noteworthy at a recent annual meeting of the American Anthropological Association, the introductory line in the correspondent's report was that "Evoking associations with musty, forgotten archives and spiral notebooks in the field, anthropology doesn't immediately come to mind as a discipline fully situated in the modern, wired world" (www.insidehighered.com, December 3, 2007). Yet the correspondent went on to affirm that, in fact, "anthropologists have been tackling the implications of technologies on ethnography with each new innovation, from handheld 16-millimeter film cameras and cassette tapes several decades ago to Internet and digital video in more recent times."

A second example is from a couple of years earlier. The journal *Fortune Small Business* devoted an article to the fact that, in order to understand the software needs of entrepreneurs, Microsoft had hired anthropologists to undertake field studies of small firms all over the United States (Murphy 2005). This was indeed the cover story of the issue—and on the cover, under the rubric "Pygmy hunters," was a cartoon of Bill Gates, the Microsoft founder, wearing a pith helmet. Gates' surprising involvement with a supposedly exotic line of scholarship, then, was suggested by rather antiquated tropical

headgear, nowadays hardly worn either by anthropologists or by anybody else, whether in the villages of whatever may be darkest Africa or in the small-business offices of North America.

A STRONG BRAND

The trouble, in other words, seems often to be not that anthropology is an endeavor entirely unknown to the outside world. We are perhaps more part of a popular imagination than most other disciplines.[6] The problem is, rather, that what people think they know for a fact is wrong. That is something which for a long time we may have thought of as mildly irritating, but perhaps not terribly important. I would argue that we may now find ourselves in times when we can ill afford not to take the matter seriously. We may not be able to put an end to all anthropology-bashing, but we can try harder to be clear about how we want to be understood.

My focus in this chapter on the public image of anthropology fits in here. Perhaps provocatively, in drawing on a characteristic current vocabulary, I would argue that anthropology needs to cultivate a strong brand. Those who feel ill at ease with that term, thinking that in its crassness it sullies their noble scholarly pursuits, may perhaps just as well continue to call it "public image," or even just "identity"; but in times of not only neoliberal thought but also media saturation and short attention spans, it may be that "brand" is a useful root metaphor—a word to think with in the world we live in. (It might also be noted that, these days, not only are corporations and consumer goods linked to brands—so too, for example, are cities and countries.[7]) Brands should attract outsiders, customers, visitors, members of the public. At the same time, they should preferably offer a fully acceptable identity for whoever may count as insiders, to reflect on and be inspired by.

It does not seem difficult to identify some criteria for a successful brand that could apply to the brand of an academic discipline as well. Preferably it should be quickly grasped and clearly understood. Academics, given to precise but not necessarily snappy definitions of their terms, may need to take note here. It is no good to formulate a brand in such a way that any innocent inquirer will lose interest and be half-way down the stairs before the reply is complete. The few words it needs to put together should be simple ones, understood by everybody. And the formulation, again, should not lend itself too easily to misinterpretation—remember again that striking

catchphrase, "the end of history," and then Fukuyama's later complaint about the world leaders who had failed to understand it.

Getting closer to specifics, I take it that one would be better off with a brand that is consistent, and more or less equally acceptable, or even attractive, to all the varied others one hopes to reach with it—and, not least, to insiders as well as outsiders. Consider some examples from the way we talk about anthropology. Occasionally I hear the somewhat flippant conclusion that "anthropology is whatever anthropologists do." In times when a number of disciplines may well be characterized by a great deal of internal variation and fuzzy boundaries, there might seem to be something to this, just as "history is whatever historians do," or "economics is whatever economists do." Nevertheless it is an insider joke, and it may be a little risky to take it outside our own circle. I do not think I would even expose students to it, for fear of confusing them further—at least not immediately. Moreover, I suspect that faculty deans, university rectors, and ministers of higher education would not feel well instructed, or be terribly amused. Perhaps we had better get used to looking for what makes anthropology a reasonably coherent endeavor, rather than emphasizing its apparent incoherence.

There is also a certain intellectually rebellious streak in our discipline, which we may cherish and wish to emphasize at times. This is an idea of anthropology as cultural critique which certainly goes back at least to Bronislaw Malinowski and Margaret Mead, and which made a prominent comeback in the late twentieth century. More recently yet, at a conference on teaching, I heard it affirmed that anthropology is a "subversive discipline." Again, that may appeal to many of us, and may attract some of the more independent thinkers among students. But I would not have recommended it, in the past or at present, as the best brand to take into negotiations with academic administrators or ministry officials who may struggle nervously to maximize order and predictability in their own domains.

Rather more substantively, there has been a tendency to define anthropology centrally in terms of how we work—that is, in terms of field research, or ethnography. Certainly, there is something attractively concrete about this, yet we may feel that in the end it is not satisfactory as a central image of the discipline. Indeed, in the very title of his Radcliffe-Brown Lecture to the British Academy, Tim Ingold (2007) has asserted that "Anthropology is *not* Ethnography."[8] In passing, Ingold notes the extreme case of the lowly "ethnographic researcher … tasked with undertaking structured and

semi-structured interviews with a selected sample of informants and analyzing their contents with an appropriate software package, who is convinced that the data he collects are ethnographic simply because they are qualitative." This is surely not a particularly representative instance of what we usually take ethnography to be; but Ingold nonetheless shares with many other anthropologists a dislike of the inclination, in adjacent disciplines and disciplinoids (such as cultural studies), to assume that ethnography is *all* there is to anthropology. As an evolving body of thought and knowledge, anthropology cannot be reduced to a method—perhaps some sort of qualitative counterpart to statistics.

All the same, it is hardly helpful in the long run to come up only with brand formulations that suit some audiences but not at all others, or that most strongly emphasize what anthropology is not. This is certainly also still the problem with that widely accepted but outdated image we took note of before, portraying the anthropologist either in "musty, forgotten archives" or in the jungle wearing his pith helmet. We may reject that—but when the Royal Anthropological Institute of Great Britain and Ireland commissions a volume to present a more current understanding of the discipline, it gives it the title *Exotic No More* (MacClancy 2002)—once again providing above all a negative statement, which might at worst be taken to mean that anthropology has given up its attempt to understand human lives across boundaries, and is now all "anthropology at home." The wide-ranging contents of the book in question show that this is not the case, but I would have preferred a more positive formulation up-front.

A FOCUS ON DIVERSITY

What, then, would I offer as a viable brand for anthropology, at present and for any foreseeable future? It is here that I come to the chapter title: "Diversity Is Our Business." I will admit that this phrase is inspired by another one—a long time ago, the large American corporation for which my brother-in-law worked as a young engineer used the slogan "Quality is our business—our only business." I remember that my brother-in-law used to quote it with a wry detachment, as one may be wise to do with any slogan. Yet it may still have served the corporation reasonably well, for internal as well as external uses.

I want to work through some of the implications of pushing the scholarly and practical understanding of human diversity as

anthropology's brand. First of all, I think it is a valid claim that this is what the discipline is primarily about. Since its beginnings, with its connections to natural history, we have indeed sought to map the variety of human life, even if for some time we applied this preoccupation preponderantly to what was geographically distant, exotic—expressing a shared stance toward what is "out there," not so affected by variations in theoretical orientation. Secondly, I think a focus on diversity identifies an important contribution to knowledge. Even if "diversity" could sound a little like "everything," which might be a rather questionable specialism—might, indeed, seem to come uncomfortably close to "whatever anthropologists do"—the value of understanding diversity should very soon become clear when one contrasts it with the inclination, which is still strong, to assume that what is familiar is universal, or that modernity necessarily breeds uniformity. A study of diversity remains the best antidote to unthinking ethnocentrism.

With this, I believe, follows the opening of anthropology to the development of cultural critique, even to identifying itself as a "subversive discipline." This line of effort may appeal more to some of us than to others, and I think the pursuit of it may well be left to individual choice. We should recognize, however, that, although a primary identification with the study of diversity may sound less heroic, even a little bland, just about any claim that anthropology can have to unusual critical insight is in fact based on its special relationship to diversity—to the knowledge that other ways of thinking and acting are possible. It may be that, in the line of critical anthropology that developed in the later decades of the twentieth century, such specific inspiration was more mixed with critical theory from other sources, and that anthropologists had become a bit more skeptical about the immediate usefulness of contrasts with the Samoans, the Kwakiutl, or other faraway people.[9] Yet there was still a sense that anthropology as cultural critique should be grounded in an ethnographic understanding of alternatives.

To what extent are we as individual scholars ready to identify the study of human diversity as our major concern? Possibly each one of us, when asked about our research interests, will spontaneously come up with some rather more specific answer: "Micronesian kinship," "Latin American squatter settlements," "software needs of small enterprises," "Hausa praise singing," "the transnational impact of the Bollywood movie industry." The problem of diversity as such, however we understand it, may not figure that prominently

in our personal scholarly preoccupations. But what we should be aware of is that what all these remarkably different specialisms add up to, in a *collective* intellectual enterprise, is that ever more encompassing, yet never complete, knowledge of human diversity. We all add our pieces to the very large jigsaw puzzle. And in that way, the understanding of the shared endeavor also offers us an umbrella for all our individual diversity—an umbrella which on the whole should allow us to get on with things.

Our inclination to think of ethnography, field work, participant observation, qualitative analysis as central to the identity of the discipline can also, I believe, be seen as following from a primary concern with diversity. It is because the varied forms of thought and action cannot be assumed to be known, and because we do not take for granted that we know what we do not know, that we are ready to immerse ourselves, intensively and extensively, in ways of life and in documentary materials that are initially far from transparent. We do not fully trust methodologies that limit alternatives and obstruct our exploration of whatever may be unknown.

When the services of anthropologists are sought outside academia—be it by Microsoft, development NGOs, or the Pentagon—it also appears that it is mostly understandings of diversity that are in demand, whatever they are termed; "local knowledge" is a notion that in large part stands for this.

DIVERSITY UNDER SCRUTINY

Proclaiming diversity to be our business may thus allow us mostly to get on with what we do, under an umbrella that might be recognizable and not too puzzling to the world outside. It may provide enough room, too, rather behind the scenes, for those internal distinctions and cleavages—philosophical, social, political, stylistic—which may be of intense interest to members of the anthropological community, but of little significance to most other people. Yet the brand is also likely to raise certain kinds of questions concerning our assumptions about diversity and our values; and even if we will not all be equally engaged in thinking about answers to these in some more organized manner, we can hardly all disregard them.

Probably we can agree that diversity is a notion that now figures much more prominently in public discourse than it did, say, a couple of decades ago. The fact that this idea has such a wide resonance is on the whole, I think, one reason for pushing it as a brand keyword. However, if it has on the one hand become somewhat fashionable,

and is on the other hand not entirely uncontroversial, there are also certain risks involved. We should try to stabilize and institutionalize our own understanding of it, hoping to avoid being dishearteningly stuck anytime soon with the favorite flavor of yesteryear. We should also have a sense of where there are grounds for contention. We may aim at mapping diversity, and understanding it, but are we also inclined to celebrate it, and to assume that it is limitless? There seem to be good reasons not simply to take for granted that all these things belong in one package. We should try to clarify the issues while we have a chance, publicly or among ourselves, to elaborate on our brand.

I do not think we should just accept the fairly strong tendency in some contexts of public discourse—not least in recent times—to leave diversity unexamined as something self-evidently good and valuable. Let us rather draw on whatever expertise we have to be a little more intellectually hardnosed, a little less softhearted. It seems quite likely that anthropologists do not only study diversity, or their particular slices of diversity, but are also inclined to respect it, appreciate it, even enjoy it, in moral, intellectual or esthetic terms; that in this sense they have cosmopolitan leanings (I will return to this issue in Chapter 5). Such an inclination should not prevent us from attempting some real scrutiny.

On the one hand, it seems to me that the most basic argument for diversity, or one which tends at least on a preliminary basis to work out as such, is a kind of human rights argument: a respect for people's rights to be who they are and do as they choose, within some limits of social justice and concern for the corresponding rights of others. This is an argument that gets complicated by a conflation of individual rights with collective rights; without going into that issue now, I will just say that I have primarily individual rights in mind. Beyond that, I have attempted elsewhere to bring together and make explicit other more tangible arguments for diversity that can be identified. I will not repeat them here, but only note that I came up with seven—all of them, I believe, in some need of further discussion and qualification (Hannerz 1996: 56–64). On the other hand, I think it is obvious to us, as to most people, that diversity, in the sense of difference, is sometimes a nuisance, involving misunderstanding and conflict. If diversity is our business, that part of it is also included.

In this context, too, I would like to note that, while anthropologists have gone about studying diversity in their way, since the later decades of the twentieth century there has also been a growth of

fields specializing precisely in dealing with diversity as something concretely problematic—fields of "intercultural communication" and "diversity management." I have once referred to the former, a little facetiously, as "the culture shock prevention industry" (Hannerz 1990a: 245). That may suggest the rather skeptical, and even ironic, stance of academic anthropologists to this mostly applied field of consultancy and training—to the extent that we pay attention to it at all, we have our doubts about both its theories and practices.[10] Nonetheless, it may be that, if we do make diversity our business, we should take the public demand for knowledge and insight in this area seriously, and consider further how we can meet that demand in our way.

There is also that other very big question about how far human diversity stretches. Particularly since the days of interwar Boasian anthropology, personified by Ruth Benedict and Margaret Mead, it may have seemed to be a dominant understanding in the discipline that human variation is practically limitless. That, of course, has never been entirely true, and it seems to me necessary that our claim to expertise on diversity also includes an interest in its limits. Understandings of these limits must also be taken as tentative and subject to change, not least in a dialogue with those scholars who study human beings primarily as biological beings. Clearly there has been a great deal of activity in this field recently. As social and cultural anthropologists, we may not always have been very impressed by the proposals of early sociobiologists or later evolutionary psychologists, but let us not respond to arguments in this area only with a dogmatism of our own. Generally, the credibility of our claim to expertise in the field of diversity should rest not on premature attempts to establish a consensual party line on critical issues, but on providing an arena for the best-informed discussion of them.[11]

CULTURE: A CONTESTED CONCEPT

I have waited until this point to bring the concept of culture explicitly into my line of argument. It is probably clear that just about every time I have referred to diversity, I might as well have said "cultural diversity." But I realize that culture is a contested concept, forever in the public arena, and for the last 20 years or so inside anthropology. Consequently, opinions may differ on whether it should be up-front in a presentation of our brand. The debate on the topic may no

longer be so intensive within anthropology, and it may have come more or less to an end without being resolved either way.[12]

I can see the reasons why some colleagues may feel ill at ease with some of the uses of the concept of culture—not least its essentialist, even fundamentalist, varieties employed by political extremists in public life. My own view, however, which I have already elaborated more fully in other contexts, is reformist rather than abolitionist, and fairly pragmatic.[13] Undoubtedly, we can manage to find ways of avoiding just about any concept if we try hard enough; but for my own purposes I do find it practical to use "culture" and "cultural" to refer to the fact that human beings are learning animals, using meanings to which they have access through their interactions with other humans.[14] Such a usage can be processual in its attention to stability as well as change, and does not assume internal uniformity or sharply bounded units. It does not have to succumb to "culturalism" either, in the sense of exaggerating the importance of beliefs, values or habits at the expense of factors of power or material circumstances. The goal must certainly be to analyze the relationships between culture, power and materialities, and that should not be an impossible task. As I remember, according to an old psychological finding, human beings are supposed to be able to keep as many as seven different things in mind at the same time, so it would not seem unreasonable to ask of anthropologists that they try to handle two or three at once.

Obviously, the question of what we do with the concept of culture has a particular connection to our concern with our brand, with our public image. It seems to me that, at least in some circles within the wider public, "culture" has been understood as an area where anthropologists have some expertise, where they can speak with a certain intellectual authority. If we stop using the concept, I doubt that this will have any particular effect on the public. Probably very few people will notice, and we would simply leave more room for uses which we find unacceptable. It may just turn into yet another case of anthropologists trying to define themselves by telling the world what they are *not*, what they do *not* do. I think whistleblowing, and trying to propagate our own view of culture, is a better strategy. We may remember one gentleman, fairly long ago, who proclaimed that "whenever I hear the word 'culture', I reach for my revolver."[15] I am afraid that, if he had been a certain kind of present-day anthropologist, he may have done so only in order to shoot himself in the foot.

It should follow from what I have said that I do not believe that a claim to a special concern with diversity necessarily implies any single stand with regard to what, in public life in large parts of the world, has in recent decades been labeled "multicultural-ism," whether as a politics of identity or as government policy. Such multiculturalism involves one kind of claim or other of taking diversity into account, but it can be critically examined with varied results. There is certainly a tendency to confuse cultural diversity with multiculturalism, but it is important to distinguish between diversity as a fact and multiculturalism as an "-ism"—as a policy, program, or ideology for the organization of diversity.[16] Again, it would seem useful for anthropology to provide a scholarly arena, accessible to the interested public, for debate over relevant concepts and realities. Rather unfortunately, such debate—particularly at a theoretical level—has mostly been carried out within the confines of political philosophy.[17]

THE PASSING OF DIVERSITY, AND ITS NEW GROWTH

Another consideration: Does the branding of anthropology as a discipline specializing in diversity really take us safely away from that public image which we reject, of a discipline somehow only of the past—antiquarian and itself antiquated? There is the possibility that diversity itself is seen as something mostly declining, even vanishing. Fukuyama's "end of history," with its global triumph of liberal democracy and whatever supposedly would belong in the same package, could perhaps equally well be read as an end of diversity. Over the years, illustrious anthropologists have come up with formulations pointing more or less in that direction. Again, there are those lines of Malinowski's with which this chapter began. Nearly 80 years later, Clifford Geertz (2000: 68) suggested that "we may be faced with a world in which there simply aren't any more headhunters, matrilinealists, or people who predict the weather from the entrails of a pig."

It is very likely that the range of cultural variation in the world is no longer what it has been. Insofar as anthropology has an interest in keeping a record of all the kinds of more or less patterned thoughts, activities and relationships that have at one time or other occurred in some corner of humanity, we may indeed take an interest in the past, and in documenting now what may soon no longer be around as a part of ongoing human life. Not so long ago this was what the

notion of "urgent anthropology" usually referred to, and for some of us it may still be a priority.

We may remember, too, that long-established tendency in anthropology to place the Other somehow in another time, which Johannes Fabian (1983) criticized in what has become one of the discipline's more recent classics. In any case, we must now insist that our business is diversity in the past, present and future. And our own present, of course, is not that timeless "ethnographic present" of the past, but indeed this particular period in that flow of history which does not end. That means we must resist those simplistic narratives of global homogenization that keep showing up in new versions, and attend to the sources of resilience in human modes of thought and practice that keep much diversity rather stable. To use the plural form "modernities" is to insist that there is diversity in modernity.

I have much respect, and even intellectual affection, for those colleagues who devote their labors to ever closer views of the cultural minutiae of the *longue durée*, or of vanishing tradition. Yet I think we should also take a special interest in the way that new cultural forms continue to develop, bringing about new diversity. I see a growing interest in anthropology in the future, and in ideas about the future.[18] No doubt it is wise to abstain from claims to predictive powers; the anthropology of the future can only be a subjunctive genre. Yet, like, for example, Michael Fischer (2003: 37–8) and Bill Maurer (2005), I would propose that our methodological inclination toward ethnography, toward open-ended encounters with a potential for serendipitous discoveries, should be of particular value in studying what is emergent. Rather than engaging with diversity mostly by looking backwards, anthropology can be in the avant-garde of describing what is growing and what may be coming. Does not the recent interest in such varied areas as cultural blending (hybridity, creolization), the varieties of virtuality, and science studies demonstrate that much diversity is alive and well around us, and ahead of us?

DIVERSITY IN WRITING: THE USES OF COMPARISON

These, then, are some of the matters I believe we must keep in mind in trying to update the public image of the discipline by making diversity the keyword of our brand. Such an emphasis might also have implications for the kind of writing we do. There has been much debate over writing in anthropology recently. I touched on

it in the preceding chapter, and will return to it particularly in Chapter 5. Surely there is room for much experimentation here. In the context of a foregrounding of diversity, however, I would like to put in a word for the possibilities of comparison.

It has been one of the recurrent ways of describing anthropology to say that it is comparative, which in fact is mostly another way of saying that it is concerned with diversity; but the fact is that not very much anthropology in recent times has been explicitly comparative. So perhaps this ends up being yet another way of saying what anthropology is not, or not quite. For a generation or two now, it seems to me, whether they know it or not, anthropologists have perhaps been in a silent battle with the ghost of George Peter Murdock (1949, for example), and the style of comparison connected with cross-cultural surveys and the Human Relations Area Files.[19] That kind of comparative work rather soon turned out to involve serious epistemological problems, and it was probably just as important that its dry abstractedness had very little general intellectual or esthetic appeal to most anthropologists.[20] But comparisons can be drawn in a great many ways, and I sense that there is again a wider growing interest in their potential, not least in portraying diversity, explaining it, and discussing its implications.[21]

A more widespread use of comparison might have one particular consequence for our work. As I have said, in building our overall picture of human diversity, as a collective enterprise, we tend to add to it our own individual pieces, without necessarily having so much of an immediate concern with the whole. There are ways of being just as individually engaged in comparative work, if we can draw on varied research experiences of our own—perhaps in different groups, in different places. Michael Herzfeld (2001b), for example, has offered an account of his own reflexive globetrotting, between the Mediterranean and Thailand.[22] Further back, there is the well-known instance of Geertz's *Islam Observed* (1968), in Indonesia and Morocco. Often, however, we may need to draw on the work of other anthropologists, other ethnographers, to accomplish comparisons. If we think of ethnography as a highly personal expression—much more like art, not much like science—or if we somehow see it as intellectual property where the rights of use cannot be transferred, comparison may be in trouble. No doubt there will be varied preferences within the community of anthropologists here as well, but on the whole I do not think the obstacles to a more effective sharing of our ethnographic resources must remain insurmountable. Which of the writings of our colleagues

can be used in comparisons, and how, is more likely something that we can decide only after close critical reading, rather than on the basis of overall assumptions or proclamations.

CONCLUSION: IN THE BUSINESS OF POWER

Finally, then, what might be the future for diversity as our business? What kind of receptivity can we hope to cultivate for this brand? Again, we may not be in the forecasting business, and one should not underestimate the ability of various segments of the public to stick to old, established, and undesirable stereotypes—the colonialist in the pith helmet, the arrogant bumbler, the profligate spender of ordinary people's tax money.

We should note, however, the signs of success that do show up in varied places—bringing us back to the American scene. Let me return for the last time to Francis Fukuyama, still a globally prominent public intellectual. As I have said, one might suspect that his "end of history" scenario could also be understood to entail an end of diversity. But no: that seems no longer, or not entirely, to be the way Fukuyama has it. On the website of the School of Advanced International Studies of Johns Hopkins University, where he has recently had a leading role, I find him making the point that "most of what is truly useful for policy is context-specific, culture bound and non-generalizable" (Fukuyama 2003). He complains that the typical article now appearing in the *American Political Science Review* contains much complex-looking mathematics, the sole function of which is often to formalize a behavioral rule that everyone with common sense understands must be true. "What is missing," he argues, "is any deep knowledge about the subtleties and nuances of how foreign societies work, knowledge that would help us better predict the behavior of political actors, friendly and hostile, in the broader world."

Furthermore, as I followed the 2008 American presidential campaign, I perused the statements that candidates were making about their foreign policy views and plans. "Today, understanding foreign cultures is not a luxury but a strategic necessity," argued Senator John McCain (2007: 24); and more concretely, he proposed setting up a new agency patterned after the World War II-era Office of Strategic Services (OSS), which could draw together "specialists in unconventional warfare, civil affairs, and psychological warfare; covert-action operators; and experts in anthropology, advertising, and other relevant disciplines from inside and outside government."

That old Office of Strategic Services, we may remember from the history of anthropology, was where Ruth Benedict, Margaret Mead, Gregory Bateson and others were active during World War II. Of course, we may worry about the suggested company.

On the other hand, there was Senator Obama, who may have regretted what he said in California about the people of Middle America, and apparently engaged in some anthropology-bashing as he voiced his regrets. But earlier on, according to one news website, at a public meeting during the primary campaign in New Hampshire, a questioner made passing mention of a famous anthropologist, and Obama's response was, "the Margaret Mead reference I am always hip to" (Shapiro 2007). And he then went on to say that his country's policymakers had a problem understanding non-western cultures:

> This is a chronic problem in Washington. It has to do with our 30-second attention span. You want to get to know a country and figure out what are the interests and who are the players. You can't parachute in. We don't have good intelligence on them. And we're basically making a series of decisions in the blind. And that is dangerous for us.

Since then, we may all have become aware that Barack Obama's late mother actually earned a doctorate in anthropology, at the University of Hawaii.[23] In the recently published version of her dissertation, which is in large part about Javanese blacksmiths, we can learn that they, like their colleagues here and there in the world, are understood to have certain mystical powers; the master smith's role "overlaps with that of the magician, ritual specialist, puppet master, poet, priest, and even musician" (Dunham 2009: 43). We may perhaps wish that some of these unusual powers have rubbed off on their ethnographer's son. He may need them in the office he has moved on to; and even if he does not take up his competitor's suggestion of a new-style OSS, we must also hope that he uses his powers with good intelligence.

Meanwhile, as the *Fortune Small Business* story on Bill Gates and his pygmy hunters concludes, "anthropology marches on."

4
Field Worries: Studying Down, Up, Sideways, Through, Backward, Forward, Early or Later, Away and at Home

Perhaps anthropologists always worried about "the field"—it is central to their way of knowing. In the past, however, when elders tended to be secretive or at least vague about the field experience, and when first field work was thus indeed like a rite of passage into professional maturity, they may have worried in a more private way. That field was also usually a rather fixed entity to worry about, a "tribe," a village, some place you could get to know by covering it on foot and engaging with its people face to face. And it was self-evidently a matter of "being there"—*away*, rather than "here." Now we do not seem to be so sure what the field is, or where it should be, if it is real or perhaps virtual, and even if there has to be one at all.

There may still be limits, or at any rate proclaimed limits. When some colleague, not least a young one, or a youngish one beginning to stake a claim to seniority, comes up with what he or she sees as a new kind of field, or a new way of approaching a field, possibly the elders might curtly say, "It's already been done"; but they could also snap, "That's not anthropology." And that would mean it would not serve as a rite of passage. But I am not convinced that the current generation of elders are actually themselves so sure either of what is what.

Here I will try to look at some understandings of fields and field work as they have developed in the last few decades, in the postclassical period of social and cultural anthropology. This will in part be my own story, partly because I have reached the stage where one's point of view becomes increasingly retrospective and where one is quick to grasp occasions for nostalgia—but mostly, I hope, because my field experiences, basically four of them, can illustrate reasonably instructively some of our shifting notions of, and arguments about, proper locations.[1]

It seems that, most of the time I have been in anthropology, there have been key terms of direction, mostly of expansion, suggesting important ways in which the discipline has continuously reinvented itself. If classic anthropology was a matter of "being there," away, an expatriate anthropology, calls for an "anthropology at home," field work without malaria pills, already signaled an expansive innovation. As on reflection it was soon understood that much of that anthropology "at home" had become a matter of "studying down," it was proposed that we should do more "studying up." But then, as some anthropologists focused their ethnographic curiosity on people with practices not so unlike their own, this could readily enough be labeled "studying sideways." And when it was understood that the construction of fields could involve tracing webs of relations between actors, institutions and discourses, a notion of "studying through" was close at hand.[2] Recently, moreover, the field has sometimes been "here and there," in many sites, "trans" or "multi" something or other. If we bring time in as well, to complicate things yet more, the classics may have tended toward the construct of an "ethnographic present," but even in the distant days when Radcliffe-Brownean strictures on conjectural history were still a reigning orthodoxy (although certainly more in British than in American anthropology), there were those who quietly went on with their ethnographic excavations of the past. Later on historical anthropology indeed became a growth area, but it was also understood to include a "history of the present."[3] And again—as I pointed out in the last chapter—if anthropologists have tended to be mostly skeptical about predictions, one can spot here and there an interest in the future, and in people's ideas about the future: in terms of hope or despair, or in terms of scenarios.[4] So anthropologists now study not only here or there—and up, down, through or sideways—but also backward and forward.

Assuming, as I do, that the overall agenda of anthropology involves the mapping of a continuously changing human diversity, that is all fine. It is an agenda where most of us can fit in somewhere, with all the particularities of our interests, temperaments and situations. Our paths through this expanded terrain of anthropology may be very personal, revealing themselves only cumulatively, depending on practical circumstances and experiences as well as on debates within the discipline. At that level we may mostly take a few steps at a time, dealing with problems pragmatically as we encounter them, less concerned with what these individual moves in their

aggregate mean to anthropology. In large part that is the kind of story I will tell.

But to the extent that the discipline is also some sort of community, perhaps as some of these particular moves are added up, and as they combine with more general conjunctures, they may lead toward more collective worries. And then the practical and technical issues may turn out somehow to have become mixed with moral matters. I will suggest that we have seen this happening recently, with "the field" serving as a symbol of tensions which also have other dimensions. Moreover, I will have some comments on life after first field work. And I will end by pointing to some circumstances which affect our relationships to "the field," and that perhaps we need to think more about.

FOUR FIELD STUDIES: FROM GHETTO TO GLOBAL

I have always been an expatriate ethnographer, in the routine sense of not being in the field, in any organized way, in my own country. I went into anthropology because of an early engagement with Africa, at about the time Harold Macmillan, British prime minister, was proclaiming that a "wind of change" was blowing through the continent; a wind of independence. We might describe it as a period of Afro-optimism. I had already done a little traveling in West Africa before, but by the time I was ready for serious field work the country I had in mind, Nigeria, was descending into civil war, and it seemed wisest to think of something else. I was offered a position as staff anthropologist in an applied sociolinguistic project concerned with African-American dialect in Washington, DC, and so I spent two years in the latter half of the 1960s hanging out in a black neighborhood in that city; a ghetto neighborhood, as one would have said, at least then (Hannerz 1969; see also Hannerz 2004a).

At that point in time, the location may have been rather unconventional for an anthropologist, but the field work was mostly according to the rules. The social and political climate was not such that I wanted to have a high profile as a researcher, doing more or less formal interviews or even surveys. Instead this was participant observation in a quite strict sense, mostly within one city block and its immediate surroundings. Above all, in the eyes of neighborhood people, I was a young white foreigner, and a student of some sort; no doubt noticeable enough, although I tried to be unobtrusive. Since then, as anthropologists have become more self-conscious about their ways of being in the field, the

notion of the observer as a "fly on the wall" has come in for much ridicule. Indeed, we really could seldom be so inconspicuous, and it would be entirely unnatural to be somehow present but not engaged in human interactions. Nonetheless, in the field in my Washington neighborhood, I preferred to let people walk their walk and talk their talk, to have everyday events as far as possible take the course I believed they would have taken without me— in that sense surrendering to the field. This seemed least risky in terms of my personal acceptability, and furthermore, since it was a central purpose of the study to understand how the modes of thought and action of black ghetto dwellers differed from those of mainstream America—indeed, whether they differed—more active interference on my part would have seemed quite counterproductive. In summary, my Washington experience seems to me to have been a case of field work by immersion, of the classical type—or even in certain ways a somewhat extreme version of it.

If anything worried me about my Washington field, consequently, it was hardly the general nature of my endeavor. I was aware of questions of personal safety, but less because I was a conspicuous outsider than because people in the neighborhood were themselves a bit preoccupied with the threat of violence—the management of danger indeed became one research topic (cf. Hannerz 1981). Perhaps more relevantly to the kinds of matters we are dealing with here, I occasionally felt some dismay, especially early in field work, about the hours I spent sitting around in the semi-darkness, watching bad TV programs with neighborhood friends. Standard ethnographic practice in the 1960s was not yet about media use. But I will come back to that.

Two developments in the growing self-consciousness and intensifying debate among anthropologists about their fields occurred about the time of my work in Washington, or soon after. One was the emergence of that notion of "anthropology at home"—there had always been some, but now it became increasingly recognized as a tendency, and perhaps also professionally legitimized.[5] "Away" was no longer self-evidently where a field would be. But what was "at home"? I was rather surprised, and a little amused, when I received an invitation to a conference on that topic on the basis of my Washington experience—I had hardly thought of my Afro-American ghetto neighborhood in quite that way. But since I did not attend the conference, I did not get to voice my doubts. The underlying assumption may still have been that any field work in an urban, industrialized, western setting could not be quite "away." At least

unless you were a non-Occidental anthropologist yourself—other ethnographers, other Bongo Bongos.

The second development could be readily identified with one particular anthropologist, and one particular chapter in a book criticizing established anthropology as it was. In her contribution to *Reinventing Anthropology*, Laura Nader (1972) argued that it was time for anthropologists to "study up"—for various reasons they had been "studying down," observing people rather less powerful and privileged than themselves; but, not least in order to understand how powerlessness and poverty were shaped, one must scrutinize the activities of the people at the top. And so, in these terms, what I had done in Washington was merely one more case of studying down.

As things turned out, in my next field, after a fashion, I did study up. After completing my PhD I spent the next year doing my long-postponed Swedish military service (mostly in an office, coding and decoding secret messages of which there were very few, and therefore having much undisturbed time for reading), and just after I got out of that, I had an invitation to a conference at the University of the West Indies in Jamaica, again largely relating to my Washington study. Since I was not due back in Stockholm until the next academic year began, I had time to spare, and decided to stay on in the Caribbean for that period. Where I went was in large part a matter of accident. When I had made a brief excursion from Washington to Jamaica a few years earlier, the airline booking agent had regretted that I could not get on a non-stop flight from Jamaica back to the US, but had to have a brief stop on the Cayman Islands—but that way, at least, I became aware of their existence. As this was in the late 1960s, this small lingering outpost of the British empire had not yet earned a reputation for either tourism or more-or-less shady offshore banking. I could quickly conclude that little anthropological work had been done there. So to the Cayman Islands I returned, for a slightly longer stay (Hannerz 1974).

