EDITED BY GUSTAVO LINS RIBEIRO and ARTURO ESCOBAR

# WORLD ANTHROPOLOGIES

## DISCIPLINARY TRANSFORMATIONS WITHIN SYSTEMS OF POWER

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# World Anthropologies

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# World Anthropologies

## Disciplinary Transformations within Systems of Power

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Oxford · New York

En memoria de Eduardo Archetti, compañero
incomparable y antropólogo extraordinaire

# Contents

# Preface

This book is the outcome of an International Symposium of the Wenner-Gren Foundation for Anthropological Research, held in Pordenone, Italy, 7–13 March 2003. It reflects a process of several years involving the symposium organizers and other colleagues. Between 1992 and 1995, one of us, Gustavo Lins Ribeiro, was a member of the Advisory Council of the Wenner-Gren Foundation. This service left him with the recognition that a broader knowledge of what had been produced outside North Atlantic anthropologies was greatly needed. His intention met with enthusiastic support from Sydel Silverman, president of the foundation at the time, and Richard Fox, then editor of *Current Anthropology.* To address this issue, he formulated a first proposal for a symposium on the international dimensions of anthropology, to be held in 1996.

The project, however, was to crystallize only several years later. In 2000, at the ninety-ninth annual meeting of the American Anthropological Association in San Francisco, Arturo Escobar described to Ribeiro the idea of a "world anthropologies network" he was then beginning to develop with Marisol de la Cadena and Eduardo Restrepo at the University of North Carolina–Chapel Hill. This conception closely matched Ribeiro's previous project. The two of us had been engaged in other collaborative endeavors and decided to propose a symposium to Richard Fox, who by then had become president of Wenner-Gren.

Fox immediately supported the idea and encouraged us to go ahead with the symposium. We are grateful for his support of a project aimed at affecting our discipline on a global level. Without his encouragement and critical participation, we would not have accomplished our goals. We want to thank the Wenner-Gren Foundation for its generous support, and especially Laurie Obbink, without whose help and kind and effective presence everything would have been much harder. We also want to thank Verena Stolcke for her thoughtful and constructive role as

discussant during the symposium and Michal Osterweil, a PhD student at UNC–Chapel Hill, for her careful assistance during both the symposium in Italy and the final preparation of the book manuscript. Finally, we express our deep gratitude to each of the symposium participants for assuming the project with so much interest and commitment to the collective idea.

Parallel to our organizing the "World Anthropologies" symposium, we became increasingly involved in organizing the World Anthropologies Network (WAN), an experiment in global cooperation that has been growing over the past few years. WAN has become an inestimable source of inspiration for all of us who remained in close dialogue after the symposium: the late Eduardo Archetti, Marisol de la Cadena, Susana Narotzky, Eduardo Restrepo, Sandy Toussaint, Shiv Visvanathan, and many other colleagues, and our respective graduate students in Brasilia and Chapel Hill. We believe our anthropological practices can be much richer if we take into consideration the great variety of anthropological perspectives currently extant worldwide.

# Participants at the 2003 Wenner-Gren Symposium

Eduardo P. Archetti, University of Oslo
Eeva Berglund, independent scholar, London
Marisol de la Cadena, University of California–Davis
Arturo Escobar, University of North Carolina–Chapel Hill
Johannes Fabian, University of Amsterdam
Esteban Krotz, Universidad Autónoma de Yucatán
Susana Narotzky, Universitat de Barcelona
Paul Nchoji Nkwi, University of Yaoundé
Michal Osterweil, University of North Carolina–Chapel Hill
Gustavo Lins Ribeiro, Universidade de Brasília
Josephine Smart, University of Calgary
Verena Stolcke, Universidad Autónoma de Barcelona
Sandy Toussaint, University of Western Australia
Nikolai Vakhtin, European University, St. Petersburg
Otávio Velho, National Museum, Rio de Janeiro
Shiv Visvanathan, Centre for the Study of Developing Societies, Delhi
Shinji Yamashita, University of Tokyo

# World Anthropologies
## Disciplinary Transformations within Systems of Power

## *Gustavo Lins Ribeiro* and *Arturo Escobar*

In this book we explore the diversity of anthropologies being practiced around the world in the early twenty-first century and the ways in which the pluralizing potential of globalization might enable anthropologists worldwide to benefit from this diversity. Some of the discipline's most fundamental transformations in the twentieth century were due to changes in the subject position of its "object of study" par excellence—native groups all across the planet. After the various rounds of critique in the field over the past decades, we are convinced that the present can be another moment of reinvention of anthropology, this time mostly linked to changes in the relationships among anthropologists located in different parts of the world system. A smaller world has meant an increase in international exchanges of knowledge. In consequence, we are enthusiastic about the possibility of establishing new conditions and terms of conversability among anthropologies on a global level. We intend this volume as a contribution to the making of a new transnational community of anthropologists. As such, it is part of a larger endeavor that we call the "world anthropologies" project.[1]

We see a tremendous transformative potential in embracing this project. Whether it is conceived of in terms of diversifying anthropological practices while maintaining a unified sense of the field or in terms of adumbrating a "post-anthropological era" in which the idea of a single or universal anthropology is called into question, we believe great gains are to be made by opening the discipline up to new possibilities of dialogue and exchange among world anthropologies.

Doing so, however, requires significant epistemological and institutional changes in current practices. It should be clear from the outset that any inclusive and participatory movement that aims to increase diversity is bound to disturb those who have benefited from its absence.

Anthropologists have always been prone to internationalism, because anthropological research in many places has often meant traveling abroad, and anthropology itself developed through the worldwide dissemination and expansion of Western university systems. And ours is not the first discussion of the discipline's international dimension (see, for instance, Cardoso de Oliveira 2000; Fahim 1982; Kroeber 1953; and issue 47 of *Ethnos*). The present endeavor is different in four main ways. First, we believe that with globalization, heterodox opportunities have opened to the academic world. Second, we believe that through concerted political action, a more heteroglossic, democratic, and transnational community of anthropologists can come into existence. Third, we write from no particular national viewpoint. And fourth, we believe we can understand the dominance of some styles of anthropology only if we relate them to unequal power relations. Such a perspective may well stem from our own locations and experiences within the academic world system. Both of us carried out our doctoral studies in North American universities. One of us, Escobar, has long worked in U.S. universities while maintaining strong ties with Colombian anthropology; the other, Ribeiro, has long worked in Brazilian universities while maintaining strong ties with North American anthropology.

## Changing World Systems: Anthropologies and Diversity

Applying the Wallersteinian notion of "world system" to the investigation of the character of the social sciences and the academy suggests that these, too, are structured by power relations and by Eurocentric capitalist expansion (Gulbenkian Commission 1996). This geopolitical and epistemological argument may be reflected, for instance, in notions of "peripheral anthropologies" (Cardoso de Oliveira 2000) and "anthropologies of the South" (Krotz 1997). More recently, it has appeared in the work of the Japanese anthropologist Takami Kuwayama, who, inspired by the Swedish anthropologist Tomas Gerholm's (1995) notion of a "world system of anthropology," argued that the United States, Great Britain, and, to a lesser extent, France constituted the core of such a system: "Even though there are internal differences, their collective power is such that other countries, including those in the rest of Europe,

have been relegated to the periphery" (Kuwayama 2004a: 9). He wrote further:

> Simply put, the world system of anthropology defines the politics involved in the production, dissemination, and consumption of knowledge about other peoples and cultures. Influential scholars in the core countries are in a position to decide what kinds of knowledge should be given authority and merit attention. The peer-review system at prestigious journals reinforces this structure. Thus, knowledge produced in the periphery, however significant and valuable, is destined to be buried locally unless it meets the standards and expectations of the core. (2004: 9–10)

Kuwayama was incredulous about explanations that posited a language barrier as the main factor hindering the dissemination of knowledge produced in the periphery (2004a: 27–29). Aware of the problems arising from dualistic readings, he recognized the complexity of center-periphery intra- and interrelations and the existence of elites in the periphery closely connected to those of the center (2004a: 49–46).

The world systems approach has recently been enriched by two other important perspectives: those of the "geopolitics of knowledge" and the "provincializing Europe" projects. The geopolitics of knowledge is a notion developed by Walter Mignolo (2000, 2001, 2002) as part of a radical critique of Eurocentric readings of modernity on the basis of the concepts of "border thinking" and "coloniality of power" (see Escobar 2004a for an extended presentation of this perspective). In close dialogue with the Peruvian sociologist Aníbal Quijano and the Argentinean philosopher Enrique Dussel (see, for instance, Quijano 1993; Dussel 1993), Mignolo related economic geopolitics to the geopolitics of knowledge in order to stress the idea that the locus of enunciation of the disciplines is geopolitically marked. Eurocentrism can be transcended only if we approach the modern colonial world system from the exterior, that is, from the colonial difference (modernity's hidden face). The result of such an operation is the possibility of embracing epistemic diversity as a universal project—that is, embracing something we might call "diversality," a neologism that reflects a constructive tension between anthropology as a universal and as a multiplicity.

Whereas the world anthropologies project incorporates diversity as a key notion for global cross-fertilization, we also associate our project with Dipesh Chakrabarty's attempt to "provincialize" Europe—that is, to show that European thought and experience are particular and historically

located, not universal as is generally assumed. For Chakrabarty, "European thought is at once indispensable and inadequate in helping to think through the experiences of political modernity in non-Western nations" (2000: 16). While transcending Eurocentric modernity is one of his goals, he asserts that the project of provincializing Europe

> does not call for a simplistic, out-of-hand rejection of modernity, liberal values, universals, science, reason, grand narratives, totalizing explanations, and so on ... It cannot originate from the stance that the reason/science/universals that help define Europe as the modern are simply "culture specific" and therefore belong only to the European cultures. For the point is not that Enlightenment rationalism is always unreasonable in itself, but rather a matter of documenting how ... its "reason," which was not always self-evident to everyone, has been made to look obvious far beyond the ground where it originated. (2000: 42–43)

In his dialogical stance, Chakrabarty avoids a romantic, dualistic position, because he recognizes that without Enlightenment universals "there would be no social science that addresses issues of modern social justice" (2000: 5). However, he also underscores the fact that in a world of globalized scholarship, the translation of a multiplicity of forms of understanding life into universalist European categories is problematical (2000: 17).

These debates inform our understanding of world anthropologies and resonate throughout the present volume. They also place us immediately within fields of global power, that is, in spaces shaped by unequal exchanges between hegemonic and nonhegemonic centers. In this connection, we see the world anthropologies project as one of establishing and consolidating new modes of relating among different anthropologies, modes that will result in the enrichment of theory beyond what is possible in the present ossified structure of the anthropological world system, which hinders more complex forms of cross-fertilization.

As many of the contributors to this volume make clear, ignorance of the magnitude, complexity, and diversity of international anthropological production is a common problem everywhere. How can anyone make a complex synthesis of world anthropologies' contributions to epistemology, theory, and methodology if we know so little about them? Such ignorance is a crucial part of the current problem. This is why the world anthropologies project also requires concrete initiatives (see note 1) for fostering awareness of other trends in anthropological knowledge and for granting them visibility. Information and communication

technologies today enable more horizontal communication among anthropologists worldwide and, consequently, more complex modes of exchange.

We should emphasize, however, that given its ultimate objectives, the world anthropologies project is more accurately seen in terms of a political and theoretical stance called "interculturality" than in terms of multiculturalism. Consider Néstor García Canclini's (2004: 15) statement on this issue:

> Multicultural conceptions admit the *diversity* of cultures, underscore their differences, and propose relativist policies of respect that often reinforce segregation. Dissimilarly, interculturality refers to confrontation and entanglement, to what happens when groups establish relationships and exchanges. The terms suppose two [different] modes of production of the social: *multiculturality* supposes acceptance of what is heterogeneous; *interculturality* implies that those who are different are what they are in relations of negotiations, conflicts and reciprocal loans.

In this connection, we advocate that all anthropologies (including, of course, the hegemonic ones) are capable of dialogically contributing to the construction of a more heteroglossic and transnational knowledge. We do not claim that the pluralization of power, histories, and knowledge is a goal in itself. Rather, we see it as a step toward a post-identitary politics (Clifford 1998) under the umbrella of diversality.

It is not our intention to propose an abstract model of what world anthropologies should be. Rather, we hope to foster debates and new modes of interaction among scholars and all those interested in diversity by suggesting political and social opportunities and means that might enable more complex forms of global anthropological scholarship. Even if we wanted to do so, it would be impossible to write a synthesis of the plural contributions of unknown histories or of histories of collaborations that are yet to happen. This book is one of the few works in English in which a variety of world anthropologies is discussed (we say more about language later). We need many other volumes to make fully visible to global audiences the "anthropologies without history," to use Esteban Krotz's ironic expression (1997: 240), and what they might have to offer in the construction of a plural anthropological knowledge. We also need to foster more heterodox initiatives of scholarly networking and publishing (especially translations) if we are to benefit from the internal global diversity of our own field of scholarship. In short, rather than offering purely theoretical assumptions about how world anthropologies might be, we argue that changes in the communicative practices and

modes of exchange among world anthropologists will result in changes to and enrichment of the epistemological, theoretical, methodological, and political horizons of the discipline.

Ours is not a project of enriching hegemonic anthropologies but of creating other environments for the (re)production of the discipline, environments in which diversality might lead to a richer set of anthropological perspectives. Our critical standpoint on the monotony and incompleteness of the current international anthropological landscape, as it has come to be structured by hegemonic forces, stems from the conviction that assuming their own diversity is a crucial (political, scholarly) strategy for anthropologies if they are to reproduce and enhance themselves in a globalized world. Why should we cherish heterogeneity and diversity more than homogeneity and uniformity in anthropology? We should do so not only because we are professionally sensitive to issues of cultural and political difference but also because, as scholars, we know that diversity and creativity feed on each other, that a greater pool of different perspectives represents greater capacity for invention (see, for instance, Lévi-Strauss 1987 [1952]).

The world anthropologies project thus aims at pluralizing the existing visions of anthropology at a juncture in which hegemonic, North Atlantic–centric discourses about difference prevail. It stems from a recognition that this is the right moment to discuss transformations of the field worldwide. In sum, "world anthropologies," as a concept, a project, and a book, is a contribution to the articulation of diversified anthropologies that are more aware of the social, epistemological, and political conditions of their own production. Toward this end, our volume has two interrelated goals: first, to examine critically the international dissemination of anthropology as a changing set of Western discourses and practices within and across national and international power fields; and second, to contribute to the development of a plural landscape of anthropologies that is both less shaped by metropolitan hegemonies and more open to the heteroglossic potential of globalization. We also see this effort as part of a critical anthropology of anthropology, one that decenters, rehistoricizes, and pluralizes what has been taken as "anthropology" so far.

## Disciplinary Transformations

Tight connections always exist between world systems of power, the development of social theory, and changes in particular disciplines such as anthropology. The various critiques of the discipline of the past

few decades have made us newly aware of these interrelations. Since its inception, anthropology has been deeply linked to the dynamics of the world system, mediated by questions of colonialism, imperialism, nation building, and the changing role of otherness in national and international scenarios. As Krotz (1997) put it, anthropology reflects regional, national, and international "structures of alterity." The connection between anthropology and world politics applies to all anthropologies, often in contrasting ways, and it applies with particular poignancy to hegemonic anthropologies. By hegemonic anthropologies we mean the set of discursive formations and institutional practices associated with the normalization of academic anthropology chiefly in the United States, the United Kingdom, and France (see Restrepo and Escobar 2005).

The crisis of hegemonic anthropologies after the 1960s, brought about by decolonization, anti-imperialist struggles, the civil rights movement, and the rise of Third World nationalisms, is well known. Anthropology's "age of innocence" (Wolf 1974) came to an end as the relationship between knowledge and power became more explicit. Critiques of anthropology became a "literature of anguish" (Ben-Ari 1999: 400), intensifying the ambivalence of anthropology's self-representation (Wolf and Jorgensen 1975) as either an ally of imperialism (Gough 1975), a child of violence (Lévi-Strauss 1966), or a revolutionary field always ready to question Western claims to superiority (Diamond 1964, quoted in Wolf and Jorgensen 1975). In some of the most farsighted ensuing critiques, one finds a questioning of the epistemological, institutional, and political foundations of Anglo-American anthropology. Some critics (see the contributions by Hymes, Scholte, and Diamond in Hymes 1974) even questioned the permanence of "departmental anthropology" and opened a discussion about moving toward a non-academic anthropological practice. Others argued for an emancipatory anthropology that would begin by recognizing that all anthropological traditions are culturally mediated and contextually situated (Scholte 1974). These efforts constituted a critical anthropology of anthropology, and to this extent we may find in them the notion of "world anthropologies" *in statu nascendi*.[2]

Later, other critics argued for a radical anthropological praxis sensitive to the liberation struggles of Third World peoples (e.g., Harrison 1991) or for the development of indigenous anthropology as a partial corrective to anthropology's Eurocentrism (e.g., Fahim 1982). Even better known are the analyses and proposals of the 1980s that focused on the kinds of representations embedded in realist ethnographies,

with a concomitant call for reflexivity, a questioning of ethnographic authority, and innovations in "writing culture" (Clifford and Marcus 1986; Marcus and Fischer 1986). This "postmodern" moment, as it came to be labeled by some of its critics, influenced an entire critical trend in regard to the prevailing objectivist, normative, and essentialist conceptions of "culture," emphasizing, conversely, the historicized, polyphonic, political, and discursive character of any "cultural fact" (e.g., Comaroff and Comaroff 1992; Dirks, Eley, and Ortner 1994: 3–4; Gupta and Ferguson 1997a; Page 1988; Rosaldo 1989).

Seen in retrospect, these critiques had their limits. As the South African anthropologist Archie Mafeje wrote (2001: 54–66), they generally took for granted the academic environment in which anthropology existed, and they sheltered taken-for-granted academic practices (see also Fox 1991; Kant de Lima 1992). Most critics failed to see the role of the colonized in decolonizing forms of knowledge, and they were largely silent about nonhegemonic anthropologies (Mafeje 2001). Some of these drawbacks were addressed in feminist critiques of the book *Writing Culture,* including the subsequent debate over feminist ethnography (see, e.g., Behar and Gordon 1995; Gordon 1988, 1991; Knauft 1996: 219–48; Visweswaran 1994). Participants in this trend rightly articulated insights from earlier Marxist-feminist anthropology and the critique of epistemology coming from feminist theory and so-called postmodern anthropology with the social critique coming from women of color and Third World women. In addressing the question of "what it means to be women writing culture," they joined a critical epistemological reflection—including the relationship between anthropology and feminism (echoing an older argument by Strathern [1987])—with a political reflection on power relations among women. These were important steps toward a world anthropologies perspective.

Generally speaking, the question of the diversity of anthropologies has not been adequately treated in previous critical analyses. International cross-fertilization, for instance, has often involved a limited universe of exchanges. Although the diversity of faculty members' and researchers' nationalities may have increased in some locations (in the US academy, for instance), this increase has rarely corresponded to an active incorporation of diverse anthropological productions and theories. This is crucial because, as Krotz put it (2002: 399), "in spite of the fact that the main impulses for the production of anthropological knowledge continue to come from the countries where this science originated, such impulses are also increasingly happening in places where those who until recently were the favorite anthropological objects live. This

requires the creation of new structures of knowledge production that … do not subordinate cultural diversity to a [unique] model."

## Anthropologies and Transnationalism

Most anthropologies have always been transnational to a greater or lesser extent. Nevertheless, as Eduardo Archetti put it in his contribution to this volume, the consolidation of an "international anthropology" has usually been an affair of the "center," rather than of the peripheries. Yet the different histories of world anthropologists' immersion in transnational processes—and the resulting disciplinary mutations—cannot be easily accommodated in a simple narrative of imposition, diffusion, borrowing, adaptation, or contestation. Transnational dynamics in the world system have acted as both unifying and differentiating mechanisms, depending on many factors, from nation building and national structures of alterity to institution building and opportunities for exchanges. In what follows, we restrict our remarks to the most general aspects of transnationalism in anthropology.

Russia, Japan, China, and, in Latin America, Peru and Mexico offer revealing cases of transnational dynamics in anthropology and their relevance for world anthropologies, as illustrated in the chapters in this volume by Nikolai Vakhtin, Shinji Yamashita, Josephine Smart, Marisol de la Cadena, and Esteban Krotz, respectively. The famous Jesup North Pacific expedition to Siberia (1897–1902), led by Franz Boas, is an early and telling example of the role of transnational connections in the development of a national anthropology. During this period, Western and Russian ethnology developed in tandem, reinforcing each other. Then, from 1917 until 1989, this transnational dimension was significantly altered. What developed in Russia during the Soviet period was an altogether different kind of anthropology, which had its avatars with changing political regimes. After the collapse of the Soviet Union, the arrival of relatively large numbers of foreign scholars to do fieldwork in Siberia again changed many aspects of anthropological research, from the objects of study to theoretical paradigms, affecting existing Siberian anthropology in different ways. Rather than a purely Western set of agendas asserting itself, these more diverse practices seem conducive to greater anthropological plurality.

There is perhaps no clearer case of the way in which empire, nation, and transnationalism have intersected to foster a different anthropological trajectory than that of Japan. In this development, there is no direct link between, say, an international influence and the turn taken by

anthropology. Despite being under the influence of Western hegemony, Japanese anthropology developed into a distinctive entity in ways that often escaped the awareness even of Western anthropologists working in Japan. Chinese anthropologies have seen much more limited development, but they, too, are marked by the interaction between nationalism, transnationalism, and reactions to Western hegemony. A principal factor has always been an ambivalence toward anthropology, motivated by its Western origin. As Smart writes in chapter 4, despite increased internationalization of Chinese anthropology after the 1980s, no linear path led toward its integration into a Western-dominated discipline. On the contrary, the forces pushing toward indigenization of the field on the basis of national priorities—specifically, the priority accorded to rural development and ethnic minority studies—continue to be important.

Anthropologies in Mexico and Peru have been significantly influenced by their connection with hegemonic anthropologies, particularly those of the United States. Less well known is that they have also been largely shaped by networks that developed specifically out of Latin American experiences. The sizable presence of indigenous peoples, strong nation-building projects, and persistent debates over race and culture in both countries enabled national and transnational institutional developments that greatly conditioned their anthropologies. As de la Cadena shows, one of the most formative elements was the articulation since the 1920s of an inter-American network of intellectuals, with its most important hubs in Peru and Mexico, gathered around an anti-imperialist, Indo-American or *indigenista* project built on shared pre-Columbian and Hispanic pasts. The intersections between this network and North American and French anthropologies were complex but not inconsequential for all parties—for instance, there was a tacking back and forth between Latin American notions of *mestizaje* and North American theories of "acculturation." A different rendition of the histories of these two Latin American anthropologies indicates that although they certainly borrowed and adapted notions from their hegemonic counterparts, they had moments of autonomy, creativity, and independence—that is, moments of being "world anthropologies." With their radical agendas, today's inter-American networks of indigenous politics are posing questions for a politics of world anthropologies perhaps of greater relevance than those arising from the centers.

Kirin Narayan's formulation that anthropologists are best seen today "in terms of shifting identifications amid a field of interpenetrating communities and power relations" (1993: 671) is well supported by

the cases discussed in this book. Some of them show that although anthropologies have always been prone to transnationalism, in the past they have been unable to tap the vast potential that the globalization of the discipline has created. World anthropologies may thrive under the conditions existing at present. We hope it will be increasingly clear that whether one is talking about Russia, Japan, China, Mexico, or Peru, what is at stake is less the maintenance of dichotomies such as Western and non-Western or central and peripheral but, as Yamashita puts it, the consideration and creation of common spaces in which anthropologies have met and can meet in the future in order to foster the pluralization of the discipline, even under the pressure of particular hegemonies. Transnational networks and events along these lines can become important elements in fostering the perspectives of world anthropologies.

## Uneven Relations: Inverted Provincialisms and Cosmopolitanisms

The existence of anthropologies totally isolated from Western anthropologies is an impossibility even under authoritarian regimes, as again the cases of China and Russia exemplify at various historical moments. Even nativist perspectives have had to go through a process of validation that is largely Western mediated. Conversely, the fact that anthropology expanded from the North Atlantic to other corners of the world does not mean it has been uninfluenced by its many developments elsewhere. We agree with Yamashita (1998: 5) that "if cultures travel, as James Clifford (1992) puts it, anthropology travels too. Through traveling the world, it can be enriched and transformed by its encounters with different local situations." But it is also true that there are different travelers and ways of traveling. Hierarchies of knowledge are predicated upon hierarchies of social and political power. Furthermore, it is necessary to keep in mind that the international circulation of ideas may "have the effect of constructing and reinforcing inequality" (L'Estoile, Neiburg, and Sigaud 2002: 23).

Uneven exchange of information and anthropological diversity are often depicted under different labels: central versus peripheral anthropologies (Cardoso de Oliveira 2000); anthropologies of nation building and of empire building (Stocking 1982); hegemonic and nonhegemonic anthropologies (Ribeiro and Escobar 2003); anthropologies of the South (Krotz 1997), and so forth. Such classifications are helpful in thinking about some characteristics of existing inequalities. Yet as Verena Stolcke

remarked during our symposium, they are insufficient for understanding the current transnational orders.

This is especially the case with Stocking's well-known distinction between anthropologies of nation building and anthropologies of empire building. This distinction implies that practitioners of so-called imperial anthropologies prefer research abroad over research at "home." The role of anthropology in nation building is well known (for the Brazilian case, see, for instance, Peirano 1991a). Suffice it to say that anthropologists often contribute to (re)creating ideologies of national unity or diversity, anchored in academic authority, that are reflected in the educational and cultural policies and interventions of states and nongovernmental organizations. As Sandy Toussaint shows so vividly in chapter 11, the dilemmas Australian anthropologists face concerning their authority in Aboriginal land claims exemplify the intricate relations between anthropology, state apparatuses, and the discipline's self-representation, especially regarding its scientific status.