I had a very modest research grant which at least allowed me, to begin with, to rent a bicycle—although after having found that one stretch of country road was inhabited by some particularly mean dogs, I began to rent a car for selected excursions instead. It would take me hardly more than an hour to drive from one end to the other of Grand Cayman, the largest island, where most of the population lived and where I did the greater part of my research. It was a flat, in large part bush-covered island. Unlike most Caribbean islands, this had never really been a plantation society. Mostly the population had at one time made its livelihood from fishing and

sea-turtling, and then a large part of the male population had become commercial seamen, on the high seas of the world, thus absenting themselves from island life for long periods. By the time I came to the Cayman Islands, however, the economy was changing, and local politics with it.

My grant had not required very precisely identified research goals, so when just about a week after my arrival a political crisis erupted, I could readily enough focus my attention on that. The British colonial administration had announced plans for regulating land sales and construction more strictly than in the past. The beginning growth of tourism, the rise of new hotels on the sea front, and the perceived interest in plots for foreigner-owned second homes appeared to require such planning (even if I could still discover that during the summer off-season I could have the entire marvelous Seven Mile Beach more or less to myself for an afternoon). A variety of local entrepreneurs, however, probably with expatriate interests in the background, much preferred to continue a less constrained style of business into the future, and they mobilized local opinion. There was heated agitation, and a protest march unique in the history of the islands. Then one morning a British gunboat was seen anchored off the small harbor, and rumors were flying that soldiers were hiding in the bushes inland. Eventually there was a dramatic all-day meeting of the Legislative Assembly, some kind of compromise was hammered out, and the islands could return to their ordinary tranquility.

While the crisis lasted, I had had a close-up view of local-level politics and of the styles of populist leadership exhibited by some of the prominent agents of protest. During the remainder of my stay in the Cayman Islands, much of my time was taken up by trying to grasp the character of recent political history in the territory, after an administrative link to Jamaica had been cut as that larger island moved toward its own independence, through a short-lived attempt at party politics, to the current state of individualized flux, with each political actor standing or running for him/herself, projecting a personal image. After the first period of intense observation, consequently, followed a phase where I did extensive interviews with the politicians involved, with people in the administration, and various other observer-commentators. Some of the politicians had had their egos or their reputations bruised during the preceding events and were eager to talk; the conversations could become both long and intentionally or unintentionally revealing. Apart from these encounters, I sought out various written materials—what

little there had been of a Caymanian press, official documents, old manifestoes and the like which some politicians could retrieve from half-forgotten personal collections. Working my way from the present into the past, then, I was studying backward. But, to repeat, I was also studying up, insofar as I was dealing in large part with the Caymanian political elite. Yet such things are relative. These leaders were mostly petty entrepreneurs in a small-scale society, struggling to rise a little above the level where one merely makes ends meet. One could insert here that much anthropology from the classic early or mid-twentieth-century period is indeed ambiguously placed in terms of studying up or down. Certainly it was often about kingship, chieftaincy and rule. Even so, in the context of the power relationships of colonialism in which they were entangled, anthropologists may have found themselves inevitably studying from the top down.

For my next field engagement I was back in West Africa, where I had intended to start my career as an ethnographer, in Kafanchan, a Nigerian town which had grown around a new railroad junction in the colonial era. But by now my notions of what I wanted to do had been influenced not least by my Washington field experience. If, like much urban ethnography, that had involved a smaller unit within a city, in this case a neighborhood, my plan in Nigeria was to try and deal with an entire urban community, even if not a particularly large one. (In this case, if I took a couple of hours, I could in fact walk around all of it.) Although ethnicity and occupational structure were my main dimensions, in a way I tried to maximize ethnographic diversity. I should add that it was also in a period when, for one thing, I was strongly influenced by the work of the "Manchester School" in Central Africa, and while my Nigerian town was of a different kind than the urban communities which the Mancunians had dealt with on the Copperbelt, I found their conceptual innovations and methodological expansiveness appealing. (I will come back to that briefly in Chapter 7.)

The result was that in Kafanchan I tried to combine more conventional participant observation and informant work with a wider methodological battery, some of it invented on the spot, and for the first time I worked with field assistants—in one period, several of them at the same time.[6] I also realized that, in a way, in this diverse and segmented community, I was juggling with several mini-fields. Conceptually, that might be a matter of keeping them together, placing them in a coherent structure. Practically, it was sometimes a challenge of keeping them apart, as their inhabitants

would not always mix well. When the hard-working, well-mannered Igbo Methodist minister would come to the guest inn where I was staying—"in order to greet me," as the saying went—and found the funny, outspoken but often not so sober Cameroonian tailor already there, there was some possibility of embarrassments. At one or two points when such salutational visiting seemed to become just too intense and too varied to handle, I escaped to the old regional capital to spend some time in the archives while things cooled down a little. The materials in the archives were from the colonial period, and there I could find documents like the handwritten or typed reports from the young, eager British district officers who had been stationed in my town in the 1930s, '40s and '50s to their superiors in the colonial hierarchy. Returning to the town, I enjoyed confronting my archival notes with the oral history I could still retrieve from local veterans.

By the time I started my project in Kafanchan, I was in a phase in my academic career which would not so easily allow full-length single field stays. So I expected to do what probably many anthropologists do in that situation—you view the field as an on-and-off thing, expecting to continue work over many shorter stays, perhaps even building that time dimension into the research design in some explicit way. Indeed, I came back to the town a number of times. Meanwhile, however, the economy and politics of Nigeria again took a turn for the worse. I had started my project during the early oil boom. When I came on what turned out to be my last visit, the prevailing mood among townspeople was not so upbeat. The elected, civilian government was seen as incompetent and corrupt. "Our best hope now," said one of my friends, a petty trader with a stall in the market place, "is that the military will take over again— but why would they want to take on this mess?" Yet about a month later the soldiers did indeed return to power. And for almost the next two decades they stayed on, and the mess just got worse and worse.

By then, however, my interests were again going through a shift in scale. If moving from the Washington neighborhood to the Nigerian town entailed a turn from an urban part to an urban whole, while I was in that town I became increasingly drawn to yet wider issues. It was a time when, back in the debates of the "First World," terms like "cultural imperialism" were in the air, while in academia "world-system theory" was increasingly influential across several disciplines; but neither of these views seemed really to illuminate the contemporary Nigerian culture I had around me. Consequently I began to cast about for ways of dealing with the latter, ethnographically and conceptually.[7] And thus I was on my way to a new

focus on what, a decade or two later, would be increasingly often summarized as "globalization." At the time, however, the key idea which attracted me, as it began to show up in more varied settings in anthropology, was rather "creolization." Some of my linguistic colleagues in Washington had been creolists, so I was reasonably familiar with the uses of creolist concepts in sociolinguistics. Among the ideas I found promising, bringing such understandings into an analysis of the social organization of culture, was not least the sense of a more or less open continuum of cultural variation, ordered across borders through center-periphery relationships (Hannerz 1987, 1996: 65–78). The point was to get away from what was the still-dominant tendency in anthropology to delimit fields as bounded local units; it was a time to start thinking more in terms of "flows."

While not being in the field much for an extended period, and not particularly attracted by the prospect of a return to a Nigeria under the haphazard domination of soldier thugs, I kept working away, mostly at my desk, on the broader implications of global inter-connectedness for anthropological thought and practice (Hannerz 1996). But then, as more absences from the office, and more presences in some sort of "field," again became a real possibility, I also felt free to think about what kind of entity that might be in the context of a global ecumene. In retrospect, what I had drifted into doing in the Nigerian town could be seen as a variation on the theme of "the global and the local"—a kind of story increasingly often told in late-twentieth-century anthropology. No doubt it has been worth telling, but perhaps it fairly soon became a little predictable. Furthermore, it seemed to me a rather intellectually and method-ologically conservative reaction of anthropologists to globalization, insofar as it often allowed them to continue on with their cherished local field research practices with only rather limited adjustments.

Perhaps that is conservative, too, but I believe I have been reasonably consistent over time in sticking to the idea that social anthropology, conceptually, is primarily about social relationships; only derivatively, and not necessarily, about places. And if globalization from that point of view involves a new balance in the combination of local and long-distance relationships, it seems to make sense to seek out field entities which illustrate that development, and which are not in themselves defined in territorial terms. By the time I got that far in thinking about more field research on my own part, several of my colleagues and students in Stockholm had already proceeded to take literally the expression "globalization at work"—they were doing ethnographies of transnational

occupations, and distributing their field studies over several sites.[8] So joining that developing departmental specialism, I embarked on my most recently completed project, a study of the work of news-media foreign correspondents (Hannerz 2004b). Not only was this a project involving a group dispersed across the world; it is also an occupation with a major influence on public understandings of the world and its parts. And the foreign correspondents drew my curiosity not least because, while the circumstances of their work are surely quite different, they are somewhat like those of many anthropologists in that they entail reporting over distances which may be not only spatial but also social and cultural. Consequently, this was a project of "studying sideways."[9]

In large part, field work in this study consisted of extended, rather free-flowing interviews, of a kind I prefer to think of as conversations, with foreign correspondents in their postings, especially in Jerusalem, Johannesburg, and Tokyo. I had a number of meetings with correspondents or ex-correspondents in other places as well, but my preference was to get together with them where they were currently practicing, because in that way the conversations could go most concretely into the day-to-day vicissitudes of the craft. I also preferred talking to correspondents in postings sufficiently exotic to their audiences that their reporting would be likely to involve some amount of cultural interpretation, of representation of Otherness. For such reasons I was more interested in "Africa correspondents," "Asia correspondents" and "Middle East correspondents," and less so in those many correspondents reporting, say, between New York and London, or between Brussels and Stockholm.

This, then, was multi-site field work—as I have put it elsewhere, a matter of "being there … and there … and there!" (Hannerz 2003b) I also talked to some foreign news editors, in New York, Los Angeles, London, Frankfurt and Stockholm, to get the view from headquarters. Moreover, certainly, I followed the foreign news reporting itself in the media, although scrutinizing the end product was not as dominant a component in this study as it tends to be in most media research. I should add, too, that I found the sizeable body of autobiographical writings by the foreign correspondents themselves another noteworthy source of understanding.[10]

CHANCE, AND RISK

So these are my four fields, ghetto to global—and now what do I think I can say on the basis of these experiences? First of all, perhaps,

I would note the part that chance or unforeseen circumstances played, repeatedly, in the choice of fields and field problems. The first field I had had in mind, in Nigeria, I did not get to, at least not then; I went to an urban neighborhood in another continent instead, because somebody in Washington remembered me. I went to the Cayman Islands not because of any particular commitment to Caribbean studies but because it seemed at one particular moment to be an optimal use of time and limited resources. And to repeat, going there had something to do with an unintended airline stopover a few years earlier. If that gray gunboat had not then appeared on the horizon so conveniently soon after my arrival, and if that protest march had not occurred, I would probably not have turned my attention to local politics—of which, most of the time, there was little of any very noticeable kind.

Then, when I did go to Nigeria, it was indeed a matter of research which had involved much planning and preparation, but the choice of that particular town was again a matter of coincidence. On an early, non-anthropological, journey through Nigeria I had been on a train that stood still at the Kafanchan railway junction for a long time, and I had been curious about the great many people milling about, seemingly in the middle of nowhere, on the savanna. Soon after, I came across what amounted to a brief ethnography of the town in an early Nigerian novel, by an author who had grown up there (Nzekwu 1962). Thus, when I had a notion of what kind of town I wanted for this project, I thought I would go and look at Kafanchan first, and that was where I stayed. I returned several times, but not as many as I might have if what in Nigeria has passed for politics had not intervened. When I began thinking seriously about the foreign-correspondent study, it helped that a couple of American colleagues had kinship connections in the business which gave me a favorable start in some pilot interviews in New York. As I got to the stage of thinking about the selection of the more central sites for the study, the one I was quite sure of from the beginning was in South Africa, whether it would be Johannesburg or Cape Town, because that would be where "Africa correspondents" would primarily be located, and in a way I was still an Africanist at heart. Yet it also had to do with the fact that, interested as I was in South African society in itself, I belonged to a generation who had mostly chosen not to go there during the apartheid period; and so this was a desirable chance to make up for that loss, to some degree.[11] Beside South Africa, however, there were some number of other possibilities for field sites. I decided on Jerusalem quite early, in

part because when I planned the project I had recently been on a lecture tour in Israel, and had realized that Jerusalem had a sizeable number of correspondents covering a mostly quite compact Israeli-Palestinian beat. But then, after one national election campaign and one minor ailment had twice upset my plans for a period in Delhi, I went to Tokyo instead, partly because it could be combined with an invitation to an academic workshop.

I enumerate these circumstances not because I think they are particularly unusual, and certainly not to claim that I have been to any unique degree either a victim or a hero, but rather to suggest that anthropology is often like that. We seize on experiences and openings which somehow come in our way, and we are vulnerable to happenings in the world over which we have no control. If we can draw any lessons from this, one might certainly be that we try to manage risk, long-term and short-term, in our selection of fields, and that we might do well to point out to those at the beginning of their professional lives that they might be wise to do the same. It may seem brave to go out there and confront risks, and at times it may be worth doing, but it can also turn out to be costly. On a more positive note, I would conclude that it might often help to be well prepared, which means broadly prepared, for the serendipities of the field experience. With an Internet café in every town, and academic libraries going electronic, we can perhaps now more often quickly read up on the kinds of things we were not prepared for, wherever we find ourselves when they confront us. I still think it is important to cultivate a certain willingness to seize unforeseen opportunities, a general sensibility toward ways of making anthropology out of realities which might otherwise remain mere distractions. And that also entails some readiness to depart from research plans and research designs we carry into the field when we run into opportunities that simply should not be missed.

DIVERSIFIED ENGAGEMENTS

Looking back at these field experiences, too, I see a fair amount of diversity. They have involved studying down, to a degree up, and sideways. More importantly perhaps, I have worked in rather different ways, ranging from the fairly strict participant observation approach in the Washington neighborhood, to the foreign-correspondent study, where there was little of that and much more interviewing. Usually, however, combinations of approaches and materials have been involved. In both the Cayman Islands and

Kafanchan, I got myself into studying backward to some degree, looking up relatively recent historical materials (from about a decade earlier in one case, and about 50 years in the other) to see what light they could throw on the present.

I began the Washington study being somewhat uneasy about time spent in half darkness, as my friends, my informants, my "natives," whiled time away in front of the TV screen. What was I doing there, wasting my precious research time, watching westerns or soap operas or boxing matches? To begin with, I explained to myself that it was necessary simply because otherwise I would probably not be on the spot when something really worthwhile happened. Then gradually I realized that there were some interesting things going on right there. People were making their own choices of what they would watch, and then they would comment on it, often humorously (with some rather sarcastic skepticism), at other times admiringly, occasionally angrily. I was making my way to the realization that media reception can be something active, and culturally shaped.

Indeed the African-American Washington I got to know was a media-saturated society. Perhaps one did not pay a whole lot of attention to the print media—apart from the fact that some would want to check the local tabloid newspaper for the horse racing results, to find out what had been the winning combination in the numbers game, the illegal gambling form which provided some of the excitement of each day. But the young men would frequently take along their large transistor radios, the kind which appropriately became known as "ghetto blasters," when they had to walk somewhere, and the young children, and sometimes the adults as well, would take a few dance steps, or more, as they heard a hit tune from a gramophone or the radio, on the sidewalk, or inside the house. After all, this was the golden age of soul music (James Brown, Aretha Franklin, the Supremes), and the local black radio stations played it continuously. The disc jockeys were among the celebrities of black Washington, and with time I came to recognize that they had an important part in turning Afro-America into what we may now, in Benedict Anderson's (1983) phrase, term an "imagined community." But of course Anderson's work, informed by an understanding of the role of print media in the growth of nationalism and national identity, was not yet around at the time.

What was around, on the other hand, was Marshall McLuhan's *Understanding Media* (1964), and I happened to read it while in the field. McLuhan may have become an obscure or even unknown figure to more recent generations, but in the mid 1960s, his was

a major cultural presence, emphasizing the importance of media communicative form rather than overt content, and the revolutionary shift in consciousness and social relations which would follow as electronic media became dominant and the role of print media declined. There were striking insights and formulations here—the term "the global village" was one of them—but there were also a great many bizarre speculations; and, surely, McLuhan sometimes simply got his facts wrong.

I did fairly little with current media in the Cayman Islands (where there were at the time a weekly newspaper, a magazine, and not yet any local radio or television). But by the time I got to Kafanchan I just could not disregard the role of the technologies of culture in making imagined worlds a great deal larger than the town itself—and this included the global dumping of old American TV sitcoms, as well as the teacher and his students in a dusty hole-in-the-wall commercial school together studying a torn copy of *Macbeth*. I suspect that it may now indeed be difficult to produce an ethnography of any way of life, just about anywhere, without paying at least some attention to media habits, as if everything happened in a face-to-face world. But, of course, in my foreign-correspondent study, following news-media reporting was itself central. The story of my own engagements with media in the field at least to a degree mirrors a more general development—by now, when I open my e-mail, I find new exchanges on the list of the media anthropology network of EASA just about every day.

One might also say that the project on foreign correspondents was different from the others in being multilocal or translocal, rather than single-site. Looking back again, I can in fact now imagine how a couple of those earlier field projects could conceivably have taken a turn toward multi-site work. When I was in the Cayman Islands I could stand in the harbor of George Town, the capital, watching the fishing boats and the sea, and think of the historical network of small Anglophone communities on islands around the western Caribbean, of which the Cayman Islands had also been a part. Unlike the Cayman Islands, still a British colony, most of them belonged to various Central and South American mainland nations. Their interconnections may largely have faded away in recent times, yet it could even now be an intriguing ethnographic adventure to search for what remains of them, in local cultures and collective memories. I did indeed play with the idea, although never quite seriously, of spending field time on more or even all of them for such purposes; but then I went on to other matters.

As I have already hinted in a minimal way, one could argue that Kafanchan in its complexity, with its varied groups, institutions and settings, really became a little like a multi-site field, although in a limited space. Furthermore, that study had one more multilocal aspect to it methodologically, insofar as I also did a couple of interviews in London, with elderly ex-colonial officers whose mid-century reports on Kafanchan and its environs I had read in the Nigerian archives. These conversations, too, contributed to the oral history. But, furthermore, as I began to think about cultural processes in the town in terms of that analogy with sociolinguistic understandings of creolization, I envisaged a spatially extended cultural continuum stretching from metropolitan centers such as London, by way of larger Nigerian cities, to towns such as mine (and beyond, to villages in "the bush," the ultimate periphery) (see, for example, Hannerz 1987, 1996: 65–78). It was hardly difficult, on the basis of fairly general knowledge of West Africa and the world, to recognize what other kinds of sites could be located along the continuum, and what kinds of relationships and cultural processes occurred between them. Yet some concrete ethnography from such locales and strategic groupings in them could no doubt have allowed some further elaboration of the idea—and helped make clear that the root metaphor of creolization is something more than a fancy way of saying "mixing."

FIELDS AND THE QUALITY OF RELATIONSHIPS

Probably, however, the increasing diversity, and lack of orthodoxy, in the ways I have been going about field work again in large part mirror more general developments in the discipline. And while I have been reasonably satisfied with my own experience, I have sensed that, at times, there has been some debate, and some unease, over certain of these changes.

In the classical style of field research, the task of the ethnographer may have been taken to be to study the "entire culture and social life" of the people assigned to him (at the time, rather less often her). Being around for at least a year, he could make observations during all seasons, and he would work in the local language (although it would probably be true that it was a language which in large part he had to learn during that year).

Anthropologists, it has also been observed, typically want to be "out there" themselves, in an immersion mode, involved with all their senses. They have not been inclined toward hired-hand

research. During the period in Kafanchan when I had several local assistants conducting interviews and observations for me, I did not particularly enjoy having a fair amount of my own time occupied by interpreting their notes, and organizing and typing these, instead of wandering through the streets, or talking to traders at their market stalls. Field work of the immersion type can be an intellectual, emotional, and esthetic pleasure—the kind of experience we feel makes both our minds and our hearts grow. It may involve memorable personal encounters: I am not likely to forget Sonny and Beejay, street-corner intellectuals in my Washington neighborhood, World War II veterans, probably drinking themselves to death much too early, bringing a bricolage of unexpected references into our exchanges: Ernest Hemingway! Oscar Wilde! Rabelais! They reminded me in a way of Victor Turner's (1960) key Ndembu informant, Muchona, whom the ethnographer could imagine as an eloquent, incisive, agitated Oxbridge don. If deep personal sympathy may not always grow out of these relationships, they may still give some special insight into how circumstances shape human beings, at the same time as these themselves try to shape their circumstances.

Yet I think we need to be wary of allowing the routine assumptions and imageries from established styles of field work to carry over and dominate arguments about newly emergent styles. Take multi-site studies as an obvious example. If you are involved with two, three or even more places in much the same time span that classical anthropology would allow for one (which, for various practical reasons, may now be the case), what can you actually do? I do not want to assert that no problems of depth and breadth arise, that no dilemmas are inevitably there to be faced. Yet such studies should hardly ever be seen as a matter of somehow squeezing several conventionally defined local field studies into one single ethnographic package. They tend not to involve the same kind of social units and relations as classic single-site fields.

In my study of the work of foreign correspondents, I was in Jerusalem and Johannesburg and Tokyo, and more marginally in several other places, but I was clearly not trying to study the "entire culture and social life" of these three cities. I was merely considering their characteristics as working environments for foreign news-people, and trying to get to know some number of these people who were stationed in them. In fact, I was not trying hard to get to know these individuals particularly intimately either; what mattered

to me about their childhood or family lives or personal interests was how these might affect their foreign correspondent work.

Anthropologists often take a rather romantic view of their fields and their relationships to people there. They find it difficult to describe their informants as informants, because they would rather see them as friends, and they may be proud to announce that they have been adopted into families and kin groups—not only because it suggests something about their skills as field workers, but also because it carries a moral value. They have surrendered to the field, and have been in a way absorbed by it. Perhaps it is for somewhat similar reasons that I much prefer describing my encounters with correspondents as conversations, suggesting a more personal quality, rather than as interviews—although I certainly also want to convey the idea of only rather mildly structured exchanges, with room for spontaneous flow and unexpected turns.

There is no doubt a time factor involved in how relationships evolve. Yet I believe most multi-site studies really also entail built-in assumptions about segmented lives, where some aspect (work, ethnicity, or something else) is most central to the line of inquiry, and other aspects less so. The ethnographer may be interested in the embeddedness of a particular line of belief or activity in a wider set of circumstances, but this hardly amounts to some kind of "holistic" ambition. It is a pleasure if one discovers a kindred soul, but one keeps hard-nosedly in mind what more precisely one is after, and what sorts of relationships are characteristic of the field itself, as one delineates it.

To some extent, personalizing encounters in the modern, multi-site field comes not so much from deepening particular interactions as from the identification of common acquaintances—from placing the ethnographer in the translocal network of relationships. Meeting with foreign correspondents, I sensed that it was often appreciated when it turned out that I had also talked to friends and colleagues of theirs in some other part of the world—perhaps more recently than they had; or even to their editor at home. As I tried to include informants from the same news organization in different postings, to develop my understanding of its operations and as a kind of triangulation, such connections could be discovered fairly often and easily. It was a matter of establishing personal credentials.

Again, in my foreign-correspondent project, interviews—albeit long, informal and loosely ordered—were a large part of my field materials. I did sit in on a daily staff meeting of the foreign desk at one Stockholm newspaper, and from Jerusalem I went on a

reporting trip to the Palestinian West Bank with one correspondent. More materials of these and other kinds would no doubt have been of value, but for practical reasons I did not pursue some such possibilities, using the time at my disposal rather to ensure diversity through the interviews. (I tried to include different kinds of media, although with an emphasis on print correspondents, and I wanted to include a reasonably broad range of nationalities.) Also, because in those three main sites, and to a more limited extent in a couple of other places, I met with correspondents as they were involved in the activities of a particular beat, the interviews could be detailed and concrete.

Probably many multi-site studies depend rather more on interviews than single-site studies, for a couple of reasons. If the researchers have to handle more places in the time classic field work would devote to one, they may be in more of a hurry. Language skills also probably play a part. In interviews, it is more likely that you can manage in one or two languages. Whether the correspondents I met with were American, German, Italian or Japanese, my conversations with them were in English; to Swedish compatriots or other fellow Scandinavians I spoke Swedish. In those three main sites, for many of the correspondents—particularly those who were expatriates, rotating regularly between assignments—English was their working language as well.[12]

Yet this is surely not to say that multi-site ethnography must rely entirely on interviewing and informant work. This still depends on the nature of research topics. Studying ballet companies in Stockholm, New York, London and Frankfurt, and the connections between them, Helena Wulff (1998) could view performances and sit in on endless rehearsals. Although she could not very well "participate" in the public performances, her own dance background also meant that she still had a particular empathetic insight into the more practical, bodily aspects of dancing lives.

If pure observation, or participant observation, has a more limited part in some multi-site studies than in the classic model of anthropological field work, moreover, that may not have so much to do with sheer multi-sitedness as with the fact that such studies tend to involve settings of modernity. There are surely a great many activities where it is worthwhile to be immediately present, even actively engaged, but also others which may be monotonous and isolated. What do you do when "your people" spend hours alone at a desk, perhaps concentrating on a computer screen? (When I worried about time spent in front of a television screen in my

Washington research, it was at least a sociable activity, in the company of several others.) Difficulties of access also matter—and if you do manage to get access to one or two significant arenas, it is in the segmented nature of modern social life that you do not therefore necessarily get in smoothly everywhere.

Interactions in the field, in other words, are often limited, regulated, and timed. We may indeed have thought of the classic ideal of participant observation as "anthropology by immersion," an involvement so deep that the supposed risk was one of "going native" (hardly anybody did). In contrast, we now hear of "anthropology by appointment"—with some irony or self-irony no doubt intended.[13]

That does not sound particularly appealing. Yet we should perhaps think of field work as "the art of the possible" (even if the expression was first used about politics, by the Prussian statesman Otto von Bismarck). Studies in contemporary, complex fields can also be seen as offering a range of sources and channels of knowledge and insight not available when Bogoraz took on the Chukchee, or Evans-Pritchard the Nuer. I am reminded of the more recent reflections of Hugh Gusterson (1997), moving on from an ethnography of one California nuclear weapons laboratory to a study of the entire American "nuclear weapons community," and looking intermittently at the counterpart Russian community as well. Inside the gates of the California laboratory there was no opportunity for participant observation whatsoever, except for meals at the cafeteria, although Gusterson could socialize with its scientists in local churches, bars, social clubs, or hiking parties. But then he could also collect data eclectically, in many different ways, from a disparate array of sources, attending carefully to popular culture, and reading newspapers and official documents. And in his continued, more multi-sited research efforts, he complemented site visits with field work by telephone and email.

In a slightly provocative formulation, Gusterson concludes that anthropologists should distance themselves from "a fetishistic obsession with participant observation." The key summary term could instead be "polymorphous engagement," referring to the ever-shifting diversity of the field worker's craft.

With "polymorphous engagement," skills of synthesis may become more important than ever. We may keep looking for those social occasions where people do indeed get together, and where we can join them—in contexts of modernity, it would seem that "meetings," ranging in scale from the morning get-together of the

office staff to the annual World Social Forum, are central among them. Media, again, differ in their combinations, in single-site as well as multi-site fields. In her ballet study, Helena Wulff describes the varied ways in which dance videos are used in the transnational dance community, including instruction as well as marketing. In my foreign-correspondent study, the print, sound and screen reporting itself naturally made up a large part of my materials, interweaving with my interviews. A growing interest in the ethnography of documents appears to relate most closely to a research interest in state machineries, and power/knowledge constellations (Riles 2006).

We can look at all this as a matter of interesting methodological challenges, which frequently go with those expansive ambitions of studying upward, sideways, backward or whatever. Yet if anthropologists have recently been inclined to worry about fields and field work, I am afraid we must look for some of the sources of unease in circumstances which have less to do with field technique as such, and more with the discipline as not merely a professional but a moral community, with its divisions and its wider environment. In the tradition of that community "the field" has stood for a rich and at the same time demanding experience, and a central question underlying our worries may be whether some of the membership of the community now miss out on that experience, or cheat by circumventing it. There may be a suspicion that if one does something more like anthropology by appointment, one misses out on some of that deeper personal engagement, and also fails to face up to whatever kinds of tangible or intangible hardships tend to accompany such experience. And so, to those who feel that they have come through all that, such a person may seem not quite a real anthropologist, not a full member of the community, not a peer.

That sort of suspicion may at times have been difficult enough to deal with, around the seminar table or in corridor talk, if it appeared on its own. But then the suggested difference may have been aligning itself with other large or small irritations in and around the community. It could appear as a conflict between generations, as the espousal of other kinds of fields—other ways of doing field work—seems to devalue the symbolic capital of elders, who in their turn hint that their juniors make it easy on themselves. If that is the problem, it will presumably not be enduring, as the younger generation will always have the last laugh (at least until the next generation comes along). But the preoccupation with what should

count as field work has also seemed to show up in the concern with policing the border with cultural studies, where practitioners, it is fairly widely held, too often get away with "ethnography lite."[14] In the discipline as a transnational community, too, controversy over understandings of the field have perhaps also occasionally, in a rather stereotypical fashion, set Americans against others. Much of the critical scrutiny of old assumptions about the field, and many of the innovations in defining it, have come out of American anthropology, while elsewhere their reception may have been mixed with some resentment of a hype of newness. Yet as time goes by, reinventing field work has clearly become more of a shared concern in anthropology's world.[15]

Getting away from certain varieties of moral overtones and animosities, perhaps we could try to spell out more explicitly our assumptions about different kinds of fields and different kinds of field work. It might help if we find a conceptual apparatus which bridges the gap between types of fields, allows us to compare them more precisely, and renders similarities and differences identifiable and more literally debatable. For one thing, we may try to cultivate an understanding of the connections between the kinds of relationships we study and the kinds of relationships we ourselves have in the field.[16] If anthropology by immersion and anthropology by appointment are actually often about different kinds of relationships between other people, what sort of depths of experience and interpersonal closeness we can reach in them ourselves is perhaps really a little beside the point.[17] And instead of dismissing some ways of being in the field only as deficient with regard to true field experience, we could ask what would be the long-term consequences for anthropology in its relationship to the world out there if conceptions of field and field work were not allowed to vary and change. Again, we have our different priorities in what we want to do with our anthropology. I have heard one colleague describe anthropology as "the study of intimacy"—if that is what you want to do, perhaps you can stay in one place and get your ethnography entirely out of face-to-face encounters. But I doubt that everybody will agree that this is what all anthropology should be about, and what would worry me rather more would be if we insisted on defining the discipline in terms of a methodological standard operating procedure, and thereby painted ourselves into a corner from which we could not reach out to much of contemporary human life.

FIRST AND LATER FIELD WORK

Looking back at my own field experiences, let me point to one more issue—but I will then begin further back yet. In 1950, Professor Edward Evans-Pritchard, not yet "Sir" but certainly a central figure in the mid-century discipline, gave a radio lecture on the BBC Third Programme where he outlined what an Oxford man would properly do to become an accomplished field worker in social anthropology. Having prepared himself meticulously for a couple of years, and if fortunate enough to get a research grant, the anthropologist-to-be would proceed to his chosen primitive society to spend there usually two years, preferably divided into two expeditions with a few months in between, if possible in a university department where he could think about his materials. Having returned home, it would take the anthropologist at least another five years to publish the results of his research, so the study of a single society could be reckoned to require ten years. And then, Evans-Pritchard concluded, a study of a second society was desirable—lest the anthropologist would think for the rest of his life in terms of a particular type of society (Evans-Pritchard 1951: 64ff.).

Even as anthropologists went up, sideways, through, backward, forward, and home, something much like the model for field work which Evans-Pritchard enunciated seems to have remained more or less the only fully publicly acknowledged model for field work, and for becoming and being a real anthropologist. To a degree, I conformed to it myself in my first field study in Washington, with two continuous years in the field immediately followed by concentrated writing. Then, however, there was that brief sojourn in the Cayman Islands, the results of which took a little longer to write up, as I was distracted by other matters. After that, the Evans-Pritchard model did not work at all. Between my first field visit to Kafanchan and my last, there were almost ten years; and five years passed between the pilot interviews I did for the foreign-correspondent study and the final interview. (Incidentally, it took place in Washington, with an ex-Middle East correspondent with whom I had just missed getting together once in Cairo, but who a couple of years later was reporting from the United States for Swedish radio—from a studio within walking distance of the neighborhood where I conducted my first field study.)

The trouble, of course, is that the Evans-Pritchard model assumes an individual who has no other commitments, occupational or domestic, which compete seriously with field work—at best, a young

PhD student. Beyond that, it seems as if you are on your own. For a fair number of professionally qualified anthropologists, in and out of academia, it is true, that first field study also becomes the last. Some of them have such a rich harvest of materials from that experience that they can go on working on them, and publishing them, for the rest of their careers.[18] But doing field work once during one's relative youth, and then going on talking about it, lecturing about it, and writing about it for decades afterwards may just seem a little pathetic, and a great many anthropologists, no longer in the first phase of their careers, certainly do engage in a greater variety of spatial and temporal practices as they continue to keep an active research interest alive. It is hardly possible to formulate any single, clear model of such work to place next to the Evans-Pritchard model, since it will again involve that art of the possible, fitting things in and putting things together in perhaps forever new ways.[19] It may entail discovering new possibilities of polymorphous engagements while more concentrated field research is on the back burner; or it may be a matter of relating the rhythms of the field to the rhythms of home or campus in what has been termed "yo-yo field work" (Wulff 2002). In any case, I think it may be useful, as we go on thinking and arguing about the field, to be more explicit and more systematic in our discussions of how one continues to be an anthropologist, and to contribute to the buildup of new knowledge. The point has been made, after all, that it is precisely in second or third field work, conducted in unorthodox ways, that some anthropologists have made their most innovative contributions.[20]

What I have labeled here the Evans-Pritchard model for field work has one further aspect. It shows the field worker as a lone wolf. This has certainly been a dominant practice; the main, fairly prominent exception has been married ethnographer couples engaging in research together. No doubt many anthropologists as field workers would be unhappy about being in the same site with a platoon of other ethnographers, getting in one another's way and preventing personal immersion in a sensitive social context. Yet, to a degree, the emphasis on single-person field research may also be importantly influenced by the fact that first field work is supposed to result in a doctoral dissertation which is to be assessed as an individuated product. Later on, this may at least be less important. Moreover, there may be some variety of possible team-working styles, both in the single field and in multi-site studies. I will touch on this again in this book.