Furthermore, the limits of this distinction may well be transcended if we remember that in modern times, behind empire building there has always been a nation-state. Indeed, anthropologies of empire building are also anthropologies of nation building, although the converse is not necessarily true. Moreover, there are cases in which "national anthropologies" became internationalized without becoming empire-building anthropologies, such as the Australian, Brazilian, Canadian, and Mexican cases. Writing about Brazilian anthropology, the Portuguese anthropologist João de Pina Cabral (2004: 263) suggested a fifth tradition, different from the American, British, French, and German ones, a tradition "that identifies itself with none of the imperial projects that have historically moved scientific development." Eduardo Archetti, in this volume, also shows that a hegemonic anthropology such as that of France may be geared toward nation and empire building at the same time. The Japanese example is interesting in that it shows that a given anthropology may shift between being national and imperial— and indeed, today, post-imperial—over time, depending on external constraints (Askew 2003).

This means that one can envision the development of postnational and post-imperial anthropologies on the basis of key power reversals (Ribeiro 2003). For the Latin American cases, given the need to "provincialize" the United States, one might envisage research projects focused on North American subjects, especially those enacting ideologies of power and prestige. It is time for Latin Americans to do research on the elites of the North, studying "up" in more ways than one (Ribeiro 2003,

2005). From a different perspective, in addressing Stocking's dichotomy, Otávio Velho (chapter 13) expresses concern over the possibility that Stocking's classification might lead to neo-orientalist perspectives that posit "national anthropologies" as the source of sui generis alternatives and knowledge. It is as if, given a certain crisis of imagination in the "center," the inventiveness of the "periphery" were the solution for a new round of North Atlantic hegemony.

Unlike exoticizing positions, our critique of the unequal exchanges among anthropologies supposes going beyond existing power structures to open the way for heteroglossic and dialogic cross-fertilizations stemming from the many other existing subject positions. At some levels, there still exists an unmarked, normalized, and normalizing model of anthropology that militates against this project and creates a sort of asymmetrical ignorance (Chakrabarty 2000). We see such asymmetry in terms of the tension between what we call "metropolitan provincialism" and "provincial cosmopolitanism."

Metropolitan provincialism is the ignorance that anthropologists in hegemonic centers have of the knowledge production of practitioners in nonhegemonic sites. Provincial cosmopolitanism refers to the often exhaustive knowledge that people in nonhegemonic sites have of the production of hegemonic centers. An expression of this asymmetrical ignorance is the fact that whereas the history of universal (i.e., hegemonic) anthropologies is studied in depth in nonhegemonic sites, the development of "anthropologies without history" is seldom taught in hegemonic centers—and often not even in those anthropologies' own countries, where the "classics" include only works by foreign anthropologists.

Metropolitan provincialism and provincial cosmopolitanism need to be understood in relation to the language issue. In an unpublished paper, the Brazilian sociologist Renato Ortiz (n.d.) showed how the English language framed sociological debates worldwide. He concluded that "the more central a language is in the world market of linguistic goods, the smaller the proportion of texts which are translated to it" (p. 27). In the United States and the United Kingdom, he found, fewer than 5 percent of publications were translations from other languages, while in France and Germany the figure was around 12 percent, and in Spain and Italy it grew to 20 percent. This points to the sociolinguistic basis of metropolitan provincialism.

It is doubtless that English has become both a hegemonic language and the main global means of intellectual communication. However, this should not lead us to overlook the existence and important roles of

regional language markets in, say, Spanish, Arabic, or Chinese. Linguistic diversity is part of any world anthropologies project. As Rainer Enrique Hamel (2003: 24) warned, "scientific monolingualism might not only deepen the existing inequalities in the access and diffusion of scientific findings" but also threaten scientific creativity and conceptual diversity. Hegemonic anthropologies often testify to this threat. Many cases bear out Hamel's observation that "most authors from English-speaking countries and their former colonies who write about the world as a whole do so without quoting a single non-English language text in their vast bibliographies" (2003: 20). This is a particularly pressing problem in a discipline that praises diversity.

Polyphony in anthropological production should above all entail the recognition of the sizable production of anthropological texts in different parts of the world and the deliberate adoption of steps to remedy existing imbalances. The translation of more works into English is a necessary but insufficient step in gaining access to the global production of anthropologists. We also need to promote the dissemination of the works of nonmetropolitan anthropologists in languages other than English to further enhance horizontal exchanges. We need more heterodox conversations and meetings, channels of communication that can be meaningful and interesting to authors everywhere, in order to construct and consolidate more heterogeneous professional networks and projects. It is time to strive for polycentrism and heteroglossia in lieu of monological hegemony. A world anthropologies perspective is precisely about reworking existing divides without obliterating the real differences that exist.

## Epistemological and Disciplinary Predicaments

The social and institutional contexts within which anthropologists operate have changed significantly over the past two decades as a result of heightened transnationalism, neoliberal pressures, and rapid globalization (Berglund, this volume). The same can be said of the epistemological concerns that occupy anthropologists in many parts of the world. The concept of world anthropologies has a special niche for questions of knowledge and alterity, because their potential for triggering animated theoretical and political reflection continues to be great.

Much has been written, for instance, about the relation between anthropology and colonialism. The various reactions of scholars outside the Euro-American centers to anthropology's universalist pretensions

and its association with Eurocentrism have been less studied. In Africa, this issue has resulted in a debate over the need for African knowledges and epistemologies. With more intensity than in India, where anthropological debates took place in the context of postcolonial discussions of nation building (Visvanathan, this volume), in Africa calls for native epistemologies were marred by the paradox, noted by Mafeje (2001), that there is nothing more Western than discussions about epistemology. Otávio Velho suggests in his chapter that claims to authenticity may be a kind of self-imposed orientalism and that the absence of foreign scholars specialized in Brazil might have positively contributed to hindering the development of nativist approaches in that country. The question of alternative epistemologies, however, is far from settled. For Shiv Visvanathan, it is imperative to recognize the existence of a multiplicity of times, livelihoods, and epistemologies as the basis of plural world anthropologies.

So where does one start a discussion about epistemology? The farthest-reaching arguments seem to place anthropology within the structures of modernity. Anthropology, it has been said, is an integral part of the modern intellectual division of labor among the academic disciplines, which accorded it the "savage slot" (Trouillot 1991)—that is, the role of studying the "primitive," or what was not "the West." This division of labor is a fundamental feature of the modern episteme (in the Foucauldian sense, that is, referring to the existence of a structure that determines the character of knowledge without the knowers' consciousness of it). Modern knowledge is based in logocentrism, that is, the belief in logical truth as the only valid foundation for a rational knowledge of the world—a world made up of knowable and orderable things (Heidegger 1977; Vattimo 1991). Modern knowledge is also Eurocentric in that it suppresses and subalternizes the knowledges of those seen as lying outside the European totality (what Mignolo [2000] and others have referred to as the "coloniality of knowledge"). It is in this broad context that we can most fruitfully locate discussions of epistemology and, more radically, epistemic difference in relation to world anthropologies.

Questions about knowledge are questions about modernity. To put it bluntly, hegemonic anthropologies, like the rest of the social sciences, have fallen into the trap of believing that there is nothing outside modernity. It is all very well to show how modernity is negotiated, contested, and hybridized on the ground throughout the world, as many ethnographers based in "center" countries have done with eloquence; yet these ethnographies still shelter the idea that modernity is an

inescapable universal (Kahn 2001). This might well be the case, but there are clues in the practices of many social actors that caution against this widely held assertion (Escobar 2004a, 2004b; Mignolo 2000). We are not talking only about the many cases of indigenous knowledge—for instance, in fields such as collective land claims, ethnic rights, and biodiversity conservation or in the rich debates over intellectual property (e.g., Strathern 1999a). In these cases, as Toussaint shows in her analysis of Aboriginal land claims in Australia, anthropologists are confronted not only with multiple knowledges produced in multiple places and going in multiple directions, up and down the power hierarchy, but also with issues of translation and (in)commensurability that are most relevantly seen in epistemological and epistemic terms. Today, as Toussaint demonstrates, these issues are pushing anthropologists in unprecedented directions, which we associate with possibilities for world anthropologies.

Debates over interculturality among members of indigenous and ethnic social movements and certain institutions (e.g., UNESCO) bring questions of translation and (in)commensurability to the fore. When, in the 1960s, the Peruvian writer cum anthropologist José María Arguedas articulated a practice that was at once modern and nonmodern, Western and not, which he refused to see as "hybrid," he was engaged in a multiple ontologism fueled by magic and reason alike, and he made an early statement on this problematic (de la Cadena, this volume). In peripheries, where the hold of logocentrism is weaker than in the center, there is more room for plural epistemological-political debates; at this level, for instance, liberal multiculturalism in the United States finds a counterpart in radical interculturality in the Andes. The articulation of modernity and indigeneity that the notion of interculturality presupposes, as de la Cadena writes, poses an ontological and epistemic challenge to the assumption of an all-determining modernity. Intercultural subjectivities might not necessarily be on their final and definitive journey toward modernity, even if they might find sources of value and even allies in many of modernity's features. This is why, for many indigenous and ethnic movements, the creation of alternative knowledges and even alternative centers of knowledge (like the innovative Intercultural University in Quito, established by members of indigenous movements) is an important aspect of their struggle. At stake here is the premise that the world is populated by a multiplicity of times, livelihoods, and epistemologies; this seems to be clearer today to social actors engaged in struggles for cultural difference than to many academics. As a dialogue of cultures in contexts of power, interculturality presents anthropologists

with politically rich opportunities for epistemological engagement and for practicing anthropologies in the plural. This notion also applies to the multiple modernities within the West, as Eeva Berglund's analysis of the British case (chapter 9) exemplifies.

This does not mean that modernist epistemologies and politics based on realist assumptions are irrelevant. Susana Narotzky forcefully states in her chapter that it is impossible to do away completely with universals or to eschew unitary frameworks, in order for shared political projects to take place. Yet at this level, the conditions for the politics of epistemology and the epistemology of politics have also changed. First, it is now accepted that there is a plurality of politically engaged possibilities for knowledge production—that is, for projects of social transformation against hegemonies on the basis of alternative orders of knowledge. From a world anthropologies perspective, even notions of political engagement within realist paradigms would challenge long-established anthropological practices such as the objectification and detachment characteristic of much participant observation. At the same time, this perspective would elicit notions of responsibility that go beyond engagement during the field stay. Researchers working from a world anthropologies perspective would consider an array of diverse knowledges with diverse political agendas, all of them situated in their respective processes of production.

Cognitive pluralism has certainly existed throughout history, as Archetti shows in the case of the French colonial anthropologist Michel Leiris (see also Nugent's discussion [2002] about the persistence of alternative canons throughout the history of the discipline). Were this pluralism to become more prominent today, it might be an effective antidote against the most homogenizing forms of professionalization.

## Anthropology Today and World Anthropologies

Anthropology, as Visvanathan says in his chapter on India, is not only a Foucauldian practice but also "a compendium of alternative dreams"—an open-ended project of entertaining multiple possibilities for both itself and the world. This statement applies to all anthropologies, to a greater or lesser extent, and at various points in their variegated histories. All anthropologies have had their dissenting figures, alternative knowledges, failed experiments, and occasional epochs of creativity and revolt. It could be said that in most times and most places, anthropology (in the singular) has functioned within established bounds even if, as many of our authors contend, the histories of the field are not exhausted

by scientific, institutional, or market logics. But it is in moments of marginality, dissent, or daring creativity beyond those bounds—and before such moments get domesticated or normalized—that we can see more clearly elements of, and for, world anthropologies.

To give an example, again from Visvanathan's chapter, it is in the rootedness, eclecticism, and plural imagination of the development anthropology of the Lucknow school—before it was defeated by the anthropology of the Delhi School of Economics, with its dreams of planned development after independence—that we find a plural land-scape of anthropological possibilities. To give another example, in treating the work of the French Indologist Louis Dumont as an open text, whether in India, Norway, or Brazil, as several of the contributors argue, practitioners moved in open-ended directions. In doing so, they worked through unresolved predicaments or absences in the Dumontian conceptualization.

It may well be that the concept of "world anthropologies"—as distinct from "international anthropology"—is a problematic for which we have not yet found enough questions, if by this we mean the fuller emergence of an anthropological space that operates as a multiplicity more than as a single authoritative practice, a shared matrix, or a contested universal. These issues are underscored in Johannes Fabian's chapter. The questions he asks about how world anthropologies might challenge the established who, what, where, when, and how of the discipline are a place to start. To this list we need to add the crucial "what for," that is, the ethico-political question, What is anthropology for?

We also need to learn to ask questions that arise from beyond the regnant academic domain. The least we can say in this regard is that the cultural-political actors from outside the center who have emerged over the past two decades make it clear that they are producing knowledge about cultural and social processes that must be taken seriously.[3] There is little indication that hegemonic anthropologies are broaching this issue. To begin with, as we know, anthropologists from the metropolises have rarely taken seriously their counterparts in the peripheral countries where they work. Several of the contributors to this book make observations, if not complaints, about this failure, whether about local anthropologists being taken merely as informants by their colleagues from the centers or about their writings or political positionings being ignored.

To develop world anthropologies means that the "where" of the discipline must undergo a radical change. No longer "elsewhere," the sites of anthropological work could certainly be "here and elsewhere" and their interconnections. It remains to be seen whether the pluralization

of the place of anthropology will mean its definitive release from the grip of the "savage slot." If this were to be the case, then anthropology in the singular (that is, as a modern form of expert knowledge concerned with otherness) would ultimately break free from the international division of intellectual labor established since the end of the eighteenth century, and it might actually be the first field to do so successfully (Restrepo and Escobar 2005). The result would be a plural landscape of world anthropologies no longer constrained by the universals of modernity but aimed at a variety of competing and open-ended universalisms and struggles around them.

The multiplication of subjects, field sites, and knowledge producers has also been at the heart of anthropology's transformation. We suggest that one of the answers we should give to the "what" question—that is, What should we study?—in order to move toward world anthropologies is "each other." This requires, first, the writing of the histories of those "anthropologies without history." As many of the contributors to this volume indicate, most versions of the history of anthropology are histories of the centers; we know little about the histories of nonhegemonic anthropologies and of nonhegemonic practices. Luckily, in some centers, feminists and ethnic minorities, such as Native Americans and African Americans, have begun writing these other histories. Turning to each other with an attentive eye to epistemic, epistemological, and political differences is a sine qua non for world anthropologies, that is, for decentering the existing world system of anthropology. Along the way, we may discover other topics of anthropological relevance and other methods and perspectives with which to study them. So the "how" of anthropological practice should also be affected by this change in the "what." The world anthropologies project aims at the construction of polycentric theoretical frameworks. Such a move, like polycentric multiculturalism (Shohat and Stam 1994; Turner 1994), calls for a reconceptualization of the relationships among anthropological communities.

This brings us to the last question, the "what for" of our practice. And there, despite the fact that most anthropologists in the world are politically oriented, we find much contention and disagreement. Ideas range from the need to produce knowledge that is applicable to pressing human needs or relevant to particular political situations to the more open-ended goal of knowledge for radical social transformation. Few, if any, of this volume's authors would endorse Clifford Geertz's famous answer, "to enlarge the universe of human discourse" and to make available to all the answers that others have given to cultural

predicaments so as "to include them in the consultable record of what man has said" (1973: 30). Few, moreover, would stop at the by-now standard poststructuralist position of critically analyzing the claims to truth embedded in particular discourses and practices and how they function to authorize particular agendas. These positions are meaningful but insufficient. But there is no clarity about how to go farther, and certainly no single answer that is good once and for all.

One example of what anthropology might be for is described by Paul Nkwi in his chapter. He argues that the turn toward applied development anthropology in Africa was a means to rehabilitate a discipline so tainted by colonial administration and thought as to be useless—even an impediment—for nation building; it was also a way to have an effect on development interventions. For Nkwi, African colleagues did not anguish much over the academic-versus-applied debate as they attempted to develop a professional and political practice under existing constraints. As a result, the divide between the two approaches was weakened.

Other African academics argue for the need to subvert the existing politics of knowledge and to take plural, place-based anthropological discourses seriously. The concept of a "post-ethnological era," proposed by Mafeje (2001), points in this direction. Mafeje suggests a number of moves in this regard, including the development of a deconstructionist approach from an African perspective; nondisciplinarity—a sort of free borrowing from any field without concern for disciplinary rules or methods; a nonepistemological approach, beyond the adherence to a general "discursive method"; a new practice of ethnography as made up of the subject's own texts; and a "post-ethnological" approach to theory building—one that goes beyond anthropology's objectifying and classifying imperatives. New practices such as these, though debatable, might produce "new styles of thinking and new forms of organization of knowledge" (Mafeje 2001: 60; see Restrepo and Escobar 2005 for a discussion of this proposal).

Berglund and Toussaint both describe how the turn toward non-academic, policy-driven, and at times politically oriented work in the United Kingdom and Australia, respectively, arose from peculiar sets of pressures. In these cases, convergences have taken place between anthropological subjects and matters of heightened public concern, such as indigenous land titles in Australia and questions of minority and Euro-British cultural identities in the United Kingdom. These convergences happen under less than ideal circumstances, to be sure, including budget cuts, audit requirements, productivist discourses, and

the rise of consultancies, but they push the "what for" of anthropology in decidedly more political directions. This creates conditions for anthropologists to be newly on the move, so to speak, even if their actions place them in cultural and political minefields. At issue here is the accountability of knowledge claims, sometimes under conditions in which the "others" are not so clearly different from "us" or in which they might have political power over anthropological performance. As a result, the kinds of knowledges being produced, the methods used, and the rules of accountability have all undergone some mutations. When the grip of concepts such as "informants" and "participant observation" is loosened, one may say that a hegemonic practice begins to fade away, and world anthropologies that provide other ways of grappling with ethical and political issues, as these two chapters indicate, have a chance to emerge.

The purpose and use of anthropology can be discussed from more familiar epistemological and political-economic perspectives. Some of our authors summon subalternist arguments toward this end. For some, an important task of the world anthropologies project is to bring epistemic and ontological differences to the fore and to put them into dialogue with Western forms of constructing the world. Marisol de la Cadena's framing of this inquiry in both the history of anthropology (her discussion of Arguedas) and the theoretical-political discourse of interculturality in the contemporary Andes demonstrates that the domestication of alterity effected by modernity is not a foregone conclusion. Faced with a frontal challenge by those who were previously considered anthropological objects and who are now intellectuals in their own right, practitioners of a world anthropologies approach might respond with new concepts and statements of purpose. Whether "relational epistemologies," "epistemological engagement," and "epistemic difference" are workable responses to these new situations remains to be seen. The important point is to reawaken the question of radical difference, its politics, and its epistemology. Indeed, an entire emerging Latin American research program and perspective is centered on just this issue. Based on a redefinition of modernity from the perspective of coloniality—understood both as the systematic suppression of subaltern knowledges and cultures since the European conquest and as the constitution of spaces for thinking other thoughts and imagining other worlds, or "worlds and knowledges otherwise" (e.g., Coronil 1996; Escobar 2004a; Mignolo 2000)—this intellectual trend has, as we have seen, particularly valuable contributions to make to a world anthropologies perspective.

Narotzky's entry point into the issue of the "what for" of anthropology is an incisive analysis of the participation of anthropologists in local political projects. This issue pertains to all anthropologies but has special overtones for those conceived as subaltern or peripheral. There is some truth in the assumption that empire-building and nation-building anthropologies position their practitioners differently in knowledge and political fields. Indeed, practitioners of the former have tended to study distant others, and those of the latter, their own societies, including their own internal others. As the Colombian anthropologist Myriam Jimeno (2003) argues, the implications of this difference go beyond political locatedness and commitments; it affects theory production, because those working within their own societies have to adapt, adopt, or transform established concepts from the center or create new ones within a much more politicized context. Jimeno goes so far as to say that this dynamic results in the production of different anthropologies (on the relationships between politics and a Brazilian style of doing anthropology, see Ramos 1990). Of course, this view does not describe all peripheral practitioners, many of whom follow received scripts in the name of a universal science, venturing at most a pragmatic adaptation of models.

Narotzky's argument has several salient edges. She finds disturbing certain practices on the part of some Northern anthropologists that speak to the core of the political dimension of anthropology. They not only have overlooked local struggles in countries such as Spain but have often failed to notice local anthropologists' commitments to those struggles and the ways in which those commitments influence their work. Also interesting in Narotzky's account is the observation that Spanish colleagues see their political projects as part and parcel of a scientific anthropological endeavor. There are disciplinary and institutional reasons why hegemonic anthropologies find it hard to understand this approach. What needs to be ascertained further are the conditions under which anthropologists might be successful at developing a more clairvoyant practice linking the exercise of power with the production of truth in real-life situations of dominance and exploitation.

Granted, not all anthropologists from every anthropology are, will, or even should be engaged in such intellectual-political projects. The minimum requirement, however, especially for those arriving in fieldwork locations from center countries, is to develop a significant awareness of the multiple, locally situated knowledges that the foreign anthropologist is likely to find in the field. These local knowledges,

including those of local anthropologists, have developed in the midst of epistemological and political tensions that cannot be assessed solely in canonical academic terms. The obstacles to fulfilling this requirement are enormous, as Narotzky constructively discusses; they range from the epistemology of political engagement to issues of communicability and commensurability of worldviews and approaches between anthropologists and local constituencies and among anthropologists themselves.

It may well be that world anthropologies should remain a floating concept, as Fabian advises. As we have shown, it already exists on some levels of discourse and practice. Vakhtin's metaphor is telling: for him, world anthropologies may be likened to a garden in which many species proliferate, and we should only nourish it without aiming to control it. As Verena Stolcke put it during our symposium, anthropology today has a twofold driving force: a shared humanity and the consciousness of historically marked differences. We have traveled a long way since anthropologists discussed the former in terms of modernist notions of humankind; shared humanity has taken on more complex dimensions today, ecologically, culturally, and politically. It has been only recently that we started developing languages for referring to historical differences appropriate to the global situation; concepts such as diversality are a way to start.

## Some Concluding Remarks

In a recent text, the Brazilian anthropologist Alcida Ramos (2005) mused over a utopia of world anthropologies that resembled the multilingualism of certain Amazonian groups for whom the rule of linguistic exogamy created "communities of multiple voices, a kind of organized and solidary Babel." In this plural landscape, "all would contribute languages, ideas, solutions and proposals without any of the partners losing his/her identity or local character, which would be preserved as symbolic capital at the service of the collectivity" (2005: 2). Although we are not close to reaching this goal, in Ramos's opinion we can already envision it.

The project of world anthropologies certainly has utopian reverberations, especially if we consider, along with Paul Ricoeur (1986), that utopias are struggles in the present over the meaning of the future. As we have attempted to show, world anthropologies entail both an intellectual and a political project. In other words, we are not talking only about more inclusive social diversity (as in multiculturalism);
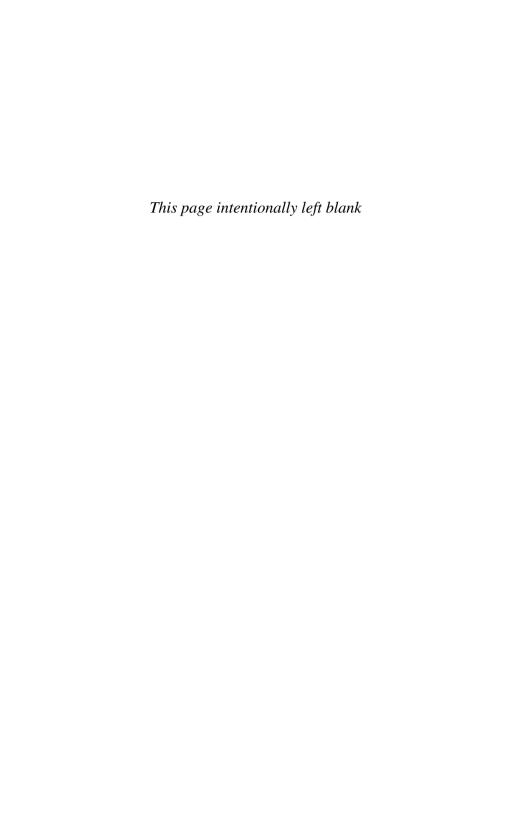
in principle anthropologists would easily accept social and political equality. We are further suggesting that anthropologies everywhere can benefit from the scholarship already existing in globally fragmented spaces. To do so, as we have argued, entails a series of changes and concerns—from attention to uneven exchanges among anthropologies to consideration of multiple histories, trajectories, languages, conceptual frameworks, political commitments, experiences of transnationalism and networking, and so forth. Taking these considerations seriously would open up new dialogic possibilities, other avenues of engagement. This process would apply whether one believed in the unity of the field or whether, alternatively, one favored the idea of a multiplicity; indeed, both perspectives are represented in this volume. If the former, we are talking about anthropology as a unified field, but in a nonhegemonic way, an open-ended unity that admits of diversity. If the latter, we might see world anthropologies as fostering diversality—understood as a giving up of classical notions of universality and seeing in diversity the main principle of creativity. We suggest that we must keep these two visions in tension.

This is why it would be ironic if the project of world anthropologies came to be seen as a new attempt on the part of the "periphery" to strike back, as in some simplistic interpretations of the aims of postcolonial theory vis-à-vis the former imperial powers. On the contrary, we think that the present is a moment of enlargement of anthropological horizons that will make our scholarly practice a richer cosmopolitics, one that is capable of dealing with the challenges arising in the twenty-first century. The concept of world anthropologies provides a space of opportunities for all those who understand that difference goes well beyond inequality and that diversity is an asset to be cherished on epistemological, cultural, social, and ecological grounds.

Change has been a constant in the history of anthropologies everywhere. Anthropologies' multiple deaths and rebirths reveal an ability to transform themselves and redefine their interests and goals. Anthropologies are finely attuned to the sociological changes of different periods and places. In a globalized world this calls for diverse international voices and perspectives actively participating in any assessment of, and at, the frontiers of anthropological knowledges. Indeed, a globalized world is a perfect scenario in which anthropologies can thrive, because a basic anthropological lesson is respect for difference. Anthropologists who, in accord with deep anthropological traditions, praise plurality and diversity are surely fostering these standpoints within their own milieu. The time is ripe for world anthropologies.
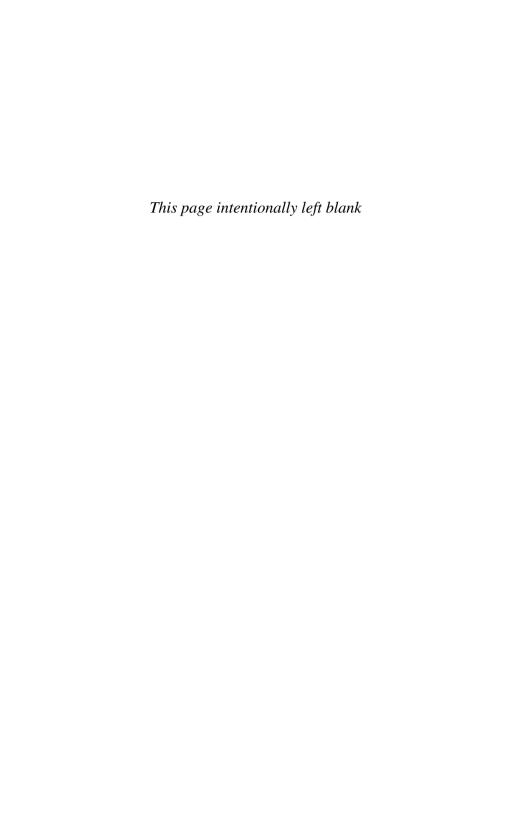
# Notes

1. See "The World Anthropologies Network" website (www.ram-wan.org); see also WAN Collective 2003 for a statement about the project. Events related to this initiative include, besides the international symposium leading to the present volume, sessions at the 2002 meeting of the American Anthropological Association, at the First Congress of Latin American Anthropology (Argentina, 2005), and at the Colombian Congress of Anthropology (2005), as well as conferences and debates in Australia, Mexico, China, and Japan. In 2004, in Recife, Brazil, Gustavo Lins Ribeiro and Paul Little organized a meeting, funded by the Wenner-Gren Foundation, of fourteen representatives of anthropological associations to discuss global cooperation in anthropology. The presidents of the associations for Australia, Brazil, Canada, France, Great Britain, India, South Africa, and the United States were present. Japan sent the director of international relations of its association. The presidents of the European Association of Social Anthropologists, the Latin American Association of Anthropology, the Pan African Anthropological Association, and the International Union of Ethnological Sciences were also there. The representatives' enthusiasm showed that the time was ripe to create more horizontal modes of interaction and exchange on a global level. This meeting resulted in the creation of the World Council of Anthropological Associations (WCAA), with the primary goal of promoting more diverse exchanges among anthropologists worldwide (see the founding agreement of WCAA at www.wcaanet.org). A by-product of this meeting was a debate among Australian, Brazilian, and Canadian anthropologists, at the twenty-fourth Biannual Brazilian Meeting of Anthropology in 2004, over the many problems and issues surrounding the engagement of anthropologists in native people's struggles for land.

2. For a more contemporary analysis of this topic, see Ben Ari 1999, Mafeje 2001, Trouillot 1991, and van Bremen and Shimizu 1999a.

3. At the University of North Carolina–Chapel Hill, an interdisciplinary research group on social movement, anchored in anthropology, is based on the notion that social-movement activists should be taken seriously as knowledge producers. See http://www.unc.edu/smwg/.

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# Part 1

# Transnationalism and State Power

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# Reshaping Anthropology
## A View from Japan

## *Shinji Yamashita*

Anthropology is usually considered to be a Western discipline developed by constructing colonial others in non-Western societies. However, as Akitoshi Shimizu (1999: 115) has argued, if Japan as a non-Western country has its own anthropology, this view cannot be maintained. One has to redefine anthropology in a way that goes beyond the dichotomy between West and non-West, and beyond the orientalism that has prevailed throughout the history of the discipline. Doing so will lead us to create an open, interactive space in which various world anthropologies can meet in order to understand diverse human societies and cultures.

The Japanese Society of Cultural Anthropology (formerly the Japanese Society of Ethnology), the main association of Japanese sociocultural anthropologists, currently numbers approximately 2,000 members.[1] This is far smaller than the American Anthropological Association (about 11,000 members) but larger than the European Association of Social Anthropologists (about 650 members). In addition, there are some 800 members of the Anthropological Society of Nippon, the association of Japanese biological anthropologists. In other words, the Japanese anthropological community is one of the largest in the world.[2]

Nevertheless, the achievements of Japanese anthropology are little known outside Japan. Although some reviews of Japanese anthropology are available in English, such as the one Chie Nakane wrote in the 1970s (Nakane 1974), some of the latest reference works still make no mention of Japanese anthropology—for example, the *Encyclopedia of Social and Cultural Anthropology* (Barnard and Spencer 1996). In order

to fill this gap, in this chapter I first review the history of Japanese anthropology since its foundation in 1884 and highlight its distinctive features and achievements.[3] I then examine Japanese anthropology in the wider contexts of world anthropologies and the main issues that contemporary Japanese anthropology faces. The experience of Japanese anthropology may provide us with some important pointers about how anthropology can be reshaped as a global discipline.

As for my position in undertaking this review, I must make it clear that I am not a representative of Japanese anthropology, nor do I wish to be. I have been greatly influenced by Western anthropologies, especially through studying abroad as a visiting scholar at universities in the United States and Europe, although I was educated in Japan and am based there.[4] I am a Southeast Asia specialist, having carried out fieldwork mainly in Indonesia and Malaysia. My approach to Japanese anthropology, therefore, is not national but transnational. In this sense I am located intellectually somewhere between Japan and the West, or between Japan and Southeast Asia. For me, as for transnational migrants in the contemporary world, what is important is not where I am from but where I am *between* (Clifford 1997: 37). The project of a multiplicity of world anthropologies for me derives perhaps from within myself, as an anthropologist based in Japan but with a transnational background.

## A Short History of Japanese Anthropology

One can divide the history of Japanese anthropology into five developmental stages, encompassing, respectively, the years 1884–1913, 1913–34, 1934–45, 1945–64, and 1964 to the present. The boundaries of these historical stages are somewhat arbitrary, because the actual historical process is of course continuous. In what follows, I highlight briefly the main features of each stage (I review this history in more detail in Yamashita 2004).

Anthropology in Japan began in 1884 when a group of young scholars formed a workshop called Jinruigaku no Tomo, or "Friends of Anthropology" (Terada 1981: 7). The founding of this group was stimulated by the theories of Edward Morse, then professor of biology at the University of Tokyo. Morse had excavated a shell mound in Tokyo and had proposed, on the basis of the remains he found there, that cannibalism had been practiced in ancient Japan. The central figure of the Jinruigaku no Tomo was Shōgorō Tsuboi, who was offended by Morse's cannibalism thesis. The group advocated that the origins of Japanese

culture should be investigated by the Japanese themselves, not by foreign scholars (Shimizu 1999: 126). In this sense, Japanese anthropology was born as a product of nationalist consciousness. Two years later the workshop evolved into Tokyo Jinruigakkai (the Anthropological Society of Tokyo), and later it became Nihon Jinruigakkai (the Anthropological Society of Nippon). In 1892, after studying anthropology in England for three years, Tsuboi became the first professor of anthropology at the University of Tokyo. He led the debate over the origins of the Japanese people in the early years of the twentieth century.

Tsuboi died in 1913. In the same year, his successor, Ryūzō Torii, published an article in which he argued that "ethnology" (*jinshugaku* or *minzokugaku*) should be separate from "anthropology" (*jinruigaku*) (Torii 1975: 480–83). Because of his extensive field research abroad, Torii was much more concerned with cultures outside Japan's national boundaries than Tsuboi had been. Torii had conducted his first fieldwork in northeastern China in 1894, followed by research in Taiwan, the Chishima Islands (off Hokkaido), China, Korea, eastern Siberia, Manchuria, and Mongolia. His fieldwork clearly reflected the colonial expansion of the Japanese Empire into other parts of Asia, including Taiwan in 1895 and Korea in 1910. In his 1913 paper, Torii proposed the establishment of a discipline to be called *tōyō jinshugaku* (literally "the study of the Oriental race") or *tōyō minzokugaku* ("Oriental ethnology"). In this way he advocated the study of the ethnology of the Orient by Oriental scholars, because they were assumed to be in a better position than Western scholars to study these regions (Torii 1975: 482–83).[5] The article marked a new historical stage in Japanese anthropology, in which Japan began to observe others and not merely to be observed (see Shimizu 1999: 116) and in which the object of study shifted from Japanese people to neighboring colonial others in Asia.

Further Japanese colonial expansion led to an interest in a wider geographical area. Japanese colonial power reached Micronesia in 1919, Manchuria in 1933, and Southeast Asia in 1941. As it expanded, the Nihon Minzokugakkai (Japanese Society of Ethnology) was established in 1934. Interestingly, its establishment was stimulated by the First International Congress of Anthropological and Ethnological Sciences, held in London that year. As the prospectus for the establishment of the society tells us:

> Ethnology in Japan has had a history of several decades. However, we have not yet reached the international standard … Ethnological studies in Japan have been concerned with native culture and ancient cultural

survivals in Japan under the name of *minzokugaku* [folklore studies]. But we have to develop the discipline in comparative perspective with other cultures, to consider the origin and diffusion of culture using the fruits of the development of ethnology in the West. In particular, through participation in the First International Congress of Anthropological and Ethnological Sciences held in London this summer, we have realized that we should promote ethnological research in Japan. This is the reason why we are reorganizing the former Minzokugakkai [Society of Folklore Studies] into the Nihon Minzokugakkai [Japanese Society of Ethnology].[6] (Minzokugaku Shinkōkai 1984: 4)

Two things are important about the Japanese Society of Ethnology. First, that it was formed under the stimulus of an international congress means that the society was itself a product of the global development of anthropology during the 1930s. Second, the newborn society was quite interdisciplinary at the start: its founding members included specialists from disciplines such as rural sociology, Oriental history, linguistics, Japanese folklore studies, Japanese classics, comparative religions, and archaeology. Kurakichi Shiratori, the founding father of Oriental history in Japan, was its first president.

A year later, however, in 1935, Kunio Yanagita founded the Minkandenshō no kai (Folklore Workshop), which specialized in Japanese folkways and later developed into the Japanese Society of Folklore Studies. There, anthropology took a more nationalist turn. As a result, scholars specializing in Japanese folklore studies (*Volkskunde,* in German) became separated from those specializing in comparative, or foreign, ethnology (*Völkerkunde,* in German). This division of labor between nationalist and internationalist anthropologists in Japan continues to the present.

In 1943, the Institute of Ethnic Research (Minzoku Kenkyūsho) was established under the Japanese Ministry of Education and Culture to carry out research that could contribute to the ethnic policies of the empire. Major Japanese ethnologists at that time were involved in this institute, though its history is still largely unclear (Nakao 1997). The life of the institute, however, was short: it was closed at the end of the Second World War, in 1945.

After the war, Japan lost its colonies. The regional concerns of Japanese ethnology were once more confined to Japan. The defeat also raised the issue of the national character of the Japanese people. The Japanese translation of Ruth Benedict's *The Chrysanthemum and the Sword* was published in 1946, selling millions of copies over the years. In 1948,

Eiichirō Ishida organized a workshop on the origins of the Japanese nation, and it attracted considerable public attention. The general public found Namio Egami's thesis that the imperial family had originated among the northern Asian equestrian peoples in Korea particularly sensational. Anthropological fieldwork during this period was limited basically to peoples on the fringes of Japan, such as the Ainu in Hokkaido and the Okinawans in the Ryūkyū Islands.

In 1964, the Olympic Games were held in Tokyo. This was also the year when restrictions on overseas travel for Japanese people were removed. These events marked the end of the postwar period, and Japan entered into a period of rapid economic growth and overseas economic expansion. In parallel with this development, Japanese anthropology once again became focused on other cultures outside Japan, while interest in Japanese culture decreased. The mainstream of current Japanese anthropology has continued to move in this direction.

Looking at this historical process, several things can be noted. First, Japanese anthropology began as a search for the origins of the Japanese and of Japanese culture, in response to the theories of a foreign researcher. This gave Japanese anthropology the character of a nationalist project intended to clarify the nature of the Japanese rather than of the whole of mankind. This research paradigm remained popular until the 1970s.

Second, Japan had a history of colonization in Asia and the Pacific, and Japanese anthropology developed as part of this colonial experience. This history was similar to that of Western anthropology, although Japanese anthropologists saw their colonial others in a distinctive way, to which I turn later. Under this perspective, comparisons were made with other parts of Asia, in order to clarify the origins of the Japanese people and Japanese culture.

Third, the regional concerns of Japanese anthropology have varied historically, depending on the fluctuating boundaries of the Japanese nation and Japanese influence in the wider world. Analyzing the articles published in *Minzokugaku-kenkyu* (Japanese Journal of Ethnology) from 1935 to 1994, Teruo Sekimoto (1995: 138–39) pointed out a centrifugal trend within Japanese anthropology: in each period in modern history, Japanese anthropologists have tended to study others in frontier or peripheral areas in relation to Japan's national boundaries. In colonial times this meant that they studied Taiwan, Korea, and Micronesia, whereas during the early postwar period, when travel was difficult, they concentrated on Hokkaidō and Okinawa. Since 1964 they have been concerned with cultures increasingly distant from Japan, in parallel with Japan's economic expansion into the remotest parts of the world. It was

during this period that Japanese anthropology became "anthropology in a global perspective" (Shimizu 1999: 161), extending beyond both the Japanese nation and its former empire in the Asia-Pacific region. The interests of Japanese anthropologists now extend to Africa and Latin America, areas of little importance for the Japanese economy.

In short, the history of Japanese anthropology reflects Japan's changing position in the modern world system, with the result that Japanese anthropology has at times adopted emphases different from those in the West.

## Nationality and Transnationality in Anthropological Traditions in Japan

Anthropology in the West has commonly been defined as the study of others and other cultures. As we have seen, anthropology in Japan started from an interest in Japanese identity and culture.[7] Later, in parallel with modern Japan's colonial expansion, Japanese anthropological interests extended to the colonized areas, but still often in search of clues to understanding the cultural roots of Japan through research into cultural similarities. As we have seen, Ryūzō Torii redefined Japanese anthropology at this stage as "Oriental ethnology." This characterization was based on the assumption that Japanese were better able than Westerners to understand other Asian peoples and cultures, because the Japanese were themselves Asians.

Yet there was an element of "orientalism" in Japanese anthropology, too. By seeing its Asian and Pacific colonies as "backward" and "primitive," Japan could strengthen its claim to being an "advanced" and "civilized" country on a par with the Western world (see Kang 1996; Kawamura 1993; Yamashita 2004). In this regard, it is interesting to consider the difference in anthropological stances taken by the West and Japan in the study of Southeast Asia. For instance, in studying the cultures and societies of the Dutch East Indies (now Indonesia), Dutch anthropologists in the 1930s developed a form of structuralism—an attempt to understand the principles of the "human mind" that foreshadowed Lévi-Straussian structuralism of the 1960s. In contrast, the Japanese Society of Ethnology carried out a research project in the late 1950s to search for the origins of Japanese culture in mainland Southeast Asia, particularly in the rice cultivation zone of Indochina. This had been a preoccupation of Japanese anthropology from its inception (Minzoku Bunka Sōgō Chōsadan 1959).

Despite this, Japanese anthropology was also transnational. From the beginning, Japanese anthropologists were keen to learn from anthropological theories developed in the West. Tsuboi, the founding ancestor of modern Japanese anthropology, went to England to study the discipline before he was appointed professor of anthropology at the University of Tokyo. Torii, the initiator of Oriental ethnology in Japan, did not study in the West, but Masao Oka, a key figure in Japanese ethnology during the wartime and postwar periods, did. In Vienna he studied the ideas of the German and Austrian school of historical ethnology, which he used to reconstruct the various stages of ethnic and cultural history in Japan through comparison with the Asian and Pacific regions.

In colonial Taiwan, Inezō Utsushiwaka, professor of ethnology (*dozokugaku*) at Taihoku (Taipei) Imperial University, was a Harvard graduate who had studied with Roland Dixon. His student, Tōichi Mabuchi, who became a leading anthropologist in postwar Japan, did his fieldwork among the aboriginal peoples of the Taiwan highlands under the influence of Western sociocultural theories of the period. He also had a life-long interest in Dutch anthropology because of his involvement in research in Indonesia during the Japanese occupation (1942–45).

In Japanese Micronesia, Kenichi Sugiura carried out fieldwork in the late 1930s and early 1940s on the land tenure system, under the influence of Bronislaw Malinowski's functionalism. In colonial Korea, Takashi Akiba, professor of sociology at Keijō (Seoul) Imperial University, carried out research on shamanism using a Durkheimian perspective gained from studying in Europe. His student Seiichi Izumi, who became another leading anthropologist after World War II, carried out his fieldwork on Jeju Island off the southern tip of the Korean peninsula, drawing inspiration from Malinowski's *Argonauts of the Western Pacific*.

It is therefore wrong to consider Japanese anthropology an isolated phenomenon: it developed along with anthropology in the rest of the world. The Japanese Society of Ethnology itself was formed under the stimulus of an international meeting in London. Furthermore, the colonial model was also translocal. As Jan van Bremen and Akitoshi Shimizu pointed out (1999b: 8): "In Japan, research in French Indo-China served as a model for the first series of governmental studies that were made of Okinawa. In turn, these studies, together with models taken from research in British India . . . inspired the research projects that were subsequently carried out in Taiwan. In their turn, the Taiwanese

studies served as a model for research projects carried out in Korea and Manchuria."

## Japanese Anthropology in the Academic World System: Does Japanese Anthropology Exist?

Takami Kuwayama (1997, 2004b) has argued for what he calls the "world system" of anthropology. According to him, the core or center of this system is occupied by the United States, Great Britain, and France, which have the power to determine which kinds of knowledge are most desired in anthropology. These countries dictate the nature of the anthropological discourse that scholars from peripheral countries must use if they wish to be recognized. Within this framework of core and periphery, Kuwayama characterizes Japanese anthropology as "semi-peripheral": it is peripheral in relation to the West, but central in relation to other parts of Asia.

This argument has drawn criticism. Van Bremen, for example, criticized Kuwayama for "the excessive weight given to center-periphery relations and positions and the static view taken of them" (van Bremen 1997: 62). The binary opposition of center and periphery looks static if one considers the two to be substantive entities. In reality, it is often difficult to determine where the center is. For instance, there often appears to be a center-periphery division within the center itself. Some universities in the United States, the most powerful core country in anthropology, may actually be more peripheral than the University of Tokyo or other leading institutions in Japan.

Moreover, today there are many students and teachers from peripheral areas in the anthropology departments of U.S. universities. For example, the University of California–Berkeley, one of the most powerful anthropology departments in the world, had faculty members from Africa, China, Japan, Malaysia, and elsewhere during my stay as a visiting professor there in 1998–99. Similar situations can be found at other major centers, such as Harvard and the University of Chicago. In reverse, many scholars with PhDs from universities in Europe and the United States teach anthropology at Asian universities, in places such as Singapore, Hong Kong, and, increasingly, Japan. Furthermore, globalization and internet communication have tended to make the center-periphery opposition increasingly meaningless: close neighbors and the farthest parts of the world are the same distance away in cyberspace. The center and the periphery are currently intermingled in complex ways.

Nevertheless, national boundaries still exist in scholarship. Among them, perhaps the most critical structural problem for Japanese anthropology in the anthropological world system is the boundary of language. Japanese anthropologists practice mainly in Japanese, a minor language in international academic communication. They are well aware of the main trends in Western core anthropology: the references cited in *Minzokugaku-kenkyu* are mostly works of Western literature. Students are required to read works in English and other European languages, in addition to Japanese. Articles by Japanese scholars, however, are mostly written in Japanese, which makes access to them difficult for non-Japanese readers. In this respect, Japanese anthropology, unlike the Japanese economy, imports too much and exports too little.

In his article "Japanese Cultural Anthropology Viewed from Outside," Jerry Eades (1994), a British-born social anthropologist based in Japan since 1991, asked why Japanese anthropology had not had greater influence outside Japan, despite the number of anthropologists and the volume of research they carried out. He argued that part of the answer lay in the institutional processes by which Japanese research and publications were produced. Japanese students and researchers generally did not compete with their Western counterparts in their academic careers. Writing in a foreign language did not necessarily help in getting a job in a Japanese university, and so people wrote in Japanese for the Japanese market. Japanese anthropology in this sense formed a sort of closed island with its own audience.

Such differences in audiences sometimes create divisions, and sometimes conflicts, between Japanese and foreign anthropologies. Gordon Mathews, an American anthropologist specializing in Japan and teaching in Hong Kong, recently examined the differences between Japanese and American depictions of Japan (Mathews 2004). He noted that "what an American audience, professional or lay, seeks to know about Japan will likely be very different from what a Japanese audience seeks to know." A topic that might interest an American audience might be boring for a Japanese readership. He also pointed out that "few American researchers pay attention to the research conducted by Japanese folklife specialists, looking for remnants of Japanese traditions; this research is for the most part completely outside American interests." This, he argued, led to an imbalance in intellectual power relations between American and Japanese anthropology. Japanese anthropologists act as if they belong to a colonized country, rarely researching the metropolis (the United States), but importing American and European theories to use in their own work.

The problem goes back to the question of power relations in the academic world system, which Kuwayama stressed. In a paper on "native anthropologists" (1997), he examined the discord between foreign and native anthropologists with special reference to Japanese studies inside and outside Japan. He pointed out that the discord arose from the structure of the production of knowledge in the world system of anthropology, rather than from personal and emotional conflicts. In this system, Japanese anthropology lies in the shadow cast by Western hegemony. This raises another vital question: does Japanese anthropology exist at all as a distinctive entity?

Before answering this question, let me mention briefly my own experience of differences in perception between American and Japanese anthropologists. In 1993, Jerry Eades and I organized a session called "The Dynamics of Identity Fabrication: The Interplay of Local, National, and Global Perspectives" at the annual meeting of the American Anthropological Association (AAA), held in Washington, DC. The session dealt with issues of cultural identity in Japan, Indonesia, India, and Egypt; it did not focus exclusively on Japan. Nevertheless, in the audience I recognized a number of Japan specialists who had come to see "Japanese" anthropologists. During the discussion, a member of the audience asked what was the "Japanese twist" in anthropology.[8] We were perplexed by this question because our session was not concerned with "Japanese" anthropology as such. We had thought we were simply practicing anthropology, but our American colleagues saw us as "Japanese" anthropologists.

After returning to Japan in 1994, I organized a panel titled "Does Japanese Ethnology Exist?" at the annual meeting of the Japanese Society of Ethnology, in order to answer the question that had been posed at the AAA the previous year. Motomitsu Uchibori, the chairperson of the panel, summarized the conclusions reached (Uchibori 1995), and I reiterate the important points as follows. The phrase "Japanese anthropology" refers to two main bodies of work. The first consists of Japanese researchers' studies of their own society and culture, including the type of work known as *nihonjinron* (discussions of the nature of Japanese identity), a genre popular in Japan. The second is research by Japanese scholars carried out in the rest of the world. It is there, in relation to the world system of anthropology, that Japanese research is often perceived as peripheral. This peripheral character in relation to the Western anthropological center is a problem not so much of institutions as of language. Deciding which language to write in is much more than an individual choice. It is related to the identity of Japanese

anthropologists, which oscillates between two poles: the anthropological academic universe and the local world in which anthropologists live. It would be absurd for Japanese anthropologists to publish all their papers in English, because anthropological inquiry involves internal motivation that is rooted in the local world. Japanese anthropologists may have no other way to write than in Japanese if they wish to reflect their Japanese identity. If Japanese anthropology exists at all, it is based in the identity of Japanese anthropologists.

Identity, however, is a complex thing: it can be multiple rather than single. Kirin Narayan argued against the fixity of a distinction between "native" and "non-native" anthropologists and suggested, "We might more profitably view each anthropologist in terms of shifting identifications amid a field of interpenetrating communities and power relations" (1993: 671). Referring to the "enactment of hybridity," she points out that "we are all incipiently bi- (or multi-) cultural in that we belong to worlds both personal and professional, whether in the field or at home" (1993: 681). In this perspective, it may be unproductive to stick to the dichotomy between Japanese anthropology and Western anthropology. What is important is to create a common space in which anthropologies in the contemporary world can meet for the future.

## Japanese Anthropology in the Contemporary World

Anthropology today is at a critical juncture. This is the case in Japan as well. In particular, in studying and teaching anthropology, students and teachers often find it difficult to relate their contemporary interests to classical theories in the discipline. In these situations, how do we reproduce anthropological knowledge for the future? In order to look at this problem against the background of changing social needs today, in 2000 the Japanese Society of Ethnology set up a committee to examine anthropological education in Japan. I chaired this committee, and we examined issues such as education in anthropology at major Japanese universities, anthropology textbooks, the anthropology job market, and institutional restructuring. Our findings can be summarized as follows.

### *The Niche of Anthropology*

Anthropologists must develop an academic niche in order to meet new social needs in the contemporary world. The main interests of anthropologists have shifted over time, beginning with the historical reconstruction

of human culture in the late nineteenth century and proceeding through the structural-functional analysis of culture and society in the first half of the twentieth century; Lévi-Straussian structuralism, Victor Turner's ritual process, and the Geertzian interpretation of cultures in the 1960s and 1970s; and contemporary issues such as "development," "medicine," "education," "ethnic conflict," "globalization," "identity," and even "September 11." Research into contemporary issues—what was once labeled "applied anthropology"—has become the basic anthropology of today. It is urgent, then, that we develop an academic niche in Japan that is relevant to radical changes in the contemporary world (see Ahmed and Shore 1995).

In studying the contemporary world, however, anthropology may overlap with other disciplines such as sociology and cultural studies.[9] What is peculiar to anthropology is its approach to the subject of research and its way of understanding human reality. In this respect, we must remain committed to our method of ethnographic participant observation in relation to a specific community or cultural practice, because most of the other social sciences have tended to adopt statistical methods based on quantitative data. In other words, anthropology is the only discipline that still tries to construct sociocultural theory through participant observation in the micro social worlds in which people live.