WHERE OUR FIELDS ARE—OR, AWAY WITH "AWAY"?

On the whole, I have suggested, changes in the way anthropologists do field work have tended to reflect a logic of expansion, a drive to take on more and more of the range of human diversity, and to adapt their ways of going about things to new research questions, and new circumstances. And I hope I have made a case for keeping some room in field work for chance, serendipity and improvisation. As far as all that goes, there may have seemed to be little need for real worry.

Yet, in the end, we must also remind ourselves that past, present and future engagements with the field have been and will be as much determined by practical, material and organizational constraints as by our own scholarly ideals and internal debates (see Parkin 2000: 107). The landscape of future field work may place new risks and obstacles in our way. Could it be that we are worrying about the wrong things? It seems at least some fields are becoming increasingly regulated. Changing conceptions of intellectual property rights can come to limit our access to, and use of, information. Ethics codes that require us to specify in advance precisely what we will do and ask about may not go well with our ideas of the reasonable way of doing ethnography, in a manner we still find very satisfactorily ethical.[21]

What I want to dwell on in closing, however, is some worry over the future geography of field experiences. If we are used to thinking of anthropology as a discipline with a special relationship to global space, we may still notice that the distribution of fields in that space has kept changing. Looking at the discipline over time, we have seen streams of expatriate anthropologists moving between regions and continents. I went into anthropology, as I have said, with a particular interest in Africa; but while that interest is still with me, I have not worked there recently, except for my sojourn with the Africa correspondents in Johannesburg. As practical conditions of work changed, and perhaps as Afro-optimism turned at least for an extended period into Afro-pessimism, many other Africanists may also have become more strictly speaking ex-Africanists, at least as far as active field engagements are concerned. Perhaps we can discern that there have been other streams: in and out of Papua New Guinea, not unrelated to problems of law and order; mostly out of Afghanistan and areas of the Middle East; and partly out of North American Indian reservations. In one period, it seemed, particularly American anthropologists found themselves not very welcome in

India—so they hurried to Nepal instead. As mainland China has opened up for foreign researchers, in some ways and to some degree, those who for several decades crowded into Hong Kong and Taiwan have had some chance to spread out again. And recently anthropologists from elsewhere have also headed into Eastern and Central Europe and Russia, on a scale hardly conceivable as long as there was something called an Iron Curtain. On a visit to Tokyo, I find a young Japanese colleague catching the Vladivostok flight to bring him to the site of his study of education in Siberia.

A chapter in the history of anthropology may be written about these collective exits and entries, and their implications for the discipline.[22] Of course they have not just involved our scholarly or personal whims or fashions, but have had much to do with our particular vulnerability to political and other changes in the world. At present, however, I wonder if—rather paradoxically in what is supposed to be an era of globalization—the most marked tendency is not to head for some new, accessible region out there, "away," but to find one's field "at home."

It is true that, even in the era of classical anthropology, back in colonial or early postcolonial times, and in the old heartlands of the discipline, the latter was not altogether consistent in its conception of "away" as the proper place of ethnographic work. Of the students attending Bronislaw Malinowski's famous seminar at the London School of Economics, who wrote the well-known book about the Kikuyu people, *Facing Mount Kenya* (1938)? Jomo Kenyatta, a Kikuyu, who never became a professional anthropologist, but Kenya's first president instead.[23] And which Oxford-trained anthropologist studied chieftaincy in Ashanti? Kofi Busia (1951), from an Ashanti chiefly family, who later became, rather briefly, prime minister of what was by then Ghana. Expatriate anthropology in those days was apparently rather for the pale-faced natives of the center. The famous Indian anthropologist, M. N. Srinivas, who learned some of his anthropology at Oxford, did his pioneering research in India, "at home"—although mostly with people very different from himself, thus showing at the same time the inexactness of that notion (see, for example, Srinivas 1966). But let us for the moment again accept the simple assumption that "at home" is in one's country of residence.

It is also a fact, as I have noted before in this book, that much of the rapid growth of anthropology in the late twentieth century occurred in countries where research would mostly be conducted locally. In these instances, there has been no switch away from

"away." Perhaps, on the other hand, we see real tendencies toward such a switch in countries, mostly in Europe and North America, where going abroad for field work has been a well-established choice. "My own generation of anthropologists," writes George Marcus (2005: 676) in an interpretation of American anthropology early in the new century, "represents one of the last that came to the discipline in the template of 'people and places' anthropology … The older ethnological concerns with the archive of peoples and places may be revived in the future in a fresh way, but this is not on the present horizon."

So anthropology could perhaps become a bit like the "World Series" in baseball: not particularly global. But then one's present horizon may depend a bit on which campus one is at; I suspect that at those universities that maintain stronger interdisciplinary area programs, the situation is not quite the same.

There could be various reasons for a turn toward more anthropology "at home." Sometimes there may just be a sense that the current frontiers of real intellectual excitement in the discipline can be reached without a passport; a conviction that if the task of anthropology is to explore diversity, there is enough of it close at hand.[24] Not going away may have to do with limited funding for field work, or with personal commitments such as to family life, or with a feeling that staying closer to home is generally less risky than going anywhere abroad. I suspect, however, that it is also a tendency which again involves those widespread changes in the structuring of academic life, not least in Europe. Current official pressures toward speeded-up passages through doctoral degree programs make field work of the classical type difficult. It is hard to fit the intellectual preparations for working in an alien culture, including learning another language quite possibly from scratch, as well as the practical arrangements both for going to another place and for being away from home, into that very limited time frame that, according to principles of academic mainstreaming, is likely to be uniform for all disciplines. Conceivably, even if the conditions of postgraduate education would not favor more exotic fields, those who have passed that hurdle could turn to them later on, less hurriedly. But this is not what has usually happened recently: on the contrary, more people who have gone away first have stayed at home with their field work later. In Europe, too, the increasing political and economic integration of the continent, with attractive new sources for research funding and academic exchange, may also entail turning scholarly attention and exchange inward, toward

other member countries, at the expense of involvements with the rest of the world.[25]

I have never been among those who would argue that all "real anthropology" must be expatriate anthropology. I have not seen any convincing intellectual arguments for that, and I have been happy enough with a situation where expatriates and local scholars mingle in the same territories. That may sometimes generate tensions, relating to the worth of local knowledge and to the center-periphery relationships of international academic structures, but it can also bring new dialogues and fresh insights. I would be less happy, on the other hand, with the prospect that national anthropological communities, or local communities such as that of a university anthropology department, should come to be made up just about entirely of people who have done all their anthropology "at home." We could continue teaching students about the Trobrianders, and the Kwakiutl, and the Swat Pathan, but if you looked around the table of the local seminar room, you would see only the faces of people who have had their fields in Sweden, or the United States, or Italy, or in whatever other country that seminar room happens to be located in. Perhaps it was always rare, even an anthropologist's utopia, to have around that table a gathering of colleagues who together represented a global ethnographic experience. It may sometimes even have been fairly concentrated to some corner of an empire which had, for field work purposes, become the departmental turf. Yet even that would have been, in Clifford Geertz's phrasing, "another country heard from" (1973: 23). If all or most of what was heard in the room were the voices from home, anthropology would seem to become much like other academic disciplines as normally constituted in European universities: sociology, history, political science, or whatever. What has generally been an expansive discipline would for once be contracting.

Here and there this may already be the situation. In other places, perhaps the geographical redistribution of field experiences will never reach that point. Some enthusiasts will insist on battling with the difficulties placed in their way. It could be, too, that the effects on academia of transnational migration will be an increasingly important factor in maintaining anthropology "away," after a fashion. Writing the emergent history of anthropology, one may identify the rise of diaspora anthropologists—what in some cases and contexts have been described, by themselves or others, as "halfies"—to growing prominence in the 1980s and 1990s; more pronouncedly in North America than in Europe, but noticeable

on the latter scene as well.[26] For them, what their local colleagues will see as "away" will in one way be also "at home," although in another country. That situation may entail its own complexity, of experience and in relationships. Yet even this will not quite involve anthropology as we have known it (or at any rate as many of us have known it), with its personal passages into distant and alien cultures.

Perhaps anthropology will continue to reinvent itself, attenuate that special relationship to global space, and make "away" a less significant term for the orientation of its interests. The scrutiny of its central concepts might lead ethnographic explorations in yet other directions (and vice versa). But that would involve a fundamental change in the imagery with which "the field" as an anthropological keyword has so far been associated—within the discipline, in the academic division of labor, and in public culture. Thinking forward, whether we like it or not, it is a scenario we should not disregard. In the next chapter we will come back to some of its implications.

5
Making the World Transparent

Let me first introduce a nineteenth-century compatriot of mine.[1] Fredrika Bremer is known in Sweden primarily as a pioneer of Swedish feminism, and as a novelist and essayist. But she was also a traveler and travel writer, journeying widely in Europe, as well as in the New World and the Middle East, and reaching a fairly sizeable audience among the educated Swedish public with her reporting.[2] Around the New Year's holiday in 1851 she was in New Orleans, and witnessed a slave auction, and the ethnic diversity of the French market, and a very lively African-American religious service. She went on from the United States to Cuba, noting there the cultural differences between African groups, such as the Congo and the Lucumi.[3] Then, in the late 1850s, Fredrika Bremer was on the road again, this time in Palestine. Coming ashore in Jaffa, she vividly depicts a market scene perhaps not so entirely different from the one she had experienced in New Orleans, although with another ethnic mixture: Copts, Abyssinians, Nubians, Turks, Arabs, Jews, Russians, Armenians, Greeks. She joined a group riding on horseback up to Jerusalem, and found the holy city in a bad state—dirty, rundown, overcrowded, not well taken care of by the declining Ottoman empire. Yet the black Abyssinian slaves seemed to her to be treated better by their owners in more prosperous Muslim households than were the slaves she had seen in New Orleans, and while she argued that Muslim society must change its view of women, she pointed out that the Prophet had not been entirely without a sense of women's rights. Visiting the harem of a sheikh, she found a senior wife who seemed to run the household as competently as any of her sisters in the Swedish bourgeoisie, and she suspected that the sheikh was actually a bit of a *toffelhjälte*, a "slipper hero"—that is, a henpecked husband.

While it was her own Christian piousness that had brought Fredrika Bremer to Jerusalem, she was dismayed by some of the religious practices that she encountered there: too much spectacle, too much hypocrisy, too much dubious business in handling the pilgrims flocking into the city. As she got to the Church of the

Holy Sepulchre, however, containing what is reputedly the grave of Jesus, she was touched by one scene. Three wild-looking men came rushing in. They removed their fur caps, and she noted that one of them had a shaved head with only one long tuft of hair on the top. They leaned down, kissing the cold stone slate covering the grave again and again, and pressing their foreheads to it with very solemn looks on their faces. And so she wondered, was it actually a good thing that her own sensible Protestant church had banned all outward signs of adoring love?

I will leave Fredrika Bremer there for the moment, where she was 150 years ago, at the holy site in Jerusalem. Anthropologists have been inclined to claim sharp-eyed travelers from Herodotus onwards as forerunners of their discipline, so I like to think of her in her travels as an itinerant proto-anthropologist, reporting from one part of the world to another on varieties of human life. Mostly since her time, anthropology has developed to do something like that in a professional, academic manner, while at the same time the world has kept changing, becoming more intensely and diversely interconnected.

So where are we now? Where do anthropologists fit into the present worldwide exchange of messages, portrayals and conversations? We observe, we listen, we record and reflect, we speak, we write—more often, now, we show pictures as well. But with whom, about whom, to whom? How, and why?

TRANSPARENCY, AND TOOLS OF SEEING

Writing in the late 1980s, Clifford Geertz contrasted some main roles of anthropologists through the discipline's past—and suggested that they did not fit into the present. There had been a nineteenth-century "up-from-the-ape, study-of-mankind sort of business"; then "the role of intercultural middleman, shuttling back and forth between the Euro-American centers of world power and various exotic elsewheres so as to mediate between the prejudices of one and the parochialisms of the other"; and perhaps a more or less contemporary "transcultural theoretician, bringing odd beliefs and unusual social structures under general laws." I am not really sure that either of these have entirely disappeared, or that we will never see new versions of them again. In any case, Geertz's own preference was another one: "the next necessary thing," he argued, was "to enlarge the possibility of intelligible discourse between people quite different from one another … and yet contained in a world

where, tumbled as they are into endless connection, it is increasingly difficult to get out of each other's way." (Geertz 1988: 1,146–7)

This is a conception of what anthropology can do, indeed should be doing, that I can sympathize with: assisting in building intelligibility where both diversity and connection are ever-present realities. I believe it goes with a kind of cosmopolitan stance that is widely shared among anthropologists—and cosmopolitanism here, as so frequently, is a two-faced concept. On the one hand, there is a concern with humanity as a whole, and its condition; a moral and at times political engagement with community, society and citizenship at a more or less global level.[4] On the other hand, cosmopolitanism involves an awareness, and often an appreciation, of diversity in meanings and meaningful forms. These two faces may occur separately from one another; and one may often be a cosmopolitanism with a worried face, trying to come to grips with very large problems, while the other is perhaps more often a cosmopolitanism with a happy face, enjoying new sights, sounds and tastes, new people. At best, however, I think there is an affinity between them, and it should not be hard to find this kind of double cosmopolitanism among anthropologists.

In this chapter I will pursue such a notion of our work by first shifting to a notion of "transparency," a keyword of our times, with the capacity of keywords to lead on to a range of associations. But then I will also play with some other visual and optical metaphors, to identify certain variations in the anthropological line of work, and in its contexts of relationships. Such a vocabulary has attracted anthropologists before—Clyde Kluckhohn's *Mirror for Man* (1949) was a classic of mid-century anthropological outreach, and rather more recently James Peacock has attempted to summarize what the discipline is about in a short book named *The Anthropological Lens: Harsh Light, Soft Focus* (1986), while Michael Herzfeld has used Greek ethnography to inspect *Anthropology through the Looking-Glass* (1987). One should always be wary of metaphors: they emphasize certain features in what is under discussion and lead us to ignore others, and they can be in some ways misleading. Johannes Fabian (1983) has pointed to a visualist bias through much western thought which has also influenced anthropology. Yet I hope a few terms out of our everyday experience, for what we see, and what we see with—tools such as mirrors—can help us in a rather simple way to organize our understandings of some of the anthropological endeavor.

I first came across the notion of transparency as a quality in social life rather long ago, in a brief ethnography of his own Igbo (also spelled Ibo) society by the Nigerian anthropologist Victor Uchendu (1965: 17–18). For once, we might note parenthetically, the young African graduate student at a major American university was not automatically sent back to do his first field work at home; Uchendu's advisors sent him to the Navajo. So there he was, under the high skies of the plateau lands of the American Southwest, in his Igbo robes when the weather permitted, in order to emphasize his foreignness; trying to make his way into a reservation society where he was actually one in a continuous line of ethnographers, identified among his hosts as people "who run around asking questions about old ways" (Uchendu 1970).

But again, for the moment at least, that is parenthetical. Nonetheless, even before Uchendu got to his doctorate, he pulled together his thoughts and his mixed insider-professional knowledge of Igbo life. And one of the characteristics of that life, he noted in his first chapter, was exactly transparency. The Igbo, according to Uchendu, washed their dirty linen in public; they dreaded any loss of face; their leaders had to be accessible to all; secretive people were held in contempt; the host would taste food or drink meant for the visitor.

One might feel that in the small-scale, face-to-face society of the past, the Igbo probably had a better chance to realize their value of transparency in the ongoing contacts of everyday life. Much of what they might want to know about each other was more or less immediately at hand. And yet Uchendu already implies that they sometimes had to struggle for it, and also to make a show of it.

Then, sadly, one also finds evidence that real transparency did not fare so well in the turbulent Nigerian society of which the Igbo had become a part. After many years in American academic life, Victor Uchendu returned to his country of origin. And early in his campaign to become mayor of his town, in 2006, he was assassinated, and it seems the crime has remained unresolved.

But meanwhile, to repeat, transparency has made it big as a term of the times—standing for a desired state in a modern world, the complexity of which really renders it quite opaque. For one thing, "Transparency International" is now the name of a prominent NGO devoted to fighting corruption.[5] More generally, it has become a keyword belonging with others such as "trust" and "accountability." Perhaps we have our doubts about it, and some of its uses—mere seeing is not necessarily understanding; strictly

speaking, intelligibility may require transparency plus interpretation. And at times it can seem rather like the transparency of a one-way screen, where the conduct of the people on one side is rendered more observable and controllable, while what those on the other side do is not visible at all. Yet there are also contexts where transparency is celebrated for its contribution to peace in the world, or in parts of it. In what he describes, somewhat idiosyncratically, as "the postmodern world," the British diplomat Robert Cooper sees the state system as collapsing, although not into disorder, but into greater order.[6] And Cooper, at one time identified as Prime Minister Tony Blair's foreign policy guru but also long centrally placed within the European Union political machinery in Brussels, sees contemporary Europe as now well into this order: "It represents security through transparency and transparency through interdependence" (Cooper 2003: 37). The system of mutual surveillance, he says, helps the continent get away once and for all from suspicion and confrontation, from the battlefields of the dark past.

One may feel that what Cooper describes as postmodernity overlaps significantly with what in other contexts would be described as neoliberalism, and that nations, having been major building blocks of our social and cultural imagination for some time, retain a considerable power over minds. Even among the personnel most directly involved in negotiating the European Union agenda of mutual transparent openness, there can still be a jockeying for national advantage and honor.[7] Nonetheless, one can sympathize with the general idea. Something good seems to come out of a sense that, to a degree, people have a clear understanding of each other's motivations, and of the principles on which they act. Opacity in culture and social life can lead to suspicion, rumors, conspiracy theories, even panics and paranoia.[8]

On the whole, in the small-scale society it may be secrecy that requires more effort; in a large-scale, even worldwide society, it is transparency that must be worked at. It may be that any way of life, any culture, is too multi-layered and labyrinthine ever to be entirely transparent even to its native users. So anthropologists can hardly expect to make it, through their ethnographic efforts, so perfectly clear to others, in audiences thousands of miles or even just a few blocks away. And perhaps both anthropologists and other people even prefer that there is some mystery left in life, some challenge, something to explore. All the same, it seems like an attractive prospect that, if the world can be made a little more transparent to its twenty-first-century inhabitants, a place

where they feel they understand what is going on, and thus a place of more mutual confidence and trust, anthropologists can contribute something to it. Mostly, again, they would presumably do it through their writing.

CRAYFISH TO COUNTERPARTS

So we come back to that set of relationships I pointed to early in Chapter 2—between the anthropologists and the people they write about, and the audiences they reach out for: basically a triangle.[9] Again, the proto-anthropologist Fredrika Bremer fits in here, reporting on the Creoles of New Orleans or the Mohammedans of Jerusalem to the Stockholm bourgeoisie. Our part as anthropologists as helpers of worldwide transparency, as men and women in the middle, however, has not itself been altogether transparent—it has, rather, been complicated and changing. In the varied forms it has taken, and in the ways it has been embedded in wider contexts, the triangle has also offered rather different kinds of global conversations. A glimpse or two of the past, and an occasional sideways glance toward some related enterprise, may offer food for thought about the way we do anthropology.

   Consider a few lines from one of the ancestral figures of American anthropology, Alfred Kroeber, originally published more than 70 years ago. Primitives, Kroeber argued, helped make anthropology scientific. No doubt it was the exotic qualities of their cultures that first attracted attention:

> But, as time went on, these small, apparently insignificant, easily mastered cultures were found through their remoteness to be much more readily treatable objectively than any others: they came to be accurately and dispassionately analyzed much as a biologist dissects a worm or a crayfish (Kroeber [1936] 1952: 76).

We note once more that anthropology, particularly in its early years, had its links to natural history; that may have had some advantages and other disadvantages. Compared to what went before, it may have led to a greater respect for facts as opposed to flights of fancy, more preoccupation with diversity among living things, a commitment to close observation out there in the terrain. Perhaps it also was more inclined toward looking, and less toward listening. Alfred Kroeber—with a childhood, we are told, of microscopes, beetle and butterfly collections, and cages for living creatures—may

have felt some rather unreflected affinity with this approach to the varieties of the human species as well (although that was probably not obvious in the way he did field work—he was too multifaceted a person for that).[10] By now, however, we may be sufficiently ill at ease with treating others as Others. Comparing them to worms or crayfish might seem utterly appalling.

Then, later in the anthropological classics, we can find the field, or at least one of its inhabitants, imagined in quite another way. I referred to it in the preceding chapter: Victor Turner (1960) toying with the idea of Muchona, a Ndembu medicine man and a superb key informant, as a gowned don, scoring academic debating points from a dais. But that, after all, was only a playful Turner fantasy. Muchona never made it to any of the ancient universities where such a scene could have taken place.

Move on to a much more recent ethnographer, Lorand Matory (2005), investigating in history and in the present the development of a Yoruba ethnic identity through the continued traffic across the South Atlantic between West Africa and Brazil. As part of his long-term, multi-site research, Matory attends conferences of devotees and scholars of what has become something like a world religion, centering on the Yoruba *orisha* deities (the descendants of Fredrika Bremer's Lucumi in Cuba would also be among the followers); and he finds that the first of these international conferences was organized in the early 1980s by a Nigerian professor and sometime university head who was, incidentally, also a *babalawo*, a diviner. So it appears that Victor Turner's fantasy of Muchona in academia was somehow not so far-fetched.

The distance between anthropologist and native (or whatever we had better call the representative of a group under study) seems by now often rather smaller than that between the crayfish and the biologist. In fact there may be hardly any distance at all. The late Marianne Gullestad (2006), discussing this in the context of her own Norwegian society, with its strong egalitarianism, starts out from an observation that, while anthropologists may still be inclined to use the term "informants," some of the latter now find the term demeaning, and prefer to be called "consultants." When she notes this to her colleagues, however, they are a little disturbed: if the relationship to these people should now be all about dialogue and equality, what is left in it for us? What is expertise? What is scholarly authority? Yet Gullestad is emphatic about what is the desirable quality of the relationship, and this also implies that the researcher

should be open about the possible sources of, and influences on, her perspective: she must aim to be transparent herself.[11]

This reduction in distance is also apparent in the turn to the ethnographic study of sophisticated elites in finance or in science. Holmes and Marcus (2005), among the pioneers in such research, prefer to think of members of such groups as "counterparts," and envisage more fully collaborative forms of ethnography and interpretation.[12] Sketching work on how key personnel in central banks actually operate, they identify what they describe as "para-ethnographic" practices: "ways of knowing that are normally repressed, subordinated, and considered slightly illicit—the ways of knowing relegated in such technocratic organizations to the realm of the anecdotal, of hype, of intuition, of experience" (ibid.: 237). It is over these, then, that collaboration can take place. (Although, after the financial chaos of 2008, one could perhaps be allowed to wonder how great that para-ethnography really was?) Elsewhere, Rees (2008: 118), closely associated with this turn in ethnography, describes one of its new organizational ventures as an attempt to reflect explicitly on "encounters and engagements with counterpart others who are, almost like the anthropologist/ethnographer, concerned with problems of the emergent, of knowledge-production, of institution-building, of strategic decision-making."

The relationship between the student and the studied hardly gets any intellectually closer than this. Holmes and Marcus (2005: 249–50) voice a certain concern that in work along such lines the ethnographer may become just a bit too closely involved with the pursuits of the collaborators; an updated version, it would appear, of the old notion of "going native." In any case, while we may feel that the distinction between what we have called emic and analytical concepts is important to maintain in principle, it could well under such conditions become rather blurred in practice.

With regard to the other kind of relationship in the triangle—that between the anthropologists and their audiences—the assumption of the past was obviously that it was separate from that with the studied. Crayfish and worms do not read books. But that separation could not be taken for granted in the long run. By the early 1990s, one anthropological book title signaled that, in a way, the triangle was changing, could even sometimes have collapsed: that title was *When They Read What We Write* (Brettell 1993).[13] Gullestad, too, points out that—not least in her relatively compact, and highly literate, Norwegian society—audiences merge and overlap, so that one can never be sure who reads what. Moreover, when people read

about themselves in anthropologists' accounts, whether these were written for them or not, their reactions can be of different kinds.

Matory (2005: 222), for his part, notes that what American and European anthropologists and other scholars have written about Yoruba religion over the years has sometimes constituted important sources for its New World priests and priestesses as they engaged in their projects of transnational renewal.

DIVERSITY AT HOME

Changes in the quality of relationships in the basic triangle, of course, have had much to do with changes in where in the world anthropologists come from, and where they are going—if anywhere. Again, Gullestad's comments were indeed set within the context of research with her own compatriots; and, as we have seen in earlier chapters, there has been a tendency—fairly steady to begin with in some places, and growing in recent decades in others—for anthropologists to work "at home," mostly understood to mean within the boundaries of the countries where they already live. As I noted in Chapter 2, this fact can become somewhat complicated, particularly in parts of Europe, by the durable coexistence of two formally separate academic disciplines, originally distinguished—to put it a bit too simply—by their sites of research. The practitioners of one had tickets, by ship and later by air, to exotic destinations; those of the other had bus or rail tickets to rural villages, and perhaps later subway tickets to the suburbs. To remind ourselves of the terms used by George Stocking (see Chapter 2), the former were, to begin with, into empire-building, the latter into nation-building. In German (and the distinction had German beginnings), they were a *Völkerkunde* and a *Volkskunde*—that is, one dealt with peoples, the other with *the* people.

Under whichever label, anthropology at home tends to mean that the triangle between topic, anthropologist, and audience shrinks in space. Yet when they have thus been inside their polities (whether these have strictly speaking been nation-states or not), to what extent, in what sense, have these various scholars really been at home, immersed in their very own way of life, while observing it, reflecting on it, and reporting on it? To a great extent, they have really not. The original nineteenth-century *raison d'être* for *Volkskunde* was that it documented the world we had lost, or were about to lose—the past was turning into a foreign country. In the twentieth century, a major preoccupation for later generations in that

discipline, as well as for their contemporaries in the anthropology with a more exotic orientation, was the recognition of diversity nearby, in much of Europe as well as in North America—a diversity sometimes already long-present, but frequently a recent fact of local life, and quite likely still growing; a diversity marked by ethnicity and often caused by transnational migration. In the United States, it was to begin with a matter of a rediscovery of *The Other America*, as one early book title had it (Harrington 1962): a country of enduring poverty since centuries before, largely inhabited by black people.[14] In Europe after World War II, there were the growing numbers of North Africans coming ashore at Marseilles and other Mediterranean ports; and the arrival at Tilbury, outside London in June 1948, of that famous first shipload of hopeful Jamaicans on the *Empire Windrush*, marking the beginning of large-scale migration from yet more distant lands. Soon, in European societies which had become used to assuming or even celebrating uniformity (and which might not so long ago have used both soft and violent methods to achieve it), the focus of research was increasingly on newcomer groups and their adjustments to their new surroundings—but often it was understood soon enough that it was neither sufficient nor fair to problematize only the exotic customs of the migrants. A large part of the ethnographic work had to deal with offices, classrooms, courts and clinics—the institutions run by native bureaucrats, officials and specialists. And thus not least in Europe, where newcomers (whether labor migrants or refugees) faced strong states that were both nation-states and welfare states, and which impinged on their everyday lives both intensively and mysteriously, anthropologists could also be led to a more active inquiry into the workings of the modern state itself—something which had seldom been particularly prominent among their scholarly interests before. Here, one could argue, it was the newcomers who pointed the native anthropologists toward home.[15]

One could also suggest that, once diversity had been established as a strong figure of thought through the conspicuous presence of migrants and ethnic minorities, it was not so difficult to start thinking in similar terms about other divides—of class, gender, sexual orientation, age, region, lifestyle and more. Moreover, as groupings defined within the wider field of diversity increasingly entered academia with their own representatives, "anthropology at home" could also take yet another form, insofar as these scholars became the organic intellectuals of their groups, and developed their

styles of auto-ethnography, speaking now to internal audiences, next to professional colleagues, or to wider audiences.[16]

So it turns out that "anthropology at home" is in large part a geographical mini-version of that global project of building intelligibility in a context of diversity and connection. Yet, even when the point really is to stay in place, among one's own, perspectives tend to become a little complicated. There is, so it is said, a risk of home-blindness. It is also true that ethnography of what turns out to be mostly known can sometimes feel painfully trivial. Consequently, ethnographers of the everyday adopt interpretive frames which allow more surprises, more afterthought, a sense of renewed wonder.[17] The mirrors used turn out to be not quite ordinary mirrors; the binoculars are reversed to establish a greater distance.

The classical anthropological example of the trick of defamiliarizing the familiar, making it more interesting by looking at it from afar, is Horace Miner's (1956) essay on body ritual among the Nacirema—spell that backwards, and you find that it is about bathrooms and tooth-brushing among Americans. Or when you understand that the everyday is passing into yesterday, the frame may be one of nostalgia, even an emergent nostalgia from the future. Or you may be made to admire the heroics of the ordinary and anonymous, "the little people." Or a view toward something routine may involve a critical stance toward the powerful and the bureaucrats, in an examination of the implications of policy.

Irony is a tempting tool for anthropologists wishing to signal a measure of distance. In one of the more conspicuous recent examples of a proclaimed anthropology at home, *Watching the English* by Kate Fox (2004), it takes the form of a "grammar of Englishness," where characteristic modes of behavior are styled as rules—food rules, rules of sex, dress codes, rules of pub-talk, the emerging rules of cell-phone talk. Fox may not be a card-carrying academic anthropologist, but she identifies her work emphatically with the discipline and has indeed been socialized in an anthropological household, so she knows it rather from the inside.[18] And her book has evidently been a great success with a wide audience, sold from airport book stalls, with blurbs from the British popular press on front and back covers. Generally, one might sense that irony could turn out to be a somewhat treacherous instrument, if there is any uncertainty about just who is who in the triangle of observed, observer, and audience. But if it turns out that they are all of much the same kind, so that irony also becomes self-irony and everybody can be amused together, it may work well enough.

One can do very different things, however, with one's anthropology at home. When Marc Augé, well-known Africanist, in a later career phase takes to observing Paris, he does it in the style of a flaneur, although underground and not so much on foot, *In the Metro* (2002). The personal is made to mix with the professional and the collective. Augé ponders how the map of subway lines and stations can also map biographies, as stations and the street-level neighborhoods above them stand for periods in a life. But when strangers mix in trains and on platforms, they have no insight into each other's metro worlds. And while station names—Austerlitz, Solférino, Bir-Hakeim—may carry the common memory of the city and the nation, generations relate differently to the suggested history through their personal experiences. *In the Metro* is actually a long essay, where Augé also momentarily plays with the idea of what a *real* ethnological study of the subway system would have to include. As he moves freely between his glimpses of concrete and largely anonymous social traffic, his reflections on its opacity, and his own associations to people, places and social theory, however, the subway journey turns out to be a journey through a life and through the world.[19]

THE NATURAL SUPERIORITY OF OUTSIDERS?

Anthropologists at home, then, have their different ways of opening their surroundings to inspection: moving between levels of awareness, between the implicit and the explicit, between the habitual and the reflected, between the private and the public, zooming in and out. What is not quite transparent can be inspected from below, from above, from behind. And when you have done that, perhaps, in a way, you can never quite come home again, to the entirely natural, familiar, taken-for-granted.

Nonetheless, we are still inclined to think that going away, in a quite physical sense, has a major part to play in anthropology; but as time passes, we may need to think again about what that part amounts to, in the shifting relationships of the observed-observer-audience triangle. In the classical era of anthropology, which was also the colonial era, researchers may have gone to foreign lands to build up knowledge about them in large part simply because nobody else was there, locally, to do it. There were no professionally trained Trobriand or Kwakiutl social scientists. One might say that the societies of the non-western world were involuntarily, and largely unknowingly, outsourcing the organized buildup of certain kinds

of knowledge about themselves to the scholarly institutions mostly located in the heartlands, the "mother countries," of empires. But those days are long gone. Now just about every country has its own research institutions, and its own anthropologists, largely pursuing "anthropology at home." So what can at present be said, if anything, in favor of the expatriate anthropologist, the ethnographer who travels to his topic, and probably between it and his audience?

With time, as a matter of professional perspective, anthropologists have been remarkably successful in persuading at least themselves that special insight comes from being an outsider. Yet there has been a fair amount of debate over the value, and the legitimacy, of insider and outsider perspectives. Take one example: in the little introductory text on social anthropology which he wrote fairly late in his life, the almost-classical British anthropologist Edmund Leach (1982: 124–9), after scrutinizing three works on China by anthropologists of Chinese origins, takes the overall view that

> field work in a cultural context of which you already have intimate first-hand experience seems to be much more difficult than field work approached from the naïve viewpoint of a total stranger. When anthropologists study facets of their own society, their vision seems to become distorted by prejudices which derive from private rather than public experience.