## The Matrix of Teaching

In parallel with the change of interests in anthropology in recent years, the subjects that Japanese anthropologists study have diversified. In this situation, how can anthropology retain its integrity and identity as a unified academic discipline? The answer lies in trying to relate the "primitive" to the "civilized," the traditional to the modern, the periphery to the center, and classic anthropological knowledge to the contemporary world by looking at the two elements of each dichotomy not as different worlds but as segments of the same modern world system. What is important is to connect classic and contemporary topics of research with each other. In doing so, we must explore areas of research that link these segments of the world system—for example, as Raymond Firth (1992: 211) once pointed out, "development in Tikopia society" and "questions of kinship in some sectors of modern London."

## The Pluralization of the Discipline

It is inevitable that anthropology in Japan will become increasingly interdisciplinary, because the objects of our research are complicated phenomena that can be analyzed only by using a combination of disciplinary approaches. If we study "development," we require knowledge of macro-level political economy, government policy, and regional sociology. This interdisciplinarity is related to the job market as well. Jobs specifically for anthropologists are becoming fewer these days in Japan. Instead, there is a growing number of positions in gender studies, development studies, area studies, and so on, all of which stress interdisciplinary research. At the University of Tokyo, the Department of Cultural Anthropology now belongs to a larger unit called "Interdisciplinary Cultural Studies," since the restructuring of the graduate school in 1996. Generally, in Japan it is rare for anthropology to constitute an independent department; usually it is integrated into wider interdisciplinary departments with labels such as "International Cultural Studies," "Global Social Sciences," and "Asian and African Studies." This situation is sometimes good for the discipline, because we can carry out interdisciplinary research and teaching on the contemporary world in accordance with the complexity of the research topics. But it may also mean that we lose our anthropological identity through diversification and research into an increasingly wide range of topics.

## The Internationalization of the Discipline

Anthropology is in principle an international science. However, anthropological practices usually have national boundaries, and practitioners do not yet have a "global anthropology." That is why we need to internationalize the discipline, something that has already been happening in Japanese classrooms.[10] Japan now has many students from foreign countries. At the University of Tokyo, approximately 40 percent of the graduate school students are from abroad, mostly from East Asian countries such as China, Korea, and Taiwan. In my graduate seminar in 2002, six of the twelve students registered were from foreign countries, including China, Korea, Taiwan, Hungary, and the Netherlands. We also have visiting scholars and professors from various parts of the world, and the job market in Japan is opening up to foreigners as well.[11] It is within these transnational situations that we must reshape anthropology.

*Applied Anthropology*

Japanese anthropologists are exploring the use of anthropological knowledge in a wider range of contexts, including practical fields such as the work of international development agencies, public sector institutions that promote intercultural understanding, and nonprofit organizations involved in social and cultural issues. For instance, the Japan International Cooperation Agency (JAICA) was established in 1974 to promote the Japanese Official Development Assistance (ODA) program. Its projects often require anthropological expertise, and some anthropologists have worked with the program. Further, the number of students who want to work in practical sectors is increasing. Although this field is less fully developed in Japan than in the United States or Europe, it seems potentially to be a major source of employment for anthropologists.

## Beyond Orientalism: An Asian Network of Anthropologists

Following on from this committee, Jerry Eades and I organized a session called "The Reproduction of Anthropological Knowledge and the East Asian Future" at the AAA annual meeting in New Orleans in 2002, to discuss the issue in the wider context of the East Asian anthropological future. For this purpose, we invited Joseph Bosco, of the Chinese University of Hong Kong, and Kim Kwang-Ok, of Seoul National University, to present papers on Hong Kong and Korea, respectively, in addition to the papers dealing with Japan. We recognized that anthropologies in Asia were different from one another because of differences in historical background as well as in the social position of anthropology in each country.

Another participant, William Kelly, of Yale University, pointed out that despite these differences, many problems were shared across national boundaries. He emailed me after the meeting: "What most struck me was how similar are Japan and US anthropologies. All five features that you emphasize as challenges to Japanese anthropology [those summarized in the preceding sections] face us as well and serve as foci of debate. This is just a random and immediate thought, but perhaps at some point, it might be interesting to think of a bi-national panel that would take these five points as a basis and organize the session as a series of presentations on each of the five points, with each presentation done as a collaboration or dialogue by a Japan anthropologist and a U.S. anthropologist" (personal correspondence, 25 November 2002).

The problems the discipline faces today are, therefore, problems not only for Japanese anthropologists but also for U.S. anthropologists and perhaps for those in the rest of the world. This might lead us to a kind of "interactive anthropology" at the global level, a position I advocate later. But before proceeding to the global level, let me consider the possibility of a regional Asian network of anthropologists, because Asia is the area with which Japanese anthropology has been most deeply and intensively concerned.

Among anthropologists in Asia, communication and cooperation have so far been limited. Kuwayama (2004b), in his discussion of relations on the periphery of the anthropological world system, quoted Thomas Gerholm and Ulf Hannerz (1982: 7), according to whom "the map of the discipline shows 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." In this anthropological world map, the residents of the peripheral islands always look toward the central mainland, rather than toward each other. This metaphor is applicable to Asia.

In 1995, a symposium titled "Cultural Anthropology and Asia: The Past, the Present and the Future" was held at the annual meeting of the Japanese Society of Ethnology in Osaka. Its organizers' aim was to discuss the place of cultural anthropology in Asia and to pursue the possibility of regional cooperation. Asian anthropologists from China, Korea, Japan, Taiwan, Philippines, Indonesia, and Malaysia participated, and Nur Yalmann, of Harvard University, who is of Turkish origin, gave the keynote speech. This was the first attempt to bring Asian anthropologists together at the annual meeting of the Japanese Society of Ethnology.

Research exchange programs also exist at Japanese universities and research institutions, including the National Museum of Ethnology in Osaka, one of the most important centers for anthropological research in Japan. A number of joint research projects are supported financially by the Japanese Ministry of Education, Culture, Sports, Science and Technology, the Japan Society for the Promotion of Science, the Japan Foundation, the Toyota Foundation, and others. The Asia Center of the Japan Foundation was established in 1995 especially to promote mutual understanding among Asian peoples and cultures. Furthermore, Japanese universities have recently pursued the international exchange of students much more actively than before. Given this trend, I can propose several possibilities for future cooperation among Asian anthropologists.

First, as I pointed out earlier, Japanese anthropology in the colonial past resulted in a Japanese version of orientalism, in which the peoples of Asia were seen in negative terms as *dojin,* or "indigenous peoples." Shifting perceptions of Asia within anthropology could be examined by Japanese and other Asian scholars as a joint project on the history of colonialism in Asia. Japan appeared "Asian" in relation to the West, but it practiced a sort of orientalism toward other Asians during the colonial period. In order to overcome the tendency toward "orientalization" within Japanese anthropology, we must discuss it together with Asian colleagues.

Second, the recent growth of Japanese anthropology, especially the increase in fieldwork carried out in Asia, has been closely related to Japanese economic expansion during the postwar period. "Understanding other cultures" became more necessary as the influence of the Japanese economy expanded. This does not mean that anthropology is once more playing a role in Japanese expansionism. However, it may be possible to investigate the differences between Japanese anthropology and other Asian anthropologies, in the same way one might investigate the gap between Japanese and American anthropologies discussed by Gordon Mathews (2004). For example, Japanese anthropology has shown less concern for development issues than some other Asian anthropologies. This gap has to be bridged if we are to develop academic exchanges.

Third, it is clear that anthropology in each Asian country has its own national characteristics. What kinds of proposals, then, can we in Asia make to address the postcolonial situation of anthropology in the contemporary world? This is what we will be debating with Asian colleagues in the near future. In order to answer this question, we need to develop an Asian network of anthropologists, though this has not yet been realized. If it were to be set up, it could hold regular meetings, like the European Association of Social Anthropologists, which developed in the late 1980s. Common problems that we are now facing in Asian regions could be discussed, such as development, the environment, migration, and ethnic conflict. Such a network would enable us to send messages from Asia to the rest of the world, rather than just receiving messages from the centers of North America and Europe.

## Beyond the Center-Periphery Dichotomy: Toward Interactive Anthropology

At a session called "Anthropology: A Critical Review from Japan," which Jerry Eades and I organized at the annual meeting of the AAA

in San Francisco in 1996, we discussed future links among Asian anthropologists. Our discussant, Stanley Tambiah, warned about the dangers of isolationism in Asian anthropology, which might be harmful to the development of a world anthropology. Of course we do not want to be isolationist. We are not advocating Asian anthropology in opposition to Western anthropology. Rather, we are seeking the possibility of an "interactive anthropology" by taking Asia as a test case.

The Japanese Society of Ethnology set up a special committee to promote international links in 1996. The society has already attempted to internationalize by inviting distinguished scholars from abroad to its annual meetings: David Maybury-Lewis (1995), Benedict Anderson (1996), Stanley Tambiah (1997), Marshall Sahlins and Richard Fox (1998), and D. F. Eichelman (1999). Even though it is useful to listen to major scholars from the center, we also believe it is important to organize substantive meetings on particular topics in Japan together with overseas scholars, and to participate in overseas meetings so that Japanese scholars can gain exposure to the international anthropological community.

In order to broaden these efforts to internationalize the society, a new journal in English, *The Japanese Review of Cultural Anthropology,* was launched in 1998, to provide a channel of communication and to make Japanese anthropology more visible internationally. This is only one of a series of recent initiatives. To mention just two others, a new journal called *Asian Anthropology* was established in 2002; it is published by the Chinese University Press for the Hong Kong Anthropological Society and the Department of Anthropology at the Chinese University of Hong Kong. Jerry Eades and I have started a monograph series, "Asian Anthropologies," with Berghahn Books, also aimed at Asian scholars. The reason for these initiatives is that anthropologists based in Asia wish to have their voices heard within the wider community of anthropology. These attempts will form the basis of an interactive anthropology. In this way, we want to create anthropologies beyond national boundaries in Asia.

Furthermore, we should create an open academic forum in which to develop interactive anthropology at the global level. Following Kuwayama (1997: 541), by "open" I mean "the kind of representation that posits a diverse audience, both native and non-native, which contrasts with the 'closed' representation that has assumed, as in the past, a homogeneous audience from one's own cultural community." For this purpose, as Kuwayama also suggested, we may need a new journal "in which native scholars comment on articles by non-native scholars,

who in turn reply to the comments they have received, thereby re-conceptualizing their ethnographic observations in both native and non-native contexts" (1997: 541).

The major problem for Japanese anthropologists, however, remains that of language. We know that few foreigners, apart from specialists in Japan, can read works written in Japanese. This is an obstacle not only for American and European audiences but even for audiences in other countries of Asia. On the other hand, if we write in English, then Japanese readers are reluctant to read us. Furthermore, by writing in English, we may simply play along with Western academic hegemony, because modes of thinking and presentation differ according to language. We Japanese are handicapped in this respect because we lack experience in both speaking and writing English, the language of hegemony, and in Western styles of presentation and publication. But we do not want to close up the country—which in any case is no longer an option in this transnational age. There will be no future for Japanese and Asian anthropology without a transnational and global perspective in which we can create our anthropology by negotiating our identities with others, just as is the case with other cultural practices in our age.

## Conclusion

In a paper titled "Anthropological Futures," Adam Kuper (1994: 115) suggested the importance of developments in anthropology outside the Western metropolitan centers. Japanese anthropology is one such non-Western anthropology with its own history and its own character. However, as I hope I have shown, the meaning of "own" may be complicated. If Japan has its own anthropology, it is only so within this complexity, and if the experiences of Japanese anthropology that I have discussed can provide some insights into ways of reshaping anthropology in the future, then it is again so within the complexity of Japanese anthropology.

The focal point of this reshaping is to create and develop an open forum in which various anthropologies in the world can meet on equal footing. There are two points to note in this regard. First, while admitting the hegemonic role of Western anthropology, one needs also to emphasize that the gaze from the periphery strengthens the system academically. Peripherals can play a positive and critical role, and this is important especially in the world that now exists after September 11, 2001. We, the anthropological others for the West-centered academic world system, should play a more assertive role in helping to create a global anthropology, rather than simply criticizing Western hegemony.

Second, although anthropological traditions may vary between countries, anthropology is also transnational. Throughout its history, Japanese anthropology has not been an isolated phenomenon but rather a product of the intersection of various anthropological traditions in the world. The multiplex and hybrid identity of Japanese anthropology may be important, because an open forum of world anthropologies should consist not of representatives of national anthropologies but of transnational anthropologists who are located somewhere in between. In this sense, the anthropology of the future will be constructed on the basis of the "glocal" (Robertson 1995), or "global-local," interaction.

# Notes

1.  In April 2004 the Japanese Society of Ethnology changed its name to the Japanese Society of Cultural Anthropology. The main reason was that "cultural anthropology" is much more extensively used than "ethnology" (or "social anthropology") in contemporary Japanese society. In Japan, sociocultural anthropology and biological anthropology form separate associations, with no single umbrella association to link them.

2.  The *Anthropology Newsletter* of the American Anthropological Association (vol. 43, no. 8, November 2002, p. 10) mentions the work of the Japanese Society of Ethnology and the Anthropological Society of Nippon in promoting ties with other anthropological organizations across the globe.

3.  I draw on material from a paper I co-authored with Joseph Bosco and Jerry Eades (Yamashita, Bosco, and Eades 2004), together with an earlier book-review article on anthropology and colonialism in Asia and Oceania (Yamashita 2001) and a paper presented at the annual meeting of the American Anthropological Association in New Orleans in November 2002 (Yamashita 2002).

4.  I was a visiting scholar at Cornell University in Ithaca, New York, from July 1981 to March 1983, at Cambridge University in England from April to July 1983, and at the University of California–Berkeley from September 1998 to August 1999.

5.  During this period, Torii was associated with the Department of Oriental History of the University of Tokyo, founded by the historian Kurakichi Shiratori, the founder of *tōyō shigaku,* or "Oriental history," in Japan (see Tanaka 1993). I presume that Torii established his "Oriental ethnology" under Shiratori's influence.

6. Rather confusingly, there are two words pronounced *minzokugaku* in Japanese, though they are written differently in Chinese characters. *Minzo-kugaku* can therefore mean either "ethnology" or "folklore studies," depending on the characters used.

7. Generally speaking, anthropologists in non-Western countries study people in their own countries rather than foreign others. Malaysian anthropologists, for instance, study Malaysians, and Indonesian anthropologists study Indonesians. This is different from Western colonial anthropology, in which the British studied Malaysians and the Dutch studied Indonesians as colonial others. Japan combines the two traditions: a flourishing school of research on Japan, particularly in folklore, coexists with anthropological research outside Japan, which arose out of colonialism.

8. Of the word "twist," Kuwayama (1997: 521) noted that it might imply the "inauthentic" character of Japanese anthropology, in comparison with "authentic" Western anthropology.

9. Lévi-Strauss (1967: 344) pointed out some fifty years ago that anthropology was not distinguished from the other human and social sciences by an area of study that was peculiar to it alone.

10. At the AAA annual meeting in November 2001 in Washington, DC, Judith Freidenberg and June Nash organized a panel called "Institutionalizing the Discipline of Anthropology in International Arenas." This may have been an attempt to internationalize American anthropology, especially after the events of September 11, 2001. For a cynical criticism of this panel, see Moeran 2002.

11. One might mention here the development of the Anthropologists of Japan in Japan (AJJ) network, which consists mainly of foreign scholars working in Japan, together with some Japanese scholars trained overseas. It currently has between 60 and 70 regular participants.

# Transformations in Siberian Anthropology

## An Insider's Perspective

### Nikolai Vakhtin

The idea of anthropological (ethnographic) research came to Russia from Germany in the early mid-nineteenth century, together with the Romantic idea of the nation-state (Schweitzer 2001). The vast expanses of Siberia, populated by several dozen indigenous peoples, were, along with Central Asia and the Caucasus, natural fields for anthropological research.[1] Siberian anthropology was particularly reinforced by the Jesup North Pacific Expedition (1897–1902), intellectually designed and led by Franz Boas. This event and its consequent research and publishing shaped, to a great extent, the Russian anthropological paradigm in the first two decades of the twentieth century, making it part of the international anthropological scene (see Krupnik and Vakhtin 2003).

Simultaneously, the specific character of Russian colonialism, as well as the theoretical mainstream of Soviet ethnography, determined the development of Siberian anthropology in two respects: it was ethnohistorically biased and it had an emphatic eschatological disposition. Russian (Soviet) ethnographers viewed the objects of their research as people who would soon become "like us"—hence their restraint from studies of contemporary conditions of "the native peoples" and their tendency to study ethnic history. And because the objects of study were supposed to disappear soon and merge into a homogeneous mass (whether "citizens of the empire" or "the Soviet people"), ethnologists' primary mission was to record this vanishing past—an approach Susan Gal (1989) called "pastoralist."

This tendency was reinforced in Soviet times by fierce ideological pressure: the present was to be described solely in accordance with ideologically approved prescriptions. This was true for descriptions of the past as well, but the margin of free choice for social research into the present was much narrower. It was safer to turn away from the present and focus on the past. This trend, together with ideological censorship and a language barrier, created a deep breach between Russian and Western anthropological traditions.

The situation in Siberian research changed after 1989. Siberian anthropology has once again become internationalized through fieldwork done in Siberia by scores of Western anthropologists (with a large share of joint projects), as well as by intensive academic contacts. This new development has revealed interesting discrepancies between the two traditions, such as different approaches to the object of study, contrasting theoretical frameworks, different attitudes toward sharing outcomes of research, and different ethical procedures and requirements.

## Siberian Anthropology at the Turn of the Twentieth Century: An International Enterprise

Although until the late eighteenth century foreigners made up the majority of Siberian researchers, they were, as a rule, employed by the Russian state, usually by the Academy of Sciences, and they worked in close contact with Russian scholars. Some "foreigners" stayed in Russia for relatively short periods and viewed their assignments there as temporary; others spent their entire professional lives in Russia and considered themselves part of Russian science (Schweitzer 2001: 268ff.).

Individual travelers such as Mattias Kastren, Karl von Ditmar, and Gerhard Maidel visited the "Land of Siberia" in the 1850s and 1860s and left valuable descriptions of it (see, for example, Kastren 1860; Maidel 1894; von Ditmar 1901; compare with Schweitzer 2001: 112–16). Still, in the first two-thirds of the nineteenth century, Siberia, a vast and remote country with a sparse population and few literati, was not a particularly fascinating field for regular research in Russian science. Only in the last third of the century did ethnology and ethnography develop in Russia into independent and popular academic disciplines. In 1889 the first issue of *Etnograficheskoe obozrenie* (Ethnographic survey) was published, and in 1890, the first issue of *Zhivaya starina* (Living antiquity).[2] In 1894 the academician Vladimir Radlov became director of the *Kunstkamera* museum in St. Petersburg, which became, under

his leadership, an active and modern research institution (Schweitzer 2001: 138–42).

In the 1880s, interest in Siberian research increased with the growth of the urban, educated population there. Sections of the Russian Geographical Society were established in Siberia (first in Irkutsk and later in the Far East; Shirina 1983, 1993). In 1888 the first Siberian university was opened in Tomsk (Schweitzer 2001: 137). The famous Jesup North Pacific Expedition (JNPE) played an important role in encouraging ethnological research in Siberia (especially in its northeastern part), as did the fact that several Russian scholars were able to take part in its work.

The JNPE, planned, organized, and carried out by Franz Boas, was a major enterprise (for details of the expedition, see, e.g., Fitzhugh and Krupnik 2001; Freed, Freed, and Williamson 1988; Schweitzer 2001: 153ff.; Vakhtin 2001). It so happened that two, and later three, Russian scholars became members of the expedition—and simultaneously became informal students of Boas's. These men, who had when young participated in the revolutionary movement, were members of the Narodnaya Volia (People's Freedom) insurgent party; in the 1880s they were arrested and exiled to Siberia for ten years. There they became interested in indigenous languages and ethnographies, conducted field research, and collected anthropological data. They returned to St. Petersburg just at the time Boas wrote to Radlov asking him to help find specialists in Siberian ethnography for his expedition.

These three men—Vladimir Bogoraz, Vladimir Jochelson, and later Leo Sternberg—became, during the 1910s and 1920s, the "founding fathers" of Siberian studies. Bogoraz and Sternberg organized the famous Institute of the People of the North, and all three were active members of the Committee of the North (1921–35). They not only determined, in the early years of the Soviet regime, the directions, format, and theoretical framework of anthropological education and research but also considerably influenced Soviet policy toward Siberian indigenous peoples and their languages in the 1920s (on Soviet national and language policy, see, e.g., Alpatov 1994, 1997; Kreindler 1984; Silver 1974; Slezkine 1996; Vakhtin 2003).

Among other things, the JNPE produced for the first time in Siberian anthropology a stream of dozens of contributions under a common agenda that were written, edited, translated, and delivered across language and political barriers for almost thirty years.[3] The partnership established during the years of the project was seemingly on its way to being extended to the second generation of scholars nurtured by the original JNPE members—a development that never happened (Krupnik and Vakhtin 2003), as we will see later.

During this period, Russian and Western research in and on Siberia went hand in hand, supporting and nourishing each other in both theoretical approaches and field data.[4] The new academic discipline, ethnology, developed triumphantly in Western Europe and the United States and strongly influenced Russian ethnological thinking, the mottoes of which at the time were pragmatism and descriptive fieldwork (Slezkine 1993: 114; compare Schweitzer 2001).

## An Air-Tight Vault: Post-1917 Soviet Siberianists

After the 1917 revolution in Russia, Russian and European–North American ethnologies took different paths. In the first years after the revolution, certain innovative tendencies emerged in Russian ethnology that had been formed on common grounds with, and not without influence from, those in Europe: "Scholars moved from diachrony and historicism to synchrony, function, and structure" (Slezkine 1996: 830). From a different perspective, these tendencies can be defined as "internationalist."

Without going into details, let me simply refer to one example from a "hard-boiled evolutionist," Leo Sternberg. In an address given in 1921 before the annual meeting of the Geographic Institute, Sternberg formulated the scholarly paradigm that he and his colleagues had made the backbone of the institute's teaching program—the essence of its ethnological education and research.[5] Although this paradigm was clearly evolutionist, it regarded ethnicity not as an intrinsic characteristic of all people but rather as a surface representation of inner unity. The cornerstone of the paradigm was the idea of a united humankind: equality and fraternity of all peoples, regardless of their place on the "ladder of civilization." According to Sternberg, ethnology was a science that was supposed to demonstrate, through exact analytical methods and numerous collected facts—the "inexhaustible treasury of facts about the life of all peoples, all stages of culture, all epochs"—the universal character of human culture (Sternberg n.d.: 25).

Other scholars of the time formulated similar ideas: the historian N. M. Pokrovskii, the linguist N. Ia. Marr, and those linguists who were active in the 1920s in the language policy movement.[6] Publications of the time about "the national question," languages, writing systems, and alphabets were full of statements emphasizing the necessity, value, and advantages of giving equal support to all cultures and all languages, regardless of numbers of speakers (see, for example, publications in the official bulletin of the Ministry of Education).[7] Instruction in and on

native languages was introduced in all non-Russian primary schools in the country—this was, at the time, the leitmotif of language and ethnic policy.[8]

The ideas of the "internationalists" were no doubt connected to political tendencies of the time—tendencies that quickly became less favorable. The archaeologist S. N. Bykovsky wrote: "Zealously looking for ethnic or national features of a culture … an educated archaeologist … is 'scientifically' endorsing the right of imperialists to snatch some territory or other" (1934, quoted in Shnirelman 1993: 56). As Victor Shnirelman rightfully commented (1993: 58), "ethnogenetic studies— that is, attempts to trace specific ways of formation of individual peoples—were impossible under Stalin's internationalism. A scholar who risked doing this would be accused of imperial chauvinism or local nationalism."[9]

Efforts were made, of course, to reestablish the true international format of Siberian research, but in this context they looked naïve. Franz Boas made one such effort, emphasizing in a letter to the Russian Academy of Science that "at the present time, the contact between American and Russian science is insufficient … It is, therefore, highly desirable that an exchange of young scientists should be developed. This is particularly necessary in the domain of anthropology." The remainder of the letter offered an exchange program for young scholars.[10] In 1928 Boas was still trying to restore lost contacts with his Russian colleagues; the letter, to my knowledge, was never answered.

Attempts to reestablish contact were made from the other side as well, especially from what might be called "the second Jesup generation" (Krupnik and Vakhtin 2003). A student of Bogoraz's, Alexander Forstein, went to Denmark in 1936 as a research fellow at the National Museum of Copenhagen.[11] From there he wrote to Boas (30 June 1936) inquiring about an opportunity to come to the United States on a long-term research grant. "Any interruption of our connections with America would be a very painful loss indeed," he said. Boas answered on 20 August 1936, saying, "Pardon the long delay of my answer to your letter … I did not know what to answer. I have retired this year from active teaching … I believe that work in America might be very useful for you but I do not know just what to suggest."

The era of internationalism ended by 1934, when, at the seventeenth Communist Party Congress, Stalin announced that the principle enemy now was local nationalism. In 1936 Pokrovskii's approach to history was purged, together with many historians; the concept of the Russian people was rehabilitated as a legitimate object of research (Shnirelman

1993: 58). The orientation of Soviet ethnography (as well as archaeology and linguistics) changed from internationalist ideas to concepts of ethnic specificity and research into the histories of individual ethnic groups. Shnirelman (1993: 54–56) connected these changes with changes in Stalin's general politics: at about this time, hope for the world revolution died out, and it became clear that the only plausible policy for the Bolsheviks was to establish a strong Soviet state—to "recreate, under the guise of the Soviet Union, the political and administrative structure of the [Russian] Empire." Similar changes took place in language policy. With all the violence of Stalin's methods, a turn was made to predominant support of the national language, Russian—that is, in Vladimir Alpatov's terms (1994), to "normal" language policy in a multiethnic state moving toward industrialization.