In its general form, that might seem to be a judgment that in principle should apply not only to anthropology at home, but to most of western social science, which is similarly immobile. When anthropology becomes increasingly an enterprise "at home," however, there may be fewer people, even within the discipline, to buy unquestioningly this somewhat counterintuitive claim of the superiority of outsider insight.[20] As it happens, one of the Chinese scholars on whose work Leach commented (a bit more gently than on the others) was Fei Xiaodong, who had been a contemporary and friend of Leach's in Bronislaw Malinowski's 1930s seminar at the London School of Economics. Fei, who spent most of his working life from the 1950s onward in China (his scholarly career was interrupted during the Cultural Revolution, when he was sent out to do farm labor in the countryside, and he died in 2005, aged 94), returned to the question in his contribution to a symposium celebrating his eightieth birthday (Fei 1992). After some fond comments on his deceased friend, Fei noted that their teacher Malinowski had not shared Leach's doubts about doing

field work in one's own society. Fei himself could indeed see the problem that Leach pointed to. But then, as a Chinese born in the early years of the twentieth century, a time of drastic change and grave national crises in his country, he had turned to anthropology, and given up a study of medicine, when he became aware that "to try and bring happiness to the millions meant more than curing individuals of their illnesses." For westerners, Malinowski had written in the preface to Fei's first book, *Peasant Life in China*, anthropology could be "a romantic escape from [their] over-standardized culture"; and Fei himself, at the time of the symposium much later, thought that "in Western anthropological circles there are at least a few who look upon the discipline as a stage for strutting one's talents, or on the mundane side, as an intellectual game or exercise, or even a pastime." So who, then, is motivated by public experience, and who is driven by private prejudices? But Fei realized that, when he followed his own concerns to their theoretical and methodological conclusions, he risked leaving his foreign colleagues behind. A later book published abroad in English, *Earthbound China*, adopting his own comparative approach to Chinese communities, had not attracted much attention, and he suspected that his western colleagues had come to view him "as an untamed horse that had jumped the fence and was running wild across different disciplines."[21]

All the same, Fei valued his interactions within the worldwide anthropological community. In a formulation remarkably similar to that from Geertz cited above, he concluded that what anthropology was now about was how people "shaped by different cultures with different attitudes towards life are crowded into a small world in which they must live in complete and absolute interdependence."

Occasionally it has been held to be one of the evils of postmodernism that, in various new branches of scholarly pursuit, only insider perspectives are accepted, and outsider views are delegitimized (see for example Kuper 1994). I do not think this view has ever been entirely accepted in anthropology. If the debate is finally subsiding, perhaps it is rather because a more complex understanding is emerging. So many variations, so much flux, such wide gray zones have been identified that "insiders" and "outsiders" may by now be understood to be almost inevitable as gross, general, preliminary categories, but too imprecise for analytical purposes and for assertions about intrinsic worth.[22] As Fei points out, insiders and outsiders may have different priorities, look for different things. It seems unreasonably harsh to insist that insiders must forever

be incapable of thinking outside the box, although some may be so rather more than others; among the latter, one may sense a creative capacity to shift between levels of reflexivity and modes of interpretation, where the intellectual traffic in insider knowledge could indeed be a resource.[23] I cannot resist quoting Malinowski's reminiscing, in his introduction to another insider ethnography by one of his students, about the latter's participation in the famous London School of Economics seminar:

> He was thus associated in research and discussion, in original contribution and extempore critical activity with a number of brilliant, experienced and highly competent young scholars, many of whom had done their own term of field-work, and all of whom had had years of previous academic training. In this group he was able to play an active, indeed creative part, giving us illuminating sidelights, inspired by the inside knowledge of an African, but formulated with the full competence of a trained Western scholar.

This was in Jomo Kenyatta's *Facing Mount Kenya* (1938); and the first few words in the Introduction were indeed "anthropology begins at home."

One should beware, too, of the assumption that "insider" and "outsider" are ascriptive characteristics—outsiders can become insiders, and perhaps one must also consider the possibility that insiders, whether they accept it or not, become outsiders, if they are away too long, forget, or try to rely on outdated local knowledge. And if it seems reasonable enough to accept that a visiting anthropologist can get something wrong at one level or other, one should also note the kind of field sites—probably more common nowadays—where there are no real natives, or at any rate fewer of them, sharing a lifetime's localized experience and collectivized understandings. There are more people who are themselves more like strangers. I find thought-provoking James Ferguson's (1999: 208) comment on what ethnography on the urban Zambian Copperbelt was like toward the end of the twentieth century:

> Here there is much to be understood, but none of the participants in the scene can claim to understand it all or even take it all in. Everyone is a little confused (some more than others, to be sure), and everyone finds some things that seem clear and others that are unintelligible or only partially intelligible … Anthropological understanding must take on a different character when to

understand things like the natives is to miss most of what is going on.

This problematizes the relationship between "native" and ethnographer knowledge again. Do things become easier for field workers if their informants also find the world opaque, or more difficult as they have to understand not only the structure of knowledge such as it is, but also the nature and social organization of ignorance and misunderstandings?

VISITORS' INSIGHTS

It is noteworthy that some societies, at least, have been quite generous in attending to, even accepting and celebrating, the understandings and opinions offered by visiting observers.

Going outside recent professional anthropology, and returning again to nineteenth-century proto-anthropological instances of the triangle, the most striking example may still be the reception of the young French aristocrat Alexis de Tocqueville's interpretation of democracy in America (1835). This was first of all a European traveler's reporting home—but over time the reporting has drawn at least as much attention from the country visited, where it has indeed been taken to show the value of an outsider point of view. Let me come back here also to Fredrika Bremer, traveling in North America some 20 years after de Tocqueville. (While de Tocqueville was surely an insightful analyst, I would say that Bremer was a livelier ethnographer.) When I decided recently to have a look at her study of *The Homes of the New World*, I found no recent Swedish edition available, and so I had to consult a precious copy of the original edition under the watchful eyes of a senior librarian at the Stockholm University library. On the other hand, when I typed in "Fredrika Bremer" on the Amazon.com website, I quickly found a current American edition, of an English-language translation, for sale. So here again was evidently a case of an outsider's reporting held to be of enduring value.

Perhaps I could add that this proto-anthropologist's account has indeed also broken, however marginally, into professional anthropology itself. In his book *The Myth of the Negro Past* (1941), arguably one of the most important examples of early American public anthropology (although hardly so well known among European anthropologists—but I will come back to it in Chapter 7), the Boas student Melville Herskovits cites Fredrika Bremer

repeatedly, and quite extensively, on the practices of plantation slavery, in the United States as well as Cuba.[24]

For yet another example of an outsider's perspective drawing the interest of insiders, let me turn to anthropology itself. Ruth Benedict's *The Chrysanthemum and the Sword* (1946) has been a controversial work within the discipline—with research carried out "at a distance," toward the end of World War II, when Benedict was actually unable to go to Japan. The methodology may be regarded as both original and debatable, and the Japan specialists of later years in anthropology have not always held the book in high esteem.[25] Yet if Benedict had lived to experience its public reception, she might have cried all the way to the bank. In the United States itself, *The Chrysanthemum and the Sword* has apparently been sold in some 350,000 copies, which is hardly bad; but the Japanese translation, published a few years after the end of the war, apparently sold 2 million copies in the following 50 years. So, in one way or other, it would seem, the Japanese found it a worthwhile read.

Some may argue, a little cynically, that the outsider gains such popularity most safely by largely confirming the story that the insiders tell themselves about themselves. Yet we might hope that there could also be a special intellectual niche for the perceptive outsider, a way of finding new openings between levels of awareness, a way of making fresh connections between what locals habitually keep apart, sometimes a daring overview. Perhaps perspectives can be complementary. And possibly anthropologists at home and expatriate anthropologists can explore the ways in which this can be a basis for more collaboration, rather than friction, or mutual or one-sided disregard.

REPORTING HOME

All this, however, involves the possibility that an outsider's particular insight can be of value to the insiders—a peculiar kind of mirror in which the insiders gain a new view of themselves. Yet in the ordinary triangle of anthropological reporting and commentary that I have referred to, the outsider is not primarily reporting back to the natives, not some kind of in-house ethnographer. The expected audience—for the proto-anthropologists Alexis de Tocqueville and Fredrika Bremer as well as for Ruth Benedict—was back there, at home.

This can have its reflexive effect. Consider again the reporting of Fredrika Bremer. I have already mentioned her account of the

scene in the Church of the Holy Sepulchre in Jerusalem, where she described the pious emotionality of three wild-looking men at the grave of Jesus, and compared it favorably with her own unexpressive Protestantism. Before that, in New Orleans, as she attended the Afro-American religious service, including spirit possession, she seemed somewhat offended by the "shouting and jumping and noise-making." Yet she also felt sympathy for the outburst of warm sentiment, and of longing and intuition—the "inner life of souls on fire." And then, she wondered out aloud, how many in her own cold, northern country had not, at least in their youth, sensed an Africa of religious life, something that could have brought wonderful flowers and fruits if it had been allowed to live, if it had not been suffocated by the snow of convention, imprisoned in the state church of life?

I do not want to idolize Fredrika Bremer's qualities as an ethnographer. There is much of quite ordinary nineteenth-century European bourgeois ethnocentrism in her writing, the kind of formulations we would very likely reject if we now came across them in the reports of our first-year undergraduate students. But some of her reporting is rich and perceptive, and the point I particularly want to make here is that she shows herself, in her accounts of religious life in both New Orleans and Jerusalem, as a forerunner of the particular kind of cultural critique which anthropologists—not least people like Ruth Benedict and Margaret Mead—have for some time made their specialty: bringing in other, distant cultures as a thought-provoking contrast to what their audiences take for granted in their own. The foreign is used as mirror. We can remind ourselves what was said about such matters in Chapter 3: perhaps in the religious climate of nineteenth-century Sweden, Fredrika Bremer's reporting was a bit subversive?

We may be a little wary of such tricks of comparison, too, not least if they are taken to be a main purpose of anthropology. Marshall Sahlins (2000: 505) voices his doubts about a self-serving, and perhaps eventually self-centered, use of knowledge about others: "a morally laudable analysis that can amount to using other societies as an alibi for redressing what has been troubling us lately … It is as if other peoples had constructed their lives for our purposes, in answer to racism, sexism, imperialism, and the other evils of Western society."

In any case, the larger point should be an obvious one. If there is an advantage to having a stranger brought in from far away to describe a way of life previously unfamiliar to her or him, then that

advantage may or may not be so noticeable to people themselves engaged in that way of life, the people who may have become at once the topic and at least a part of the audience. On the other hand, it should be more clearly to the advantage of that distant audience whom the observer, the interpreter, the mediator, the ethnographer knows well. Hardly any native of Jerusalem or New Orleans would probably have bothered, in the 1850s, to reach out to tell their own story to the distant Swedes. Fredrika Bremer, the remarkable lady adventurer, took the trouble to travel and to write about it. Moreover, she reported in a form that was most likely more accessible and more attractive to her Swedish readers than anything that the locals born and bred in either of these two cities could have accomplished. A rooted cosmopolitan, as it were: open to the world, but knowing where home is, and what it is.[26] The outsider observer becomes insider reporter, insider storyteller.

CONSIDERING GUS

Let me fast-forward now, to the present or at least near-present, and revisit a project of mine. My study of news-media foreign correspondents, briefly described in Chapter 4, was, as I said, partly a matter of studying sideways, looking at an occupation in some ways resembling our own, again often reporting from one part of the world to another (Hannerz 2004b). One difference, surely, was that their field sites, their beats, were mostly larger than ours—they were Africa correspondents, Middle East correspondents, Asia correspondents. Another recurrent difference, however—the one I will focus on here—was in their relationship to their audiences.

I will draw my concrete example from the conversations I had with the foreign news editor, and some of the veteran correspondents, of the *Los Angeles Times*, at the time one of a handful of major American newspapers maintaining a respectable international news coverage, largely through its own extensive network of correspondents. I met *Los Angeles Times* correspondents in Jerusalem, Johannesburg, and Tokyo.

But the paper was also very conscious of its own dominant position in the news market in Los Angeles and in southern California. "This is basically a one-paper town," said the foreign editor, "You should try to write for the academic and the bus driver, because both read our paper." And there was a sign in the newsroom admonishing the journalists to "Consider Gus," a mythical figure probably much like that bus driver. But the goal, said the editor, was to make stories

clear even if they were complex. Dumbing down would do little to educate those who wished to be informed, and would lose the informed readers. Consider Gus, then, but consider also Sherry or George or Akhil or Purnima, the local professors who preferably should likewise read the *Los Angeles Times*.

Generally, however, those far-flung correspondents had not to forget that they were writing for readers in Southern California. This was one reason why this paper, like many other prosperous news organizations in North America and Europe, preferred to send out its own correspondents to major news areas, rather than relying entirely on agency materials and the like. Correspondents who had a sense of their readers, the way Fredrika Bremer knew her Swedish compatriots, would know what understandings one could appeal to, even what stories might be of special interest. The Johannesburg correspondent of the *Los Angeles Times* would write a vivid feature story for his utterly car-dependent Southern Californian readership on the troubles of getting a driver's license in South Africa. There was a "native's point of view" waiting for him at home as well.

Can we as anthropologists learn anything from this? I would suggest that we could, indeed should, if we take seriously that task of helping worldwide conversations along. Through much of the past hundred years or so, the professionalization of anthropology has involved, for one thing, the development of a point of view and a vocabulary which aim at not being ethnocentric, not conspicuously or treacherously constrained by values and assumptions of the anthropologists' own mostly western culture. No doubt that approach continues to be valuable, as long as we speak mostly to each other. Certainly, too, in any scholarly discipline, the internal conversations among peers must take up a fair share of time and effort. The question is whether we have given enough thought to the price we pay at the same time by making ourselves remote to other audiences—particularly lay audiences at home.

Perhaps, to do our part in supporting more inclusive transparency in the world, we, too, should make a greater effort to "consider Gus"; to think anew about what wider variety of styles of writing we would need to reach people who are not part of the captive audiences of Academia.[27] The recent critique of writing in anthropology, and the resulting experiments, have been aimed largely at an internal audience, made up of colleagues and always intellectually news-hungry graduate students. Certainly this has been one of the sources of vitality in the discipline, but mostly such

efforts do not reach out. It may be that, in a very large national academic system—notably the American one—with a considerable degree of autonomy, where the attention and esteem of colleagues are what matters most in continued career mobility, there is a particular logic to the concentration on internalist writing.[28] But under current circumstances particularly, when we may need to consider the public image of anthropology (see Chapter 3), it could be a good idea to think of other styles of writing as well. Again, this need not entail dumbing down. It does require thinking about accessibility, and about topics and perspectives of interest to an educated general audience.

We must of course be aware that the state of play is now rather different from what it was in Fredrika Bremer's times. This is a period when the flow of images and information is much wider, and more intense and rapid, than it was then. In recent years we have had little chance to forget about either New Orleans or Jerusalem. We might see it as a matter of representational crowding, in print and in other media, but it is a volatile field. When I began my study of foreign correspondents, I innocently assumed that their line of work was one with a great future in an era of globalization; but already in one of my pilot interviews, one of America's best-known journalists, a Pulitzer Prize-winner for his international reporting, told me to keep in mind that this was "a dying occupation." And indeed the *Los Angeles Times* is not the only large news media organization, in the United States or elsewhere, that has significantly reduced its own investment in foreign coverage since I did my study. (For one thing, it is true that such coverage is expensive.) Yet if newsprint is doing less well in reporting on the world, there are websites and blogs, and feature films and documentaries.[29] Moreover, readers may satisfy some of their curiosity about distant places by reading world fiction: Rohinton Mistry about India, for example, or Haruki Murakami about Japan, or Chimamanda Ngozi Adichie about Nigeria.[30]

Under such circumstances, in the contemporary division of communicative labor, what can be the particular voices of anthropologists as messengers and interpreters between the corners of the world? There is surely room for experimentation here. Clifford Geertz (1988: 148), pointing out that past anthropological genres did not seem to fit readily into a world that had become "a gradual spectrum of mixed-up differences," concluded that "something new must appear on the page."[31]

Indeed it seems as if thoughtful anthropologists, working in new ways, have distanced themselves from the kind of blanket rejections of journalism that have been common in the past.[32] Some journalism is very bad, and some very good; I have at times been impressed by the mini-ethnographies skilled foreign correspondents sometimes manage to fit into the constraining frames of much newswork: so many column inches, or so many seconds on the air. Probably, if anthropologists do wish to reach out to readers outside their own circles, they can draw some inspiration, and pick up some useful tricks for their trade, from journalists—including both late-twentieth-century "new journalists" and more recent "new new journalists"—and other "creative non-fiction" writers.[33]

But we may also need to think a little more about doing justice both to what we know and how we know it. Media presentations—fast news—may still be too inclined to divide humankind out there into two categories: adversaries and victims, terrorists and sufferers. Neither kind of person should be ignored, but anthropologists can contribute something different with a more nuanced and multifaceted portrayal of people and their circumstances, and are thus more likely to offer slower food for thought. Compared to the kind of writing we do for one another, I suspect that finding our way to other audiences requires, for one thing, more of the varieties of concreteness—more of the sights and sounds, and generally of the senses; more situational analyses, to use one old genre identification of ours; more person-centered ethnography in a wide sense, such as life-stories, to use another; and more vivid comparisons.[34] Generally, our relative consensus on the value of rich, fine-grained ethnography may just be, to a degree, a result of the inward-turning of academic anthropology. It is a value that comes naturally to the connoisseur-ship of skilled craftspeople and their apprentices in training. In our contributions to a wider culture, however, where audiences may just be somewhat impatient with our in-house enthusiasms, our ethnography may need to be fitted into more mixed genres.

There is also the big question of what to do with narrative.[35] It seems the human mind is particularly receptive to storytelling. The matter has been under debate among historians as well, but undeniably they use the narrative format extensively, and equally undeniably some of them are and have been very successful in reaching lay audiences. Nonetheless, one should also consider the limits of narrative. Again, to do justice to our own particular mode of understanding, we might ideally try to blend it with a clear yet imaginative rendering of our sense of structure.

TELESCOPES AND DARK GLASSES

Starting out with a rather utopian notion of anthropology as an effort to increase transparency in the world, my optical imagery has become a little more complicated as I have moved on. The outsider's picture of insiders can become one sort of mirror for them; the picture which the traveler brings of a distant form of life can likewise be a sort of mirror for the audience at home. Somehow, transparency seems to be accomplished partly through the construction of a house of mirrors. Mobile anthropologists (and reporters like Fredrika Bremer) offer themselves as binoculars for publics with more sedentary habits. Observing the Nacirema, on the other hand, we turn the binoculars around.

Still, this discussion is admittedly incomplete, insofar as it has tended to dwell only on more-or-less conventional, place-bound research and its particular problematics. Not all anthropological reporting is necessarily from-here-to-there, or from-there-to-here; or for that matter from-here-to-here. In addition, that global state of being "tumbled into endless connection" means that some of the reporting must be, and now is, on that connectedness itself—the networks and overarching institutions stretching between here and there. There is also a related question of "the big picture": What, if anything, can a discipline mostly so committed to ethnography credibly say about larger wholes—even the world as it has been put together, and is forever being put together anew? Problems of scale must be confronted in experiments with the anthropological imagination. So perhaps we need telescopes as well.[36]

There are also other complications which we must not forget, shadows over utopia; or perhaps this time we look through dark glasses. If we hope that transparency can help build trust, presumably it had better be voluntary. Again, if recent European political history can serve as an example of some relative success, the idea is that the participating units, in this case countries, choose to be open to each other's inspection. But this is not always the case, and we know of the various instances, since the time of Franz Boas, when the uses of anthropological knowledge have become controversial. To begin with, at least, Ruth Benedict's *The Chrysanthemum and the Sword* was clearly an instance of adversary anthropology, in preparation for a hostile takeover. There are more complex circumstances when we do not foresee the uses to which audiences may put our reporting. Georges Condominas' *Nous avons mangé la forêt* (1954) and Raphael Patai's *The Arab Mind* (1973) are two very different

books, but they share the fate of having been recycled long after initial publication for purposes which the authors hardly intended—at the time of the Vietnam War in the former instance, and during the more recent Iraq war in the latter.

In this context, I find the career of Patai's book worth thinking about.[37] Raphael Patai, born in 1910, was a Hungarian Jew who studied Semitic languages in Budapest and Breslau (now Wrocław, in Poland), and left for the growing Jewish society in what was then the Mandate of Palestine in 1933. Arriving in Jerusalem, Patai found to his dismay that the Arabic he had learned in Europe was not of much local use—initially, at least, he could not make himself understood, and did not understand other people. He continued his studies in Jewish history and folklore at the fairly new Hebrew University of Jerusalem (he seems to have been the first recipient of a PhD from there), and meanwhile also became a self-made anthropologist. Moreover, he explored local Arab society, and formed a friendly relationship of intellectual exchange with one of its scholars in particular. But life in the *yishuv*, the Palestinian Jewish society, was uncertain, and Patai never succeeded in establishing a comfortable academic foothold there. So, after having cultivated contacts in American anthropology by mail in the mid 1940s, he left for the United States on a fellowship in 1947, just before Israel was established. He remained in America, but mostly in rather peripheral academic appointments, for the rest of his life (with occasional visits to Israel).

Yet Patai continued to write and publish extensively. It is somewhat ironic that, while much more of his active research effort was in Jewish studies, his wider reputation has mostly to do with his writings about the Arabs—especially *The Arab Mind*, which was first published in 1973. The author could hardly have foreseen it, but this was just in time for a period of rapidly growing American and European public, political, and indeed military interest in Middle Eastern affairs.

Notably, only few references in the book are to what were, by the time of its appearance, more recent anthropological publications about the Middle East. Patai does not really seem to have engaged much with current anthropology in the latter half of the twentieth century, and so it may not be entirely unfair to describe *The Arab Mind* as an unfortunate mixture of Orientalism with psycho-analytically inspired ideas from the early culture and personality school in anthropology, combined with largely secondhand political reportage. The style was accessible, however, and there

were numerous new editions. The book then entered the news, in 2004, as the investigative journalist Seymour Hersh broke the story of the American maltreatment of prisoners at the Abu Ghraib prison. Patai's book, according to one academic Hersh had talked to, was "the bible of the neocons on Arab behavior," and, as Hersh quoted the book, sex was "a prime mental preoccupation in the Arab world." The same source told Hersh that, in these political circles, two themes emerged in discussions: "one, that Arabs only understand force and two, that the biggest weakness of Arabs is shame and humiliation."

It is not easy to determine what role *The Arab Mind* really had in the practices of its policymaking and military readers. At least it can hardly have served as a torture manual. In any case, it continued to have its admirers. The most recent edition of *The Arab Mind* appeared in 2007, a little over 30 years after the first; had Patai lived (he died in 1996), he would by then have been 97. There was a new Foreword by Norvell De Atkine, who was enthusiastic in his praise: "Not only is it one of the finest books ever written on Arab culture, it is the only one in English that delves deeply into the culture, character, and personality of the Arab people." De Atkine, a retired colonel, had been a student at the American University of Beirut in the late 1960s, taught for 18 years at the Special Warfare School at Fort Bragg, North Carolina, and came to Iraq in 2003 with a Psychological Operations unit.

A couple of years after this last edition appeared, I looked for Patai's book on the website of the publishing house involved. I found items such as *How to Hug a Porcupine: Easy Ways to Love the Difficult People in Your Life*, and *The Complete Guide to Navy Seal Fitness*, and *What to Do When Your Therapist Isn't There*, and *My Fat Dog*. *The Arab Mind* was no longer there, but the list of titles suggests that this press had a leaning toward how-to books.

Can we learn anything from the fate of Patai's book? One would be that knowledge, or what passes for knowledge, can be a dangerous thing; sometimes even when it enters a western mind. We must continue to debate the uses of cultural transparency. Nevertheless, it also seems remarkable that the book, offering a sort of anthropology that an important group of lay readers somehow turned to at a critical point in history, was already a quarter-century old; in terms of its theoretical underpinnings, actually more like half a century old—an intellectual contemporary of Ruth Benedict's *The Chrysanthemum and the Sword*, linked to a much earlier war. Perhaps it was unfortunate that the military had not found their way

to anything more current to help them understand Arab life? It just might have been a good thing if more people with regional expertise had considered Gus—even if this particular Gus was a colonel.

Such circumstances as those of Patai's and Condominas' books also take us, however, to the still troubling image of the one-way screen, offering transparency of a very incomplete and partial kind. And here is another shadow over utopia. The ideal of building intelligibility in the world—indeed, an anthropology of world-building, rather than either nation-building or empire-building, as in Stocking's terms—does not seem to be fully realizable as long as opportunities for observation, reflection and reporting remain very unevenly distributed, and unevenly controlled. Anthropology is no longer entirely the west's study of the rest, the study of Others as if they were worms or crayfish. Even so, the center-periphery relationships of the world still exercise a major influence on who gets to travel, and under what circumstances, and this also affects that triangle of topic, reporter and audience in anthropology.

Just possibly, both non-western anthropologists and their home audiences feel that they already know too much about the West—or, in current usage, the North—so that, compared to what these scholars have to do at home, there is no urgent anthropology in this for them.[38] There may be a special part to play here, again, for those anthropologists of the diaspora, perhaps doubly rooted cosmopolitans, who can report in both directions. Mostly, so far, they have described their old home to audiences in their new home; Victor Uchendu, portraying transparency in Igbo society in a book easily accessible to American college students, was one of their forerunners. But they may well also report back to where they first came from.

Yet, in the early twenty-first century, we also sense ongoing shifts in the world, in power and prosperity. At least we can imagine that these could make a difference—in the future, will we in Sweden or Slovenia or South Dakota inspect ourselves in mirrors brought by anthropologists from Shanghai, Bangalore, Singapore, or Abu Dhabi?[39]

6
Flat World and the Tower of Babel: Linguistic Practices in a Global Discipline

You who are native English-speakers, living and working in a largely English-language environment, may want to skip this chapter—but then if you do not, you may find some food for thought about how the international scholarly community functions (or malfunctions), and some of you may get a sense of what you are missing.[1]

Here and there in these pages, I have found a point of departure, seriously or a little mockingly, among what I think of as the "global scenario writers"—those academics and journalists who have appeared especially since the end of the Cold War to pronounce on the present and future conditions of the world. In Chapter 3 I counterposed the political philosopher Fukuyama's notion of the "end of history" to the recurrent worry about an end of anthropology. In Chapter 5 I drew on the diplomat Cooper's notion of a "postmodern" world of transparency, which he found politically best represented by Europe as it had evolved after World War II. I have in fact cultivated a more systematic interest in these scenarios (partly as a consequence of my study of foreign news reporting), and in this chapter I will again find a starting point within this genre.[2]

Thomas Friedman, former foreign correspondent, currently *New York Times* international columnist, has shifted his emphases a little over time, but one of the more recent installments in his series of bestselling books, published in 2005 and subtitled "A Brief History of the Twenty-First Century," has the main title *The World is Flat*. For some time, of course, we have mostly believed that this is not so, but, relying on extensive reporting work in South Asia, Friedman comes to his new conclusion. It is actually a reformulation of a statement by one of his sources, a leader in the Indian information technology industry, in an interview in Bangalore: "Tom, the playing field is being leveled" (Friedman 2005: 7).[3] What Friedman and his interlocutor emphasize, obviously, is the global power of information technology—computers, software, e-mail, networks,

teleconferencing, and all that. But "what the flattening of the world means is that we are now connecting all the knowledge centers on the planet together in a single global network"; and, moreover, that it places people in that network "on a more equal footing than at any previous time in the history of the world" (ibid.: 8).

Well, yes and no. It would seem to matter a great deal that much of the reporting in *The World is Flat* is from India; for one of the great advantages of India, as it has made its great leap into a world held together by, and dependent on, information technology, has been that the national language of its modernity, English, is also the dominant world language. This matters to the engineers graduating from the high-powered Indian Institutes of Technology, and to the people whose courteous voices are now heard at the call centers, responding from places like Bangalore, to queries from everywhere.

LANGUAGE AND THE WORLD OF RANKING

What language, or languages, we use also plays a part in the academic world, and in anthropology's world. Thomas Friedman sees the world turning flat by means of a leveling of the playing field—and this is not just a metaphor of connectivity and collaboration, but also of competition. One of the ways competition, and estimates of competitiveness, is now expressed in the world, and in global academia, is in the form of ranking lists. Let me choose an example that is really "anthropology at home" in the strictest sense.

Some years ago, the rector of my university—a thoroughly modern rector (or vice-chancellor, or president: whichever title you prefer), and at the time still relatively new in his office—noted on his blog that his institution was ranked 93rd in the world in the international ranking of universities just published by the Shanghai Jiao Tong University. It was doing reasonably well, but not, he thought, well enough. Aware of how such rankings are produced, he suggested that his academic staff had better do more of its writing in English, rather than Swedish, so that its publications would count, and that it had better write journal articles, where citations would also count, rather than books, which tend to disappear in this kind of process.[4]

When I consulted the website of the Institute of Higher Education at the Shanghai Jiao Tong University (http://ed.sjtu.edu.cn), which produces the annual ranking list (for the ultimate purpose of assessing the gap between Chinese universities and world-class universities), I could see that its working group was aware of certain biases in its procedure. It noted that the scientific prizes to which it paid

attention were in medicine, chemistry, physics and mathematics, and that publication practices tended to favor medicine, while placing social sciences and humanities at a disadvantage. The results of such biases were evident in the list. I noted, for example, that the highest-ranking Swedish institution was purely a medical school, while the London School of Economics, an institution of undoubtedly high international esteem but specialized toward the social sciences, was not at the time among the first 100 entries. The Shanghai website also noted that, since the first ranking had been published two years earlier, it had had over 1 million visitors from all over the world—an average of some 2,000 visitors per day.

In this chapter I am concerned with language diversity and its part in anthropological work. Perhaps, with Thomas Friedman, we can, in some sense and to some degree, imagine the world becoming flat; but above that landscape there is still the contour of the Tower of Babel, that edifice in the Old Testament of the Hebrews, reaching toward heaven but so displeasing their god that he condemned them, and all of humanity, to linguistic diversity and mutual incomprehension forever after. I am struck again and again by the fact that the language diversity of which the Tower of Babel has become an enduring symbol remains a very effective obstacle to the global flow of knowledge and ideas. Generally, I would say that the commentary on social and cultural aspects of globalization has given less attention to this than one might have expected. The "global" tends rather to be contrasted to the forces of nationhood and nationalism, to the established political forms and boundaries, or something vaguely termed the "local." But languages fit into the global order in their own ways, which need to be scrutinized and debated.

"Anthropology's world," I pointed out in the Introduction to this book, can mean two things: it can be the entire world which anthropologists are engaged in studying, and it can be that smaller social world of the community of anthropologists and their immediate, everyday habitat. Through their ethnographic explorations of the wider world, anthropologists have become acquainted with complex multilingual situations, whether in the Amazon rainforest or in the highlands of New Guinea. Through research or through personal experience, they are also aware that, in the modern world, how language skills combine in individual repertoires depends on education as well as migration, travel and media use—aware, furthermore, that these skills may vary between generations. In sum, the road away from the Tower of Babel obviously involves

multilingualism, or at least bilingualism. But the language question is also one with its own significance for the way anthropologists relate to each other within their own global scholarly community, and for the way they relate to other audiences. Is the ivory tower also a Tower of Babel? Perhaps because the most active participants in debates over the contemporary politics of anthropology have largely been Anglophones, who can take for granted the use of a single language in most contexts, this question may have been a bit neglected. But now ranking practices, and anxieties, may affect the issue in new ways.

When I was first preparing what has become Chapter 3 in this book, it was for a presentation in a Jensen Memorial Lecture Series at the Johann-Wolfgang-Goethe Universität Frankfurt am Main, honoring the memory of Adolf Jensen, who had played a part in the revival of German anthropology after the Nazi era and World War II; so I wanted to reacquaint myself with Jensen's work. His book *Mythos und Kult bei Naturvölkern* had in fact been on my shelf since my student days in the early 1960s, admittedly in the English-language translation published by the University of Chicago Press (Jensen 1963). Some 40 years later, I was hardly less impressed than I had been at first by the way Jensen could refer to the work of not only German but also French, British, Italian, Dutch, Austrian, American, Danish, Finnish, and indeed some of my compatriot Swedish scholars, and probably others as well, in at least six languages.

I am not sure one can find such a list of references in any very recent anthropological publication, whether book or journal article. What I will dwell on here, connecting more realistically to present-day scholarly circumstances in the discipline, are the relationships between two or three, or possibly four, languages, or perhaps classes of languages, in the working life of an anthropologist. Some central materials to think with are in the analysis of the "world system of language" by Abram De Swaan (2001), the Dutch sociologist who has developed a comprehensive understanding of the global language situation. De Swaan notes that the system has different tiers. Again, English is obviously now *the* dominant world language, with a large number of native speakers, but, more importantly, much the largest number of people able to use it as their second language. He also points out, however, that there are a number of other languages that are more than just national languages. They have many native speakers, as well as secondary users. If English is hypercentral, these are supercentral. The languages De Swaan lists in

this category are Arabic, Chinese, French, German, Hindi, Japanese, Malay, Portuguese, Russian, Spanish and Swahili. In lower tiers are a great many national languages, with variously many native speakers, but rather few who use them to any significant extent as a second language. Scandinavian languages are, of course, among these. And it is hardly an accident that Dutch, Abram De Swaan's own language, is also here—his personal experience has no doubt sensitized him to the issues involved. Let us keep in mind at least the three tiers thus identified as we proceed.

With regard to my rector's request to the members of his academic staff, the underlying assumption would seem to have been that, if they had just switched languages, from Swedish to English, citations would increase, and this would improve the standing of his university. (Precisely how this better standing translates into more tangible assets for his employees may be a more complicated issue.) Yet we can be sure he realizes that things are not quite so simple.

I will leave aside here his suggestion that his academic staff should write articles rather than books. Different disciplines not only have different traditions in this regard, but really work on rather different logics; and this cannot be easily ignored. This matter of course also draws widespread comment in the academic world. Despite the proliferation of journals in recent decades, there are colleagues who may not be entirely happy to give up the dream of writing the Great Anthropological Monograph. Anthropologists are still very much a people of the book.[5]

But on the question of language, again: we do have a fairly good idea that some scholarship travels easily, and some is more context-bound, culture-bound, nation-bound, which tends to mean language-bound. Work in the natural sciences is often in the former category, together with much of economics and psychology. Work in the humanities and much of the social sciences, on the other hand, is more concerned with, or linked to, peculiarities of history, institutions, personalities and other matters anchored in particular areas of the world. Since most people are not entirely cosmopolitan in the distributions of their interests and curiosities, the audiences for writings on such topics tend also to be somewhat localized.

We may expect, then, that a Swedish political scientist who writes on the special characteristics of Swedish municipal elections will often assume that his or her audience will consist in large part of Swedish colleagues, and perhaps also of some politicians and others with a practical interest in the phenomenon. So he or she writes in Swedish. Yet, on the other hand, this scholar may have a notion of

an internationally dispersed group of researchers with an interest in the comparative study of local elections, and therefore an article or two (or, perish the thought, a monograph) may be produced in English. We may strongly suspect, however, that this is something more than a choice of language, a matter of simple translation. Choosing to reach for an audience knowing little about Sweden, having little intrinsic interest in the country, and possibly receptive rather to comparative, theoretical and methodological issues, may entail a quite different intellectual practice. In fact, whether he is entirely aware of it or not, this might be what the university rector is really proposing.

THE RESPONSE OF LINGUISTIC NATIONALISM

So far I may have made it seem as if the only major current pressure on academic practices is from the side of globalization: the neoliberal culture complex and its associated academic wing. If I have let my university rector personify some of this, he is of course not at all unique in his preferences; I am sure he has many counterparts, in various positions of authority and leadership, in many countries. Yet in Sweden, and probably also elsewhere, there are identifiable counter-pressures from what one may describe as the lobby of linguistic nationalism. What is at issue here is not merely which is the more promising writing language for good rankings in scholarly citation indices. Questions are raised, rather, about the more general expansion of English in higher education settings. There is some inclination (although I doubt that it is yet really strong) to make English the language of instruction, particularly at the graduate level, and occasionally the undergraduate level as well. The argument is partly that international exchanges of teachers and students simply make this necessary. There are apparently other instances, however, where English is used even in classrooms and seminar rooms where everybody present is a Swedish-speaker. The rationale then is that the students need to practice functioning in English, for the sake of their future careers in international contexts. While certainly there are numerous academic settings in the world where the academic language is not the language of everyday use— not least in postcolonial countries—in Sweden this is a new and controversial situation.

So let me introduce another actor here, whom I will allow to personify the Swedish-language lobby. While prominent enough, again he certainly does not stand alone, and I am sure his arguments

have their parallels elsewhere in the world—not least in Europe.[6] This is Dr. Horace Engdahl, until recently the permanent secretary of the Swedish Academy ("permanent" in this context usually meaning for a decade or so)—the body best known for awarding Nobel Prizes in literature, although it is also recognized as the official guardian of the Swedish language, publishing, for example, the official Swedish dictionary. Dr. Engdahl himself is a distinguished literary scholar and critic.[7]

While in office as academy secretary, Engdahl has made himself a spokesman for those who argue that Swedish must be a language for all purposes, situations and subjects.[8] In this context, such a stance should not be construed, I believe, as one hostile to immigrants and immigrant languages, but precisely as a reaction to the encroachment of English in central, high-status fields of activity such as business and scholarship. There is a danger here, so it is argued, of "domain loss" in the Swedish language. Personally, I sometimes get a feeling that such arguments draw on an view of a language as a living organism with rights and needs of its own, which is somehow maimed if it lacks words for something, and humiliated when foreign terms are mixed into it. More concretely, perhaps, from a user's perspective, they assert a right to be monolingual, to encapsulate oneself in a mother tongue and still be able to learn about everything. In its most fully developed form, the position of linguistic nationalism is that all fields of current knowledge must be accessible in Swedish. It would appear to be an underlying premise that, otherwise, the national culture will also be incomplete.

There is another aspect, however, to the advocacy of language nationalism: that of acceptable skill. Engdahl argues that one can really only become fully fluent in one's mother tongue, and that attempts to speak and write in other languages will in the end be imperfect approximations. Coming from him, in particular, I find this a surprising argument. I would have expected him, as the person most directly responsible for one of the major literary awards in the world, to recognize that a fair number of recent recipients of such awards (many from "the Global South") have received them for works in what was not their first language; the point is occasionally even made that some of their particular linguistic creativity has to do with their ability to combine linguistic frames. But it may be that linguistic nationalism is often not so well grounded in understandings of actually existing bilingualism or multilingualism.

In a less absolute sense, nevertheless, I can see that Engdahl has a point. Not all Swedish academics or students, or for that matter

all Swedish business-people, are going to achieve perfect fluency in English, and it may be, for example, that its use in teaching situations when it is not really necessary may be grueling, and even an obstacle to effective communication and learning. I would imagine, too, that, with his sensibilities as a writer and literary scholar, Engdahl's standards for the use of any language may be very demanding. But perhaps these standards are not actually needed in all academic contexts, and I suspect that what constitutes skilled communication differs considerably between disciplines. Does it not take less, in terms of the breadth and subtlety of purely linguistic skills, to produce a piece of scholarship, say in economics or psychology, than in history, comparative literature, philosophy, or anthropology?

QUESTIONS OF TRANSLATION

Engdahl's solution to the problem of international scholarly exchange is also one that I find rather unrealistic. He certainly accepts that scholarship must not fragment into a profusion of monolinguistic communities, but he argues for professional translation on a large scale. Let most people, even scholars, remain in the Tower of Babel, then, and leave linguistic bridge-building to specialists.

I doubt that this is a real possibility. There is the problem of skill. Certainly, there are professional translators who earn fame for their virtuosity; occasionally we hear the claim that their translations are really stylistic improvements on the originals. Most of the time, however, the translators available to needy scholars are not of this quality. Occasionally even quite ordinary phrasings are misunderstood, with remarkable results. I have a couple of illustrative examples from my own experience. The first comes from editing a book, in the early 1970s, on the external relationships of local communities ("the global and the local," to use a more recent vocabulary), made up mostly of Swedish translations from English-language texts. I never met the translator personally. He was hired by the reputable Swedish publishing company and was probably reasonably fluent in English, but without a particularly sophisticated and context-sensitive knowledge of the language. When, in his translation of a text about wild-rubber tapping among the Mundurucú Indians of the Amazon River region, I came across a mention of "the poor tax payers of the Amazon," I was puzzled, as I could not remember them from the original I had selected. When

I checked there, I found that the author had referred to "the lower tributaries of the Amazon."

My other example is from a more recent text of my own, originally written in Swedish, which a Scandinavian colleague had asked for permission to include in an edited English-language volume. He arranged for a translation to be made by a foreign student at his university, a native English-speaker, probably more competent in another Scandinavian language than in Swedish. In a section on diasporas, in order to avoid repetition, I had once chosen to use a near-synonym, meaning most nearly "dispersal" (although, parenthetically, I could add that the same word can also mean "embezzlement"). The Swedish word was *förskingring*. So the translator has to struggle a bit. *Försking-ring*? A *ring* is a "ring." *Försking*? Maybe *forskning*, "research." So diaspora became "research ring." I should add that neither the poor Amazonian tax payers nor the research ring actually made it into print, as I caught them in time.

It is also true that not all translators can handle the particular idioms and assumptions of all disciplines. Most of us can probably offer examples of insufficiencies, and sometimes ludicrous errors, in both written translations and conference oral translations, which occur when translators are not really familiar with the disciplines concerned.

The problem of professional translation of academic writings on a large scale, however, is unlikely to be only one of linguistic competence—there is very probably also a financial issue involved. When we look over the budgets allowed by such modest research grants as we may have, I suspect we would seldom see much chance of their covering the cost of really qualified translators. That our funding bodies might somehow come up with significant additional funding, earmarked for translation, does not seem particularly likely. Whatever funding there may be will probably come from sources that, in the final analysis, could otherwise have been available for funding actual research; and scholars who insist on writing in their mother tongue, and then require a translator to get their work into another language, thus become rather expensive colleagues.

So what can we do? There is probably no such thing as a perfect and realistic overall solution, only a variety of situationally relevant solutions or part-solutions, perhaps involving some risk. If a translation seems feasible at all, I believe we will mostly want to go for it. Over time, I have been happy to see various writings of mine, whether articles or books, translated into a number of

languages, several of which I do not understand at all—so I cannot myself judge the quality of the translations. I remember once having a spirited but initially rather puzzling exchange with a colleague from another country about some conceptual matter in one of my books, as translated into his national language—until I realized that the translator had been slightly too creative. If the foreign language in which we want our work to appear is one we feel reasonably at ease with (which, given the overall situation De Swaan describes, is likely to be English), we might do best to write directly in that language, and then perhaps have someone even more proficient check it for formal or stylistic errors. Or we may write the text in our native language first, and then produce a translation. Either way, I suspect that much of the time, the best we can do is still to try and muddle through, more or less on our own.

THE THIRD TASK

So much for one of the mundane realities of life in the international community of scholars. I will return in a moment to questions of translations, or the absence of translations, within the world order of languages. First let us consider at slightly greater length the premise that only work in the national language is really a full part of the national culture, and that this language must therefore be an all-purpose language. This argument has not been brought to bear, in debates, on the practices of anthropologists specifically; it is of a much more general nature. Even so, I think it may prompt us to raise the question of how well this discipline—with its particular publication practices and sense of its audiences—integrates itself into national cultures.

It so happens that, when my rector asks me and my colleagues at his university to write in English, this already fits fairly well with a certain anthropological logic. Unlike that political scientist writing on Swedish municipal elections, an anthropologist is in a discipline thoroughly accustomed to boundary-crossing. To the extent that the historical development of anthropology has been in a globalizing direction—so that, in principle, scholars from anywhere can do their research anywhere, and be interested in human life everywhere—there should be little reason to assume that most of the academic audience for an anthropologist's writing based on that research must be in his or her home country, or share his or her national language. If I write something based, say, on my field research in Nigeria, I might hope that it could be of some interest to

Nigerians, whose national language is English, and for that reason I might write in English—which might also be seen as a matter of courtesy. Yet it is at least as likely that I am writing for Nigerian*ist*s everywhere, possibly dispersed across the world, and thus I write in English because it is the language through which I am most likely to reach them.

If I follow my inclination to write for my colleagues in the international anthropological community (as in large part I have done), I may consequently gain my rector's approval rather as a byproduct. It is also a fact that I hardly risk losing the attention of any of my Swedish anthropologist colleagues, since I can assume that they also read English. Whether or not they read my work will mostly not depend on my language choice.

Just a little further out in the academic terrain, however, I already see a risk. My writings may face something which I would describe as a two-boundaries rule, and which I would hypothesize is prevalent in knowledge-acquisition in national academias. Starting out from their own discipline, practitioners are reasonably familiar with what goes on within it nationally; may extend their knowledge of that discipline to reach out to some degree internationally; and may also have a rough sense of what is what in neighboring disciplines, to the degree that they are readily at hand in the national culture. That is to say, you can cross a national boundary within the discipline, and you can cross a disciplinary boundary within the nation. As a rule, however, you do not cross two boundaries, between nations and between disciplines, at the same time. To the degree that anthropologists, through their publication habits—writing in a language other than the national language, and publishing through other than easily available national outlets—distance themselves from the national culture, they place themselves in a sense two boundaries away even from their academic compatriots.

The more important question may be what is the contribution of anthropology, thus internationally oriented, to the culture of ordinary lay people. One will mostly not run into our writings in local bookshops or libraries; and in any case the majority of people may not be particularly comfortable reading foreign-language books. Thus we come back to some of the concerns raised for example by the academy secretary, Dr. Engdahl, and also here and there in earlier chapters of this book. What kinds of audiences do we reach? And how does a tendency not to reach some audiences affect the public understanding of what anthropologists do?

You cannot be around in Swedish university life very long before hearing of *tredje uppgiften*, "the third task." This translates more or less as knowledge-based public service—the involvement with the wider society that is expected to accompany the first and second tasks, of research and teaching students. In principle, it tends to be spoken of highly; in practice, current assessment exercises in academic management, in Sweden as in other countries, have a rather strong tendency to ignore such efforts. Yet we may feel that they should be taken seriously, both for civic reasons and because, in the slightly longer perspective, it may help if people—citizens, taxpayers—know what we are up to.

There is of course that divide, which we have already discussed, between "anthropology at home" and anthropology abroad, in some traditions between *Volkskunde* and *Völkerkunde*. It may well be generally easier for those who do "anthropology at home"—to repeat, an imprecise notion—to reach their wider audience, a public which, again, may not consist solely of cosmopolitans, treasuring diversity, curious about the far-away. To the extent that they are specialists on things abroad, matters distant and alien, anthropologists may have knowledge for which there is really little demand, and which attracts only limited interest from the audiences nearest to them. It appears to me that, on the few occasions when an anthropologist has established him- or herself as more of a public intellectual on a national scene, this has tended to be in countries where they, and the majority of their colleagues, have practiced "anthropology at home."[9] Yet we may hope that there can be a demand—and that we can foster a demand—for understandings of the elsewheres of the world.

Then again, more or less regardless of what language situation we are in, we face issues of writing style. While anthropologists are inclined to be area specialists, there has also been a long-standing tendency to try, at least *within* the discipline, to identify more general problematics, and to seek a shared analytical vocabulary capable of transcending cultural boundaries. This is no doubt difficult, and there is always a lingering suspicion that this vocabulary and its underlying ideas are Eurocentric, after all. The attempt to find such a vocabulary, however, may also come at the cost of making professional anthropological communication, turning inward toward the discipline, rather inaccessible and unattractive to outside readerships. I touched on this toward the end of Chapter 5; writing in that way, we tend to lose Gus.

IN THE SHADOW OF THE TOWER

But let me now come back to the questions of language more specifically, to different arrangements of monolingualism and language diversity, and in the end to the issue of translation. In the shadow of the Tower of Babel, how does our handling of language divides affect the workings of the worldwide anthropological community?

In the kind of situation I have sketched, a Swedish anthropologist—in the field of tension personified by an eagerly globalizing, international-rank-seeking university head on the one side, and a guardian of the national-language treasure on the other—may perhaps logically pursue a strategy of active bilingualism, with a fairly clear division of labor between the national language (in the third tier of the linguistic world order) and the dominant world language. He or she will produce purely professional, intradisciplinary work in English, and whatever may be aimed at some wider public only at home in Swedish. I suspect that such a choice is frequently faced by scholars in that sort of situation—Scandinavian, Dutch, Polish, or whatever. For reasons of personal inclinations or priorities, certainly, individual scholars may aim more of their publishing efforts in one or the other of the two main directions, some of them writing more for colleagues abroad, and others more for less professional audiences at home.

In contrast, those scholars with a primary attachment to languages in what De Swaan identifies as the second tier of the linguistic world system may find themselves in a rather different situation. Obviously several of these languages—as far as I can tell, at least Chinese, Japanese, French, German, Portuguese, Russian, and Spanish—are also functioning academic languages, in anthropology as in other disciplines. (In the cases of Portuguese and Spanish, of course, large parts of the language communities are in Latin America.) Anthropologists who have one of these as a first language may be entirely comfortable using it in writing and publishing work clearly destined for readers within the discipline, as they can find quite large professional audiences at home, as well as some abroad.[10] The question whether they write one text or other for colleagues in their discipline, or for peers in the national academic community, or for a wider public, may, at least at times, not be so sharply defined, insofar as no active language selection is involved. They contribute to national culture, as it were, by default. Some of them may largely pursue "anthropology at home"—as I understand it,

the writers in Chinese, Russian, Portuguese (Brazilian anthropologists), and Spanish (other Latin American anthropologists)—but that need certainly not be true of those writing in French, German, and Japanese, who often do their field research abroad. Thus far at least, in certain instances, officialdom in their states may continue to insist that the languages involved have full international stature for academic purposes as well, and may even actively discourage writing and publishing in any other language. These writers have been less likely to face university rectors asking them to switch. Whether, in the long run, the lure of the ranking lists such as that of Shanghai Jiao Tong University will change this situation remains to be seen.

That may all seem like a rather comfortable situation to the scholars themselves, but the question is what it does to that idea of the flat world—"connecting all the knowledge centers on the planet together in a single global network." So we come back to the question of translation. My impression is that we have little organized and detailed knowledge of the redistribution of access to scholarly work through translation; but at the most general level it would seem safe to say that, compared to all that is said and written in the various languages of the world, translations remain scarce. Moreover, we can consider again the hypercentrality of English. Since so many people speak and understand English as a second language, one could logically draw the conclusion that translating *from* English for publication in other languages would be rather superfluous. That, however, is not at all how things work. According to one authoritative website devoted to translation issues, 50 percent of all the books in translation now published worldwide are translated *from* English, while only 6 percent are translated *into* English (www.wordswithoutborders.org). In combination with the distribution of active bilingualism, this global structure of translation activities means that, beyond English, the world as a space of intellectual activity remains remarkably opaque—not only to native Anglophones, but to all others as well.[11]

With regard to how things work out in the discipline of anthropology specifically, my own somewhat scattered impressions suggest that translation often depends on particular entrepreneurs and small circles of activists, with energy and a broad perspective, and connections to publishers and perhaps funding sources. But when it comes to what is translated from which language into which other language, I suspect that this field fits fairly well into the overall pattern. Just about everything of mine that has been linguistically mobile, as it were, has been translated from English,

rather than from Swedish (with one exception, referred to above). Rather little anthropological work is translated into Swedish from any other language, no doubt because the Swedish-language market is rather small, and made up substantially of people who can also read—and may often prefer to read—original texts in English, or even translations from other languages into English. I would expect such factors of language market size and the relative strength of second-language skills will recurrently shape decisions concerning translation and publishing in many national settings.

In some cases, a more specific determinant may be whether, in particular national markets, university students can be expected— even required—to read materials in any but the national language. If not, this may create a relatively safe market for translations. At the same time, however, one may discern that home languages can be zones of experimentation, where, to meet your particular needs or preferences in teaching, you put together materials in new or unorthodox ways. Again, I have a few examples from my own experience. The first is a volume I have already mentioned—the reader on how local communities relate to the world, from the 1970s. The book was indeed made up mostly of translations from the work of British and American anthropologists (with a rather lengthy Introduction of my own); but I do not believe that, as a whole, it had an English-language counterpart at the time; whether it had in any other language, I do not know. But it was a response to a perceived demand in a local teaching situation, at a time when undergraduate students were noticeably impatient with what they saw as an anthropological habit of looking at local communities in isolation.

My other examples involve more entirely local products, all edited multi-author volumes. One was a book on "media and cultures," growing out of my membership of a small national interdisciplinary research committee. In was triggered partly by my early field experience in Washington, when I had wondered about what to do with the strong media presence both in my field and in my data—there was hardly yet a recognized "media anthropology." The research committee project took some time, with various mishaps and delays; yet when the book appeared in 1990, it was still by most standards an early example of a strong input of anthropology into media studies, including chapters of ethnography from Kenya, Poland, and Sweden by compatriot colleagues. (As it has been quite widely used over an extended period, moreover, it might possibly be

my most successful intervention in "national culture," as narrowly conceived.) Another was a book mostly by Stockholm colleagues on multi-site field studies—that way of going about field research had its forerunners, and was just taking off on a larger scale, but probably this was the first entire volume on the topic anywhere. A final example was a book comparing anthropology and journalism, an editing project I undertook as an offshoot of my own study of the work of foreign correspondents, but inspired by my finding that a number of graduate students and younger colleagues in Swedish anthropology departments had some experience of their own as practicing journalists. Once more, it was a book intended for use in our own teaching curriculum, and it was taken on by a local textbook publisher, but I am not aware of exact counterparts in other languages.[12]

I should perhaps hasten to say that none of these various volumes simply had to be done in Swedish. Probably most required readings for Swedish anthropology students are in English. The students were there, however, if not as a captive audience, at least as one that was rather readily accessible; and what was written for them might spill over to find other readers, perhaps curious about what anthropologists—not least local anthropologists—were doing. I would expect that colleagues from elsewhere, in the second and third tiers of the world system of languages, could tell me of similar endeavors. Together, they would seem to contribute to a certain diversity among national anthropologies.

On the whole, I think we must conclude that the Tower of Babel still stands. The anthropological world is not yet very flat. What can we do about it?[13] One argument could be that we should all try to be not monolingual, not merely bilingual, but more multilingual—more polyglot. Some of us do achieve that, but I would not be very optimistic in general. It is true that many anthropologists, particularly those conducting their research in fields that are in no sense "at home," are at least trilingual—using a mother tongue, a field language, and English. Such skills may arrange themselves relative to each other in different ways, and of course it may be that the anthropologist's skills in the field language are not necessarily quite comparable to those in a language used for academic purposes. Yet the case of Adolf Jensen, referred to earlier in this chapter, probably has rather few counterparts in the twenty-first century discipline. Even if it would not require full, active linguistic competence at the levels of writing and speaking,

but only those of reading and understanding, I am inclined to doubt that a large number will manage this at a demanding academic level in many languages.

One could see all this as a matter of justice, or rather injustice. De l'Estoile (2008: 123), the French anthropologist, has playfully described his personal utopia, where in international meetings everybody would speak in a language other than his or her own. Appealing as this might be from the point of view of equality, perhaps it is not so practical. It may be a mixed pleasure to hear American or British colleagues trying out their French, German or Spanish while the rest of us probably speak English. (Moreover, I remember hearing anecdotes about expatriate anthropologists picking up rather rustic dialects in their fields, then showing up with them in academic seminars in the same countries, prompting rather amused reactions.)

Furthermore, perhaps one should not jump too quickly to conclusions about who benefits from the existing world linguistic order. Perhaps the most fortunate of our colleagues are those who are native speakers of a language in the second tier, if that happens also to be an academically well-endowed language, and who in addition have that rather readily acquired knowledge of English. A German or Japanese Africanist, say, is likely to have general access to writings on Africa in English, and then also privileged access to the substantial number of scholarly writings on that continent in his or her own language (which, incidentally, would be inaccessible to most Africans). In principle, the best-informed expertise on a region or some other kind of specialism would then be likely to be found among the readers of a language of that rank. On the other hand, unless the work of these scholars is somehow made available in the only fully functioning academic world language, which is English, they are not really pulling their weight in the global scholarly community, and will probably be somewhat painfully aware of the fact.

It might be, then, that if we are concerned primarily with expanding interactions among anthropologists in some particular region—East Asia, Latin America—we may be interested in the further use of some second-tier world language, and in stimulating translations between languages none of which is English. But if we aim at really building a global community of anthropologists, the path leading away from the Tower of Babel at present seems most realistically to entail, not doing more translations *from* English, but

somehow getting more of our work *into* English. That is not just a way of bowing to the demands of the heads of our universities, or to the technologies of measurement in the academic version of the neoliberal culture complex—or of further privileging those of our colleagues who happen to be Anglophone monolinguals. In the present circumstances, at least, it seems to be the best we can all do for each other.

7
Before and After:
Exploring the Usable Past

Max who? Marcel who? Melville who? (No, not that Marcel—his *The Gift* will probably still be remembered.)

Anthropology at present has an uneven, and largely rather forgetful, relationship to its past.[1] Opinions are divided, of course, on what disciplines should do with their histories. In the view of some, one should honor the accumulation of knowledge and insights through time, and venerate the ancestors—latecomers reach higher only by climbing on the shoulders of giants. Others argue that disciplines will only progress by learning to ignore the forerunners.

But what actually happens may not be entirely the result of a deliberate management of memory, but more a matter of circumstances—and these can vary between academic environments. Fairly long ago already, the American anthropologist Robert Murphy (1971: 17) observed that "the breadth and diversity of American anthropology, as opposed to British and Continental anthropology, are not so much a matter of intellectual influences as a function of the prevailing rules of tenure and modes of academic organization." He went on to point out that while in Europe (as prominently exemplified by Great Britain) there was *the* Professor who held *the* Chair, American university departments were less pyramidal in structure. Normal advancement in the latter would bring a greater proportion of the personnel to tenure rank and full professorships. There might even be "more chiefs than Indians." Departmental chairmanships brought no particular distinction, but would more likely be seen as bureaucratic nightmares, to be rotated every few years to new victims. In this system there would be no dominant figures, no respected arbiters or final authorities, more people doing their own thing. Meanwhile, in European academic life, according to Murphy, there was more hierarchy, more dependence on trickle-down patronage, more use of critical skills to enforce intellectual conformism; consequently, a pull toward the center.

Murphy's contrast did not incorporate much of a time dimension, but one may see it as implicit. Greater diversity is likely to result from more innovation, less preoccupation with precedents and tradition.

His was also an American view, but from the other side of the Atlantic a more recent but retrospective interpretation of British anthropology by Jonathan Spencer (2000) seems largely complementary—slightly more nuanced, in pointing to the spirited intellectual rivalry between senior figures in one important center of the discipline or other, but still emphatic in its understanding of the steady reproduction of disciplinary foci and boundaries. The key mechanism in maintaining this continuity and coherence through the classic period, Spencer observes, was the weekly departmental research seminar—"a setting for certain stylized kinds of performance, and for the passing of, often tacit, judgments" (Spencer 2000: 19).[2] Seminars play a large part in the collective memory of British (and British-trained) social anthropologists— at least of a somewhat older generation, which remembers the occasionally, or recurrently, outrageous conduct of some prominent participants. Yet perhaps one should not underestimate the role of the tutorial either, where it has existed and for as long as it has existed, as an academic institution for keeping ideas, assumptions and standards going through focused personal interaction between generations.

By now, in the early twenty-first century, the difference between American and European academic structures may not be as marked as in Murphy's sketch. British and other European universities have taken on more of the American type of departmental structure, with multiple full professorial positions, and individual promotions not entirely dependent on positions becoming vacant further up in the hierarchy, through retirements or deaths.[3] Perhaps not all British departmental seminars are what they once were, while something resembling them has become part of the scene elsewhere as well. Nonetheless, one difference may have become more noticeable. Since the late twentieth century, the American academic job market, in its upper regions at least, has indeed seemed more like a marketplace, with its particular logic of competing offers and geographical mobility, and of creating stars and fashions. There may already be less of that, to the extent that universities face a harsher economic climate. When scholars, whether junior or senior, have to offer themselves as desirable commodities, however, a presentation of oneself as standing modestly on the shoulders of giants may not seem like a good promotional strategy.[4]

FROM GENERATION TO GENERATION

Whether the past is kept alive or not—as an integral part of the present, or as something rejected and willfully ignored—can also depend on more particular conjunctures. Historical breaks may come about as old and established modes of thought become exhausted, or as new and strong academic generations enter the scene. Such breaks may be largely local, or they may occur on a wider scale.[5]

Take as an example the reminiscences of George Marcus (2008: 10), one of the prominent innovators in late-twentieth-century anthropology, about his own graduate student days, at Harvard University in the early 1970s. Although it seemed to him that anthropology had exciting potential, he thought the teaching of it at the time was stale. So there had formed, not least among anthropology students (although it had counterparts elsewhere on campus), an "invisible college" where other readings were discussed: Foucault, Barthes, Habermas, Althusser, and "the literary turn." These sources of inspiration from outside the discipline left a mark on the new anthropology that gathered strength and became a focus of attention in the 1980s.

Take as another, and earlier, example from American anthropology the changing scene at Columbia University around mid-century— drawing on a recent brief history by William Peace (2008). It was the time of the arrival, quite widely across American colleges and universities, of the World War II veterans, coming to the university on the "G.I. Bill" allowing ex-soldiers to further their education. At Columbia, this generation included, among others, Eric Wolf and Sidney Mintz. Columbia had long been perhaps *the* major center of the discipline in the country; but at the time, notes Peace, it was still "reeling from the death of Franz Boas." The type of softly humanistic anthropology exemplified in the department by Ruth Benedict, a rather mediocre teacher, had little appeal for these worldly, hardnosed, often radical, not-so-young students, mostly inclined toward materialism and evolutionary thinking, and issues of class and power. The perspective of Julian Steward, newly hired faculty member, was a great deal more to their liking (although he was apparently not altogether at ease with them). In any case, they also formed their own, somewhat subversive and apparently rather macho, informal reading group: the Mundial Upheaval Society (there was perhaps some sense of irony even in that generation). Peace, their historian, does not say very much about their outside

sources of intellectual inspiration, but it is clear that they read a certain amount of history and archaeology that had an affinity with their own views. It is likewise clear that they engaged in a quite systematic campaign to expand their influence in the discipline.

The G.I. Bill generation was probably large enough, and experienced and self-confident enough, to leave its own mark in many sites in academia. (In another part of the latter, Clifford Geertz was also one of those who found his way to a scholarly life through the G.I. Bill.) Marcus' generation, at Harvard and elsewhere, would seem to have been more or less the "baby boomers" (to the perhaps slightly earlier Swedish counterpart of which I happen to belong myself). Large in numbers and perhaps collectively self-confident, it, too, was capable of redefining the situation, deciding for itself what was worth reading and what was not, and assembling its own intellectual world. I suspect, moreover, that if one scratched the sometimes suntanned surfaces of some members of this generation of anthropology graduate students in the United States, one would also find returned Peace Corps volunteers, bringing their own experiences of the world out there. And although the Peace Corps was an American invention, it soon had its counterparts elsewhere, with related implications for the recruitment to a growing anthropology.[6]

Altogether, I have a sense that the picture of continuity and change in American anthropology is really rather more blurred than the imagery of ruptures suggests. Individuals and institutions move at varied speeds. Even an old and established department—Columbia at one time, Harvard at another—might go through some major shift, but then, as the saying has had it, the question is whether it will play in Peoria. In the sizeable American department where I taught for a year in the early 1970s, it did not seem that the preferred readings of the graduate students were the same as those to which their Harvard contemporaries were making their way. Tutoring, too, can tend to be a conservative force—a sanctuary from chasing the news; not only in Great Britain, but even in America, and elsewhere in anthropology's world as well. Occasionally one comes across situations where a faculty member, at a late career stage, perhaps having received tenure (or whatever may be its nearest local equivalent) in a rather distant past and not having updated his or her view of the discipline very much thereafter, still takes on the task of advising new, fresh, and not so independent-minded graduate students—who may then innocently be inducted into an anthropology which passed its expiration date rather long ago.[7]

But even if Columbia University around 1950, or Harvard University some 20 years later, were not all there was of anthropology, or even of American anthropology, in these periods they seem to have exemplified a tendency of strong generations to distance themselves from existing scholarly universes that may seem to be yielding diminishing returns, and to offer little scope for their own ambitions. Insofar as moving away could at the same time entail confrontations, such shifts may not always be harmonious (perhaps they seldom are). Westbrook (2008: 81), a friendly observer of at least one recent variety of American anthropology (Marcus'), suggests that the 1980s discussions between age sets moved, not dispassionately, from "certain intellectual problems in cultural anthropology" by way of "a critique of the work of the preceding generation of anthropologists" to "trashing my life's work."

In the African village communities of the past, we have been told, if schisms between lineages became too deep, if there were too many witchcraft accusations, a dissatisfied group could choose to march off into the bush and clear new ground to cultivate. The equivalent in academia could be to start a new department. During the extended period of expansion of the university industry in the latter half of the twentieth century, the chances to get rid of what seemed like an undesirable past and celebrate intellectual rupture could sometimes indeed be increased if there were even new universities to colonize, or at least existing campuses where new departments could be set up, or more or less defunct old ones refashioned. In Chapter 2, apropos of editing, I referred to the fact that there are scholarly journals which are more like department stores, and others which are more like boutiques—and that the same can be true of university departments. To make a fresh start, unburdened by too much history, would seem much easier if you can decide to have a boutique department. Yet it may be a risky strategy. Department stores—the real thing—may by now seem like rather old-fashioned institutions, and the occasional demise of such temples of consumption usually draws much attention. But the average life-span of boutiques is really much shorter, and in the scholarly marketplace boutique departments may also show a curve of more rapid rise and decline. I was once, briefly, a visitor in a department that had its own take on the future of anthropology, and which headed full speed in that direction, into a blind alley (with some faculty members admittedly shaking their heads, if rather discreetly).

TOWARD A LIVELIER PAST

Among the less credible reasons for a neglect of earlier anthropology that I have come across is that it was all "before globalization"—with which the world has changed so much that the old stuff has all become irrelevant. That is a bad argument in at least two ways. First of all, even if the term "globalization" is mostly fairly recent, and the forms of world interconnectedness have changed in important ways, various contacts over continents and across oceans are not a new thing. But a more important reason why anthropologists have so often approached globalization through the formula of "the global and the local" is precisely that "the local" often turns out to be quite resilient, even as it changes, and that a grasp of its earlier forms is thus very valuable in understanding the present. Personally, I have certainly devoted much attention to the current and emerging forms of global connections, and that has in itself been one source of my interest in the writings of past generations of anthropologists.

There are other present-day facts of life, however, that affect practical access to the written word even of not so long ago. Bookstores are concerned with turnover, and do not much care for slow-moving titles from the past—even quite new titles have a short shelf-life. As library use becomes increasingly electronic, early journal volumes that have not been retroactively digitalized will not be sought out in those increasingly unfamiliar, forgotten library stacks, and many books will perhaps never be digitalized at all. It seems too early to tell what effect Amazon.com and Google can have in the long run on the temporality of academic texts. As it is, it seems difficult even for new and notable work to establish durability beyond those fifteen minutes of fame—to have any chance of establishing itself as part of a new canon.

But, again, whether it has to do with the force of technical and organizational circumstances or more intentional generational changes of direction, whether it is due to more or less benign neglect or to a more active preoccupation with badmouthing the ancestors, anthropologists mostly do not now engage too seriously with what went before them in their scholarly field. This may go with the rhetoric of crisis noted in Chapter 3—if one detects an end of anthropology as we have known it, to save the discipline there must be a radical renewal. Judging from well-known book titles, this has become a genre in itself: from the previous century into this one, *Rethinking Anthropology*, *Reinventing Anthropology*,

Recapturing Anthropology. Perhaps such reforming goes on in all disciplines—I know of at least one *Rethinking Sociology*, and it would not be so difficult to imagine a *Recapturing Economics* or a *Reinventing Geography*. Yet the before-and-after imagery seems to be recurrent from the beginnings of anthropology. Before Boas? Before Malinowski?