For ethnology, this had serious consequences: it brought a "sharp downturn in the fortunes of unorthodox intellectuals in the Soviet Union" (Brandist 2002a: 9). Whereas in the 1920s anthropologists carried out extensive fieldwork in spite of the country's financial condition, by the mid-1930s their work was almost totally stopped, and ethnography "became nothing but a theory of primitive communism" (Slezkine 1993: 120). In 1932 N. M. Matorin, a leading Soviet ethnographer, declared that to continue fieldwork under modern conditions was imperialism. He also claimed that ethnographers had no right to study contemporary issues: there was nothing specifically "ethnographic" about modern *kolkhozy,* or collective farms (Matorin 1931: 20–21, quoted in Slezkine 1993: 120). Ethnology and ethnography in the Soviet Union were declared, for almost ten years, redundant and useless (indeed, the Russian epithet for them, *vrednyi,* is better translated as "evil," or "sinister"). Serious research was terminated. For many years beginning in the mid-1930s, the main research topic for Soviet ethnographers became ethnogenesis, that is, archaeological and ethnographic, and partly linguistic, investigation of the formation of "ethnoses," or ethnic groups. From this point onward, mutual understanding between Soviet ethnography and Western anthropology began to decrease (Shnirelman 1993: 52). Soviet ethnographers did not do research on "cultures"; rather, their main task was to capture, understand, and glorify the imperceptible "ethnos" (Slezkine 2001: 362–63).

In 1932, a Moscow meeting of Soviet archaeologists and ethnographers approved a resolution that laid out immediate tasks for ethnographic research. Ethnographers, according to the document, were to study the following:

(1) the process of ethnogenesis and territorial distribution of ethnic/national groups; (2) material production in its specific (ethnic) variants; (3) the origin of the family; (4) the origin of classes; (5) the origin of various forms of religion and art; (6) forms of deterioration of primitive communism [and] feudal society in capitalist surroundings; (7) forms of transition from precapitalist formations directly to socialism; and (8) the construction of culture, national in its form and socialist in its content. (*Sovetskaya etnografiya,* vol. 3, 1932, quoted in Slezkine 1993: 119)[12]

This emphasis on ethnic groups, combined with political pressure, gave birth to another interesting feature of Soviet ethnography in the 1930s (and later): ethnographers became agents of the state. Yuri Slezkine wrote: "With the coming to power of the Bolsheviks, the essence of national policy became a fight to converge ethnic borders with administrative ones, which meant that most ethnographers had to become administrators" (Slezkine 2001: 342). Ethnographers had to study ethnic groups? Well, those ethnic groups first had to be constructed, delineated, and made rigid. Another writer, David Anderson (2000b: 135), supports this observation: "A hallmark of Soviet State ethnography has been a concern with establishing those [ethnic] boundaries between people which later became real administrative borders." Later he says, "The fluid boundaries between identity groups … have been made solid and impermeable partly through official ethnographic action" (2000b: 141).

In other words, in the late 1930s Soviet politics, ideology, and then science returned to the idea of ethnoses, and scientists began to study ethnogenesis and to describe differences between ethnoses in "material" and "spiritual" culture (see Anderson 2000a: 77ff.). They thereby contributed to constructing the ethnoses themselves and, in doing so, to laying the basis for future ethnic conflict. As S. M. Shirokogorov wrote in the 1930s, "The ethnographers' intention to show the inferiority of these small nations and the need of 'protecting them' results from the general attitudes of the great nations… while they seek impartial truth, they are thus preparing the most perfected weapon for the ethnic struggle in the future" (Shirokogorov 1933: 168).

Beginning in the late 1930s, Slavic-Russian ethnography became the main field of studies for ethnogenetic research. The purpose of these studies was to prove the self-driven formation of a resourceful and productive early Slavic culture and to demonstrate that Germanic cultures had been underdeveloped, and their influence on neighboring cultures, totally negative. This was an answer to the "ethnogenetic expansion"

of German ethnologists: Soviet authors of the 1940s were ready to discover "ancient Slavs" almost everywhere. Soviet authors set apart for the Germanic peoples as insignificant a place as German authors of the 1920 and 1930s allotted to Slavic people (Shnirelman 1993: 62–63). All this "academic research," especially from the late 1940s through the early 1950s, was aimed at intimidating the defeated Germany, to "prove" the primitiveness of ancient German culture, and to honor Russia above its Western neighbors (Shnirelman 1993: 63)—that is, it had all sorts of goals, but they were not scientific. The roots of ethnogenetic research in the Soviet Union were in "the struggle for the past" (Shnirelman 1993: 64), in the craving to establish a Slavic ethnogenetic myth.[13]

The journal *Sovetskaia etnografiia* for 1946 through 1955 demonstrates the following quantitative distribution of papers: 151 papers on ethnogenesis, ethnic statistics, ethnic cartography, and research on ethnic composition and ethnonyms; 294 papers on various aspects of subsistence and material culture; 56 papers on social structure and family and marriage relations; and 182 papers on the folklore of different ethnic groups (*Sovetskaya etnografiia* 1956). Most papers on specific ethnographic or linguistic issues contained in their titles references to ethnicity or ethnogenesis.

After 1936–37, when unrestricted terror began and many ethnographers were arrested, "uncertainty and horror were so strong that those ethnographers who were not yet in prison almost lost the ability to speak" (Slezkine 1993: 122–23). Ethnography—in Matorin's and Bykovskii's version—was revived only after World War II, and ethnographers returned to studying what was required by the resolution of the 1932 meeting mentioned earlier.

The Soviet Union thus became "the first State in history that legalized ethno-territorial federalism, classified all citizens in accordance with their 'biological nationality,' and formally subscribed to a policy of governmental preference by ethnicity" (Slezkine 2001: 330; see also Brubaker and Cooper 2000). As another author put it, "reduction of national to ethnic is a characteristic feature of Russian social sciences" (Malakhov 2002: 12).

## The Dam Breaks: Cultural Anthropologists in an Ethnographic Field

One of the inevitable consequences of the claustrophobic development of the Soviet state was the isolation of scholars, including those who did ethnographic and linguistic research in Siberia. I do not mean isolation

from new theoretical achievements in Western science; even in Soviet times there were scholars who watched carefully what was going on in the West. The problem was that Russian ethnographers could work only within the approved theoretical paradigm, and Russian and Western scholars could not work together in the field. For many decades, the Siberian "field" was, officially, completely sealed.[14]

In reality, a few individual Western ethnologists did sometimes find a way to work in Siberia, even before the dissolution of the Soviet Union. In 1975, Marjorie Mandelstam Balzer managed to get to northern Siberia. In the mid-1980s she began fieldwork in western Siberia among the Khanty people and in Yakutia among the Sakha people. Caroline Humphrey did fieldwork among the Buryats in southern Siberia in the late 1970s. Piers Vitebsky, too, began to do fieldwork in Yakutia fairly early, in 1986.

But fieldwork by Westerners in Siberia during Soviet times had "official"—that is, approved and controlled—status. Western scholars, who had to be formally invited by the Academy of Science, were carefully passed on from one academic institution to another. They were told where they could go and where they were not allowed to go; their contacts were strictly monitored and controlled by what was then called "the competent organizations" (a Soviet euphemism for the KGB). Besides, their stays in Siberia were usually brief.

From the turn of the 1980s through the 1990s, the serene landscape of Soviet Siberian studies was shattered by the unanticipated arrival of "other" foreign anthropologists, who came in a different manner. They were young people, postgraduate students in anthropology, from all over the world, magnetized by the sudden accessibility of a vast anthropological "field." In full accord with the traditions of British and North American anthropology, they came for long periods of time, usually for a year but often more. They moved about on their own, uncontrolled by state officials; they made friends with local people, lived in villages and camps, stayed with families, roamed with indigenous reindeer herders on the tundra, fished and hunted with indigenous hunters and fishers in the taiga, and generally behaved like free people, cheerfully breaking, in the eyes of the bewildered local administrators, all the unwritten rules of the Soviet era.

It may not be an exaggeration to say that in the late 1980s there appeared in the West, and especially in North America, a real craze for Siberia. From that time into the mid-1990s, more than a dozen young anthropologists who had graduated from universities in the United States, Canada, Great Britain, Germany, Norway, and France managed

to complete serious fieldwork in Taimyr and Yakutia, in Chukotka and Sakhalin, in Kamchatka and Yamal, to collect data for their PhD projects.[15] By the end of the 1990s, this work had begun to produce results: publications appeared—first articles, then books.[16] The authors found jobs in anthropology departments at universities in the United States, Canada, the United Kingdom, Finland, France, and Germany. They became regular participants in academic conferences on Siberian anthropology both in Russia and abroad. Russian scholars who did research on the cultures and languages of Siberia began to discern a metamorphosis of their academic landscape.

This metamorphosis can be perceived in several respects. First, the (invisible) international academic community of Siberian anthropologists has grown considerably in numbers and has become much younger. Approximately three dozen Russian anthropologists and linguists who did research in and on Siberia have been reinforced by a strong and active cohort of foreign scholars, comparable in numbers, whose advent has markedly transformed the community. Alongside this "mechanical population increase," the number of Russian anthropologists has decreased steadily, due to natural causes, to the departure of Russian scholars from the underpaid discipline (and in many cases, from the country), and to young Russian students' apparent unwillingness to enter a discipline that, in the early 1990s, seemed to have no career prospects at all. Against the background of decreasing numbers of research staff in the Siberian departments of various institutes of ethnology and in the linguistics departments of the Academy of Science, the Universities of St. Petersburg, Moscow, Tomsk, and Novosibirsk, and other traditional research centers, the advent of ten to fifteen young, energetic scholars from the West was a major transformation.[17] Interestingly, among the Western Siberianists there are no "generations": all of them are approximately the same age. This creates special relations both within the group and between the group and its Russian colleagues.

The second important change in the Siberian academic landscape is the change in the object of anthropological research. Russian (Soviet) ethnological research, as I mentioned, was always connected with the past, with tradition, with studies of cultures, languages, and societies that were "on the verge of extinction" and thus had urgently to be recorded before it was too late. This view was not, of course, a Russian invention: as Susan Gal (1989) noted, claims about the inevitable disappearance of cultures, languages, and dialects, soon after they were discovered, were constant and central rhetorical figures of European ethnography of the twentieth century. Scholars often were in search of remote rural areas,

of elderly people; they were looking for archaic, unchanged, and hence "genuine," authentic elements of culture. They interpreted changes as distortions, as losses of this "genuineness." Gal called this approach "pastoral": the past is the model, the present is regarded as the "spoiled past." Although many scholars explicitly rejected this approach, it still influenced their results (Gal 1989: 315–16).[18]

The opposite approach to the "ethnographic field"—namely, studies of the contemporary social, cultural, and linguistic features of modern communities—was, as I mentioned, totally washed away in the late 1920s by studies of "ethnogenesis" and "material culture." The new cohort of young Western anthropologists brought back this approach: their work strongly emphasizes the present as something of value in itself. They study *contemporary* reindeer herding, *contemporary* ethnic identities and conflicts, *contemporary* power and gender relations. This, as well as the fact that international foundations emphasize their support for research into contemporary topics, has caused Russian scholars to adjust their research along the new—or rather, forgotten—lines.

Another important transformation is what one might call changes in "property relations" among Soviet Siberian anthropologists in dividing their domains of study. For decades, these relations rested undisturbed: everybody knew who studied what, and who had the "right" to study what. In a way, Siberia was divided into "spheres of influence" among scholars from Moscow, Leningrad, Novosibirsk, Tomsk, and some other cities; it was rare that two scholars, whether linguists or ethnographers, did research in the same area or on the same language. This division of Siberia into "hunting sites"—where each scholar had his or her "legitimate" area, people, or language, and trespassing was definitely unwelcome—can be explained partly by the small number of scholars: the field was too vast and human life too short to take more than one indigenous group or language for serious research. Partly, however, this situation was maintained consciously, and not always for purely academic considerations.

The arrival of Western anthropologists changed the situation considerably. In the 1990s almost every Russian ethnologist discovered that he or she had acquired a colleague (or a rival, depending on one's point of view) who worked in the same area, in the same villages, and often at the same time. The "natural monopoly" of Russian ethnologists over "their" people, area, or language came to an end.

The theoretical paradigm of Siberian research also changed. Soviet ethnologists worked, willingly or not, within pseudo-Marxist theory, developed and approved by several "recognized" scholars—and first

of all within the "theory of ethnos." Western scholars brought in new theoretical approaches. It is not that those ideas were entirely new to Soviet scholars, but the mere fact that it now became possible to *choose* between approaches, between different conceptual and terminological systems, was a breath of fresh air for Russian scholarship.

I do not mean to say that all "foreign" theories and methodologies were necessarily better, or that foreign scholars were free from theoretical stereotypes or fashions, with their inevitable references to Mikhail Bakhtin and Lev Iakubinskii, Michel Foucault and Fredrik Barth, Ernest Gellner and Benedict Anderson (references that often resemble the unavoidable references to Marx, Engels, and Lenin on the first pages of Soviet publications). But the mere fact that these were different theories and that there was now a possibility of choice affected Russian ethnography.

Finally, the rise of the system of research grants, including international ones, was also a new development. The possibility of approaching international funding agencies directly and of starting joint research projects with colleagues from other countries eroded the state's monopoly on international collaboration and led to the active development of ethnological, archaeological, and linguistic research in Siberia—at Siberian universities and research institutions. Modern Siberian ethnology in Russia is much more decentralized than it used to be.

Although the fashion for doing fieldwork in Siberia has ebbed somewhat by now, it is clear that this outburst of interest was not accidental or temporary; the changes in the Siberianist landscape are now established facts. Siberian anthropological studies are—theoretically, methodologically, and practically—much more international than they were during the Soviet era. They are, in a sense, starting over from the point in the early 1920s when cultural anthropology, brought to the country by Bogoraz, Sternberg, Jochelson, and others, began to develop as a natural part of international science, a development that was violently terminated by the Soviet power.

## What's New? Current Changes in Research Paradigms

As a result of the developments I have outlined, today's Siberian anthropological research in Russia is undergoing acute disciplinary transformations. I will limit myself to two brief examples of these transformations within two aspects of research: changes in the object of study and changes in attitudes toward ethical aspects of anthropological research in Siberia (see Vakhtin and Sirina 2003 for details).

The field methodology accepted in Russian ethnological research, as well as the programs of such research, were originally designed for the study of specific territories (Sirina 2002). With the rise of the "theory of ethnos," these methods were applied to individual "ethnic groups" ("peoples"), which caused numerous problems, especially in parts of Siberia known as "zones of ethnic contact" (Vasiliev 1985). In those zones, different "ethnoses" had lived together and influenced one another for centuries, and it was often impossible to tell where the ethnic boundaries lay, if they existed at all. Nor was this approach useful for large territories encompassing several distant groups, such as Evens, Evenki, and Nenets.

Because most of Siberia is in fact one big "zone of ethnic contact," and because indigenous demography has changed considerably in the last fifty years, influenced by the high mobility of the population, research that takes "one ethnos" as its object drives itself into a dead end: it is often impossible to define the object. This tendency is now slowly being overcome, not without the positive influence of Western anthropologists who employ in their research a territorial approach. (Again, I do not mean to say that Western approaches are necessarily better: in many cases it is only collaborative research that can yield convincing results.)

Another interesting trait of current Siberian ethnology is a shift from studies of indigenous populations to studies of *all* populations. The ethnic picture of modern Siberia is complicated and cannot be reduced to simple dichotomies such as "indigenous people vs. newcomers," "oppressed vs. oppressors," or "traditional vs. innovative." Indigenous populations are today highly structured and stratified and include those who prefer traditional subsistence, those who prefer to live in villages, and ethnic elites who occupy leading positions in social and power structures. The "newcomers" (Russians) are also diversified. They include "old settlers" who have lived in the area for the last three hundred years, people born in the area to immigrant parents, and "real" newcomers, temporary and even part-time ("shift") workers. The social, educational, and economic characteristics of all these groups are diversified. For some areas, at least three kinds of groups are defined: local administration, local industry, and the indigenous population; all three are now becoming legitimate objects of anthropological research (see Novikova 2002).

Ethical codes for anthropological research are adopted by both national and international associations of anthropologists and by indigenous communities. An interesting discussion is presently going on

in Russia about whether such a code can—and should—be adopted. Unwritten rules of conduct "in the field" have, of course, always existed in Russia, but to make them a written (paralegal) document, one needs two legitimate parties to "sign" it. The recently established Association of Russian Anthropologists and Ethnologists could be one such party; the other side is evidently "the community." But indigenous communities in Siberia are very misleading units. On one hand, many of them were created artificially during the infamous era of forceful relocations of indigenous people in the 1950s and 1960s (see Vakhtin 1992). On the other hand, many of them exist only on paper and are reduced to native elites; there are doubts whether these units can be partners in a "contract" of this kind.

Importantly, this whole discussion was initiated, although indirectly, by Western anthropologists, who obviously feel uncomfortable without such a code. They find themselves in an even more complicated situation than their Russian colleagues, because, not being Russian citizens, they cannot interfere in local politics or take part in local economic, social, or ethnic conflicts. Both Russian and Western anthropologists see their mission at that of becoming "the voice of the voiceless": to make the problems of the Natives known and heard, to help them formulate their needs in the language of the law, to help them in their struggle for rights (Argunova 2002; Novikova 2002). Joint cultural, language, and educational projects with and for indigenous people (Kasten 2002a; Koester 2002) are one possible solution, yet the ethics of anthropological research in Siberia remains a difficult issue for both Russian and non-Russian researchers.

## The Current Situation: Cooperation or Rivalry?

Western anthropologists, just like their Russian colleagues, are working today in a "field" that was, to a large extent, constructed by Soviet national (and language) policies and by Soviet ethnographic (and linguistic) research. But Western anthropologists have been brought up in an entirely different theoretical and methodological tradition from that of their Russian colleagues of the older generation. Coming to the "Siberian field," they are naturally tempted to deconstruct it, to peel off stereotypes and the "discursive crust" and identify underlying meanings and facts. But on this road, a trap awaits them. By deconstructing the object of study, they simultaneously and automatically repudiate the scholarly tradition that constructed the object. Consciously or not, they find themselves in a position of criticism of Soviet (Russian) ethnology—which, naturally, Russian ethnologists do not like.

I will finish with a story about a clash between two scholars: the Canadian anthropologist David Anderson, who now works in Scotland, and the Russian linguist Evgenii Helimskii, who now works in Germany. Both are, in their respective fields, professionals of the highest rank.

In June 2000 a conference took place in Vienna, where, unexpectedly, Helimskii presented a sharply critical review of Anderson's book *Identity and Ecology in Arctic Siberia* (2000a).[19] Two of Anderson's "sins," in Helimskii's opinion, were underrating the role, achievements, and value of Soviet scholarship and overrating the scale of ethnic conflicts in the region. Helimskii is clearly a "primordialist": for him, ethnicity is peacefully and simply inherited, passed from parents to children unchanged and unchangeable. He was enraged at the way Anderson treated the issue, accusing the latter of no less than inflating, through his research, ethnic conflict in Taimyr. Anderson, on the contrary, is clearly a "constructivist": for him, ethnicity, which previously had been insignificant in the area, was "constructed" in the mid-twentieth century, not without the help of Soviet ethnographers, and now is claimed by local elites and used as an instrument in political, social, and economic life in the area. The discussion was energetic and caused some long-lasting ripples on the quiet surface of the tiny Siberianist pond.

This polemic is a good example of the mutual "ideological" misunderstanding that is, unfortunately, widespread and impedes joint productive work by Russian and Western Siberianists. The "ideological struggle" in the discipline is further aggravated by the fact that Soviet ethnography, throughout its history, was strongly politicized and "ideologized": ethnographers received political assignments from the state. When the ideological constituent dissolved in the late 1980s, ethnographers found themselves in a methodological vacuum. At first, new Western theories began to pour in and fill in the gap, but in later years, neo-nationalist demands began to arise, and ethnography, archaeology, and linguistics were once again recruited to help support various political and territorial claims, "prove" the antiquity of an ethnic group, or "prove" the right of this or that group to self-government, to a piece of land, or to a history (compare Shnirelman 2000).

Western anthropologists are strangers to this game and are usually—and understandably—unwilling to play it. The local elites, because they cannot use them, try to dislodge them from the scene, often using straightforward methods. They accuse Western anthropologists of working for foreign intelligence agencies, of being "agents of influence" for the international oil industry, and even of attempting to cut off a piece of Siberia and proclaim it a sovereign country. Quite a few publications

making such claims have appeared in the local press; usually they end with demands "to put an end to" the insurgent activities of a certain anthropologist. Here is just one example, a passage from an email message sent to me by a colleague, NN, a social anthropologist with two years of fieldwork in Chukotka, which I received in April 2000 (translated from the Russian): "Well, Nick, this has happened: I have been refused permission to conduct fieldwork in Chukotka … the Department of Migration and Nationalities sent my papers to the Department of Agriculture for endorsement. They have a new boss there, and he disapproved." Disciplinary transformations are taking place—and the reactions of the "systems of power" are not always favorable.

All things considered, one can still say that Russian cultural anthropology is going through an international phase in its development, or at least it is entering such a phase. Unlike the 1990s, the first years of the new millennium seem to have brought Russia a growing economy and economic and social stability; unlike in the 1990s, the social sciences and humanities have again begun to receive state financial support, however modest and insufficient. Russian scholars are feeling much more confident about getting international research grants and scholarships, which put them financially on a more equal basis with their Western colleagues. Scholarly paradigms, theoretical frameworks, and methodological approaches are also becoming more level. Both epistemological and institutional relations between Russian and Western scholars are thus growing more balanced and apparently have at present a greater potential for cross-fertilization.

For me, what I have described in this chapter is more than a dispassionate history of science, or "transformations in Siberian anthropology." It is a history of a branch of science in which I have worked for the last thirty years, so my attitudes are far from disinterested. Like Peter Schweitzer (2001: 17), I feel that my view of the past is difficult to separate from my interests in the present and future of Siberian research. Schweitzer's central analytical concept was the notion of "national" versus "trans- or international" scholarship; he suggested a model according to which periods of claustrophobia in the development of Siberian studies (that is, times of "national" academic approaches) alternate with periods of openness ("transnational"). Because I am an "insider," I hope, rather egotistically, that Schweitzer's model, although undoubtedly true for the past, will prove wrong for the future—that in the future, Siberian anthropology will not again become a purely Russian research field, that other national and transnational research traditions will establish

themselves permanently in the realm of Siberian anthropology. I hope, in other words, that the claustrophobic years are in the past, whatever fluctuations the "systems of power" might undergo—although of course one never knows.

# Notes

1.  Much of this chapter, especially its second part, is but a brief overview of the main tendencies in Soviet ethnology in general, of which Siberian anthropology was merely a part. Because of my personal interests and experience, I am more familiar with linguistic research and, consequently, with the history of language research and language policy in Siberia.

2.  The journal *Etnograficheskoe obozrenie* was published until 1926, when its name was changed, first to *Etnografia* (Ethnography), from 1926 to 1929, and then to *Sovetskaya etnografia* (Soviet ethnography), from 1930 to 1991. After the collapse of the Soviet Union in 1991, it was changed back to *Ethograficheskoe obozrenie*.

3.  See the detailed bibliography of publications connected with the expedition compiled by Igor Krupnik (2001).

4.  See, for example, the obituary of S. M. Shirokogorov published by W. Muelman (*Etnograficheskoe obozrenie* 2002, no. 1), in which Shirokogorov's influence on European theoretical thought is discussed. See also Schweitzer 2001.

5.  Sternberg n.d. As far as I know, this address has never been published. Sternberg and Bogoraz established the Geographic Institute in 1916 as an ethnological research institution within St. Petersburg University. In the early 1920s it became the nucleus of the famous Institute of the Northern Peoples, later transformed into the Northern Department of the Hertzen Pedagogical Institute, the main educational institution for indigenous Northern minorities.

6.  From the modern standpoint, despite the notoriety of Marr's speculations, there was in them a grain of reason. If one sets aside his "stadial theory" (i.e., that all languages pass through the same stages of development, from primitive to developed, and the stages coincide with the social and economic stages of societies), his ill-famed "four-element" theory, and his thesis that each social class spoke a different language (see Brandist 2002b), there remain Marr's powerful thoughts about the "interbreeding" of languages, cultures, and peoples, his ideas about their mutual influence, and his point about the

unified principles determining the development of languages and cultures. According to Marr, all modern languages and peoples were mixed; besides, language, culture, race, and religion were historical categories, which meant that they changed over time (Marr 1915: 287, quoted in Shnirelman 1993: 53). We find here a completely different approach to ethnicity ("race," in Marr's terms) as a resilient category, an approach that looks more like modern ones than like the "primordial" theories of ethnicity that later became dominant in Soviet ethnography.

7. *Bulletin of Official Orders and Communications of the Ministry of Education,* 13 January 1923 (no. 7): 10.

8. See the speech given by N. M. Pokrovskii before the First All-Russian Congress of Regional Councils (26 January 1923) on the enlightenment of nationalities, in *Bulletin of Official Orders and Communications of the Ministry of Education,* 13 January 1923 (no. 10): 12. See also the presentation by Rosen in the same source (p. 15).

9. For those unacquainted with Stalinist political rhetoric, "imperial chauvinism" (*velikoderzhavnyi shovinizm*) and "local nationalism" (*mestnyi natsionalizm*) were two accusations the Bolsheviks used alternatively to charge and purge those who carried on their shoulders all cultural work. Too much attention to the general (whether in language, culture, habits, or school curricula) put the person at risk of being accused of imperial chauvinism (and imprisoned). Too much emphasis on the particular—wherever that might be—led to charges of local nationalism, with the same result. To make things utterly hopeless, only one person, Stalin, knew the correct balance. For example, what word should one suggest as normative for a schoolbook in Yupik Eskimo to name a hospital? If one used *bolnitsa,* a Russian borrowing, then one was accused of imperial chauvinism. If one coined an Eskimo derivative, *aknighvik,* or "place where one is sick," then one was accused of local nationalism. No way out.

10. This and other letters by and to Franz Boas are quoted from the collection of Boas's professional correspondence housed at the American Philosophical Society in Philadelphia. I used microfilm copies at the New York Public Library, as well as the collection of the American Museum of Natural History.