The history of anthropology has mostly become another academic specialism, with its stable set of contributors and its defined readership segment among the anthropological community's own history buffs. Yet occasionally a voice is heard from a senior generation, from someone who was there before the most recently proclaimed divide, and does not think the past was ever quite as it is said to have been. The trenchant critique of "the misrepresentation of anthropology" (especially American anthropology, by American anthropologists, and sometimes non-anthropologists) by Herbert Lewis (1999) is a notable example. Lewis, who took his first anthropology courses in the 1950s, argues that claims that earlier anthropology treated the peoples it studied as "radically alter," that it was ahistorical, and that it treated each culture as an isolated unit unconnected to any other, are all false, and he offers numerous examples to show this. And he is annoyed when some currently celebrated outside star is invited to an anthropology meeting, talks about earlier anthropology in a way that shows how little he knows about it, and is not challenged by anybody in the audience.

You may also come across a handful of somewhat younger colleagues who announce an intellectual identity as "neo-Boasians," and take pleasure in rediscovering shared concerns with forerunners of a century or so earlier (Bashkow et al. 2004). They find the intersection where Franz Boas meets Michel Foucault; they discover that boundaries, to Boasians, were quite open; and one of them reconsiders Ruth Benedict as a practice theorist. So they take Boas himself, and the first and second generation of Boasians, actually to be fellow travelers and good company in their own anthropological rethinking.

My perspective on anthropology's past world is something like that. Perhaps identifying with the Boasians is in one way safe: they are sufficiently far away in the past so that the step toward them cannot be understood as merely standing still, or following too closely behind the nearest senior generation.[8] But one could explore other parts, other nooks and crannies, of twentieth-century anthropology as well, and see what one finds that might be of current

interest. After "rethinking," "reinventing," "recapturing," perhaps there is room for a bit of remembering, retrieving, even recycling?

Again, this is not just a matter of venerating the past, or of looking for skeletons in anthropology's closets—although at times the examination of skeletons may be instructive. Perhaps my own rather insouciant stance here has something to do with the fact that Scandinavian anthropologists—of my generation, at least—were not very burdened by any local tradition actively struggling for its own reproduction. The handful of elderly museum curators who were in charge of teaching did not really insist on inculcating their kind of knowledge, and we hardly read any of their own limited writings. Our more real scholarly ancestors were those we adopted, from far away in the foreign centers of the discipline—mostly without their knowledge, or that of their more immediate heirs. So, for some time at least, we were largely free to take what we wanted, and do with it what we pleased.

That situation could of course result in a somewhat anxious provincial conformity—"Did we get that right?" But it also allowed some rather more spontaneous exploration of what was, and what had been, in anthropology's world, where the shaping of a livelier past also contributed to a richer present. The past became an intriguing intellectual resource, but it was also itself a largely unknown land. That, at least, was a view some of us arrived at, at the time rather unintentionally and unreflectively. It is, however, still one that I think has something to recommend it. So it is with that point of departure that I suggest that we should, once in a while, take time out and go browsing in that materialization of the past, the library stacks.

OBSERVING THE JAPANESE AND THE ENGLISH

The old-timers we run into, and sometimes seek out there, in the pages of books and journal volumes, may at times be familiar or at least vaguely recognizable as heroes and heroines, or as villains and fools—although it might turn out that such typifications are to a degree caricatures. Some would have done well in the citation indices of the times, had there been any; others wrote little, and were seldom or never referred to.

Browsing is by nature serendipitous, but is not without some underlying selective principles. The past that I will look out for here has less to do with specific ethnography, and does not concern more general theoretical orientations either—the latter may often

be mostly of more historical, not to say antiquarian, interest. I will attend, rather, to a past that I think may be useful, worth thinking with as we go about doing anthropology into the twenty-first century: how we do it, with whom we do it, when and where we do it, and for whom we do it. To touch on several of these issues, let me introduce Kon Wajiro.

Wajiro who? In Japanese, the surname comes first, as it were, and Kon Wajiro was Japanese. He also published in Japanese, and I strongly suspect that, to most of my non-Japanese readers, he is as unknown as, until recently, he was to me. I still have not read him in the original, or even in translation; so for my understanding of his work, I depend on a British anthropologist, Tom Gill (1996), who knows the Japanese scene well.[9] But then there is a case to be made, in the words of Richard Fox and Andre Gingrich (2002: 8), for "overcoming retrospective provincialism—that is, by introducing valuable elements from divergent national anthropologies and by expediting an international conversation for an anthropology 'of the present'." Welcome, then, to 1920s Tokyo. Kon was a somewhat bohemian professor of architecture at Waseda University, one of Tokyo's best-known. At the same time, from early on, he was an amateur follower of Kunio Yanagita, the pioneer of the Japanese discipline most comparable to European *Volkskunde*, and of an "anthropology at home" committed to the superiority of insider cultural knowledge. That line of scholarship, however, had mostly been committed to collecting the traditions of remote villages. For Kon, the transformative moment came with the great Tokyo earthquake of 1923, and the major fires that followed. As an architect, Kon was fascinated by the improvised shelters that city people were creating for themselves, and started documenting them. From then his curiosity and range of interests grew and grew. Japanese life was in a period of major change. Kon invented a new discipline for his activities—*kogengaku*, meaning "archaeology of the present"—but eventually preferred the label *modernologio*, Esperanto for "modernology."

With a rather mixed crew of collaborators—artists, journalists, sociologists, soldiers and students, apparently numbering in the dozens—Kon engaged in more or less ethnographic surveys, in the fashionable quarters of the Ginza as well as in neighborhoods of squalid poverty. If housing remained central to his concerns, clothing styles (shifting between Japanese and western), consumption patterns and the human traffic of the streets also drew his attention. He was commissioned to extend his research into other parts of

Japan as well—and these could include Korea and Taiwan, at the time under Japanese control. A trip to America and Europe, including a four-month stay in Paris, also allowed him to take a comparative view of such things as hair styles and skirt lengths. By the 1930s, however, modernology was in decline, perhaps partly due to changes in the political climate that did not encourage this sort of rather open-ended inquiry into styles and conditions of life. The last major effort was perhaps a very large-scale survey, supported by a women's magazine, of women's clothing, hair styles and makeup, taking place on May 1, 1937, in 19 Japanese cities—some 25,000 women were observed.

Japan has a long tradition in anthropology, rather like that of the heartlands of occidental anthropology, and like it, partly with colonialist origins, and devoted to a study of the culturally alien. Kon's intellectual linkages were not so much with that, but mostly with the more nation-building, "at home" variety of Kunio Yanagita. Curiously, however, as he developed his own research practice, he turned this mentor's perspective inside-out. He was, Gill comments (1996: 199), an inveterate collector, seeking out "nuggets of sociocultural information the way a magpie collects small, shiny objects." But the style of field work that went with his "archaeology of the present"—mostly observing people discreetly, without talking to them—did not really reach for anything like a native's point of view. If one American ethnographer of the 1950s playfully turned the ordinary bathroom behavior of his compatriots into an exotic ritual of the Nacirema (see Chapter 5), aiming at insight through distancing, Kon was perhaps doing something like an ethnography of the Esenapaj several decades earlier. Perhaps the concentration on the material and the observable could draw on an assumption that this was more objective stuff than whatever people might think or say; though it seems Kon was also simply a rather shy man.

Kon Wajiro was not much of a theorist, so whatever principles may have guided his collecting remained largely fuzzy. One could see him, however, as one of the world pioneers of urban ethnography: a contemporary of those Chicago sociologists—Robert Park, Louis Wirth and others—who were on the streets of another rapidly changing city, and who have earned greater scholarly—and to some extent interdisciplinary—recognition.[10] Gill, for his part, emphasizes that Kon was also a contemporary of Malinowski. But, while Malinowski celebrated participant observation, Kon did non-participant observation; while Malinowski taught the importance of extended field work, to add your part to one of Kon's surveys

"all you need is a pen, the back of an envelope, and half an hour to spare"; Malinowski's field work was typically solitary, Kon worked with teams. Kon, it seems, was the un-Malinowski.

When I learn of Kon Wajiro's field techniques, his emphasis on observation, and his use of a team of instructed but not entirely professional field reporters, I am reminded of another enterprise which made its appearance a decade or so later, likewise at the outskirts of professional anthropology, in Great Britain. "Mass-Observation" was the organizational invention of a team of three young intellectuals on the political left:[11] Charles Madge was a poet and journalist who would eventually become a sociology professor; Humphrey Jennings was a painter and film-maker of surrealist leanings; but the one who may over time have been the most closely identified with their undertaking was Tom Harrisson. He was somewhere between a (self-styled) anthropologist and an ornithologist, with field experience in the New Hebrides (now Vanuatu) and Borneo, to which he would later return. The three were rather mismatched, and seemed over time to disagree more often than they agreed. What they, and their collaborators and supporters, had in common was a desire to find out how ordinary British people—basically the working class—actually lived, and to share their insights with these compatriots. As they saw it, the kind of people who were in power in the nation did not know, and did not much care; and in any case their cultural institutions could be expected to continue to propagate an official version of the life, values and beliefs of the nation.

In opposition to such hegemony, Mass-Observation was conceived as something less like a research institution and more like a movement. Its methods were quite eclectic. It sent out questionnaires and teams of observers, and Harrisson did his own more comprehensive observational work in Bolton, a town deemed representative of the industrial belt of Northern England. Indeed, with his ornithologist past, Harrisson believed in observing rather than interviewing—"approaching the study of Britons rather as if they were birds," as he would later put it (Harrisson 1978: 13). The similarity with Kon is notable, even if the latter was portrayed not as an ornithologist but as a magpie.

But, most significantly, participants were recruited among lay people to report on their observations. Hundreds of them (apparently including many teachers, clerks and shop assistants—more or less lower-middle-class people who enjoyed this sort of

somewhat self-educational pastime rather more than the actual working class) sent in regular accounts, in a diary-like form, of what went on in their surroundings. The first major result of their efforts was a book named *May the Twelfth*—and this, in 1937, was no ordinary day. It was the coronation day of George VI, who had reluctantly ascended to the British throne, after his brother, Edward VIII, when he had to choose, preferred marrying his woman friend, the American divorcee, rather than continuing kinging. Much of the reporting naturally focused on the ceremony itself, and the parade through London streets; and there was some communication from far away, about activities in Britain's colonies. Yet the minutiae of private celebration and interpersonal commentary were also there to be glimpsed in the wide-ranging coverage:

> My mother also told us that on the bus coming back to Harborne there was a girl about 17 years old who had been backwards and forwards to town three times because she was too drunk to get off. It seemed that this time two well-dressed men took charge of her and said they would take her to their home in H— Rd, give her some tea and then take her home.

And a somewhat facetious report from what is apparently academic Cambridge, its ears turned to the radio:

> Morning. Listened in to part of commentary on ceremony in common room with 9 people (4 conservatives, 3 liberals, 1 fascist, 1 fabian). General reaction: embarrassed grins, and outright laughter when the commentator was outstandingly loyal. Fascist stood for National Anthem. Conservative remarked "bloody fool!" It was generally agreed that the Coronation was a good thing because it improved trade, gave ruling class prestige and broke down class barriers.

It is an intriguing coincidence that this major first effort of Mass-Observation, on May 12, 1937, occurred less than two weeks after Kon Wajiro's last large-scale survey of Japanese women's appearances, in Tokyo and other cities, on May 1.

Most of the time, however, Mass-Observation attended to more or less everyday life, and its institutions. One of its other more acclaimed publications was a book on *The Pub and the People*. Then came World War II, and with it a concentration on describing

how people coped with its hardships and nuisances—not least during the blitz, the 1940–41 German air strikes. There was some potential for controversy here, as Mass-Observation might turn into a government tool for surveillance of the home front. And then, after this high point had passed, Mass-Observation shifted form into a market-research outfit, and not much later basically vanished from the scene. By then, all the founders had continued on to other commitments and enthusiasms. (Toward the end of the war Harrisson was parachuted into Borneo, where he would lead a local guerrilla force against the Japanese, and then stay on as a museum director in a distant part of the empire.)

Looking back, one may have mixed feelings about Mass-Observation, its wide-ranging materials, and its publications. Without any coherent theoretical point of view to guide collections, much of the reporting, perhaps like Kon Wajiro's, may now seem mostly curious, or merely trivial. Yet there are intriguing topics and formulations, and there could be gems still to be discovered—since the 1970s, the archives of Mass-Observation have been at the University of Sussex. One may sense that, in their radical political sympathies and their ethnographic focus on working-class lives, the inventors of Mass-Observation show a certain affinity to another, later development on the British scene: the "cultural studies" emerging in the 1960s and 1970s—this, too, had some of the characteristics of a movement. But the cultural studies complex was much more clearly academically based, and had stronger theoretical interests.

Where, however, were the anthropologists when Mass-Observation was born, and flowered? In one of the first statements, published early in 1937 in an important journal of opinion and debate, the *New Statesman and Nation*, the initiative was indeed presented under the rubric "anthropology at home." In that spirit, Malinowski wrote a fairly favorable Foreword to one early Mass-Observation book; but otherwise academic anthropologists evidently preferred to have nothing to do with it.[12] As MacClancy (1995) has pointed out, at the time they were still in a fairly early stage of defining the professional nature of their work, emphasizing the more sophisticated quality of their data-gathering and analysis, just as Mass-Observation was promoting a sort of amateurism in which just about everybody could be an ethnographer. So their interests did not exactly coincide, and Mass-Observation just barely makes it into the history of anthropology.

MAX, MARCEL, MELVILLE & CO.

With the move from Kon Wajiro to Mass-Observation, however, we have already moved from east to west, from the center of one island imperium to another. From here on, the glimpses of the past are from the old heartlands of Occidental anthropology: France, Britain, and (particularly) the United States. As I pointed out in the Introduction, anthropology, in many other places, is a latecomer on the local academic scene, so there simply may not be all that much to remember.[13]

So: Max who? Marcel who? Melville who? Max, Marcel and Melville all happen to be men; but then, if I were to stick with that first-name initial, the Margaret and the Mary I first come to think of are probably still too well-remembered for any Margaret who? (Mead) or Mary who? (Douglas) So let us stick to these three—and touch briefly on a couple of their contemporaries with other first initials. Those I have in mind are certainly not people who have entirely faded away to join the ranks of the most completely forgotten—Max Gluckman, Marcel Griaule (not Mauss) and Melville Herskovits are undoubtedly still understood to have been prominent mid-twentieth-century anthropologists, in the British, French and American research traditions, respectively, of the disciplinary center. They, or the groups they represented, are now receiving their scholarly biographies.[14] But are they now mostly of interest to those members of later generations who are historically inclined, and just barely identifiable for those who are not?[15] Or do their anthropological practices include anything that is also more directly relevant to current debates or endeavors in the discipline? In what follows I will only point to some of the circumstances and activities that could still make us curious about them, and make us think about how what they did could relate to what we do, or do not do, today.

In the same way as Boas goes with the Kwakiutl, or Evans-Pitchard with the Nuer or the Azande, Griaule is linked to the Dogon people, in what is now Mali. He returned to the Dogon again and again, and this extended engagement with one field is in itself remarkable, although not unique. What can be the particular results of such a long-term commitment? In what ways, if any, does the passage of time show up in the ethnographic reporting? Griaule, however, was not the only Dogon ethnographer. The study of this people was a joint effort with several French colleagues, so the possibility of a larger collaborative ethnographic enterprise, as

distinct from the usual lone-wolf field study, is also exemplified here. Moreover, the style of Dogon ethnography has to an extent, and for some time, come to stand as particularly characteristic of a French anthropological tradition—once leading Mary Douglas (1967) to ask what would have been the result if the French anthropologists had taken on the Nuer, and the British the Dogon …

But then Marcel Griaule's name has become associated not just with the Dogon generally, but with one identifiable individual in particular: Ogotemmêli, one of anthropology's most famous field informants. Together, Griaule and the blind, elderly hunter developed an astonishingly detailed, sophisticated account of the Dogon view of the world and the cosmos (Griaule 1965).

Yet whose view was it really—that of the Dogon, or that of Griaule and Ogotemmêli making culture together? The Dutch anthropologist Walter van Beek (1991), coming to the Dogon long after Griaule, could find little of the sort of knowledge and philosophical viewpoint his predecessor had reported, and also found much of that reporting inconsistent with other Dogon ethnography from the Griaule team. So this raised, particularly dramatically, the recurrent question about the nature of ethnographic work, and especially the dynamic of the relationship with key informants: To what extent and in what way do the results reflect the viewpoint and the peculiarities of the field worker? What part did Griaule's forceful personality, and the power equation of the colonial context, play in his interaction with Ogotemmêli? As van Beek's critical scrutiny was published in the journal *Current Anthropology*—with its customary format of invited comments by scholarly peers—a variety of responses were offered. Some were more critical of van Beek than of Griaule. A couple saw parallels with the well-known attack on Margaret Mead's work, similarly occurring after its author's death. One well-known Parisian anthropologist, also an Africanist but of a later generation, argued that the article raised the question of whether French anthropology had been sufficiently capable of self-criticism.

Our second M—Max Gluckman, also an Africanist—was certainly prominent enough in his own right, as an ethnographer, a theorist, and not least a pioneer in the anthropology of law (he had legal training before he turned to anthropology.) Nevertheless, he also stands as the leader of the "Manchester School" in social anthropology, for a quarter-century or so a remarkably cohesive and successful enterprise, and it is more that school's collective contribution to the discipline than Gluckman's individual

achievement that I will focus on here.[16] One major feature of the work of the Manchester School was that, for a long period, it had two closely interconnected bases—in its British university department, and at the Rhodes-Livingstone Institute, in what was still colonial Central Africa, through which much of its field research was organized.

When Gluckman had served in an early period as director of the Rhodes-Livingstone Institute, he had set forth a comprehensive research plan—"the first plan of the kind in the British Empire," he claimed. Not everything in it would be realized, but it pointed in important directions. While there would be a number of somewhat more conventional ethnographies of rural tribal communities, the region in which the Institute worked also included the new mining communities of the Copperbelt, and so its researchers came to play a pioneering part in developing urban anthropology, and in finding methodological and conceptual tools for it.

The Manchester School had perhaps the most versatile understanding of social form displayed anywhere in British anthropology, and a developed interest in social conflict. It experimented early—sometimes successfully, occasionally perhaps less so—with modes of ethnographic exposition. Case studies, including "extended case studies," were perhaps inspired by Gluckman's legal experience, as well as by his colleague Clyde Mitchell's background in social work. Gluckman's early "situational analysis" of the opening of a new bridge in Zululand was followed by Mitchell's study of the Kalela dance, a spectacular choreographic form revealing new alignments and identifications in the Copperbelt's urban centers.[17] These were developments in the use of concrete ethnography which opened up the temporal and spatial scope of anthropological analysis in new ways. And one can sense their affinity with the interest in "social drama" that another anthropologist with Mancunian roots, Victor Turner, continued to develop in other contexts. Moreover, the Manchester School could produce an ethnographer-informant relationship which has perhaps become almost as famous as Griaule's with Ogotemmêli: Turner's with the Ndembu medicine man Muchona, to which I have already referred in previous chapters.

Last but not least, one should consider the part the anthropologists at Manchester played in refining the idea of network from a simple, rather casually used metaphor, into a more elaborate concept, or family of concepts. Although it occurred first in a study of British families and in a journal article on rural Norway, it was also brought

to Central Africa for further development. For some scholars, in sociology at least as much as in anthropology, it then became a highly technical, quantitatively oriented research specialism; for others (including myself), network notions have remained useful to think with in a more exploratory fashion.[18] In any case, as we come across them in academic tomes from varied disciplines, as well as in popular noun and verb versions ("networking"), we may remember that Manchester social anthropology once did much to place them in circulation.

M number 3: Melville Herskovits may now be referred to within anthropology, as often as not, for his critical, cultural relativist view of the Universal Declaration of Human Rights, which in the late 1940s was one of the products of that new world organization, the United Nations. By now his stance on this issue may seem somewhat quixotic and embarrassing, although it was grounded in a suspicion of western cultural hegemony which was at the time not entirely unreasonable.[19] But he left his mark on anthropology in more ways. In Chapter 4 I mentioned his book *The Myth of the Negro Past* (1941) as a significant early instance of public anthropology. Earlier yet, he was one of the co-authors of a brief "memorandum on the study of acculturation," which was a central document in legitimizing and outlining the study of situations and processes of cultural contact as a research field, at a time when many anthropologists in the United States were still preoccupied with what was basically salvage ethnography with American Indian populations (Redfield, Linton and Herskovits 1936). For a quarter-century or so, from the 1930s to the 1950s, research on "acculturation" can be identified as one of the core interests in American anthropology, at least until the term itself began its vanishing act (though reappearing in some other disciplines). Certainly, when globalization became a keyword several decades later, the world and the social and technological underpinnings of its cultural processes were sufficiently different so that a straight revival of old formulations was hardly an option; but if acculturation studies are now hardly a recognized part of the scholarly genealogy of globalization studies, that may again be an example of more recent academic amnesia. In her 1990s inaugural lecture as incoming professor at Cambridge, Marilyn Strathern (1995: 24) noted that—with material culture and technology back on the reading lists, and with a renewed interest in diffusion emerging from debates over globalization—it could seem as if anthropology at the end of the century was more like that of the beginning of that century than the approximately mid-century

anthropology she had encountered as a student. With regard to the latter of these interests, however, one could thus argue that this was more true in British than in American anthropology.[20]

After World War II, as area studies programs proliferated in American universities, Herskovits may have been most noticeable as a statesman, lobbyist and entrepreneur in African studies. A remarkable proportion of the first sizeable generation of Africanists in American anthropology were his students at Northwestern University, and it might seem as if his professorial position, according to the contrast suggested by Robert Murphy with which this chapter began, was in some ways more of the European (oligarchical) than the American (more collegial) type. Yet his former students tended to branch out in varied directions, and did not all remain Africanists. A main reason why I want to point to his example here, however, has more to do with his earlier research activities.

When *The Myth of the Negro Past* was published, the "myth" that Herskovits criticized was the notion that black Americans had not carried any African cultural heritage with them across the Atlantic. During the long period when, according to progressive opinion, the interests of racial equality would have been best served by viewing black people and white people as basically alike, Herskovits' interest in "Africanisms" may seem to have been a little risky. But, at least to an extent, that changed: by the time of my field work in Washington in the late 1960s, I could see that his book had become popular among black cultural nationalists.

In fact it had been a somewhat hurried synthesis, bringing together available written materials on the life of people of African descent in the New World. Herskovits was not himself entirely satisfied with it. But until the academic expansion of African studies came to take up his time, he (together with his wife Frances) also undertook a series of field studies in Surinam, Haiti, Trinidad, and Brazil, as well as in Dahomey (now the Benin republic)—an area of West Africa that had been prominently involved in the Atlantic slave trade. Some of the research may have been brief, and not of the highest quality.[21] Looking back at that series of studies now, however, we may see it as a groundbreaking contribution to a cultural mapping of the "Black Atlantic"—a notion that has mostly entered our vocabulary much later (Gilroy 1993).[22] Moreover, while multi-site field studies have widely been taken to be a feature of very recent anthropology, might it not make some sense—taking the long view of much of one scholarly career—to see Herskovits' extended enterprise, "following the Africanisms," as an early example?

In his case, it was a single-investigator effort (or, rather, that of an academic couple), and its results were spread over a great many publications—monographs as well as articles. But this case may bring to mind at least a couple of other relatively early research enterprises in American anthropology. Around mid-century, some of the members of that Mundial Upheaval Society at Columbia University were drawn into one of them. Julian Steward, during his fairly brief period at Columbia University—also a time when he was preoccupied with the development of area studies—organized a large project on Puerto Rico (Steward 1956), for which much of the field work was carried out by Columbia doctoral students—most extensively by Sidney Mintz, Eric Wolf, and Robert Manners. These field studies were of ecologically distinctive local communities: one was a sugar plantation, one was devoted more to coffee growing, and one to tobacco growing. These studies were integrated into Steward's theoretical framework of cultural ecology—one, as it turned out, that in the long run was not entirely to his students' liking.[23] Precisely how much collaboration there was among the participants as the project went on is not entirely clear, but Wolf (2001: 5), at least, has later recalled that between him and Mintz there was "intense correspondence."

Some 20 years earlier there had been another American project combining several field sites. Robert Redfield (a little later, one of Herskovits' co-authors of the acculturation memorandum), from a kind of evolutionary perspective, mapped what he saw as the "folk-urban continuum" onto a series of localities on the Mexican peninsula of Yucatán: the tribal village of Tusik, the peasant village of Chan Kom (close to the famous Maya pyramid at Chichen Itzá), the town of Dzitas, and the metropolitan city of Mérida.[24] Most of Redfield's own ethnographic involvement (together with that of his wife, Margaret Park Redfield) was concentrated in Chan Kom; but together they also worked in Dzitas, he recruited a junior American colleague to work in Mérida, and for much of the field effort in both the villages he relied on a young man he encountered as the village school teacher in Chan Kom—Alfonso Villa Rojas.

The published results of the Yucatán project were uneven, and the conceptual framework has been faulted. Yet we may still find it interesting in some ways. It is true that there is not much tracing of specific tangible linkages between the sites, but more of a comparison of the sites as distinct entities. There was close contact between the scholars on the team, however. Can we think of the Yucatán project and the Puerto Rico project, too, as early

varieties of multi-site ethnography, though of another kind? If we are concerned with the nature of our more or less ambiguous, unequal partnership with collaborators in knowledge-production in the field—assistants, informants—the story of Alfonso Villa Rojas also seems remarkable. Through his connection with Redfield, the provincial college dropout and village teacher went on from Chan Kom, by way of a Chicago PhD, to become one of Mexico's foremost professional anthropologists.

I also wonder if Robert Redfield's anthropology, as it developed over the years, is not of some current interest in other ways as well.[25] There is his place in one of the fairly often-discussed but rarely-practiced varieties of anthropological field research: the restudy. His first major project, and probably in the end still the best-known, had been in Tepoztlán, not far from Mexico City. Apparently it was a little hurried—there was a revolution going on—and it seems as if the results became rather dependent on Redfield's own sensibilities. A very different anthropologist, Oscar Lewis (who may now be remembered mostly as the originator of the notion of a "culture of poverty"), came to the same village later, and his disagreement with his predecessor's findings was strong enough so that the Redfield and Lewis case has become a classic of anthropological debate.[26] But Redfield also added to the restudy genre himself: 17 years after most of the ethnographic work was done in Chan Kom, he returned for six weeks, and the outcome was the book *A Village That Chose Progress* (1950). That may seem like a brief stay—hit-and-run anthropology—but the product was illuminating and readable. Although the genre has become associated with critique and disagreement (like the Griaule and van Beek case), even identified as yet another mode of generational challenge, one should not forget that, through its combination of everyday ethnography with time-depth, the restudy can also allow a significant anthropological contribution to the understanding of historical change.

If some of Redfield's early work achieved classic status, and has been roundly criticized for both its ethnography and its theoretical leanings, he would also seem to be a possible model for continued scholarly engagement, in many forms (something that, as I suggested in Chapter 4, has its particular difficulties in anthropological careers). In a later period, when he had become more of an academic statesman, he was earlier than most in giving explicit recognition, in various essays, to anthropology's balancing act between science and art.[27] In the 1940s and 1950s he played

a part as a public intellectual, supporting desegregation in race relations, and commenting on international relations. A decade or so into the twenty-first century, with a US administration committed to a renewed foreign policy of "listening," one might find it striking that in 1953, in Cold War times, Redfield contributed an article to the respected weekly, the *Saturday Review*, under the title "Does America Need a Hearing Aid?"

Toward the end of his life (he died at 61, after a battle with leukemia), forever concerned with the "big picture" of human life, he turned to developing an understanding of civilizations, mostly—in a characteristically anthropological way—from the bottom up. This took him on journeys to China (interrupted by revolution) and India (interrupted by his illness), and he began, but did not have time to finish, a book on the subject. It was a topic in which Redfield found himself in a dialogue with Alfred Kroeber, who approached it from a different angle. But Kroeber did not have much longer to live either, and in later years the concern with civilizations within anthropology has been more muted. We may think this is unfortunate—if a stronger interest had persisted, the discipline might have had a stronger voice some decades later, when the "clash of civilizations" became a slogan for a certain interpretation of the world.[28]

MEDIA ANTHROPOLOGY: FROM HOLLYWOOD TO THE COPPERBELT

At about the same time, in the middle of the twentieth century, as Julian Steward sent his students to Puerto Rico, and Robert Redfield returned to Chan Kom, one anthropologist found her field in Hollywood. Again and again, Hortense Powdermaker placed herself at the forefront of the changing discipline. Having spent her undergraduate years at a reputable women's college close to Baltimore, her home town, and then worked as a trade union organizer, she went to London and became one of Bronislaw Malinowski's early graduate students—thus participating in the same seminar that would later draw, among others (as we saw in Chapter 5), Fei Xiaodong and Jomo Kenyatta. American but London-trained, she became one of the first transatlantic anthropologists. Her first field study was in her mentor's footsteps, on a Melanesian island: the village of Lesu was on New Ireland, which had not long before been the German colonial territory of Neu Mecklenburg. Life in Lesu involved rising with the sun every day, attending feasts and funerals, working with informants on

genealogies and magic spells, and strolling through the village to chat with the women.

Her career as field worker subsequently took a turn that was then not quite what was expected of anthropologists. She went to the Deep South, to a town in Mississippi, to study the relationship between blacks and whites. According to the title of the resulting book, this was *After Freedom* (1939)—by the time of her research, some seven decades had passed since slavery had ended. Roughly the same amount of time has elapsed since then, and Powdermaker's Mississippi ethnography has thus become a document from a period of regional American history. It describes those differences of class and lifestyle on each side of the color line that also played their part in shaping the social order, and it notes that sexuality at times crossed that color line.

Powdermaker recalled that at least one prominent senior colleague at the time had reproached her for her choice of field site and topic—the right place for anthropologists remained with the exotics, the primitives. For one thing, however, her stay in the Mississippi town had made her aware of the role that popular films played in the experience and imagination of Americans everywhere. Her next two field studies, consequently, turned her into a pioneer of media anthropology (which is thus not a phenomenon with it's origins exclusively in the 1980 and 1990s). The first resulted in the book *Hollywood: The Dream Factory* (1950), the second in *Copper Town: Changing Africa* (1962). And then, looking back at her varied field experiences, she concluded with *Stranger and Friend* (1966), one of the first books in a genre that would soon grow, in which anthropologists would reflect on what field work was really like. (At the time, not even Malinowski's *A Diary in the Strict Sense of the Term* had yet appeared—it was published the year after.)

The second part of the *Hollywood* book title—*The Dream Factory*—summarizes the main issue it deals with: on the one hand, movies are important materials for the imagination, and on the other, those materials are produced by an industry. Powdermaker pays somewhat perfunctory attention to the actors, the movie stars, but has more to say about people like writers and producers. This makes the book an early case of what I have previously (see Chapter 4) called studying sideways: the people of Hollywood were, in their background and their work, not entirely different from Powdermaker's own. Reading her description of field work, I see similarities with my own work with foreign correspondents a half-century later. To begin with, some of it is field work "by

appointment." I recognize her uncertainty over whether, with informants like these, one should take one's notes openly during the interview; and also the solution she resorted to frequently of finding a place around the corner to write down a rough first version immediately after an encounter. She notes that Hollywooders were excellent interviewees partly because their level of frustration was high, and frustrated people love to talk. One particularly fruitful interview was with a producer who gave generously of his time:

> The interview lasted about two hours, and he had told his secretary that he was in conference and not to be interrupted by phone calls. He did practically all the talking with only an occasional question from me. When I finally got up to go he said, "You know, this has been simply fascinating. You must come again" (Powdermaker 1950: 6).

On the whole, however, as becomes particularly clear when she looks back at it in *Stranger and Friend*, the Hollywood experience was not very satisfying to Powdermaker personally. At the time there was no existing model for doing anthropology in a place like this—in fact, it was hardly a place: "not a structured geographical locale; studios and homes were spread for many miles in the sprawling city of Los Angeles" (Powdermaker 1966: 213). If it could be called a community at all, it was divided in interests, values and lifestyles between its different categories of people. There was not much social coherence, and people came and left—nothing like the repetitious daily round in Lesu, or even the strong sense of place of the Deep South.

But perhaps, to an extent—because this world was populated by human beings more or less of her own sort—she also felt free to be critical, or simply could not avoid it. She was disturbed by the way scripts and movies were put together, with too many people meddling, and by the lack of integrity of the individual writer's product, in which the writers soon learned not to invest too much of their selves and their emotions. "Any system," she concluded, "which employs men of talent, whether artists or scientists, and does not recognize that certain conditions of freedom are necessary for their effective functioning, is destined to destroy their usefulness and value" (Powdermaker 1950: 169). And in a way she pitied the actors and actresses, who, for all their supposed glamor, were looked down on by the other people in the dream factory.

Powdermaker then went away to write her book, and when her publisher read the manuscript he insisted that she should act the part of an anthropologist more noticeably, and insert the kind of labored exotic comparisons that we would now reject as gimmicks—to refer back to Chapter 3, she was forced to put on the pith helmet. She was subsequently unhappy about this, not least because she had thus given up some of her integrity, just as those movie industry scriptwriters had too often done in responding to commercial demands.

In Hollywood, Powdermaker was on what we now think of as one of the two major sides of media anthropology: at the production end. In her next, and final, field study, she focused much of her attention on the other side: on media reception. In the 1950s she was in a town on the Central African Copperbelt, in what was still colonial Northern Rhodesia, which was the subject of intensive study at much the same time by a number of anthropologists of the Manchester School. While she was in close contact with them, her work shows some significant differences from theirs. The mining towns were racially divided, but the researchers from the Manchester School and the Rhodes-Livingstone Institute concentrated their attention on the African inhabitants. Influenced perhaps by her previous experience in Mississippi, Powdermaker took some interest in the Europeans in the town, and also in the mine staff, and devoted a revealing chapter of her book *Copper Town* to them. In an article in the *American Anthropologist* that preceded the book, she also discussed the imagery and values of "teen-age Africans" in the Copperbelt (Powdermaker 1956). (Might this actually have been the first time that a contribution to the journal focused on teenagers?) One of her girl informants, commenting on European women, noted that "they paint their nails and lips, they don't paint lips for fun but because of the bitter cold which is at their home. Nails they paint for fun."

Her book attempted a broad picture of daily life, but while her colleagues from the Manchester School, for all their methodological expansiveness, took little interest in the media, Powdermaker was particularly curious about what the townspeople read in the papers, heard on the radio, and saw at the movies.

The circumstances of field work were rather difficult, for Powdermaker as for everybody else (partly for reasons of intensifying anticolonial politics); and because she had had to change her field site from another part of Africa at the last minute, she had not had a chance to study the major local language, Bemba, beforehand.

Thus she found herself working largely through assistants, herself talking mostly to English-speakers. But she liked going to the cinema to watch the rather invariable fare of westerns, British and local newsreels, African scenes, and farces—and to watch audience reactions, which were continuous and loud. To catch more of the commentary, she posted some of her assistants in different places in the rows, so that they could listen in and report to her. At times, the remarks they overheard showed that viewers would not always separate fact from fiction, or actors from the parts they played. That created some confusion, and irritation, when a familiar star showed up doing something that was not consistent with their character in a previous film.

The townspeople certainly included some of what Powdermaker calls "the intransigents," who were just not very interested in the media, and would rather spend their leisure time drinking beer. But radio and the movies were by now part of the leisure life of a great many. Reading was not so common, although it was regarded with a great deal of respect. The point was, of course, that it tended to go with education, and not least with knowing English, as printed matter in any of the regional languages was limited. There had been a time, not so long before, when reading had been mostly confined to the Bible, as literacy had been brought by the mission schools. This was no longer so, however. There were several local and regional newspapers, mostly small and serving official viewpoints, but they did something to broaden horizons. Copperbelt Africans did not read too many novels, but took a greater interest in the sort of non-fiction publications that could be of more immediate practical use to them. Even so, reading could do something to develop imagination and critical power. Rather approvingly, Powdermaker quoted one young man who noted the way one could come back and re-examine a text in a way one could not with the spoken word. This youth, she reflected, was in a way making the same point as a certain renowned contemporary American thinker (David Riesman) whom he had surely never heard of: "once literacy enters the environment, neither social organization nor the individual can ever be the same again."

Powdermaker carried out four major field studies in as many decades, in three very different world regions. Here, then (to return to the question of what anthropologists do with their longer-term careers), was someone who did not let her first field study become her last. In *Copper Town*, however, she complained that writing up had taken too long. She had felt a need to think and read widely, on

her return from the field, in order to draw on the conceptual and theoretical resources of several disciplines. (Due largely, no doubt, to the field circumstances, there is sometimes a dearth of closeup ethnography in the book, and rather more general arguments drawing on current social-scientific thought.) But she was also teaching at a college rather than a research-oriented university, so she had had little time for such efforts, and for the writing itself.

CONFIGURATIONS OF ANTHROPOLOGICAL WORK

Old anthropology can be boring, and it can seem quite alien— the ethnography may or may not be of a high standard (although perhaps not all ethnography of later date is impeccable either), and the conceptual apparatus may no longer appeal to us. It happens, certainly, that we like it even in those respects, but often we sense that we have come a long way since then. There are many dimensions to anthropological research and reporting, however, and some of the questions anthropologists confront tend to be more enduring—even as our responses to them may shift. We may find past anthropology most resourceful in relation to what we might loosely term ways of configuring anthropological work. Griaule and Ogotemmeli; Kon Wajiro watching the Japanese; the Mass-Observers reporting on the coronation, the pubs and the blitz; Herskovits following the Africanisms; Gluckman and his colleagues in Manchester and Central Africa; Steward and his (relatively) young graduates of the Mundial Upheaval Society in Puerto Rico; Powdermaker at the movies; and Redfield (with or without Alfonso Villa Rojas) visiting and revisiting Mexico—and having his Mexico revisited, and then exploring civilizations, and concerning himself with Americans' listening: these are all good to think with as we continue to ponder that triangle of relationships discussed in Chapter 5, between the anthropologists, the people they write about, and the audiences they reach out for. And of course it is not just a simple triangle. Looking closer, there are also relationships among collaborating anthropologists; between anthropologists and their assistants and informants, each with their own life trajectories; between the anthropologists who were first in a field and those who followed them there— indeed, many of the relationships within anthropology's world.

This is not to say that we will just rediscover old ways of doing things, and return to them. Sometimes reinventing anthropology can indeed be like reinventing the wheel; but a fresh look at these old ways may also give us a chance to think again about present

ways of doing things, and new challenges and possibilities. (Neither, however, should one underestimate our capacity to think from scratch and come up with something more genuinely new.)

Let us return to a question hinted at before in this book. Field research in anthropology has tended to be a one-person, lone-wolf undertaking. On the whole, this has remained true when multi-sited work has increasingly often been seen as desirable, as a research problem somehow ramifies in space. There are reasons for sticking to this basic organizational solution: it is simple, the scholar feels comfortably in control of his or her ethnography, and one person can be held accountable (which, again, is a good thing if the result is to be examined for an academic degree).

But there may also be disadvantages to this approach. A single researcher covering many sites may become somewhat hurried. Moreover, this single scholar may not possess the combination of cultural and linguistic skills that might be desirable in working at a set of quite different local sites. Under the circumstances, this researcher may be constrained to choose only sites that do not make too many new and inconvenient demands, even when they may not be optimal in other ways. There are also times when simultaneity matters, and when research in interconnected sites should preferably go on at the same time, especially in order to offer the best view of more quickly moving processes. When the single researcher moves over time from one site to the next, he or she is less likely to reach a clear and detailed understanding of such phenomena.[29]

In our selections from the anthropological past, we may find some materials for thinking about alternatives. Julian Steward's Puerto Rico project and Robert Redfield's Yucatán project were both divided over several sites, and based on team research.[30] It is true that both involved reasonably coherent regions, the participant researchers were not systematically recruited for differing skills, and temporality was not very important. Indeed, following up particular social connections between the sites was not a major purpose of either project. But the possibility of engaging more collaborators may deserve more attention in an era when sites within a project may be very different, and widely dispersed (in diasporas, or "global assemblages"), and when chains of actions, events and responses may connect quickly over great distances. That early activity of Mass-Observation, calling in reports on the coronation events of May 12, 1937, from all over Britain and even beyond—as well as responses to them and comments on them, all occurring more or less

at the same time—is also, in its own way, an example of multi-site field work conducted differently, with more participants.

Why might we now want to give further thought to carrying out multi-local studies in a more collaborative fashion? The potential for better-quality ethnography is one factor, and speed and simultaneity will sometimes matter. There is also the important circumstance that email and laptops can make such collaboration over distances much more practical and efficient than in earlier periods, both in assembling materials and in writing work up.[31] It might also be a consideration that some of the funding agencies we must rely on do not now seem to think that small is beautiful. They take a favorable view of larger-scale activities, and of national or even transnational team efforts. (This seems often to be the case within the European Union.) We may or may not think that these funding agencies know what they are doing, or have the subtle grasp of differing modes of research that they ought to have. Nonetheless, we could try to turn the circumstances to our advantage.

It remains true, of course, that collaboration depends greatly on such rather less tangible factors as commitment and trust. As one looks back at the varied projects in anthropology's past, one may also rethink the appropriate distribution of knowledge and skills. In the landscape of knowledge, the tradition of anthropology has been to make a sharp distinction between scholars and lay people—between those with professional knowledge and those with folk knowledge. There may have been outstanding informants with expertise in folk knowledge, but the distinction still persisted. With time, however, that distinction has become more blurred. The people who sent in their reports to Mass-Observation headquarters were perhaps not quite of "the masses" themselves. Kon Wajiro, and Hortense Powdermaker on the Copperbelt, and for that matter the Manchester anthropologists in the same field area, had helpers doing some of the interviewing and observation. Robert Redfield's schoolteacher assistant went on to become an anthropologist. We saw in Chapter 5 that there are fields in which informants prefer to be called consultants; and that as we turn to studying rather more sideways, with groups in some ways like ourselves, their members may be "counterparts," doing para-ethnography. But precisely how should we understand these collaborative arrangements to differ from that between Marcel Griaule and Ogotemmêli? Any inclination of ours to read a sort of intellectual scandal into the possibly quite creative interaction between these two men seems to rely on that assumption that folk knowledge is collective and static,

and that the blind Dogon hunter should be providing the visitor only with an expert version of it. But was he actually a counterpart of the anthropologist, doing some kind of para-ethnography?

In our time, collaborative efforts had better develop a clear understanding of where the people involved situate themselves in the landscape of knowledge—as informants, assistants, students in training, para-ethnographers, anthropologists "at home," expatriates, armchair theorists, or whatever. However—not least because there are now anthropologists everywhere, and from everywhere—the chances of putting together a resourceful team should be better than ever.

There is also, in the way we think about the anthropological endeavor, the question of how to reach audiences, and with what. In the age of blogs and print-on-demand, it may receive some new answers, but the range of old ones deserve a hearing. Mass-Observation brought its rather raw ethnography more or less directly back to the people, with varying success. Over a period of changing relations between black and white America, Herskovits' *Myth of the Negro Past* had its ups and downs as an example of public anthropology. Max Gluckman gave many radio lectures, on the upper-middlebrow Third Programme of the BBC; but mostly the members of his group certainly had academics in mind when they wrote; their book-length work typically appeared in cloth-bound volumes, in a green color reminiscent of rusty copper (appropriately, given that many of them offered Copperbelt research), from Manchester University Press. But if their situational analyses and their extended case studies were mostly for the eyes of colleagues, perhaps one could imagine more or less similar styles of organizing materials in narratives aimed at other audiences? (For that matter, "Kalela Dance—The Movie": Is it unthinkable?)

Hortense Powdermaker also did things her way. Her writing style is mostly quite accessible. We may remember, from Chapter 5, that sign in the *Los Angeles Times* newsroom, asking journalists to "Consider Gus"—the average newspaper reader. The anthropologist writing about one Los Angeles neighborhood, Hollywood, may have had something like that in mind. And her books were brought out not by university presses, but by more general publishers. The one most clearly intended for academic readers was *Stranger and Friend*, with its particular purpose of offering young anthropologists, and other young social scientists, "a case history of how an anthropologist lives, works and learns"; but "other readers may also find it useful and interesting to go backstage with an anthropologist,

and see what lies behind the finished performance"[32] (Powdermaker 1966: 15).

In an earlier chapter, we played with a number of tools for seeing that can be useful for anthropologists in one context or other: mirrors, telescopes, binoculars, dark glasses. Finally, perhaps, we should specifically add a rear-view mirror? But, of course, one should not look in the rear-view mirror all the time—mostly you look forward, sometimes sideways. Keep connecting what you see in that mirror, then, to what is in front of you. Or, in other words: think of these library stacks as promising sites for the exercise of the anthropological imagination.

If you see things that way, stereotyping past anthropology as a largely uniform body of thought and practice does an injustice to it; but, more importantly, it is a disservice to ourselves. The notes in the second part of this chapter on a number of anthropologists (or near-anthropologists), largely from two or three generations ago, should certainly show that they do not make up a cohesive, harmonious intellectual collective. Indeed, some of them were adversaries with regard to important issues of their times. The long march of anthropology has nearly always involved a loosely inter-connected crowd of coteries and loners, innovators and stragglers, curmudgeons and mavericks. It is certainly a good thing that the discipline now has its own historians, who try to discover some order in that diverse past. If you are not into that, however, some more opportunistic occasional raiding may still yield interesting results.

This could also allow us to speculate about false starts, and about (perhaps forever) unfinished business. We may play with the coun-terfactual history of anthropology: What might have been, if this or that had come to inspire followers in a way that it did not? How did organizational facts on the ground affect subsequent developments? Neither Kon Wajiro nor the central Mass-Observation team had a secure academic base where their undertakings could be turned into more durable research orientations. If they had had such a base, would things have turned out differently? What if Hortense Powdermaker had been a central figure in a department with a major graduate program?[33] Would the history and the present of anthropology have been different if someone (whether by now remembered or forgotten) had not turned to other interests, or gone into administration, or died too young?

Such questions in the rear-view mirror may also help prepare us for the unexpected, as anthropology's world moves deeper into the twenty-first century.

8
And Next, Briefly: Toward 2050

Well, enough. Someone tells me, however, that there should be a conclusion. I accept this hesitantly, as I often find conclusions rather dull and repetitive. But let me try to make it brief, and forward-looking …

Again, my own dwelling in anthropology's world has extended over close to 50 years, since the early 1960s. Given that an active life of learning and practicing in a scholarly field may extend from someone's early twenties to the age of retirement (whatever that may turn out to be), and possibly beyond, I would hope that some of you who have read this book will still be active anthropologists in 2050—assuming that "the end of anthropology" has not come to pass, and the discipline is in fact still around.

So if you are among them, what will you face? We can try to shift among our optical tools once more, from the rear-view mirror back to binoculars, to see if anything can be glimpsed from this distance. But I noted in the introduction that the time we are in now is already said to be a time of surprises, an "age of the unthinkable"—so even if anthropologists are becoming more engaged with the future, and people's ideas about the future, I will offer no predictions about what lies ahead. By 2050, new countries may have come into existence, through political decision, and old ones may have disappeared, perhaps due to political indecision in the face of climate change. The twenty-first century could have its own Atlantises. There may have been new waves of ethnic or religious cleansing, and new streams of refugees, and a birth of new diasporas. New combinations of technology with terror, aimed by a very few at the very many and thereafter haunting everybody, may very effectively and dramatically have bridged the micro-macro gap.

In the wider sense, these will have been events and processes changing the world, and therefore also anthropology's world. Under circumstances more directly and narrowly affecting your practices and possibilities, there could be new no-go zones for ethnographers, whether nations or neighborhoods. When human organs become transplantable, transportable, and transactable, and individuals

thus in a way turn increasingly into dividuals, are the anthropology of the body and the anthropology of the senses also transformed?

We cannot be sure what will be your more immediate working environment. If by 2050 you are a campus dweller, you may be teaching in a Program of Cultural Engineering. And perhaps you are lucky enough to have your scholarly home at what has now, after a steady climb over the years on that authoritative ranking list of Shanghai Jiao Tong University, become the world's leading academic institution: Shanghai Jiao Tong University. On the darker side, research ethics authorities, at one organizational level or other, may have imposed new constraints on what have long been fairly freewheeling and mostly inner-directed anthropological field work styles. Can we imagine participant observation being banned in Kyrgyzstan, or Kansas? Or will the intellectual property rights of human groups that refashion themselves in some way as corporations make it difficult for individual scholars, insiders or outsiders, to publish their ethnographies? In combination with the power of the citation index and the ranking list in academic audit culture, on-demand translation technologies may finally have taken you out of the shadow of the Tower of Babel, as anything published in Spanish, German, Chinese or whatever can be instantly available to you in English as well (though these same technologies may then also offer you some more examples of ludicrous translation mistakes, if you are in a position to catch them). Another few nouns and adjectives may have had the prefix "post-" attached to them in the vocabulary of the communicating classes.

This, however, is all ethnoscience fiction, fragments of a scenario for a still fairly distant future, and circumstances over which anthropologists are not likely to have much control. On the basis of what has gone into these pages before, what might be some plausible suggestions for life in twenty-first-century anthropology in areas over which we have more control—at least as we head for 2020?

To begin with, do not mumble. When people outside your circle of colleagues ask what you do, and what anthropology is, have a reasonably clear and simple answer at hand, to be elaborated as long as your interlocutor's attention does not seem to have started wandering. Try to make clear the connection between your view of the discipline in general and your account of your own particular research interest, too. Moreover, try to adapt that answer to different audiences, so that you can say the same thing in ways as different as they may need to be—to students on the introductory course, and media people, and faculty deans, and your neighbors.

This is obviously a matter of caring for the brand, the public image of anthropology. It is also involved in not provoking more anthropology-bashing. Do not make a nuisance of yourself with your research proposals or in the field situation. In the near future as in the present and the past, at least some members of the surrounding society will feel that any research should be of some immediate, obvious, practical use to themselves, or for some other purpose they think is worthy. Some anthropological scholarship is unlikely to match their criteria. You could try, however, to have an argument ready for why your contribution to the buildup of knowledge and thought should be of interest to other people, at least to some people—not only to you personally.

Yet, of course, trying not to provide materials for anthropology-bashing must not degenerate into sheer conflict-avoidance. In anthropology's world in the wider sense, there are probably adversaries who will not go away anytime soon: know-nothings, xenophobes, ethnocentrics … but perhaps also people who claim some affinity with anthropology in their quite strange claims to knowledge of, and insight into, the world. (As usual, with certain kinds of friends, there is no need for enemies.) To maintain the integrity of the brand, some waves may at times need to be made.

As a joint enterprise, anthropology should take care to stay world-building—to cultivate those cross-cutting ties, that mixture of research at home and away, that awareness of humanity as an interconnected but diverse whole toward which it has perhaps always been striving but which it may only rather recently have come close to achieving. Again, in the division of labor between disciplines, this may be its most distinctive feature. If it should turn into only a collection of side-by-side anthropologies at home, then that distinctiveness will be substantially lost. What is involved here is not only satisfying the individual anthropologist's desire for travel, but having, in as many countries (or whatever are the units) as possible, access to a diversity of perspectives, both inside and outside, toward itself—and to insiders who know the outside. In other words, it may be of some interest to the Belgians to know how a Bangladeshi views their society, but also of some value to people in Bangladesh to have someone around who knows about Belgium.

That, I have suggested, is a recipe for contributing to a degree of transparency in world society. But this world-building anthropology also requires that the product of these cross-cutting experiences and interactions does not remain entirely within the scholarly community. Like any academic discipline, after all, this one can hardly make its

living if its members only take in each other's laundry. There is a need to find effective ways of telling other and wider publics about what anthropologists are finding out about ways and circumstances of life elsewhere. Communicating anthropology in the twenty-first century has to be both about diversifying writing genres and about using the greater variety of technologies now at hand.

World-building anthropology would also seem to work best if anthropology at home and anthropology away make their collaborative arrangements—especially when they share a territory. Locally based anthropologists can be helpful to visiting scholars in a number of ways: opening networks, accessing institutions, filling in on contexts and antecedents. Visitors can make an effort to get to know local colleagues, read what they write, stay in touch, perhaps even work and write together with them—and, by citing their writings, make these more widely known.

Until we have well-functioning translation technologies, language plays a part here. In relation to writing, those whose everyday working language is not English have to keep thinking about what combination of language uses will serve their purposes best, both in reaching wider publics and in communicating with the international scholarly community. The Anglophones, for their part, may try to read in at least one other major language in which significant scholarly publishing goes on—and support translations, especially from other languages *into* English.

This has to do with cultivating openness. In Chapter 2, apropos of the strong presence of the American elephant, in anthropology as elsewhere in global academia (a presence both impossible and unwise to ignore), I mentioned in passing other metaphors from the scholarly zoo that might serve as models for the anthropological life: the fox, who knows many things, and the hedgehog, who knows one big thing. I quite like hedgehogs, including the real ones honoring my summer garden with their visits. It is true, too, that one may shift between means of knowledge-seeking at one point or other in a life. But let me concentrate here on what could be the academic activities of a twenty-first-century fox.

Keep your eyes open—and not only in anthropology's world as narrowly defined. Pay attention, too, to what goes on "out there," in the wider world—including to things that have not yet had the academic stamp of approval as legitimate anthropological topics of research. Anthropology may have a certain advantage in approaching emergent phenomena. Try to keep an eye on other disciplines, too. There may be possibilities of collaboration, or at least complemen-

tarity, if you sense that what they are doing to a topic is not what you would do. Note that these disciplines may also be changing in some ways, at least in some corners, so that your established views of them may no longer be up to date. Beware, then, of ill-informed sociology-bashing, or economics-bashing, or whatever.

Generally, try to make room for serendipity. Such practices can go by different names in different contexts. To repeat, in libraries and bookstores (and I would assume that there are still some of the latter left, at least by 2020), the relevant activity is called "browsing." Look not only at the shelf of your specialism. Following the line of argument of the preceding chapter, browse backward in time. Among the days or parts of days left for your own free intellectual life, make some offline days. It may be seductive just to take your usual seat in front of the screen, but much of the stuff online is ephemeral, narcissistic, not well thought through. On those days, explore the stacks instead.

Have you already been in anthropology's world for some time? Take care to stay alive as an active scholar throughout your career. If there is some period when that is difficult—due to domestic commitments, say, or organizational obligations (such as administration), try to get back to it as soon as you can afterwards. It will be good for your intellectual well-being; and, at least if you are in an academic environment (and one that is not too corrupt), it will presumably also help the ratings of the unit you are in. If you are ready for more field work, this may involve finding another kind of field, and other methods of field involvement. But it could also be a matter of other kinds of research, and other kinds of writing.

Keeping your eyes open can also involve moving around. If you are a campus-dweller, do not just do the daily round between classroom, office, and home. Go to conferences when you can. There are national academic structures which are very sedentary, so that people spend their entire careers, from undergraduate to emeritus, in the same place. There are others where you are encouraged or even required to move about. Even if, for one reason or other, you remain mostly in one place, look for opportunities for excursions: visiting appointments, exchange programs, and the like. It is a way of escaping from taken-for-grantedness, of changing your horizons. Even if you find that the grass is not greener on the other side of the fence, it may taste thought-provokingly different. (Do foxes eat grass? Those metaphors probably do not mix well.)

But all anthropologists are not campus-dwellers now, and will very likely not be by 2020 or 2050. And people who have been

trained as anthropologists and then found a livelihood outside academia are not necessarily ex-anthropologists, or quasi-anthropologists, but may be using their professional skills and contributing to the discipline in other ways. Research institutes, think tanks, consulting firms, media organizations, branches of government, and NGOs are among the other settings where people may in one way or other practice anthropology. For those of you who have found, or will find, such other niches, make sure to keep in touch with the wider anthropological community through meetings, refresher courses, online networks, or by other means. Do not expect the discipline will remain there, precisely in the same shape it was when you left your alma mater. Conversely, for academic institutions, and national and international associations of anthropologists, try to maintain links—staying in touch with alumni, paying attention to their experiences and their needs. For one thing, they may have immediate access to some of the most striking ethnography of emergent phenomena out there.

Then again, 2020 may already turn out to be entirely different; and this conclusion could then be counterfactual.

Notes

CHAPTER 1

1. For some of the discussions of anthropology and colonialism of more enduring interest, also by senior scholars who had an inside view, see Gough (1968), Asad (1973), Loizos (1977), and, a bit later, Goody (1995).
2. See for example the multifaceted discussion of "colonial legacies" in a special issue of *Social Anthropology*, edited by De l'Estoile (2008a).
3. Unless one can arrange for every part of the world to be studied by a non-native; a nice idea in a way, but so far not very realistic.
4. On methodological nationalism, see the German sociologist Ulrich Beck (for example, 2000: 64ff.).
5. My more significant considerations of global and transnational matters are in Hannerz (1987, 1989, 1992a, 1996); and then I discern the growing absorption of understandings of global contexts into standard anthropology in Hannerz (1999a).
6. On such matters, see, for example, the discussion of open access and the circulation of scholarly work by Kelty et al. (2008), and Fabian's (2008) book of ethnographic commentary, based on field materials available on a website.
7. Anthropologists, however, seem to prefer to think of themselves in the researcher's (not least the field worker's) role, rather than in the teacher's. In my period as chair of the European Association of Social Anthropologists (EASA) in the late 1990s, thinking that the association could achieve greater organizational density by building various thematic networks, and realizing that a large part of the membership had teaching as a major part of their activity, I initiated a "teaching anthropology network." Through the efforts of some devoted founding members, two valuable edited volumes were published (Dracklé, Edgar and Schippers 2003, Dracklé and Edgar 2004). But since then more narrowly defined research-oriented networks in the Association seem to have generated much more activity than that focusing on teaching.
8. For some earlier comments on these issues that I also draw on here, see Hannerz (2007). For further views of academic neoliberalism on the European side, see, for example, Shore and Wright (1999) and Strathern (2000); on the American side, for example, Marcus (2002) and Brenneis (2004, 2009). On the quickly expanding use of neoliberalism as an explanatory trope in anthropology, however, see some critical remarks by Kipnis (2007). On emergent qualities of university life (not specifically concerned with anthropology), see, for example, the largely American-based interpretations by Readings (1996), Donoghue (2008)—who also dwells on the rapid expansion of for-profit organizations in occupationally oriented American post-secondary education—and Tuchman (2009). Over the years, I have found some solace in the satirical column by the British sociologist Laurie Taylor, in the publication appearing as the *Times Higher Education Supplement*, or (over time) some variation on that title; at least things are always worse at the University of Poppleton. In his column of

February 12, 1993, Taylor announced three new scholarly journals designed to meet the needs of scholars coming up for evaluation: *The British Journal of Citations*, *The European Journal of Negative Findings*, and *The Comparative Journal of Overnight Articles*.

9. Hannerz (1970) in fact focused on men, in the low-income African-American context where their gender role has been rather more problematic than that of women; so it was an early exception to the tendency of gender anthropology to become synonymous with the anthropology of women.

10. On the American four-field approach, see Segal and Yanagisako (2005). On the somewhat related recent development of a British "holistic anthropology," particularly in Oxford, and under other historical circumstances, see Parkin and Ulijaszek (2007).

11. For examples of anthropological perspectives toward cultural studies, see Lave et al. (1992) and Nugent and Shore (1997).

CHAPTER 2

1. Clifford Geertz no doubt did much to stimulate this interest as he raised the question, "What does the ethnographer do?" and responded, "He writes" (Geertz 1973: 19). His *Works and Lives* (1988) contributed further to it. Otherwise, a number of the later explorations of anthropological writing, its phases, varieties and affinities, have come through edited volumes—see Clifford and Marcus (1986), Fardon (1990), Sanjek (1990), Okely and Callaway (1992), Archetti (1994), James et al. (1997) and Waterston and Vesperi (2009). See also Marcus (2007) on the characteristics of some recent anthropological writing.

2. Tomas Gerholm, whose other contributions to anthropology have mostly dealt with the Arab world (including pioneering contributions to Yemeni studies), unfortunately died much too early, in 1995.

3. The article authors were Satish Saberwal on India; Józef Búrszta and Bronislawa Kopczynska-Jaworska on Poland; Abdel Ghaffar Ahmed on Sudan; Gordon Inglis on Anglophone Canada; Gerald Gold and Marc-Adélard Tremblay on Quebec; and Otavio Velho on Brazil.

4. I am not sure that I have a full overview of this growing body of writings, but for a variety of examples, see Barth et al. (2005) on four central anthropologies (British, German, French and American); Manning (1983) and Harrison and Darnell (2006) on Canada; Yamashita et al. (2004a) on the discipline in East and Southeast Asia; a thematic issue of *Inter-Asia Cultural Studies* on social science in Asia, including articles by Chun and Shamsul (2001) and others; Kuwayama (2004) on Japanese anthropology; Boskovic (2008) on national anthropologies ranging from Cameroon to Russia; a similarly wide-ranging volume of the *Anthropological Yearbook of European Cultures* edited by Elfimov (2007); De l'Estoile et al. (2005) on the comparative politics of anthropologies; Skalnik (2002) on the post-socialist anthropologies of Eastern and Central Europe; Poblocki (2009) on transnational and interdisciplinary connections with an emphasis on Poland; Ntarangwi et al. (2006) on the practice of anthropology in Africa; Hammond-Tooke (1997) on South African anthropology before the transition; and the EASA-inspired volumes on European anthropology mentioned in Chapter 1, n. 7.

5. Spencer (2000) is particularly illuminating in his insider overview of the organizational circumstances of British anthropology in the late twentieth century.

The latter part of Barth's remark here obviously refers to the growth of the academic "audit culture" that affected British universities early, and perhaps particularly severely; see, for example, Strathern (2000) and Berglund (2006).

6. I am reminded here of the times when one leading American anthropologist could express some doubt whether British social anthropology was really anthropology at all (Murdock 1951); for another discussion of the old British/American divide, see Watson (1984).

7. Notably, in that volume (Barth et al. 2005), British anthropology was discussed by a Norwegian, German-language anthropology by an Austrian, and French anthropology by a Briton.

8. The World Anthropologies Network has a website (www.ram-wan.net) and its own electronic journal. See also Ribeiro (2006).

9. Only one EASA conference during the same period was held in the United Kingdom, and none at all in France.

10. See, for example, Ntarangwi et al. (2006).

11. Kürti (2002: 75–6) discusses this in the Hungarian case.

12. For views of the political history of American anthropology from World War II onwards, see, for example, Price (2008) on the early years, Wax (2008) on the beginnings of the Cold War period, Horowitz (1967) on Project Camelot in Latin America, Wakin (1992) on the controversy over involvements in counterinsurgency in Thailand, and González (2008, 2009) on the Human Terrain System in Iraq and Afghanistan.

13. On these aspects of South African *Volkekunde*, see Hammond-Tooke (1997: 119ff.).

14. To a degree, in an early period, Nordic anthropologies may, somewhat similarly, have been settler anthropologies vis-à-vis the Saami people of the north.

15. Hann (2007), with extensive Central European experience, offers one elaborated account of the difference between "the two anthropologies." I should perhaps note here that I do not quite agree with the point of view formulated by my colleague Tomas Gerholm (1995) in a later essay contrasting a "peripheral" Swedish social anthropology with a "central" Swedish ethnology, that is, *Volkskunde*. Although there is no doubt something to that point of view, it appears to me that Gerholm tends to disregard the very considerable difference in scale and organization between the "world systems" of the two disciplines—that of *Volkskunde* is largely limited to certain parts of Europe, and after a problematic history does not appear very internationally cohesive.

16. For an intriguing account of the interplay between politics and scholarship in fostering an ethnology at home in postcolonial France, see Lebovics (2004); see also the overview by Rogers (2001), and the brief comments by Archetti (2006: 123–7).

17. There were actually more than 39 section editors, as some of the 39 sections had two co-editors. In the following, in the limited numerical facts that I use, I have counted each co-editor as one half of an editor.

18. Whoever wants to look for an unusual stylistic twist in the encyclopedia could turn, for example, to the entry on "Symbolism in anthropology."

19. Apart from these, the older encyclopedia included many more biographical items, but the publisher and editors of the new work decided to be very restrictive in this area.

20. Strictly speaking, this is the Bank of Sweden prize in the memory of Alfred Nobel; it has only been awarded since 1969.

21. The imagery may be a little inappropriate, as I understand that in terms of current animal totems, more American anthropologists have tended recently to identify with the donkey than with the elephant.

22. Or, to borrow instead from the work of the more recent American folklorist Roger Abrahams (1964), and later Gates (1988): as signifying monkeys, deep down in the jungle (deftly dealing with uncertainty). The "signifying monkey" is a famous trickster of urban African-American folklore, and there may be something trickster-like about anthropologists anyway—see the next chapter, not least on the subversive implications of an interest in diversity.

CHAPTER 3

1. In his lively autobiography *An Academic Skating on Thin Ice*, Worsley (2008: 154) notes about this paper that "I was wrong—it didn't die at all," and, moreover, about the reaction of his local Manchester colleague Max Gluckman, that "his fury knew no bounds." But then Worsley's point had really been that anthropology would wither away unless it gave up its traditional focus on "tribal" societies—and of course it did, as Chapter 4 of this book emphasizes.

2. In a Jensen Memorial Lecture Series at the Frobenius Institute, Johann-Wolfgang-Goethe Universität Frankfurt am Main, on April 21, 2008. I am grateful to Professor Karl-Heinz Kohl for the invitation to participate in the series.

3. On interdisciplinary organization, see, for example, Gibbons et al. (1994) and Nowotny et al. (2001). For comments from within anthropology, see, for example, Appadurai (1996) and Marcus (2005).

4. A year or so later, another political commentator and by then president-watcher, *New York Times* columnist Maureen Dowd (2009), put things quite differently: "Barack Obama grew up learning how to slip in and out of different worlds—black and white, foreign and American, rich and poor. The son of an anthropologist, he developed a lot of 'tricks,' as he put it, training himself to be a close observer of human nature, figuring out what others needed so he could get where he wanted to go."

5. This may have been the main tendency in the public commentary around the "Yanomami Affair," occasioned by the journalist Patrick Tierney's book *Darkness in El Dorado* (2002).

6. MacClancy (2005), discussing the literary portrayal of anthropologists, points this out; he also finds that the character of anthropologists in fiction is more often pathetic than heroic.

7. I have also seen a book review by the Archbishop of Canterbury with a title describing the Virgin Mary as "a global brand" (Williams 2009).

8. But note here also Marshall Sahlins' (1993: 9) comment: "Some Cultural Studies types seem to think that anthropology is nothing but ethnography. Better the other way around: ethnography is anthropology, or it is nothing."

9. The obvious reference here is to Marcus and Fischer's important book *Anthropology as Cultural Critique* (1986).

10. I believe the dissertation by Dahlén (1997) remains one of the most illuminating overviews of the field of "intercultural communication" from an anthropological perspective.