11. Alexander Forstein was born in 1904 in Marseilles, to which his parents had emigrated from southern Russia (presumably escaping from the pogroms). In 1911 he was brought back to Russia and lived in Rostov. He entered the university in St. Petersburg in the Department of Ethnography, graduated in 1926, and got a job and a postgraduate position at the Institute of the Northern Peoples. In 1927 Bogoraz sent him to Chukotka to conduct research and to teach at a school there. In 1927–29 he lived on Cape Chaplin; in 1929–30 he went to Khabarovsk, where he married; and in 1930–33 he returned to

Chukotka, where he worked as director of social and cultural programs for the District Executive Committee. In 1933 he returned to Leningrad and became a research fellow at the Institute of Anthropology and Ethnography. In May 1937 he was arrested as a "Japanese spy," allegedly a member of a counter-revolutionary organization (clearly invented by the KGB) led by another Siberianist, Yakov Koshkin. As a "member of this organization," he was accused of counterrevolutionary propaganda among the native population of Chukotka for secession of the Far East, as well as of counterrevolutionary work among students and faculty of the Institute of the Northern Peoples against Marxist theory in science. He was sentenced to ten years in labor camps, spent the years in the Magadan area, was released in June 1947, and went to live in a small village in Kursk district. Later he moved to the Caucasus and worked as head administrator at a power plant there. He was rehabilitated in 1956. He never returned to his scholarly research. In the late 1960s a Norwegian linguist, Knut Bergsland, tried to find him and establish contact with him, but Forstein abruptly refused all contact—he had had enough of this, he said.

12. The phrase "national in its form and socialist in its content" is another famous invention of Stalin's, well known to everyone born in the Soviet Union before the mid-1980s.

13. "People often take pride in their national history, contending that ancient people are 'their' ancestors and some elements of ancient culture are 'their' cultural 'heritage.' Competition for ancestors [compare the subtitle of Shnirelman's 1996 book, *Competition for ancestors . . .*—N.V.] and heritages are often connected with political disputes between neighboring nations. Because nations and ethnic groups can be solid communities only synchronically and are diachronically challengeable, it is scientifically not very meaningful to determine which ancient or medieval communities are their ancestors. This should be better understood as a creation of myth than [as] academic research" (Tomohiko 2002: 163).

14. In this section of the chapter, I am drawing on materials from a symposium titled "Who Owns Siberian Ethnography?" held at the Max Planck Institute for Social Anthropology in Halle (Saale), Germany, on 7–9 March 2002. I am also using the text of a review of that symposium that I wrote jointly with Anna Sirina (Vakhtin and Sirina 2003; see also Gray, Vakhtin, and Schweitzer forthcoming).

15. I list here only some of these young scholars—those who worked in Siberia in the 1990s and have already completed their research: David Anderson (ethnic processes and ethnic identities of the Taimyr Evenki and Dolgan, 1992–97); Alexia Bloch (Evenki residential schools and indigenous education, 1996–98); Atsusi Esida (the social and cultural situation among the Nenets, 1995–98); Bruce Grant (Sovietization among the Nivkhi of Sakhalin, 1993–95);

Patricia Gray (current social processes and power relations in Chukotka, 1995–96); Anna Kerttula (Chukotkan newcomers, Yupik Eskimos, and Chukchi identity, 1989–92); David Koester (socialization of the young generation of Itel'mens in Kamchatka, 1994–96); Hiroki Takakura (social landscape and reindeer herding among Evens and Yakuts in Yakutia, 1996–99); Gail Fondahl (the effects of industrialization on Evenki reindeer herding, 1996–98); Peter Schweitzer (social organization of indigenous peoples of Chukotka, 1990–98). These "veterans" have been followed by a strong new wave of young social anthropologists who already work, or are planning to work, in Siberia.

16. For books based on firsthand research in Siberia, see Anderson 2000a, Balzer 1999, Golovnev and Osherenko 1999, Grant 1995, Kasten 2002b, Kerttula 2000, and Rethman 2001. Similar to these, but based on archival research, is Slezkine 1994.

17. It looks as if the decline in the numbers of Russian Siberianists has now ended, and an opposite tendency seems to be under way. Scholars from other disciplines are turning to Siberian research; new centers are opening, and the old ones are slowly recovering from the shock of the economic crisis of the 1990s.

18. Compare the excellent discussion of the difference between "rapid cultural change" and "deterioration of culture" in the book *Arctic: Contributions to Social Science and Public Policy* (1993). The book has as an appendix a document called "Arctic Social Science: An Agenda for Action," written in 1989. Much of what it says about the American Arctic applies also to Siberia.

19. The conference, organized by Peter Schweitzer, was titled "Siberia and the Circumpolar North: A Contribution of Ethnology and NGOs."

# In Search of Anthropology in China

## A Discipline Caught in a Web of Nation Building, Socialist Capitalism, and Globalization

### *Josephine Smart*

The history of anthropology in China is poorly known outside of China. Among the small body of publications on the subject in English, Gregory Guldin's monograph *The Saga of Anthropology in China* (1994) and edited volume *Anthropology in China* (1990) are the most comprehensive. Rather than repeat these historical accounts, in this chapter I explore the discipline's transformations in the context of tensions and alliances between China's nation-building agenda (past and present), as embodied in ideological rhetoric and praxis; socialist capitalism, which arose from the post-1978 economic reform; and globalization, in the form of networks and exchanges with persons and institutions outside China. In charting these transformations, I hope to convey the particularistic nature of anthropology in China and offer some sense of its future direction.

## A Brief History of Anthropology in China

Guldin described the history of anthropology in China as neatly segregated into four phases: the pre-1949 phase, during which anthropologists "adopted Western approaches wholesale"; the Soviet phase, from 1940 to the late 1950s; the Mao phase, identified with the turbulent ideological campaign of the Cultural Revolution, 1957–77; and the post-1978 economic reform phase, which saw the revival and expansion of academic disciplines (including anthropology) in parallel with the emergence of socialist capitalism (Guldin 1994: 6–9).

A recent document produced by the Chinese government in its bid to host the sixteenth congress of the International Union of Anthropological and Ethnological Sciences (IUAES) in 2008 identified a similar chronology for the development of anthropology in China, with some variations (China Applicant Committee 2003a: 1–2). According to it, the pre-1945 years were the "golden phase" of Chinese anthropology, marked by active research and publishing. It was interrupted by the Japanese invasion and the civil war between the Chinese Communist Party and the Nationalist Party (Guomindang). The 1950s were the "second golden phase," marked by the consolidation and expansion of ethnic minority studies. Researchers (anthropologists and ethnologists) were collected and housed together at the Central Institute for Nationalities in Beijing. Finally, the years from 1978 to the present were the "third golden phase," characterized by a revival of anthropological departments and research institutes throughout China and the increasing internationalization of Chinese anthropology through faculty and student exchanges. In the 1980s, selected work by Lewis Henry Morgan, Bronislaw Malinowski, Claude Lévi-Strauss, Lucien Lévy-Bruhl, Margaret Mead, Ruth Benedict, Leslie White, Franz Boas, Marvin Harris, Edmund Leach, A. R. Radcliff-Brown, Elman Service, and several Russian and one Japanese scholar were translated into Chinese (for more details, see China Applicant Committee 2003c: 6–7).

Social anthropology and ethnology—which, as I discuss later, are not a single discipline in the Chinese academic system—were first introduced to China in the early twentieth century. Nankai University in Tianjin was the first institution to offer a course in anthropology, in 1923. By 1928 the first department of ethnology and department of anthropology were created within the Academia Sinica in Beijing. In 1926, Cai Yuanpei published "Shuo minzuxue" (On ethnology), which became a seminal work for subsequent generations of Chinese ethnologists (China Applicant Committee 2003c: 2).

The early introduction of anthropology in China was dominated, however, by physical anthropology, paleoanthropology, and archaeology. The archaeological project at Zhoukoudian, headed by Johann Andersson, Davidson Black, Pierre Teilhard de Chardin, and others, confirmed the discovery of Peking Man in 1929. This breakthrough contributed to the early consolidation of archaeology as a separate and independent discipline in China (Guldin 1990: 7). In conjunction, physical anthropology and paleoanthropology were established early on within independent institutional frames.

In contrast, ethnology and social-cultural anthropology were offered piecemeal within departments of history, literature, and sociology

throughout the 1930s and 1940s. Ethnology subsequently dominated the discipline in terms of numbers of practitioners. The most recent records show a total of sixty-three departments, institutes, or research centers that support degree-granting training or research facilities in anthropology, ethnology, or both (China Applicant Committee 2003b). Of these, fourteen institutions offer PhD programs in anthropology, ethnology, or both within greater China (including Hong Kong and Taiwan). There were only two stand-alone departments of anthropology in China proper (excluding Hong Kong and Taiwan): Zhongshan University in Guangdong province (also known as Sun Yat-sen University as of 2003) and Yunnan University in Yunnan province. It is worth noting that doctoral training in anthropology in China is a post-1980 phenomenon (table 4.1).

## Foreign Influences and Local Situations

Japan was the first source of influence in the development of anthropology in China, around the turn of the twentieth century (Guldin 1994: 34). This fact speaks of two historical specificities. The first is the strong influence Japan had on the academic disciplines in China. Japan was upheld as a model of modernity in the nineteenth and early twentieth centuries. Many Chinese students received their university educations in Japan long before Europe and the United States became educational destinations. Second, Japan was the first nation in East Asia to foster interests in anthropology, especially in human evolution.

This Japanese influence was later overshadowed by Soviet, American, European, and British influences as foreign researchers and scholars arrived in China to conduct field research and to lecture in a visiting capacity at Yanjing University, Tsinghua University, Peking University, and other key institutions in the north. Many of these foreign scholars were prominent figures in their respective fields of physical anthropology, paleoarchaeology, linguistic studies, sociology, and social-cultural anthropology. In addition, the strong linkage between Yanjing University and the University of Chicago brought A. R. Radcliff-Brown (1935), Robert Park (1931–32), and Robert Redfield (1948) to China (Guldin 1994: 43–46).

Cai Yuanpei, the founder of anthropology in China, was educated at the University of Leipzig, Germany, in 1907–10. He then founded the Academia Sinica in 1928 and set up an anthropology division within it in 1934, which provided the foundation for minority studies in China (Guldin 1994: 31–33). The American model of four-field anthropology

**Table 4.1** Institutions in China with PhD Programs in Anthropology, Ethnology, or Both

| Department or Institution (Date of Formation), Location | Year Program Established | No. Faculty Members in Department or Institution |
|---|---|---|
| Institute of Sociology and Anthropology (2000), Peking University, Beijing | 1987 | 49 |
| Institute of Ethnology and Anthropology (2002), Chinese Academy of Social Sciences (CASS, 1956), Beijing | 2002 | N/A |
| Research Institute of Ethnology and Sociology (1994), Central Institute for Nationalities (1950), Beijing | 1994 | 44 |
| Research Institute of Languages and Ethnic Minorities (N/A), Central Institute for Nationalities (1950), Beijing | N/A | More than 60 |
| Institute of Vertebrate Paleontology and Paleoanthropology (1953), Chinese Academy of Sciences (1929), Beijing | N/A | 109 |
| Research Centre for Socio-Cultural Anthropology (2001), Institute of Sociology (1980), CASS (1956), Beijing | Under planning | 65 |
| Institute of Cultural Anthropology (1999), Department of Sociology (1926), Tsinghua University (1925), Beijing | 1999 | 14 |
| Research Centre for Ethnic Minorities in Northwestern China, Lanzhou University (1950), Lanzhou | N/A | 22 |
| Department of Anthropology (1997), Yunnan University (1923), Kunming | 1997 | 14 |
| Research Institute of Ethnology and Sociology (2003), South Central University for Nationalities, Wuhan | 2003 | 24 |
| Department of Anthropology (1948), Zhongshan University (1924), renamed Sun Yat-sen University in 2003, Guangzhou | 1996 | 17 |
| Department of Anthropology (1982), National Taiwan University (1949), Taipei | N/A | 23 |
| Institute of Anthropology of Taiwan (1988), Taiwan Tsinghua University, Xinzhu | N/A | 13 |
| Department of Anthropology (1980), Chinese University of Hong Kong (1972), Hong Kong | 1992 | 6 |

*Source*: China Applicant Committee 2003b.

was introduced to China through American-trained Chinese students such as Lin Huixiang of Xiamen University (MA 1928, University of Philippines; Lin was a student of Henry Otler Beyer's, who was Harvard trained), Wu Wenzao of Yanjing University (PhD 1929, Columbia; a student of Franz Boas's), and Li Fanggui of the Academia Sinica (BA 1926, Michigan; PhD 1928, Chicago; a student of Edward Sapir's and Leonard Bloomfield's in linguistics) (Guldin 1994: 30–37).

The establishment of anthropology in China was not easy, and its rocky journey of uneven development was symptomatic of ongoing tensions in China between nationalism, globalization, and reaction against Western hegemony. It is not meaningful to speak of Chinese anthropology as a unified discipline of four subfields (archaeology, social-cultural anthropology, linguistics, and physical anthropology). The unconditional acceptance of the significance of archaeological and physical anthropological knowledge for national interests has guaranteed state funding and uninterrupted advancement in these fields over time, despite the many political and economic storms the People's Republic of China (PRC) has weathered since it inception in 1949. For instance, anthropometry, the measurement of human physical features such as head size and foot size, remains an important field of study in China today, on the strength of its applied linkage with consumer product development in Chinese industries. Linguistics was underdeveloped and remains so today, with a narrow focus on non-Han languages. Throughout China, sociocultural anthropology was more commonly known as ethnology, and this label remains in use today as a result of the historical subscription to ethnology (*minzuxue* 民族学, or the study of nationalities and cultural groups) instead of anthropology (*renleixue* 人类学, or the study of people, human beings), which likely was influenced by Cai Yuanpie's 1926 article "On Ethnology."

The origin of social anthropology in Western traditions was and still is received in China with much ambivalence, ranging from rejection to creative harnessing. This ambivalence toward and suspicion about foreign knowledge was amplified during the latter half of the Qing Dynasty as a result of the repeated humiliations the Qing government experienced in the international political arena, such as its defeat in the two Opium Wars of the 1830s. A strong sentiment before and after the fall of the Qing Dynasty in 1911 was the desire to harness Western knowledge and technology for nation-building purposes. It was in this context that the utility of sociocultural anthropology was prized during the 1930s: "This social science that Chinese like Fei [Xaiotong] were going abroad to study was a field that most Chinese academics hoped

would serve China in its struggle to become 'modern' and strong. Cai Yuanpei urged that ethnology be brought to China not just to add another discipline to Chinese academia but to use it to formulate better social policy" (Guldin 1994: 46).

This nationalistic, nation-building discourse was repeated when Liang Zhaotao met with key officials in the Ministry of Education to argue for the (re)installation of a department of anthropology at Zhongshan University in 1980. Liang argued, "All other countries have this discipline; why not us? We have such a glorious culture and large population. Why not us? We Chinese must study our one billion Chinese! We must study our bountiful material—if not us, who will? We can't leave this science only to the foreigners! Let anthropology make its contribution to the Four Modernizations!" (Guldin 1994: 12).

The American vision of anthropology as a four-field discipline had a stronger influence in South China. The Department of Anthropology at Zhongshan University was established in 1948 to offer four-field undergraduate training; it was interrupted from 1949 until its reinstallation in 1980. According to Guldin (1994), opposition to its reinstallation was widespread even among trained anthropologists and social scientists in China at the time. One of the greatest barriers to the development of a unified four-field anthropology in China is intra-disciplinary resistance to merging existing autonomous institutions, which are highly diverse in their funding security, disciplinary interests, and identity.

The general underdevelopment of anthropology in China today (excluding Hong Kong and Taiwan, which I discuss later) is a consequence of the ideological struggles and centralized political system particular to the post-1949 Chinese Communist state. First, within a formal hierarchy of academic subjects, anthropology is ranked as a second-tier discipline. This state-imposed ranking defines the inferior status of anthropology vis-à-vis sociology and ethnology, which are first-tier disciplines, and it directly affects funding and resource allocations (from both the central government and provincial governments) and the types of students who elect to major in anthropology. Entry into universities in China is based on a series of elimination processes, aided by examinations and tests. Only the best performers, a fraction of the entire high school student body in any given graduating year, get admission into the university system. These students want to study something "useful" and "marketable" in preparation for improved life chances and employment prospects. Anthropology has neither the status nor the image to attract the best students.

A second factor accounting for the underdevelopment of anthropology in China is the ideological premise of Marxism and communism, as understood within the Chinese context. According to this ideology, anthropology is a "bourgeois" discipline that deserves to be curtailed. This line of thinking and action was escalated to the extreme during the Cultural Revolution (1967–77). The interruption of formal education at that time did tremendous damage to anthropology and other disciplines by creating discontinuity in human resource development and the transmission and creation of knowledge. Even earlier, in 1949, the anthropology department at Zhongshan University in Guangzhou (in the southern province of Guangdong, adjacent to Hong Kong) was closed down. It had been the only free-standing department of anthropology in the country, and it remained so after its reopening in 1980 until a department of anthropology was established at Yunnan University in 1997. Anthropology in Chinese universities is usually subsumed under or joined with history, sociology, or other disciplines in the humanities and social sciences.

As a third factor, the post-1949 PRC government did not welcome criticism and prohibited critical social studies that might call into question the authority of the state and its ideological premises. What it allowed and supported was ethnology—the descriptive study of non-Han people ("ethnic minorities") and their cultures. Ethnology was and is intended to serve the interests of the state in its governance of non-Han people within the Chinese polity. Ethnology remains to date a first-tier discipline and is an important training ground for sociocultural anthropologists in China.

The tradition of studying non-Han minorities and doing descriptive cultural studies of rituals and artifacts continues in Chinese research, even in sociocultural anthropology, but a new, permissible field of study has emerged since about 1980. That is the field of rural development. Given the rapid rate of rural urbanization and economic development throughout China after the introduction of economic reform in 1978, the new focus on rural development in anthropology is seen as coherent with state interests, so long as it remains descriptive. The development of a critical anthropology independent of state ideology and intervention may be a long time coming in China.

## Anthropology in Taiwan and Hong Kong

Anthropology in Taiwan and Hong Kong developed along paths quite different from that of anthropology in the PRC, for historical reasons.

The Guomindang (GMD) established a parallel government in Taiwan in 1949 when the Chinese Communist Party took over China. Six institutions in Taiwan today offer research or teaching facilities related to anthropology or ethnology, all of them created after 1950. There is a strong emphasis on languages, archaeology, and studies of indigenous peoples. Two institutions, the National Taiwan University and Taiwan Tsinghua University, offer graduate training in anthropology (see table 4.1). Several foreign countries have had significant influence on Taiwanese society, due to either colonial or geopolitical legacies. Taiwan was a colony of the Netherlands (1624–61) and Japan (1895–1945), and the GMD government has maintained close political relations with the United States government since the 1930s. Many contemporary Taiwanese scholars were trained in the United States. The development of anthropology in Taiwan has been influenced by past and current Japanese, Dutch, and American contacts.

Even though Hong Kong did not return to Chinese sovereignty until 1997, it has a long history of social and economic integration with southern China. The intellectual exchange between the departments of anthropology at the Chinese University of Hong Kong (CUHK) and Zhongshan University beginning in 1980 was an important juncture in the development of anthropology in post-1978 China. Until then, however, anthropology had been virtually ignored in Hong Kong.

China ceded Hong Kong to Britain in 1941. The first university there, the University of Hong Kong (HKU), was a colonial establishment that provided higher education in the English language to a small number of students selected for their high achievement in public examinations. HKU was the training ground for senior civil servants for the British colonial administration. It remained the only university in Hong Kong until the Chinese University of Hong Kong was established in the early 1970s. There was never a department of anthropology at HKU, even though anthropology had been a well-established discipline in key British universities for a long time.

The omission of anthropology in the curriculum at HKU reflects two dimensions of the colonial experience in Hong Kong. First, in the British tradition, anthropology was a servant of colonialism, in that it provided relevant information about the "natives" that might facilitate effective colonial administration. The study of colonized people was for a long time done by and for the colonizers. For British colonial administrators to learn about Chinese customs, rituals, and history in their home institutions in Britain was fine, but it was not a mandate of the colonial government to encourage colonized people to study their own social and cultural change under colonialism.

Second, there was no demand for anthropology among students or society at large. Public awareness and understanding of anthropology are still very limited in Hong Kong. This lack of appreciation for anthropology may have stemmed from two sources: anthropology's origin in the West (even though its praxis is not exclusively of Western origin) and the fact that most people's main concern about higher education is the postgraduation return for their investment in terms of employment prospects, income level, and job security. For a long time the goal among university graduates was to get a government post, which was a de facto life-time meal ticket. Since Hong Kong "took off" in the 1960s and transformed itself into a global city of affluence, university graduates are even more preoccupied with postgraduation employment prospects. Hong Kong is ruled by the motto, "Money is everything," a mind-set that is gaining currency rapidly in the prosperous coastal cities and special economic zones in China. The idea of studying something "useful" and "practical" that will guarantee employment after graduation is stronger than ever. Top students compete fiercely to go into commerce, business management, engineering, law, medicine, and accounting, on the assumption, rooted in well-established attitudes and worldviews, that these subjects will pave the way to easy employment and high income. Anthropology ranks extremely low on this market-driven scale of worthiness.

In 1973, Professor Chiao Chien, a Taiwanese Chinese trained in the United States, joined the sociology-anthropology department at the new Chinese University of Hong Kong and was given a mandate to start up an independent department of anthropology. Established in 1980, this department remains the only department of anthropology among the eight universities in Hong Kong today. CUHK distinguished itself from HKU by assuming an "American" outlook, in contrast to the British image and structure of HKU. It was perhaps this "American"-style liberalism and venture seeking that brought anthropology to CUHK. The department at CUHK admits only twenty students per year into its undergraduate program. Most undergraduate students are female. It supports a good-size graduate program at both MA and PhD levels, with students from Hong Kong and mainland China at a fairly balanced gender ratio. Most of them do fieldwork in Hong Kong, China, or other Asian locales. In general, however, graduate students in China find it nearly impossible to do fieldwork outside the country, because of limited funding, lack of support from the government for overseas fieldwork, and sometimes a lack of proficiency in foreign languages. Though all students in China and Hong Kong receive training in English, the level of proficiency is uneven, and some students cannot work effectively in

the English medium. This further limits their opportunity and desire to do fieldwork outside their home locale, even if funding and institutional support are in place.

## Ethnology or Sociocultural Anthropology: What's in a Name?

The coexistence of ethnology and sociocultural anthropology as distinct disciplines in China today, juxtaposed with officials' and academics' common use of the terms *ethnology, sociology,* and *anthropology* as synonymous and interchangeable, can be mystifying. The state's ranking of ethnology as a first-tier discipline and anthropology as a second-tier one may add to the confusion for readers who consider the two disciplines to be equals. It is useful for non-Chinese readers to remember that the contents and structure of anthropology are not identical or uniform around the world, past or present. Ethnology and anthropology departments are co-present in Europe and other parts of the world today, and their coexistence speaks of the historical specificity of the development of these disciplines in a given place in the context of cultural diffusion, colonialism, and globalization. Similarly, the situation in China today requires historical contextualization, along three main axes.

The first axis is that of the open playing field that existed in anthropology's formative years, the 1920s and 1930s. Chinese anthropologies and other social sciences were strongly shaped in those decades by multiple streams of external influence from places including Japan, Europe, Russia, and the United States. No single force of influence dominated, and there was no "Chinese" school or tradition in anthropological knowledge to challenge these diverse and rich external influences. The American conception of anthropology as a four-field discipline did not dominate the institutional structure or academic discourse in China in the early twentieth century. The labels *ethnology* and *anthropology* were both used. Generally, American influence was considered stronger in the south than in the north. By 1949, contact with the West was cut off, and academic exchange with Russia intensified throughout the 1950s, under state sponsorship.

The second axis is that of state intervention. As I mentioned earlier, social researchers and scholars from throughout the country were housed centrally within the institutional framework of the Central Institute for Nationalities beginning in 1950. They were encouraged to study non-Han nationalities exclusively, in what was officially called

ethnology. This state intervention directly contributed to ethnology's institutional dominance over anthropology in the PRC, reinforced by state-regulated funding policies and the government's tiered ranking system for the disciplines. This bureaucratic intervention reinforced the existing fragmentation of the subfields of anthropology and their competition over disciplinary boundaries and access to resources and funding. It is precisely this intradisciplinary competition, in addition to the independent development of each well-established subfield over many decades, that has been the source of greatest resistance to the post-1978 effort by select institutions to establish four-field anthropology in China. The distinction between ethnology (*minzuxue*) and anthropology (*releixue*) has been firmly established in China and will likely persist until further state intervention.

The third historical axis is the post-1978 effort to reintroduce anthropology by Zhongshan University (1980) and Xiamen University (1984). This effort gained wider currency as the opportunity for overseas studies and academic exchanges increased under the rubric of the Open Door Policy. The effort to establish anthropology in China since the 1980s is an interesting indication of two intersecting forces. The first is American hegemony over the discipline throughout the world, as measured by the universal currency of the concept of four-field anthropology and the practice of renaming what was previously called ethnology as "social" or "cultural" anthropology. The second is the growth of Western influence in post-1978 China, over everything from consumer culture to academic disciplinary structure. Chinese anthropologies had their origins in Western influences at the turn of the twentieth century but were forced to develop on their own from 1949 to 1978. Now the field is once more wide open to external influences. This time, American dominance is likely to overshadow the other streams of influence.

## "Soft" versus "Hard" Science: An Image Problem for Anthropology

The Chinese are known for their respect for education and their subscription to Confucianist ethics, perpetuated by a well-established system for the selection of government officials based on academic merits. Scholar-officials must pass a series of public examinations before they are eventually appointed to a government post, a system that originated in the Qin Dynasty, more than two thousand years ago. In preparation for these public examinations, young men once spent years reading the Confucian classics according to a curriculum stipulated by the

imperial court. These classics dealt with subjects that would be familiar to students today in philosophy, psychology, political science, social studies, folklore, anthropology, public administration, and economics. In short, the governmental bureaucracy of China until 1911 was staffed by scholars trained in the humanities and social sciences.