11. See the comments on this issue by Bloch (2005: 1–19), and in a review article by Eriksen (2007).

12. Among the enduring references in a more critical vein here are Abu-Lughod (1991) and Fox and King (2002); Brumann (1999) takes a more culture-friendly view. For a thorough overview of the uses of the culture concept through the history of anthropology up to the present, see Fischer (2007).
13. See especially Hannerz (1996: 30–43, 1999b).
14. It is occasionally suggested that we should keep the adjective "cultural," and do away with the noun "culture." I can see the point in principle, but would be very pessimistic about such a distinction succeeding in public usage.
15. The statement is perhaps now fading out of public consciousness; it is attributed to Hermann Göring, one of the Nazi leaders closest to Hitler.
16. For one case where "diversity" as a political term is largely identified with late-twentieth-century North American expressions of multiculturalism, see Wood (2003). This author is identified on the back cover as a professor of anthropology at Boston University. He recognizes that "diversity" can be understood in other ways as well, but the book is largely devoted to connecting it to identity politics, political correctness, and so on.
17. For major examples from political philosophy, see Taylor (1992), Kymlicka (1995), Parekh (2000), Barry (2001), Benhabib (2002) and Kelly (2002).
18. For some examples of anthropological interest in the emergent and the future, see the early volume by Wallman (1992), and more recently Malkki (2001), Hannerz (2003a, 2008b, 2009), Miyazaki (2003), Appadurai (2004), Guyer (2007) and the responses published with it, and Rabinow (2008).
19. The volume *Anthropology, by Comparison*, edited by Fox and Gingrich (2002), offers evidence of this—but also shows that there can be diversity among ways of comparing. Having been George Peter Murdock's departmental colleague, for a short period and long ago, I am probably more aware of his varied influences on the discipline, around the mid-twentieth century and particularly in the United States, than most anthropologists now active. A copy of the Human Relations Area Files, the major data bank of the discipline, was the proud possession of perhaps every major anthropology department; the *Outline of Cultural Materials*, published in updated editions over several decades, attempted an overall standardization of ethnographic categories. The journal *Ethnology*, now providing an outlet for quite varied ethnographically oriented writings, also had its beginnings as a part of Murdock's organizational effort to gather materials for large-scale comparisons. In another aspect of the history of the discipline, Price (2004) has offered some insights into Murdock's Cold War-era contacts with the FBI concerning suspected anthropologist radicals.
20. I am reminded here of Clifford Geertz's recollection of how, as a young and innocent graduate student about to embark on the study of anthropology, he went to the Harvard University library to find out what his new discipline was really about. He found Murdock's book *Social Structure*, took it out to read it, and soon called out to his wife and co-student, "We've made a disastrous mistake! This is not for us!" (Handler 1991).
21. See also, for example, the discussion by Moore (2005), and Rosaldo's (1999: 32–4) interpretation of Geertz's handling of comparisons.
22. This kind of work by scholars working alone has recently found new expression in multi-site research projects, which usually have a comparative dimension. We will come back to them in later chapters.
23. For some very appreciative reminiscences of this anthropologist mother by a colleague who knew her well, see Dove (2009).

CHAPTER 4

1. An earlier, shorter version of this chapter was presented as a plenary lecture at the annual conference of the Association of Social Anthropologists in Durham (March 29–April 1, 2004), and then published in *Locating the Field* (ASA monographs 42), edited by Simon Coleman and Peter Collins (2006). I am grateful for comments on that version by both the audience in Durham and the two editors. After that previous version was published, Moshe Shokeid (2007) published a somewhat comparable reflective essay on his career as a field worker in Israel and the United States, and its relationship to changes in the discipline. There are parallels and divergences between his account and mine, and a reader may find it worthwhile to compare our conclusions and perspectives. Among other more recent publications on field work are the books edited by Faubion and Marcus (2009), discussing the innovations linked to their group at Rice University, and by Borneman and Hammoudi (2009), on the complexities of the ethnographic encounter.

2. Shore and Wright (1997: 14) propose "studying through" in their mapping of an anthropology of policy, drawing from an unpublished thesis by Susan Reinhold.

3. See Lewis (1968) on the early relationship between history and social anthropology in the British tradition; and Axel (2002) for the more recent state of the art in American historical anthropology.

4. On the future in anthropological study, see the references in Chapter 3, n. 18.

5. It is worth noting that earlier, in two departments of social anthropology that soon established themselves among the centers of the discipline—those in Manchester, England, and Bergen, Norway—the founders had been quick to include local research on their agendas (see Frankenberg 1982, Barth 2007).

6. On my methodological interests in this field, see Hannerz (1976).

7. The earlier anthropological literature on "social change," "acculturation" and so on seemed less illuminating in the late-twentieth-century postcolonial condition.

8. These multi-site studies were not all transnational, and there were some others that were not devoted to occupations, but the collected effort resulted in what is probably the first entire book on such research in anthropology (Hannerz 2001).

9. I first used the term "studying sideways" in a discussion of several occupations on tracks parallel to, or intersecting with, those of anthropologists (Hannerz 1998), and at least thought it was my own; but, as Laura Nader has pointed out to me, there is indeed a reference to "sideways" in her classic chapter on "studying up" (Nader 1972: 292). Marcus' (1997) essay on the anthropological turn to studies in "power/knowledge" is also relevant to this terminological addition, as is much in the recent anthropological involvement in science studies.

10. I would note that, while autobiographies of this kind concentrate on more dramatic events and experiences, my own work has dwelt at least as much on the routines of correspondent work.

11. My interest in South Africa had even resulted in a publication which was both "studying backward" and "at a distance": an essay on Sophiatown, the famously culturally vibrant neighborhood of Johannesburg torn down by the apartheid regime in the 1950s (Hannerz 1994).

12. In an early overview, George Marcus (1995: 101) concluded that most multi-sited field studies had been carried out in monolingual, mostly English-speaking settings.

13. The term "appointment anthropology" may be Luhrmann's (1996: vii). On interviewing access to elites, see, for example, Thomas (1993).

14. For a critical discussion of cultural studies from one anthropologist's point of view, including questions of method, see Howell (1997).

15. See, for example, an issue of the *Anthropological Journal on European Cultures* (2002), and an illuminating transatlantic exchange between Marcus and Okely (2007).

16. Reading Brian Moeran's book *The Business of Ethnography* (2005), on his different field experiences in Japan, cast more light on this issue for me; for a briefer statement, see Moeran (2003).

17. Although, if personal "tales of field work" have tended to become thinner and more reticent, as Marcus (2006) has argued, it may have to do with such shifts in modes of knowledge-production.

18. Evans-Pritchard published his last communication on the Azande in 1973, almost a half-century after the related field work, and the year of his death. A short piece on Azande sexual intercourse positions, it was hardly central to his oeuvre (Evans-Pritchard 1973).

19. It may only have been Gupta and Ferguson's *Anthropological Locations* (1997) that really began to bring this variety entirely into the open; a little later, Amit's *Constructing the Field* (2000) added more to the picture. See also, for example, Ramos (2007: 62–5) on field practices in Brazilian anthropology.

20. Marcus (e.g. 1998: 233–6) has made the point repeatedly. On long-term field involvements, see also Parkin (2000). There has been some discussion of long-term research in particular sites—see, most recently, Kemper and Royce (2002).

21. On such matters, see, for example, the *American Ethnologist* forum on institutional review boards, with an introduction by Lederman (2006), and several other contributions from the United States and elsewhere.

22. For an example, see Pieke (2009) on the implications of a new "Chinese century in anthropology."

23. See also the exchange between two other Malinowski students, Edmund Leach (1982) and Fei Xiaodong (1992), referred to in Chapter 5.

24. One commentator, writing out of Singapore, notes that a website on "Understanding Race" sponsored by the American Anthropological Association, is "so America-centric that it is only marginally useful in teaching about the issue of race in places outside the United States," and that the same organization devoted much more attention to Hurricane Katrina than to the more devastating Indian Ocean tsunami that occurred less than a year earlier (Thompson 2008: 126).

25. There is a kind of paradox here: while for most disciplines previously afflicted with methodological nationalism, a turn to Europe involves an expansion of interests, for anthropology it is the other way around.

26. For varied views, autobiographical and critical, of the recent part of Asian diaspora scholars in the social sciences and humanities in America and Europe, see Assayag and Bénéï (2003) and Roberts (2003). One might keep in mind here that anthropology has long attracted migrants, and children of migrants: Malinowski's seminar, again, was largely a gathering of strangers to the British

Isles, and Boas and the first generation of Boasians (Kroeber, Lowie, Sapir) were often of German background, whether Jewish or not. Unlike the more recent diaspora scholars, however, these earlier migrants (with certain exceptions) did not carry out their field studies in their countries of origin.

CHAPTER 5

1. An earlier, shorter version of this chapter was presented as a plenary lecture under the title "Worldwide Conversations" at the biennial conference of the European Association of Social Anthropologists (EASA) in Ljubljana, Slovenia, in August 2008. I wish to thank Rajko Mursic, both for convening the session "Mutualities in Practice: Beyond Worlds in Collision" and for his major part in making the conference as a whole so fruitful and enjoyable.
2. Bremer (1853–54) is the original edition of her travels in the United States and Cuba; for her journey to Palestine, my source is a more recent edition (Bremer 1995).
3. The group known in Cuba at the time as Lucumi is that now usually referred to, in West Africa as well as in African-American contexts, as Yoruba.
4. Cosmopolitanism has for some time been among my own interests; see Hannerz (1990a) for a discussion focusing on the cultural dimension, and Hannerz (2004c) on the interrelations of cultural and political dimensions, as well as questions of distribution. These texts do not deal specifically with cosmopolitanism among anthropologists. Kuper (1994), who refers to the desirability of a cosmopolitan anthropology, does not elaborate much on his conception of cosmopolitanism, and says nothing explicitly about any idea of diversity.
5. And in one period a sometime Nigerian head of state, General Olusegun Obasanjo, played a prominent part in it.
6. In his brief but influential book *The Breaking of Nations* (2003), Cooper divides the world into three: not those we became used to in the Cold War period, but what he terms the premodern, modern and postmodern worlds, coexisting in time and impinging on one another. The premodern world is that of chaos and failed states, that where warlords and drug barons rule. The modern world, mostly orderly, has the classical state system intact. (On the whole, the United States would belong here.) In the postmodern world, with its openness between states and its wider interconnectedness, the distinction between foreign and domestic affairs breaks down. One can quarrel with Cooper's vocabulary. Even by the time his book appeared, he was having second thoughts himself about the choice of terms with which he had already gone public—especially about "postmodern," as he recognized that "it carries a lot of complicated baggage that I hardly understand" (2003: 173).
7. My Stockholm colleague Renita Thedvall, who has observed what she terms *Eurocrats at Work* (2006) in Brussels, noted that, in the development of particular indicators to measure and compare the success of nations, national representatives were still at times concerned with what Michael Herzfeld (1997) has described as "cultural intimacy," and with showing their own countries in the best possible light. We might add that, in their effort to further European transparency, ethnography was not much in evidence in their toolkit.
8. For anthropological studies in this rich contemporary field, see West and Sanders (2003).

9.   Yamashita et al. (2004b: 18) offer a slightly more developed version of this view in a two-by-two table along the axes "fieldworker and informants same/different" and "audience and informants same/different."

10. See on this the biography by Kroeber's widow, Theodora (Kroeber 1970: 14–15)—who also makes clear, however, that the Kroeber boy was socialized into a wide range of other interests as well.

11. My discussion here leaves out one significant field presence about which anthropologists have frequently not been all that transparent in their writings—that of locally recruited field assistants. On this category see Sanjek (1993). In the world of foreign correspondents, referred to in Chapter 4 and below, it has its counterpart in "fixers," most of whom are similarly rather invisible in resulting texts.

12. On the particular characteristics and problems of such work, see also the pioneering work of Forsythe (2001).

13. In one intriguing account, taking his point of departure in the problematic of Brettell's volume, Shokeid (1997) discusses the particularly complex circumstances of the triangle as it quickly emerged around the manuscript of his book *A Gay Synagogue in New York* (1995)—both a friend of his, a somewhat marginal member of the community in question, and the manuscript editor for the university press involved engaged him in intensive exchanges concerning his text.

14. It is true, as Silverman (2007) has pointed out, that there was (apart from American Indian studies) an earlier wave of anthropological studies in the United States, in the 1930s and 1940s, and focusing mostly on problems in American society; but it was not institutionalized academically.

15. For one perspective on this, see Björklund (1986).

16. For prominent examples among anthropologists in the two oldest American ethnic minorities, see Medicine (2001) and Harrison (2008).

17. Some of my Swedish colleagues in the post-*Volkskunde* discipline of European ethnology have discussed very perceptively the historically, contextually and individually shifting stances toward local materials; see, for example (if you can), Ehn and Löfgren (1996); or, for a brief discussion of one approach to Swedish materials, in the context of the homecoming of anthropology elsewhere, Löfgren (1987). For a developed ethnographic example, see also the social anthropologist Bengt-Erik Borgström's (1997) analysis of coexisting conceptions of the past in the north Swedish community where he grew up, and to which he has maintained a connection.

18. Her father is the well-known anthropologist Robin Fox, academically mostly based in the United States; and her account of family life occasionally may seem like a somewhat ambiguous, gentle form of anthropology-bashing, as discussed in Chapter 3, above: "When my mother told him that she was pregnant with me, their first child, he immediately started trying to persuade her to let him acquire a baby chimp and bring us up together as an experiment—a case study comparing primate and human development" (Fox 2004: 7).

19. There is also a sequel, a "the Metro revisited," published 20 years after the first French edition (Augé 2008).

20. I also remember hearing a Burmese colleague mentioning at a conference that an extended critique of Leach's *Political Systems of Highland Burma* had circulated among Burmese anthropologists, apparently without ever effectively reaching their colleagues abroad.

21. We can see the parallel between Fei's experience here and the dilemma described by the Cameroonian anthropologist Nkwi, quoted in Chapter 2. Another of Fei's English-language books, *China's Gentry* (1953), with an introduction by Robert Redfield, offers a further variation on the topic-author-audience triangle. It consists largely of essays written by Fei for Chinese newspapers in the late 1940s, and dictated in rough translation to Margaret Park Redfield, when the Redfields were with Fei at Tsinghua University, Beijing, in 1948. Soon thereafter, with the coming of the Revolution, he largely disappeared from western view for some 30 years, and then received the Malinowski Award of the Society for Applied Anthropology in Denver, Colorado, in 1980. Some of his endeavor to bridge the gap between Malinowski and Mao can be glimpsed in *Toward a People's Anthropology* (1981).

22. For some contributions to the discussion, see, for example, Bunzl (2004), Domínguez (1989), Gefou-Madianou (1993), Hastrup (1993), Kuwayama (2004), Narayan (1993), Peirano (1998) and Weston (1997). Merton's (1972) article is a classic sociological comment.

23. Contributing to the volume on national anthropologies discussed in Chapter 2, Satish Saberwal (1982) did indeed take a rather bleak view of the frequently confining everyday and academic experience of Indian sociologists and anthropologists; but one might also note that he was able to make use of his own understanding of Indian society with a remarkable comparative and historical imagination (Saberwal 1995).

24. I would add here that the Herskovits book was originally intended as part of the working materials for a larger study building on an invited outsider's view, Gunnar Myrdal's *An American Dilemma* (1944); and I would also note that about a quarter-century later, when I was myself given the opportunity to offer an ethnographic view of African-American life, I certainly wrote my account with an American audience in mind, giving little thought to whether it would also be read in Sweden (Hannerz 1969).

25. For some of the more recent commentary on Benedict's *The Chrysanthemum and the Sword*, see Fukui (2004), Hendry (1996), Kuwayama (2004: 87–114), Lummis (2004) and Shannon (2004).

26. On the notion of rooted cosmopolitanism, see Appiah (1996) and Hannerz (2004a).

27. In an article already referred to in this chapter, Kuper (1994: 550–1) discusses the possible audiences for ethnographic writings. His preferred audience is other anthropologists; I expect this audience to be with us as long as there is an anthropology, and do not object to it as a major choice, furthering the goals of anthropology as a social science. But with regard to his only alternative of writing for a lay audience not made up of the people written about—"the natives"—Kuper suggests that this is "to write for curious foreigners, armchair voyeurs, who want only the safe pleasures of vicarious travel." And he argues that this is something "attributed to the most corrupted of ethnographers." This puzzles me—it would seem to assume that the only reason for people not to travel all over the world themselves is sheer laziness, and that curiosity about that world among lay people is rather suspect. I would obviously argue that people may be so otherwise occupied that they have limited opportunity for extensive travel, and that if they still want to seek knowledge about more distant parts of the world it is a desire which anthropologists should seek to respond to, and cultivate. See also Sutton (1991).

28. On such matters, see also the view of anthropology offered by a friendly and well-informed outside critic, the law professor David Westbrook (2008: 108ff.).

29. One should not take too rosy a view of this media growth. As people are offered a very wide choice of sources of information and opinion, there is the possibility that some will choose to attend mostly to those that confirm information and opinions already held. This is a kind of mirror use which does not contribute much to making the world transparent; see a comment on "The Daily Me" by *New York Times* columnist and veteran foreign correspondent Nicholas Kristof (2009).

30. And of course (to avoid slipping into ethnocentrism), if these readers are not from Europe or North America but want to engage with these regions, there are some good writers from there, too.

31. The need to rethink, under such conditions, may obviously involve not only the forms of writing, but such other kinds of communicative efforts as film-making and museum work as well.

32. In an essay on "the economies of violence," Catherine Lutz and Donald Nonini (1999: 104) suggest that ethnographic work on such topics will have to look like "fine investigative journalism," in its combined use of a wide range of sources of knowledge. George Marcus (2008: 4) notes that "while their functions and sources of authority as experts are quite different from journalists, anthropologists often function nowadays like the best and deepest journalists—certainly their experiences of other places, of sites of research and reporting, are similar today." See also Hannerz (2002).

33. The volume edited by Waterston and Vesperi (2009) offers varied anthropological perspectives on, and experiences of, such "reaching out." For an interesting presentation of some of the more prominent American "new new journalists," see Boynton (2005); and for an introduction to nonfiction writing techniques that may be practically useful to anthropologists as well, see Cheney (2001).

34. Two recent books—as it happens, both by Cambridge anthropologists—offer particularly interesting examples of writing that probably has wider appeal. Piers Vitebsky, in *The Reindeer People* (2005), takes his readers to the ice, snow and temperatures far below freezing on the Siberian taiga, but also offers warm and vivid portraits of young and old individuals, depicts humans and reindeer living close together and knowing each other well, and describes the havoc caused by the Soviet system falling apart. In *Japan Through the Looking Glass* (2007)—yet another instance of optical imagery—Alan Macfarlane (shown on the cover, in a broadbrimmed hat, with a book, an umbrella and something to drink, against the background of Mount Fuji and a terraced landscape) explores the dimensions of Japanese exceptionalism. A senior academic at an advanced stage of a career based on expertise in other areas, Macfarlane began to take an interest in Japan because of a lecture invitation, but did not aim to retool as a Japan specialist. He travels widely, however, and reads extensively (not least writings by earlier travelers), and engages especially his academic hosts in searching conversations about what he sees. It is a book built largely on impressions, anecdotes, and comparisons, yet drawing on a scholarly sense of what is interesting and not trivial. In some ways the style is more like that of Benedict's *The Chrysanthemum and the Sword* than most of the recent professional work on Japan by Occidental anthropologists, and possibly it will meet with similar criticisms. Yet Macfarlane, of course, had the advantage of "being there," rather than working at a distance.

As far as reaching other readers is concerned, it is also worth noting that both Vitebsky and Macfarlane move outside the ordinary habitat of academic publishing, to trade presses, with these two books.

35. Eriksen (2006), arguing for greater outreach in anthropological writing (and drawing on very considerable experience of his own) also suggests more use of narrative formats. But then anthropologists may long have been using narrative in ethnography without always being quite aware of it (or making knowledgeable use of it). For one fairly early discussion drawing on past and present views of American Indian relationships to the surrounding society, see Bruner (1986). For a critical perspective on the social science use of stories, linked to a relational perspective with which social anthropologists may find some affinity, see the sociologist Charles Tilly (2002: 25–42, 69–76).

36. On such issues, see, for example, my discussion of "macro-scenarios" (Hannerz 2003a), Comaroff and Comaroff (2003) on the handling of scale in postcolonial anthropology, and Knauft (2007) on the place of America in the twenty-first century.

37. I draw here, for a sense of Patai's background, also on his autobiographical *Journeyman in Jerusalem* (1992).

38. There is even an asymmetry in ethnographic work within the West, as many more American anthropologists conduct field research in Europe than European anthropologists in North America.

39. According to one report from the congress of the International Union of Anthropological and Ethnological Sciences held in Kunming, China, in 2009, a number of Chinese anthropologists are now becoming interested in field work abroad (Wilcox 2009). As Chinese anthropology in the era of the People's Republic has so far tended to somewhat resemble a settler anthropology, as briefly discussed in Chapter 2, above—largely a study of non-Han ethnic minorities by Han Chinese researchers—one can perhaps discern a certain parallel with the historical development of American anthropology, turning from its initial focus on American Indians to worldwide ethnography as the United States became a world power. (Although one could also add that, in the pre-Communist period when Fei Xiaodong's early work took place, referred to above in this chapter, there was also a phase of nation-building anthropology.)

CHAPTER 6

1. The first version of this chapter was to have been presented at the International Congress of Anthropological and Ethnological Sciences in Kunming, China, in July 2008, but that entire event was canceled at short notice by the hosts. The particular session was then turned into a workshop on "Making an Interactive Anthropology in East and Southeast Asia," held at Berkeley in November 2008. I thank the organizers, Shinji Yamashita and Wang Jiang Xin, for making that event possible and enjoyable. The chapter also draws on a previous paper, "Twenty-First Century Anthropology: On National and International Practices in a Cosmopolitan Discipline," prepared for the conference "The Politics of Academic Knowledge in a Global Era: Nationalism, Cosmopolitanism, and Market Values," organized by the International Center for Advanced Studies, New York University, and the Institute of History, Hungarian Academy of Sciences, in Budapest, July 16–19, 2007. I am grateful to Professor Thomas

Bender of New York University for hosting that conference, and to its other participants for thoughtful and constructive comments on the paper.

2. See particularly Hannerz (2003a, 2008b, 2009). One major contribution to the genre that I will not touch on at all here, though it may be the one most provocative to anthropologists in its inclination to cultural fundamentalism, is the late Samuel Huntington's "clash of civilizations" thesis. (But see the comment in Chapter 7 on Robert Redfield's interest in civilizations.)

3. Friedman's conversation partner was Nandan Nilekani, of Infosys.

4. Rector Kåre Bremer's note on international ranking and publishing was posted on the Stockholm University website, www.su.se, on September 5, 2005. I might add that, of the first 20 institutions on the Shanghai Jiao Tong list at the time, 17 were in the United States, as were 53 of the first 100. Rector Bremer's institution has climbed slightly on the list in later years, although it is a moot question whether this is the result of his blog remarks.

   More recently, a news item has reported from China that local officials have been suspected of stealing files of individual educational achievements and selling them to underachievers, who can thus appropriate valuable credentials for their own job-seeking (LaFraniere 2009). This seems like another pioneering venture in opening up the educational world to market practices. Perhaps the heads of underachieving academic institutions could similarly acquire the marks of more successful competitors, and thus move up the ladder of world ranking lists?

5. See, however, the multifaceted debate by Wulff et al. (2009) over the fate of the monograph. Book publishing in anthropology is threatened not only by the weight attached to journal citations, but also by the rising cost of maintaining journal subscriptions in university libraries—the budget for books thus shrinks. And anthropologists buy too few books for their personal shelves, so print-runs often become rather small.

6. I noted in Chapter 5 that Europe, especially in European Union contexts, is often described as successful in achieving transparency, at least in political contexts. Yet it is to a great extent—officially, but also in practice—a union of monolingual nation-states, which means that much of what goes on in any one of them tends to be opaque to most people elsewhere on the continent. This certainly complicates the cultivation of togetherness.

7. In 2008, Dr. Engdahl became briefly newsworthy, particularly in the United States, suggesting (at least as it was reported) in an interview with Associated Press that there were few American candidates for the Nobel Prize in literature because the American literary scene had become rather isolated—there were few translations from other languages into English, so American writers were a little out of touch.

8. Somewhat predictably, perhaps, Engdahl's statements on the issue, as far as I know, have mostly been in Swedish. The reference I offer here (Isaksson 2006) is to an interview with him published in the journal of the union of Swedish university teachers. The title, in translation, is "Permanent Secretary Believes in Civil Disobedience"—that is, if teaching in English were to be made mandatory.

9. I would have in mind, for example, Roberto Da Matta in Brazil, or Veena Das as long as she was primarily active in India (she is now more a part of American academic life).

10. For a very illuminating discussion of the (rather marginal) language issue in Japanese anthropology, and the institutional context in which it is embedded, see Eades (2000).

11. For a further discussion of translations as a "cultural world-system," see Heilbron (1999).

12. These Swedish-language books are Hannerz (1973, 1990b, 2001, 2004d).

13. I am pleased that the journal *Anthropological Quarterly* has recently introduced a series of review essays on books written in other languages than English—not a matter of full access to the books themselves, then, but at least a window opened onto other anthropologies (Grinker and Herzfeld 2009).

CHAPTER 7

1. This chapter draws to a limited extent on my Afterword for Mark-Anthony Falzon's volume on *Multi-Sited Ethnography* (2009); I have dwelt on related themes earlier, in a lecture delivered when I was awarded an honorary doctorate at the University of Oslo (Hannerz 2006). The section on the work of Hortense Powdermaker draws on a lecture on media anthropology I gave at the Biennial Conference of the German-Speaking Social Anthropologists, on "Interdisciplinarity, Ethnology and Its Neighbours," in Heidelberg in October 1999.

2. Interestingly, Spencer describes the informal mode of learning in the seminar as an instance of the broader category identified by Lave and Wenger (1991) as "legitimate peripheral participation."

3. The shift in British anthropology sketched by Fredrik Barth, referred to in Chapter 2, may be related to this.

4. Perhaps it is too frivolous, but I happen also to be reminded here of an observation of Gregory Bateson's, first published in the context of the national character studies in the World War II era: in the families of the English upper and middle class (for example, at meal times), according to Bateson, the parents engaged in exhibitionism while the children were the passive spectators. In American families, it was the other way around (Bateson 1953: 376).

5. See also Ortner's (1984) well-known article on theoretical developments during 20 years of mostly American anthropology, where she argued that the early 1960s not only marked the approximate beginning of her own professional experience, but were also characterized by "a major set of revolutions" within the discipline—taking it away from structural-functionalism, psychocultural anthropology, and varieties of evolutionism to symbolic anthropology, cultural ecology and structuralism (to be followed later yet by structural Marxism, political economy, and practice theory).

6. It is notable that Marcus, after the turn of the century, finds that

    the priorities of the best candidates had shifted … The excitement of theory and academic debate about changing social and cultural orders had receded among students in favour of activism, driven by a healthy combination of pragmatics and idealism. The "typical" highly motivated candidate today comes with experience from work in the world of NGOs and activist organizations. One can no longer count on a background in the knowledge of, or at least the desire for, the theories and debates that brought students into anthropology previously … (Marcus 2009: 16–17).

7. Or, to put it differently, perhaps they may be standing most directly on the shoulders of dwarfs?

8. I am reminded here of the British anthropologist Michael Thompson's (1979) "rubbish theory" (inspired by Mary Douglas' interpretation of dirt), according

to which what is not of the present, and not old enough to be respectably antique, is devalued: merely rubbish, junk. Such a time perspective will obviously be mobile, but perhaps, by that measure, the real rubbish in anthropology now derives from the middle third of the twentieth century?

9. But see also various references to Kon Wajiro's work by Harootunian (2000), a specialist in the cultural history of modern Japan.

10. The literature on the classical Chicago School of urban sociology is voluminous; for my own attempt to discuss it in relation to more recent urban anthropology, see Hannerz (1980: Chapter 2).

11. There is a considerable body of writings about Mass-Observation, including a vivid essay in the *New Yorker* (Crain 2006), but mostly not by anthropologists—the most thoughtful recent study here is by MacClancy (1995), who also notes that postmodernist anthropologists with interests in ethnographic production seem largely unaware of this early initiative. For a discussion from a social historian's point of view, see Summerfield (1985); and for some Mass-Observation products, Jennings and Madge (1937), Mass-Observation (1943), and Harrisson (1978).

12. One may be reminded here of Malinowski's opening line in his preface to Jomo Kenyatta's book of the year before, quoted in Chapter 5: "Anthropology begins at home."

13. Unless one extends one's interest to early proto-anthropologists, as I did with Fredrika Bremer in Chapter 5.

14. On Griaule, there is Fiemeyer's (2004) biography. On Max Gluckman, and his colleagues in the Manchester School and at the Rhodes-Livingstone Institute, there is a monograph by Schumaker (2001) and an edited volume by Evens and Handelman (2006), which also includes contributions on the uses of case studies in later anthropology. I have focused on Copperbelt urban anthropology in a chapter in my *Exploring the City* (1980). There are recent biographies of Herskovits by Gershenhorn (2004), and—to include here a couple of the people referred to below—of Redfield by Wilcox (2004), and of Steward by Kerns (2003). These works include full references to other writings mentioned below, so I will not attempt any more detailed bibliographic coverage here.

15. "Were we to ask graduate students and younger faculty at major American departments of anthropology whether they knew anything substantive about this approach," write Evens and Handelman (2006: 5–6) in their discussion of Manchester-style situational analyses and extended-case studies, "our educated guess is that nearly all would reply that they do not."

16. I will not discuss here Max Gluckman's major later endeavor, the Bernstein Project, which played a major part in developing anthropology in Israel. For an insider perspective on this, see Shokeid (2004).

17. For a Manchester School member's discussion of these approaches, see a chapter by van Velsen (1967) in a book on anthropological method that bears the mark of the School. A view of a later development of the extended-case method is offered by Burawoy (2009), a Berkeley sociologist with his own Manchester and Central African antecedents.

18. For my own interest in network thought, see Hannerz (1980: Chapter 5) and Hannerz (1992b).

19. For a comprehensive analysis of Herskovits' thinking on cultural relativism, see an article by James Fernandez (1990), who was one of Herskovits' students; the article also offers a view of Herskovits' extensive public engagements.

20. About the same time as the term "acculturation" had its breakthrough in American anthropology, the Cuban scholar Fernando Ortiz (1947) coined a similar term, "transculturation." While there has been some attempt to distinguish between them, Herskovits (1948: 529) clearly finds them synonymous, or at least greatly overlapping. "Transculturation," however, seems to have enjoyed a certain recent revival, as Ortiz has been rediscovered in postcolonial studies (see, for example, Pratt 1992, Coronil 1995).

21. For recent comment on this, see the scrutiny by Price and Price (2003) of Herskovits' Surinam field work.

22. See also the brief reference to the work of Lorand Matory on the black South Atlantic, in Chapter 5, above.

23. For Eric Wolf's view of the Puerto Rico project some 30 years after the field studies in question, see Wolf (2001: 38–48).

24. *The Folk Culture of Yucatán* (1941) was the major book resulting from the effort. The field worker in Mérida has offered his own retrospective view of his collaboration with Redfield on the project (Hansen 1976).

25. My interest in Redfield is marginally affected by the fact that one of his last works, *The Little Community* (1955), began as a series of lectures originally given in Sweden (at the University of Uppsala). Redfield shows signs of having tried to familiarize himself with some contemporary Swedish folklore research (i.e. of the *Volkskunde* type), but his visit seems to have had no noticeable influence on local "general and comparative ethnography," and his host was a professor of sociology.

26. The central references are Redfield (1930) and Lewis (1951). As Margaret Mead put it—in a comment quoted in Collins (1976: 144)—"Redfield was interested in harmony; he was interested in what made things go well. Oscar, as everybody knows, was interested in what made things go badly."

27. Several essays in a posthumously published volume of Redfield's papers (1962) deal with this question. Redfield's student Charles Leslie (1960: 79–91) took it further, while still clearly under his influence, in an appendix, "The Comic Muse in Cultural Anthropology," in his brief monograph *Now We Are Civilized*— an early but significant study in writing anthropology. (It was drawn to my attention by Adamson Hoebel, when I visited the University of Minnesota in April, 1968.)

28. Much of what Redfield wrote about civilizations is in Redfield (1962). For an account of this work by one of his close collaborators, see Singer (1976). An essay by Gusterson (2005) exemplifies the later anthropological response to the "clash of civilizations" thesis as expounded by the international relations specialist Samuel Huntington (1996). On one somewhat marginal result of Redfield's visit to China, see Chapter 5, n. 21, above.

29. For an example of a project of simultaneous multi-site research, see Mazzucato (2009) on studying the Ghanaians at home and abroad.

30. As Silverman (2007) notes, in an essay on mid-twentieth-century American anthropology which also takes as its point of departure scrutiny of Powdermaker's Hollywood study, there were also several other larger group projects, some more tightly integrated than others, in this period. Wolf (2001: 388–90), for his part, sees similarities between the Puerto Rico project and the activities of the Rhodes-Livingstone Institute, which were well-integrated although not aiming so much at joint publications.

31. I recently had a conversation with one anthropologist who began working closely, and co-authoring, with a colleague when they both resided in the same European country. Then one of them moved to Scandinavia, and the other to the South Pacific. Yet my conversation partner said their collaboration could now work even more effectively, due to the difference in time zones. When one of these two had finished work for the evening and emailed the results to the other, it was morning on the other side of the world, and the colleague could begin attending to that mailing immediately.

32. In the final chapter of *Stranger and Friend* (1966: 303–5), Powdermaker also offers some intriguing remarks on generations in American anthropology as they appeared to her at the time, in the 1960s. There was a strong, perhaps even dominant, generation of people born between 1915 and 1930, who seemed to have "a need for certainty and a fear of ambiguity," and who sought "elegant models, formal rules, and neat diagrams"; "this generation likewise seems to be more truly a part of their own society, desiring to enter the Establishment rather than rebelling from it as did many members of the preceding generation." She evidently found such tendencies most strongly represented among the followers of ethnoscience, "the New Ethnography," which she did not much care for. But Powdermaker also sensed that the people born after 1930 were more interested in exploring "broader and deeper problems." Returning to the earlier part of this chapter, one could note that she wrote at a time when the veterans of the G.I. Bill may not yet have made their influence most fully felt, and before the baby boomers had charted their course.

33. She did, of course, pass some of her promising undergraduate students on to graduate programs elsewhere—for example, Eric Wolf to Columbia, and Nancy Scheper-Hughes to Berkeley.

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