Once they were on the job, these scholar-officials were expected to learn about the sciences relevant to water control, agriculture, mining, energy extraction, and animal husbandry, in order to fulfill their expected role in improving people's livelihoods. They were also expected to learn about law and punishment (for the maintenance of social order), astronomy, military strategies and technology, and historical documentation and to acquire other skills that would contribute to national defense and governance. Their basic training in the humanities and social sciences formed the foundation upon which they later acquired knowledge in sciences and technology. Given this historical emphasis on the humanities and social sciences, one might expect anthropology to have gained acceptance and popular appeal in China. That such is not the case requires an explanation.

In contemporary Chinese societies, both in China and overseas, it is a common observation that parents and students strongly favor certain subject matters that they consider desirable and useful. These subjects tend to fall into two major categories: the sciences (e.g., engineering, computer science, industrial chemistry, biology) and professional studies (e.g., accounting, education, medicine, dentistry, pharmacy, business management). These widely shared preferences among the Chinese are also observed in populations of non-Chinese origin; they are shared sentiments that speak of the power of the market-economy mentality, which equates educational investment with returns in income and status and which defines the worth of knowledge by its marketability. The shared concern about prospects for income-generating employment after graduation also speaks of the powerful grip of the market economy over our everyday lives, in which our existence and well-being are determined by our access to money—for most of us this access is facilitated by employment—which pays for all the goods and services we depend on.

Within the context of the market economy, one can make sense of the overrepresentation of women among anthropology undergraduates in Hong Kong. Hong Kong is modern and Westernized, and it has a high rate of female labor participation. But certain "traditional" values place differential expectations on the sexes. A man is still expected to be the breadwinner, the head of the household, the leader of the

pack. Women who marry well are admired for their luxury of staying home and having an easy life. Stay-at-home husbands are not admired in the same way. Thus, it is more acceptable for young women than for young men to study something "frivolous" and "without obvious utility"—such as anthropology. In the rest of China today, unlike in Hong Kong, the gender ratio among anthropology students is described as quite balanced. I wonder if this will change as China progresses along its current trajectory of economic development and integration with the global economy. Will the penetration of the free-market economy revive some "traditional" expectations of sex roles? Will we see in the future a steady decline in the number of male students in anthropology?

The preference for the sciences and professional degrees was established well before the contemporary period. I believe the qualitative switch from an emphasis on the humanities and social sciences to the sciences and professional degrees took place in the 1880s in reaction to China's repeated defeats at the hands of European powers, which won on the strength of their greater technology in military weapons and transport. The Qing Dynasty, labeled the "sick man of Asia," mounted a major reform effort near the end of its reign to modernize the nation. One of its efforts involved sending selected young boys and teenagers to Europe and the United States for education during the 1870s and 1880s. These boys were put through elementary, secondary, and university educations to acquire training in engineering and other sciences, so that they could contribute to the modernization of the nation upon their return. They were told to learn Western knowledge to be used in the Chinese context. The state's emphasis on the sciences during this period of reform was clearly a departure from the traditional mode of training in the humanities and social sciences expected of its chosen bureaucrats. After the fall of the Qing Dynasty in 1911, Western thought and technology continued to be viewed favorably by both Communists and Nationalists in their struggle for control and survival. In the post-1949 period of nation rebuilding, the emphasis on the sciences and "useful" or "practical" knowledge continued. The most recent group of Chinese leaders, who took office in 2002, are all men trained in engineering.

## Network Building and Institutional Linkages: Current Situations and Future Trends

Scholars in China share the opinion that institutions in northern China tend to be more conservative and politically oriented than those in

southern China, because of their proximity to the seat of administrative power in Beijing. In the south, distance from Beijing and the legacy of overseas contacts made through out-migration support a more progressive outlook. Until the opening up of China in 1978 to welcome foreign investment and technology in support of the Four Modernizations, contact with the outside world was tightly controlled. Few Chinese citizens had the privilege of traveling abroad or the means to do so. Closely supervised visiting delegates from overseas were brought into contact with carefully chosen local citizens. In the early years of the PRC, contacts between Chinese academics and overseas colleagues were closely monitored and controlled. Being close to Beijing brought the northern institutions many more state-supervised contacts with Westerners in those early years. In contrast, the south, such as Guangdong, has always been in touch with the outside world through people's personal contacts in Hong Kong and other overseas communities via visits, mail, and remittances.

Before 1980, most contacts were initiated by visitors from abroad, and the visitors either came to China with a working proficiency in Chinese or had state-provided interpreters. Foreigners either were not allowed to conduct social studies in China or were closely supervised in their work by state officials. Not until the 1980s was fieldwork in China possible, and even then, only researchers with good local contacts in their field site could get their projects off the ground. Otherwise, they got bogged down by excessive bureaucracy. Anthropologists who were interested in China but could not do research there did the next best thing: they carried out their studies in Hong Kong (known as the "gateway" or "window" into China) or in Taiwan. Hong Kong in particular attracted many English-speaking visiting academics and became a central meeting place for China experts. The Department of Anthropology at CUHK became a strategic point of contact for visiting anthropologists from the West, and it played an important role in bringing its counterparts at Zhongshan University into these Western contacts throughout the 1980s and 1990s.

How have these contacts with the West affected the development of anthropology in China? In many ways they have brought about significant internationalization—through faculty exchange (to North America, Europe, and Hong Kong), conference attendance overseas, and the successful placement of top Chinese students in graduate programs overseas. But there are reasons for caution. These internationalization processes have been limited, and their future effects on the discipline are yet to be seen. Several important issues require closer attention in our thinking about the future of anthropology in the China region.

The first is the language factor. English is the international language for trade, business, and academic interaction, but its dominance creates problems for existing faculty members in China. Those of the pre–Cultural Revolution generation were trained in Russian, a state decision that reflected China's close alliance with the Soviet Union for a large portion of the twentieth century. These people, now in their sixties and seventies, are unlikely to learn another foreign language. The generation of scholars who entered university after the Cultural Revolution are now the key members in the institutions. These scholars, in their forties and fifties, suffered an extended interruption in their pre-university education as a result of the ideological campaign that raged for nearly ten years in 1967–77. As a result, their proficiency in English and other foreign languages tends to be weak if not poor. Chinese anthropologists were mostly underprepared for the surge of connection with the West in the 1990s, yet such contacts are beneficial in broadening their worldviews about institutional structure and pedagogical traditions. Another concern about English as a working language in anthropology is the continuing dominance of English publications and textbooks in the classroom.

The second issue is whether Chinese anthropology will become increasingly "indigenized" instead of increasingly integrated into the world system of anthropologies. A debate has arisen in East Asia around the indigenization of knowledge in reaction to the hegemony over academic excellence exerted by the English-speaking traditions (Kuwayama 2004a) and around indigenization as a means to strengthen the relevance of anthropology for China and the Chinese people (Zhou 2003: 10–12). Because of China's closed-door policy from 1949 to 1978, it has much "catching up" to do in all areas of research, theory, and methods in anthropology. Yet this catching up is hampered by several factors. One is the linguistic issue I have already mentioned: many faculty members and students lack the language proficiency to comprehend publications in English. Another is the issue of access to printed material. Not only is it difficult and costly to acquire publications printed outside of China, but scholars within the country even have problems acquiring publications printed outside their home institution or city, owing to the nature of distribution networks.

Yet another factor affecting China's "catching up" is national pride. Why should the Chinese feel that they have to catch up? There are many outstanding ethnologists in China doing excellent work in the Chinese language; why should they have to look beyond their national boundaries for excellence and acceptance? Unlike Japan, China does not yet have the critical mass of scholars and students needed to support a

self-contained arena of research and publications in its own language—but it has great potential to develop such a critical mass in the future. It is too soon to tell whether China will opt for greater integration with the international body of scholars in anthropology or whether it will turn inward under a mandate of indigenization. At present, China's attempt to gain the right to host the sixteenth congress of the IUAES in 2008 is a positive sign of a move toward greater international integration.

A third issue requiring attention as we think about the future of anthropology in China is that of brain drain. There is a great deal of optimism in the country about the current generation of students, who are receiving better language training to facilitate integration with the international academic body. Some top students are being channeled into graduate programs in Europe, Australia, and North America. Whether these young scholars will make a real impact on the future of anthropology in China depends on several things. For one, will they return to work in China after their training overseas? Brain drain is a reality, and the Chinese government has formulated incentive packages to attract overseas Chinese to return to China, but with mixed success. Moreover, some students may decide to switch from anthropology to another field for better employment and income potential, thus diverting human resources from anthropology to other fields.

A final issue is that of public awareness of anthropology in China. The perception of anthropology as "lacking in utility" and less important than many other subjects is an important obstacle to the future development of anthropology in Asia. There is some hope that the Chinese government may elevate anthropology from the second tier to the first in the academic ranking system. But even without such state-imposed ranking, top students are not enrolling in anthropology. What can be done to raise public awareness of the utility and worth of anthropology in Asia? And can awareness be raised within a political context that suppresses critical theory and analysis of the state?
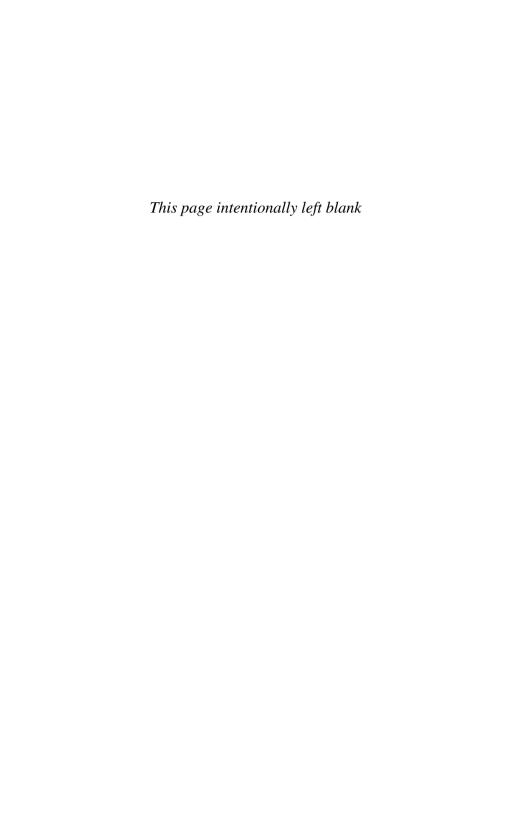
## Concluding Comments

Anthropology in China has been shaped by many factors—geopolitical, cultural, economic, and ideological—both historical and contemporary. Anthropology in China is not a single unified field, nor is it now singularly dominated by American models or Chinese indigenization. The mandate of nation building continues to be a guiding force in the development of research and teaching in Chinese anthropology, lending strength to the already established ethnological studies (*minsuxue*) and

the emergent field of rural development studies. China's increasing integration into the global economy is bound to strengthen and amplify the exchange of ideas between Chinese anthropologists/ethnologists and their counterparts around the world.

The integration of anthropological knowledge generated in China within the wider global system of knowledge calls attention to the dominance of English as a universal language of communication among scholars and businesspeople. Besides the language issue, there are issues of funding, access to overseas education and contacts, state policies, and economics. It is interesting that greater economic development and globalization have not made anthropology more attractive to students in greater China.

The future of anthropology in China cannot be assessed easily or readily. The recent boost in government support for expanding anthropology in China is an encouraging sign that things are on the upswing for the next generations of anthropologists there. Will they take on anthropology? What kind of anthropologies will they create? What shape will the integration of Chinese anthropologies within the world system of knowledge take? We will have to wait for some years to learn the answers to these questions.

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# Mexican Anthropology's Ongoing Search for Identity

*Esteban Krotz*

From a certain point of view, the history of anthropology seems to be one of continual growth regarding the sociocultural phenomena, areas, and configurations under study, topics and forums of debate, educational and research institutions, and analytical, methodological, and technical perspectives. In geographical terms, the corresponding concept would be that of extension: from a small nucleus of specialists who emerged during the second half of the nineteenth century in Europe—"a small peninsula of the Asian land mass" (Wolf 1987: 465)—and what was then its North American prolongation, anthropological practice has expanded to the point that today it is present in many parts of the world.

Yet this manner of reconstructing the development of the discipline, though in a certain sense justified, carries with it the danger of passing over anthropology's internal heterogeneity, its fractures and discontinuities, and of being unable to adequately take into account the emergence of new situations.

One such novelty of recent decades (despite having begun, in some cases, considerably earlier) is the increasingly noticeable, relatively autonomous existence and reproduction of particular anthropological traditions in many countries of "the South." These regions were previously considered solely or almost exclusively sites for the conduct of anthropological research generated in and directed by institutions located in the founding countries of the discipline.

One of the first important reflections on this topic took place during a Wenner-Gren symposium held in 1978, called "Indigenous Anthropology

in Non-Western Countries" (Fahim 1982). As Talal Asad pointed out in his contribution to the symposium volume, the existence of "non-Western" anthropologists, as they were called then, is not the same thing as the existence of non-Western anthropologies (Asad 1982: 284). And indeed, the character and properties of the so-called peripheral anthropologies, or anthropologies of the South (Krotz 1997), are yet to be defined.[1] Yet how could it be supposed that the anthropological discipline, created originally to study "others," who were so in relation to the industrialized, urbanized, and literate European world of the nineteenth century, has not changed in some way after taking root, various generations later, in precisely those different cultures and upon being utilized generally, though not always, for the study of those cultures' own internal "others"?

This chapter is meant as a contribution to the clarification of the anthropologies of the South through the analysis of one of them, that of Mexico. By this means, I attempt to identify features that might be used in elaborating a new perspective on anthropology as a "global discourse that entails an ensemble of paradigms, styles, practices and forms of political consciousness" (Ribeiro and Escobar 2003: 2).

To be able to adequately understand Mexican anthropology, it is necessary to examine the particularities of the national sociopolitical system of which it forms a part. The identity of this scientific discipline and professional practice in Mexico is profoundly rooted in the Mexican nation's own search for identity. For these reasons, I first discuss the relationship between the nation-state and anthropology as one of the keys for understanding the makeup of an anthropological tradition, and I outline some of the principal features of contemporary Mexican society and its anthropology. In the next two sections, I reconstruct the historical development of Mexican anthropology, emphasizing its treatment of indigenous peoples as its "internal others" and the dialectic between class and culture, ending with a brief characterization of the most recent decade. In the final section, I describe a series of traits and intrinsic tensions in Mexican anthropology today that might aid in comparing that anthropology with others of the South and North.[2]

## The Constituting Factors of Anthropological Knowledge in Mexico

Although the origins of the present phase of globalization can be found in the sixteenth century, and although the first anthropological scientists constituted themselves as an international (North Atlantic)

community in the second half of the nineteenth century, the nation-state remains the most important organizer of anthropological activity. The nuances of the nation-state's role depend on whether it belongs to the group of central or peripheral countries and on its general historical, cultural, and linguistic background. To the degree that the nation-state, with its peculiar tendency toward homogenizing key aspects of life, also directs the legitimation of the social order in force, it sets the limits for, and on occasion directly intervenes in, the generation of so-called scientific knowledge.

Scientific knowledge cannot be reduced to a set of declarations constructed according to certain methodological prescriptions. Rather, it is a result of the actions of concrete individuals and groups. Scientists are immersed in shifting networks of relationships—among institutions, among colleagues, with funding sources, with transmitters of knowledge, and with actual or potential users of the results of scientific investigations. These networks, as well as the ever-changing sociopolitical conditions that frame them, are not exterior to scientific knowledge but rather are factors that help constitute it.[3]

A scientific tradition or (sub)discipline defines itself principally through a specific question about reality, thereby "grasping" reality in a certain way. In the case of anthropology, this perspective is that of alterity: any particular phenomenon is considered and studied as an integral part of the sociocultural multiverse.[4] Yet because this perspective must always be elaborated from the experience of a unique, concrete, historical reality, and because the basis of scientific knowledge is precisely the reality under observation, that particular reality not only forms part of the generation of knowledge but also contributes to shaping the anthropological tradition or subdiscipline in question.[5]

The empirical field in which all social sciences develop is constantly changing. In anthropology, this experience is so strong that it has become one of the causes of the discipline's cyclical "crises": every so often, anthropology's practitioners fear the imminent disappearance of the phenomena their ancestors and classic authors dedicated themselves to understanding. But it must be taken into account that the sociocultural multiverse in which phenomena appear and disappear is, if not created by, then at least always shaped by the state in whose territory it is located. To make this statement is not to assign to the state its own essence. Rather, the state should be understood as a mechanism for "configuring" or "filtering" situations and, especially, "social problems." Sometimes the state itself generates these situations and problems; sometimes it only ranks them, makes them explicit, or tries to disperse

them, hide them, play them down, attend to them, or make them present in so-called public opinion, in expert circles, and in the actions of its apparatuses. Thus, the topics (and the perception of those topics) that anthropologists of a particular country take on as subjects of study (and to whose modification they sometimes try to contribute using the knowledge they generate about them) are established and sustained by the concrete actions of state institutions.

All of the foregoing suggests that in order to understand the configuration of any particular anthropological tradition, one must begin by considering the field of sociopolitical forces and rules in which the daily activities of the producers, communicators, and users of anthropological knowledge take place. This field is not merely the "context" for the generation of anthropological knowledge but also another of its dimensions.

For the preceding reasons, it is necessary to briefly describe the nation-state of Mexico in order to understand its anthropology as a generator of knowledge in the worldwide sociocultural mosaic. Mexico, a country of 2 million square kilometers, with nearly 100 million inhabitants and several million more nationals living in the United States,[6] is characterized by extreme social inequality. In its economy, which ranks among the top ten in the world, the legal minimum family wage in 2003 was equal to four US dollars per day.

Although Mexico is predominantly mestizo demographically and culturally, a significant percentage of its population belongs to one of the sixty-two native ethnolinguistic groups that live in Mexican territory. Of the approximately 10 million inhabitants considered by the census to be indigenous, 2 million live in large cities; the rest live primarily in the rural zones of the center and south of the country, under conditions—according to nearly every social indicator—that place them at the base of the social pyramid.

The third important characteristic of the country is its proximity to the world's principal superpower, which traditionally has exercised an enormous influence over Mexico, including its cultural environment. For example, a large number of the Mexican anthropologists who have completed academic degrees outside Mexico, as well as those who have taken sabbaticals or finished postdoctoral degrees, have done so in the United States. Until the recent past, there existed among the majority of intellectuals and in some spheres of the state apparatus a certain nationalistic tendency, which was most clearly expressed in Mexico's relatively independent foreign policy (Mexico supported Cuba and the Nicaraguan *sandinistas* and acted as mediator in the Salvadoran and

Guatemalan civil wars). However, neither the predominant consumption patterns nor many other aspects of daily life in Mexico escape the molding influence of the social and cultural norms generated in its powerful northern neighbor.

The social and political consequences of the Mexican Revolution of 1910 make up a fourth relevant feature. Not only was the 1917 Constitution one of the most progressive in the world when it was created, but it also effectively established, across more than seven decades and in spite of many manipulations and twists, an important union movement and, above all, educational and agricultural policies unique in Latin America.

Finally, during the twentieth century Mexico remained free of "national security" governments. This marked the country with a distinctive intellectual, political, scientific, and academic lifestyle and fostered, especially during the twenty most intense years of dictatorship in Central and South America, the arrival en masse of social scientists from other Latin American countries. Some of them were even permanently incorporated into the Mexican academic community.

Today, Mexican anthropology constitutes a large, well-consolidated disciplinary and professional field, although its visibility in public opinion and policy has varied over time. The Instituto Nacional de Antropología e Historia (National Institute of Anthropology and History, or INAH) and, to a lesser degree, the National Commission for the Development of the Indigenous Peoples, or CDI (until 2003 called the National Indigenous Institute, or INI), have always played important roles in this field. INAH controls almost all the archaeology in Mexico, administers the majority of history and ethnology museums, and is the source of employment for almost every archaeologist and many other anthropologists. Among the employees of CDI, professional anthropologists are a minority.

Two sets of statistics illustrate the current situation. The first is that Mexico currently has nineteen programs offering bachelor's degrees in anthropology, eighteen offering master's degrees, and nine offering doctorates, to which can be added various graduate programs that are not formally anthropology programs but which regularly allow students to write theses of an anthropological character. Although the majority of these programs are concentrated in the country's capital, one-third of the thirty-two federal entities have at least one academic program, and several more are in preparation. Between 1998 and 2000, 672 bachelor's (*licenciatura*) degrees, 235 master's degrees, and 98 doctorates were awarded in anthropology. The second is that in 2001, twenty-nine

scientific and general anthropological journals and annuals were being published in Mexico, along with a dozen specialized bulletins belonging to the different branches of anthropology.[7] In addition, anthropologists in Mexico write regularly in a good number of multidisciplinary and other social science journals, as well as in cultural supplements and even newspapers.

## The Conception and Treatment of Internal "Others"

Although modern, scientific Mexican anthropology has its roots in the political and social reordering provoked by the Mexican Revolution of 1910, it also forms yet another link in a tradition of thought that began with the Iberian invasion—namely, reflection on the confrontation of the civilizations involved in that event.[8] The landmark dispute in the sixteenth and seventeenth centuries over the human status of the inhabitants of the Americas and the consequent obligations of the colonial powers toward those inhabitants was in a sense the starting point for Mexican anthropology in terms of its principal others, the indigenous peoples.[9] These peoples would live out the centuries following the invasion separated from the Spaniards and Creoles in their "Indian republics." But in numerous social interstices, above all urban ones, a demographic sector later called mestizo grew larger and larger, augmented by the descendants of enslaved Africans.

The liberal discourse of independent Mexico did not distinguish among citizens on the basis of their ethnic roots. It did, however, legitimate the intent—only partially successful—of dissolving the geo-economic bases of the indigenous societies through the expropriation of their land. After the colonial power had almost totally destroyed all previously existing forms of sociopolitical organization, that land was possessed and worked under communal forms of organization. For this reason, and under the influence of positivist evolutionism, the intellectual precursors of the Mexican Revolution generally subsumed the indigenous population in the "rural problem" of the country, which they proposed to resolve through agrarian reform and the industrialization of the countryside.

The Constitution that emanated from the Revolution, with its extraordinary (for that period) individual guarantees and social rights, made no mention of the indigenous population. Yet two of its most significant articles, those referring to the ownership of land and to education, became the two principal axes around which indigenous activity would spin. To those axes—because of widespread ignorance about the indigenous population—intense research was added as a third.

An emblematic initiative in this regard was the action-research project on the population of the Teotihuacán Valley, the primary results of which provided the first Mexican anthropologist, Manuel Gamio, with a doctorate in 1921. It was a project of regional orientation considered to be a pilot, and it consisted of first getting to know in detail and later improving the situation of the highly indigenous population. A recent retrospective described this sort of project as follows:

> Anthropology in Mexico was born, really, from a very vivid, lacerating sociocultural reality that still exists today. From this reality comes the application of the integral method, which contemplates the study of the population in its three stages of development—pre-Hispanic, colonial, and modern—to learn its historical origins and thereby to be in a condition to help the population. In that way, in contrast to other countries where anthropology or one of its branches served for colonial ends, in Mexico it appears as a practice for the good of marginalized and traditionally exploited groups. (Matos Moctezuma 2001: 39)

Manuel Gamio had been a student of Franz Boas's, who had played a decisive role in the initial stages of the International School of Archaeology and American Ethnology in Mexico City, which was interrupted by the events of the Revolution. Later Gamio became the first head of the Department of Archaeology and Ethnography (created in 1917), which was not a branch of the education sector but rather formed part of the Secretariat of Agriculture and Development.

Indigenism expanded during the popular, nationalist presidency of Lázaro Cárdenas (1934–40), who laid the foundations for the development of anthropology in the country. In 1939 he founded INAH, which, besides being responsible for all archaeological sites in Mexico, has centers of administration and investigation throughout the country and carries out important editorial activities. During the Cárdenas administration, the First Inter-American Indian Conference took place in 1940 in Pátzcuaro, Mexico, as a result of which two important institutions were established. The first was the Inter-American Indian Institute—an institution of the Organization of American States and editor of one of the oldest social science journals in Latin America, *América Indígena*—which was founded during the conference but really began to operate in 1942 in Mexico City. The second was the National Indigenist Institute, established in 1948. The first academic department of anthropology was created as early as 1938;[10] it was incorporated into INAH in 1942 as the National School of Anthropology and History (ENAH). To this

day it is the most important center for anthropological training in Latin America. It offers, at the levels of *licenciatura,* master's, and doctoral degrees, all the subdisciplines of social anthropology, ethnology, physical anthropology, linguistics, ethnohistory, history, and archaeology.

This anthropological science was designed to occupy itself with important tasks for the country, above all with studying and conserving Mexico's pre-Hispanic and colonial patrimony and with examining and attending to the precarious situation of the indigenous groups. Whereas those who addressed the first of these tasks did so largely on a technical level, had no great theoretical pretensions, and were principally interested in shaping a panorama of ancient history, those who undertook the second task widely assumed the theoretical orientation and methodology of so-called North American culturalism. That approach was supported through the translation of its principal works at a government publishing house and through diverse research programs directed by North American anthropologists.

The emblematic figure of the epoch is Gonzalo Aguirre Beltrán (1908–96), a medical doctor turned ethnohistorian and social anthropologist, who during his long life combined directorial positions in indigenous activity with prolific writing on the theoretical and practical aspects of indigenism. According to his definition, indigenism was "not policy formulated by Indians for the solution of their own problems but rather that of non-Indians targeting the heterogeneous ethnic groups who receive the general distinction of indigenous" (Aguirre 1992b [1967]: 24). In contrast to the orientations of past generations, which Aguirre rejected as "assimilationist" or "incorporationist" and accused of lacking respect for indigenous cultures as one of the roots of the modern Mexican nation, he defined his position as "integrationist," in the sense of an "induced acculturation" (1992a [1957]: 43). Consequently, it was an indigenism that adapted the "ideology of the mestizo [as] the method and technique of national unification" (1992a [1957]: 119). With this goal, many "indigenist coordination centers" were founded. Located in the ladino urban nuclei of the so-called intercultural regions, they were to foment the slow incorporation of the indigenous population into national culture, principally through government actions in primary education, health, promotion of economic and communicational activities, and legal counsel in agrarian issues. Anthropological research would precede the establishment of such centers and would accompany the development of their activities.

Yet at almost the same time this original version of applied anthropology, which practically defined Mexican anthropology, reached

its culmination during the federal administration of 1970–76 (when Aguirre worked simultaneously as director of INI and as a subsecretary of the Secretary of Education), critiques of this form of anthropology were also becoming stronger. These critiques incorporated the dependency theories then emerging in Latin America, the rejection of US imperialism, dissatisfaction with explanations of social reality that derived from the so-called superstructure, the postulate that any local phenomenon must be understood in the frame of global processes of exploitation and domination of the Third World, and the hope for a near and radical transformation of Mexico's blatantly unjust social structures. Marxism became the articulating axis of what was frequently called "critical anthropology" or "engaged anthropology," which in a short time became hegemonic in the country. In an early manifesto, indigenism was denounced as an instrument of an unacceptable "Westernization and modernization of the indigenous" (Nolasco 1970: 85), the goal of which was to "eradicate the ethnic personality of the Indian" (Bonfil 1970: 44). Against this, the right to "self-determination" for indigenous groups and the construction of a "pluri-cultural state" was demanded (Bonfil 1970: 56). Others later rejected indigenism as part of the "capitalist project of dissolution of non-capitalist societies" (Diaz-Polanco 1981 [1979]: 37).

The development of this critique (full of internal contradictions as it was) and the search for alternative anthropologies were consolidated during a governmental period of nationalist and Third-Worldist tendencies and the intent, manifested largely in a new agrarian law, of reviving the revolutionary tradition dulled by the regimes following that of Cárdenas. As a result of all this, the indigenous population mostly ceased to be a subject of study. When it was studied, it was seen almost exclusively from the angle of its belonging to the peasantry, with which it seemed to share domestic and communal organization, submission to exploitation and domination, and the occasional protest. Simultaneously, the superstuctural sphere—customs and language, religion and cosmovision—which traditionally had occupied a central place in anthropological work, was reduced to the epiphenomena of ideology and alienation. And at nearly the same time, the dizzying urbanization that over the course of the 1970s converted Mexico, in statistical terms, into a predominantly urban country inspired a growing number of anthropologists to enter the fields of urban and labor anthropology. They brought with them the hope of finding in the mobilizations of these "new" social subjects the beginning of the longed-for social transformation, and they shared a conviction that they could contribute, with their scientific study, to that transformation.

But even after Aguirre left the indigenist organizations, his conception prevailed inside them, although they were later assimilated into the fight against what was first called "marginalization" and later "poverty." These phenomena were doubtless exacerbated throughout the country by the neoliberal policies instrumented after the explosion of Mexico's foreign debt in 1982.

In *México profundo* (Deep Mexico; 1996 [1987]), Guillermo Bonfil characterized the history of Mexico since the arrival of the Europeans as an unending battle between the Western and Mesoamerican civilizations and diagnosed the potential for updating the latter civilizations. For the anthropological community, the book was a call to attention for having forgotten the indigenous population. It reminded anthropologists that indigenous people had to be taken into account if they were to adequately understand the complexity of Mexican society and to reconstruct it on real bases of equality and freedom. Yet this, Mexican anthropology's most successful book, was unable to provoke a new indigenist policy, although it was a key point of reference in a brief interlude in which anthropologists attempted to promote a more participatory indigenism. It did contribute, in contrast, to the strengthening of Indianism and, in particular, to the self-esteem and mobilization of many indigenous organizations and intellectuals, with whom Bonfil maintained close ties and whom he promoted through different media.[11] It also contributed to cementing a more adequate concept of the "indigenous problem." The book made it clear that the "indigenous problem" entailed a questioning of the national model of the country and was not an obstacle caused by the backwardness of a demographic minority.

In the country's democratization, which grew stronger beginning in the mid-1980s and led, for the first time, to a candidate from an opposition party's winning the presidency of Mexico in 2000, the indigenous groups' situation and demands played practically no role whatsoever. In 1992, two controversial modifications to the Constitution brought them briefly back into the national limelight. On one hand, motivated by the International Labour Organization's Convention 169, concerning indigenous and tribal peoples in independent countries, and by preparations for the commemoration of the five hundred years of the American-European encounter, one addition to the Constitution made mention of the indigenous population for the first time in that document, recognizing it as the basis of the country's multiculturalism. On the other hand, modification of a different article put an end to agrarian reform and made peasants' land vulnerable to market forces, permitting allocation of the *ejidos* (the most common form of land

tenancy among the indigenous population) to individuals, as well as the rent and sale of *ejido* land.

Finally, it was indigenous people themselves who demanded the country's attention. On precisely the day that the North American Free Trade Agreement (NAFTA) went into effect, 1 January 1994, the Chiapanecan neo-Zapatistas initiated their armed uprising. Its brief military phase and heavily publicized demands provoked, over several years, an intense debate over the situation of Mexico's indigenous peoples and the national project. It also prompted a complicated process of negotiation between the rebels and the federal government, which remains unfinished even now.

At the same time, indigenist policy began to break apart. On one hand, new programs were established, and people of indigenous extraction were named to various directive posts. On the other hand, the number of uncoordinated indigenist agencies increased, the resources assigned to the indigenist apparatus were reduced, and the Inter-American Indian Institute's activities were frozen.

In the anthropological community, all these developments provoked an authentic boom in studies and events surrounding the varied—and often very "traditional"—topics related to the indigenous peoples, their cultures, and interethnic relations in the country, as well as reflections on the pluricultural character of the nation. However, anthropologists' efforts to systematize and share their knowledge with other specialists, political actors, the indigenous groups and movements, and the public in general still do not correspond to the country's objective needs. And few anthropologists are truly familiar with any of the indigenous cultures, from whom they are separated by a language barrier (among others).

In short, Mexican anthropology of the last three decades contrasts sharply with that of its first four decades as a modern science in the country. In its first stage, past and present indigenous groups were almost the only phenomena anthropologists studied, and the anthropological knowledge generated about these internal others was tightly linked to a strategy for their transformation, directed by the state, in the goal of completing the process of national formation. The second stage began with the eclipsing of this topic, but later—and in good measure due to mobilization by indigenous people themselves—the design of a model of the country that permitted their incorporation as different peoples became one of the principal topics of anthropological research and a perspective that permeated many other topics as well.

## The Culture-Class Dialectic

The history described in the previous section was interlaced with a sequence of hegemonic, paradigmatic positions in Mexican anthropology over the near century of its existence as a scientific discipline. I have already commented on the strong influence that North American culturalism exercised over Mexican anthropology in its first phase. Yet this influence kindled not mere imitation but rather a creative transformation of the culturalist theoretical orientation for purposes of understanding the Mexican situation and illuminating the formation of a national culture. Toward this end, anthropologists greatly reduced the weight of the relativist component of the original theoretical formulation, and they combined that formulation with an evolutionist perspective in which the nation-state constituted a universal stage—though one still not entirely achieved in Mexico.

According to a concept of culture inherited from the nineteenth century, the nation in Mexico could be thought of as nothing but homogeneous, especially in its superstructural sphere. Just how strongly national unity was understood as uniformity was still obvious in the late 1980s, when some Mexican anthropologists rejected the demand for indigenous autonomy, considering it unavoidably damaging to national integrity and sovereignty and even dangerous for being potentially separatist (and thereby exploitable by the United States to weaken the country in the same way it had done on the Atlantic coast of Nicaragua). At the same time, proponents of orthodox Marxist positions refused to recognize in noncapitalist forms of production anything except precapitalist survivals, and they objected to the multilineal model of evolution, contrasting it, as Aguirre had in his indigenism, with the idea of the necessary conversion of the peasants and the indigenous population into proletarians.

Because of the preeminence of the National Institute of Anthropology and History and the National Indigenist Institute within the anthropological community, the union of the four traditional anthropological subdisciplines—nuanced by the addition of an important historical component—was maintained. The historical component had already been expressed in the conception of the first nineteenth-century institution of importance for Mexican anthropology, the National Museum of Natural History, Archaeology, and History (founded in 1865), and it was constantly reinforced by the presence of archaeological vestiges everywhere, by the colonial centers in the major cities, and by the state doctrine's insistence on the mestizo character of the Mexican people. The last was represented emblematically in the "Plaza of the

Three Cultures" in Tlatelolco, where an inscription in stone defines the result of the final battle of 1521: "It was neither triumph nor defeat; it was the painful birth of the mestizo people that makes up Mexico today." The protection and reconstruction of spectacular historical sites and buildings, the creation en masse of archaeology-history-ethnology museums throughout the country, and the use of indigenist policies to achieve the cultural homogenization of the nation contributed decisively to the consolidation of Mexican anthropology. It thereby gained itself a prominent place among the social-science disciplines, an achievement rare even in those countries where anthropology was born.

Regarding the real transformations achieved in the indigenous regions by what Aguirre called the "Mexican school" of anthropology, few evaluations of these interventions exist outside of the National Indigenous Institute's reports. Yet it is striking how many "pilot projects" were never converted into projects of regional transformation. This was primarily due to the six-year rhythms of public administration, to the fact that the indigenist institutions were preponderantly limited to tasks of coordination and not execution, and to regional elites' resistance to indigenist activity. Despite expressing occasional admiration for the builders of the pre-Hispanic pyramids and temples, the elites frequently felt threatened by potential or real demands from the descendants of those builders in respect to land, financing, and public services. In this context we must remember that even today, the word *Indian* is used in Mexico more as an insult than as a plain denomination for one part of the population.

In spite of the serious conflicts it caused, the critique of "established" anthropology that appeared in the late 1970s developed rapidly into a substitute for the previously predominant theoretical orientation, especially in the university institutions in the capital (keeping in mind that at the time there were only two anthropology schools in the rest of the country, founded in 1957 and 1966, respectively). It led many Mexican anthropologists to distance themselves from indigenism (without, however, slowing down their engagement in other official agencies related to social policy). That the different branches of this "new anthropology" shared an interest in social structure and its precise position in dependent capitalism undoubtedly promoted greater attention to the previously little-known British and French social anthropology.[12]

But the general frame of debate and investigation was that of Marxism, at first rudimentary, mechanistic, and economistic, oriented by Soviet manuals, but later more flexible and influenced principally by then world-renowned French and Italian Marxists and Marxist

anthropologists. The thinking of a long series of Marxists considered "heterodox" or "renegade" by the "official" Marxism of the era, as well as the ideas of a good number of Central and South American left-wing social scientists, not a few of whom lived for periods of time as exiles in Mexico, were also important. Neo-evolutionism and multilineal evolutionism, widely fused with Marxism, also played an important role, explainable by Mexican anthropology's traditional attention to historical processes of long duration, as well as by its capacity to join diverse theoretical anthropological currents. During those years the opposition to "culturalism," which was often identified with North American "cultural imperialism," was so strong that the word *culture* practically disappeared from anthropological texts (see Krotz 1993). Even studies as well known as the analyses of the media done in South America's Southern Cone during the 1970s and the highly original critiques of capitalist ideology in general and the educational system in particular, such as those of Paulo Freire (1970) and Ivan Illich (1972), motivated few concrete anthropological studies.

Meanwhile, sociocultural aspects until then heavily researched, such as language, family relationships, normative systems, and religion, were left to one side. This was probably an extreme consequence of the search for more suitable models of social analysis that would bring to light the multiple relations of exploitation and oppression in force in the country, which manifested themselves particularly crudely in the rural countryside. Not a few anthropologists and students were tied, in one form or another, to political, social, and religious organizations and to movements of leftist orientation. Although they escaped the repression common in the rest of Latin America, they still remembered with anguish the pre-Olympic massacre of 1968. In light of all this, it is not strange that the anthropology generated from the late 1970s until the late 1980s featured a strong tone of denunciation and social criticism, almost independent from the intentions or personal preferences of many of its practitioners. The nonexistence of an industrial proletariat as the revolutionary subject predetermined by Marxist theory, on one hand, and the fascinating Cuban and Chinese revolutions, together with the ups and downs of Latin American and Vietnamese guerrillas, on the other, turned the analysis of the rural population extremely mechanical, especially the debate over the future of the peasantry and the seemingly endless controversy over the "Asiatic mode of production."[13]

In the 1980s the word *culture* slowly reappeared in Mexican anthropology, prompted by Gramscian Marxism. The concept of "popular culture" permitted a fruitful connection between the discipline's typical

micro-scale studies of urban barrios, rural-to-urban migrants, factory workers, and union movements, on one hand, and a global frame of analysis from a class perspective, on the other. As a result, the effective segmentation of work, daily life, political interests, and symbolic expressions in the largest sectors of the Mexican population came to light in increasing detail, and terms such as "cultures of work" and "urban culture" began to proliferate. At the same time, the analytical decontextualization of these social groups was avoided by maintaining a familiar, comprehensive theoretical scheme and the customary critique of the system. The significant advance in the democratization of political life beginning in the mid-1980s contributed to raising scholars' interest in the superstructural sphere, in the subjectivity of social actors, and, consequently, in the field of "political culture." Anthropologists also began to pay some attention to indigenous populations again, although for a long time only as part of the "popular cultures."

Since 1980, the institutional growth of Mexican anthropology has been continuous. Research centers have been founded, universities have opened departments of anthropology (almost always social anthropology),[14] and the numbers of museums and long-term archaeological projects have grown. Graduate degrees have multiplied (in the early 1970s there existed only one master's degree and one doctoral degree program, both at a private university). All of Mexico's anthropological institutions have benefited from increases in full-time faculty positions and from the relative ease with which conferences can now be organized and written works of all types published. Importantly, however, almost no anthropological libraries deserving of the name exist outside the capital, and no university has managed to gather a base of materials on any one theme, not even on the indigenous groups of its region, that would enable scholars to avoid going to foreign universities for any significant research in anthropology or history. Moreover, a large percentage of the research done even in academic institutions is limited to ethnographic description or historical "inventory."

Since the beginning of the 1990s, a somewhat disturbing, internally contradictory panorama has appeared. In the first of a set of contrasts, the presence of anthropology—especially of social anthropology/ ethnology—in academic institutions, on one hand, continues to grow and gain strength. Programs offering graduate degrees in particular continue to multiply and frequently attract professionals from other disciplines, social as well as medical and agricultural. On the other hand, a silent transformation of the university system—following the overall dictates of neoliberalism and the criteria of efficiency and

productivism—is leading to a decline in the importance of fieldwork, especially in teaching programs. It is also the reason many professors, most of whom represent the first generation in their family with an academic degree, have been pushed to organize their activities according to the rationality of productivism. Yet the anthropological community has made no organized response to this matter, which is highly relevant for the conception and future of science and scientific research.

A second paradox is that, on one hand, the number of publications coming out of Mexican anthropology—books as well as periodicals—is growing, and they are often of high quality. In this way, reflection about Mexican anthropology has also been augmented. Yet on the other hand, the infrastructure of the majority of Mexico's research and teaching centers does not even permit scholars to collect and familiarize themselves with the anthropological knowledge generated in their own country, much less that stemming from the rest of Latin America, the Caribbean, or other parts of the global South.

In a third contrast, although the presence of anthropologists in diverse areas of public administration and national political debate remains strong, the weakness of profession-based organizations has made it impossible for them to join forces to coordinate any internal debate or direct one toward society at large. In addition, a divide is growing between, on one side, anthropologists who work in academic institutions and tend to assume that they represent the entirety of Mexican anthropology and, on the other, those who work in state administration and frequently penetrate, in creative ways, both the public and private sectors, including the growing sector of nongovernmental organizations. In this context, the contrast between the political contents of many anthropological discussions carried out in general society and the depoliticization of university institutions also merits attention.

A fourth paradox is that, on one hand, an increasingly rich variety of topics is available for research and publication. Some matters that went uninvestigated for years, such as religion, the culture-nature relationship, life in the big cities, and art, now have a fixed place, and newer topics, such as relations between genders and generations, migration and political cultures, and juridical anthropology, have been added. On the other hand, all these investigations are carried out and written works published with almost no contact between researchers and without debate—as is evidenced, for example, by the absence of critical reviews of writings and events.[15]

Finally, Mexican anthropology's definitive recuperation of the concept of "culture"—that important identifier of the discipline—can be

highlighted as a characteristic trait of doubtless significance. Although there is no uncontested hegemony, a semiotic approach seems to predominate. Consequently, the superstructural angle (politics, law, religion, and communication) also prevails with respect to the indigenous topic. Paradoxically, what the first manifesto of the Zapatista movement denounced as "the undeclared genocidal war against our peoples," as well as the socioeconomic component of their demands—for "work, land, roof, food, health, education, independence, liberty, democracy, justice and peace" for all Mexican people (Comandancia General 1994: 35)—is left out of the line of vision. Could it really be said that the concept of culture, which was to correct the economistic and mechanistic narrowness of the sociologistic and objectivist vision previously in vogue, in the end did not remedy that narrowness but became merely a substitute for it? In effect, whereas before references to social class, the class struggle, imperialism, and revolution were almost obligatory, one now finds mention of difference, diversity, globalization, and intercultural dialogue.[16]

## Perspectives on the Identity of Mexican Anthropology

I hope I have shown that Mexican anthropological science has been so closely tied, on various levels, to the Mexican sociocultural and political system that it would be impossible to understand anthropology without reference to that system. The presence of indigenous peoples has been and continues to be defining for Mexican anthropology (certainly, it gives the term *multiculturality* a significantly different meaning in Mexico, as in all of Latin America, from that in Europe and the United States). Since the arrival of the Spaniards in the sixteenth century, these "others" have presented a problem of knowledge and of political and social action. At first the colonists solved this problem by assigning indigenous people the sort of subordinated coexistence typical of every colonial order. The liberalism of the nineteenth century then decreed the problem's inexistence. The postrevolutionary regime recognized it anew as a problem of national transition—for which reason it was not included in the Constitution—and fomented the establishment of anthropology as an instrument to diagnose the indigenous people's situation, starting from their most ancient antecedents, and to distinguish between what was conservable (which was little, according to Gamio, and more, for Aguirre) and what had to be suppressed.

   In that manner, indigenism was born as an original creation of research-action that was oriented more by anthropology's origins (the

concept of the nation in the nineteenth century) than by the model of Indian-white or black-white relations in the country where relativistic culturalism originated. In this sense, all of the most original theoretical formulations developed by Mexican anthropologists in the twentieth century—from Aguirre's concept of acculturation as a universal process and a strategy of social policy to Bonfil's model of the battle of two civilizations, in addition to different understandings of the peasant mode of production—shared an equally original, although not always explicit, multilineal evolutionist vision: on one hand, the possibility of a unique Mexican route to modernity, and on the other, the existence of a diversity of modernities.

One also can find in this evolutionary multilinearity the diverse intentions of Mexican anthropologists to identify a Mexican anthropological tradition in whose beginnings the places of Montaigne and Hobbes, Rousseau and Herder are occupied by Bernadino de Sahagún and Bartolomé de las Casas, José de Acosta and Francisco Javier Clavijero. Yet in Mexican teaching programs, Mexican anthropology (in isolation from other Latin American and Caribbean anthropologies) is presented more as an "annex" to or a simple "adaptation" of the dominant anthropology, which is seen as universal. Congruently, the occasional publication of Mexican anthropological texts in foreign journals of international circulation, all of which are published in the countries where anthropology originated, is sometimes more a mark of certification than of participation in the global process of communication.

The names of the precursors of Mexican anthropology show modern anthropological science to be the most recent link in five centuries of intellectual effort to clarify and define the collective identity born in New Spain. This task is far from being merely an intellectual enterprise— ever since the first creoles and the instigators of Mexican independence undertook it, the matter of identity has been of immediate political relevance, as it continues to be in decisions about the country's educational, scientific, and technological systems and in respect to the 10 million Mexican-born residents of the United States. The task can directly affect those who undertake it, sometimes in terms of personal identity, as when one discovers in one's own culture or even in one's own family history one of the roots of Mexican culture. Sometimes it affects people in terms of expanded or diminished employment opportunities, as when one begins to legislate on linguistic rights. And in political-cultural terms, the task always affects those who undertake it, as when one discusses the redefinition of the project of nation building. If pending legal reforms were to advance in a serious manner, then the

task of defining Mexico's collective identity would also affect people in economic terms, because scant public budgets would have to be redistributed in a new way.

The general malaise in the social sciences in many places, sometimes identified with and sometimes promoted by discourses on globalization and on the alleged end of the grand narratives, influences Mexican anthropology, too. But uncertainty about the future of democracy in the country weighs heavily today as well, beyond Mexico's economic problems and beyond the problems democracy is facing in other parts of the world. An entire generation, which pushed for and finally achieved a system more or less free of political parties, with effective vote counts, alternation in the principal positions of public administration, and the first steps toward a redefinition of the relationship between the legislative, executive, and judicial powers, seems fatigued in the face of demands for inculturation (indigenization) of the doctrine of human rights. It also seems fatigued in view of the conversion of demands for "respect for indigenous cultures" into demands for just and practicable laws and regulations. This generation seems weary of the uneven dissemination of the values of transparency, accountability, and critical participation among the social spheres. There are plenty of uncertainties about the meaning, role, and future of the state.

In this context, a parallel between the history of Mexican anthropology and that of the hegemonic anthropology portrayed in the textbooks generally used in Mexico demands attention. The hegemonic anthropology has endured, in the wake of its original paradigm of nineteenth-century evolutionism, repeated transformations of such breadth that they can be described only as ruptures. Structural-functional anthropology, diffusionism, and culturalism were understood as re-foundations of the discipline, just as Marxism would be half a century later and, three decades after that, the neo-Boasian anthropology that is sometimes called postmodern. Mexican anthropology, too, has known substitutions of this type. Curiously, unlike the first radical challenge of indigenism, which corresponded to a generational change, the recent eclipsing of Marxism was carried out without a generational change and almost without controversy.

In past decades, controversies about the dependent development of the country included the topics of the rules and cognitive characteristics of anthropological science, of its social function and its insertion into a world scene marked by cultural imperialism. Today this type of discussion is nearly extinct. This is at least partially due to Mexican social scientists' acceptance, without real questioning, of the external and internal

pressures they feel to make their activities conform with supposedly universal productivist canons—a conformity demanded, in many forms, by the national educational apparatus since the 1990s. Apparently this situation has reinforced the fact that in Mexican anthropology, the topic of its changing influence on the rest of Latin America—in reaction to which other parts of the South have criticized Mexican anthropology in much the same way that Mexican anthropologists have criticized the powerful anthropologies of the North—seems never to have posed any concern at all.

In this context it is interesting to note some indications that Mexican anthropology, independently of its theoretical perspectives or specific institutional insertions, may soon have to subject itself to a type of critical judgment that until now was thought necessary only for the hegemonic anthropology of the North. This is so because, to the degree that some indigenous cultures continue their revitalization, more observations of the type made by a Mayan ethnolinguist could emerge with respect to Mexican anthropology as practiced by non-indigenous people: "Our dominators, by means of anthropological discourse, have reserved for themselves the almost exclusive right to speak for us. Only very recently have we begun to have access to this field of knowledge and to express our own word" (Alonso Caamal 1997: 320).

## Final Comment

The trajectory and current situation of Mexican anthropology are as unique as those of any other country or of any other national or linguistic-cultural anthropological community. This anthropological tradition was not formed as the immediate effect of a simple dissemination from the main countries where the universal anthropological science originated nearly a century and a half ago. Rather, as a result of a diverse array of both exogenous and endogenous impulses, among which the diffusion of European and North American anthropology has played a fundamental role, Mexican anthropology has molded itself via a constant search to understand and to act upon the country's sociocultural diversity, which has served, until now, as its principal object of study.

Similar observations would ring true in any place where "native" anthropologists have made their appearance—native anthropologists who, in contrast with other anthropologists, principally "study or studied their own societies" (Boivin, Rosato, and Arribas 1998: 16).[17] A systematic inventory and comparison of the "anthropologies of the South" would reveal more clearly their characteristics, their weaknesses, and their

potential. This does not suggest that these anthropologies are totally distant from or necessarily opposed to Northern anthropologies.

Instead, clarifying the properties of the diverse anthropologies of the South, whose societies of origin formed part of the initial anthropological program as "objects of study," would help clear a path toward a new conception of a universal anthropology in which differences do not necessarily lead to hierarchical divisions but rather are accepted as variations on a common base—as occurs, for example, in the cases of French and English anthropology (Asad 1982: 284). In this way, the universal anthropological tradition would accept the historical challenge of assuming for itself the essential trait of the human reality it studies: to be one and diverse at the same time.

# Notes

1. For an elaboration of the concept of "peripheral anthropologies," see several works by Roberto Cardoso de Oliveira (1988, 1998); for that of "anthropologies of the South," see number 6 of *Alteridades* (http://www.uam-antropologia.info/alteridades/alteri_06.html). Some of these concepts are discussed in volume 17 of *Critique of Anthropology.* Some of the considerations of the Catalan anthropologist Josep R. Llobera (1990: 109–26) are also enlightening.

2. I am grateful to Andrés Medina and Roberto Varela for their observations on a previous version of this chapter and to Jennifer Cassel for translating it into English.

3. I elaborated this idea in greater detail in Krotz 1987. Andrés Medina (1996b) has demonstrated the intrinsic relationship between ethnography and the nation in the cases of three important anthropologists from Cuba, Mexico, and Peru, respectively. The anthropology-nation relationship also appears in several of the contributions to Fahim 1982.

4. For more on the anthropological question and alterity, see Krotz 2002: 49–76.

5. This dependence of anthropology on its available empirical objects is visible in so-called political anthropology, which emerged between the two world wars in those European countries interested in consolidating colonial administrations in areas that still had strong and even challenging cultures. North American anthropology at the time produced nothing equivalent for

the study of its preferred objects, American Indians, until World War II and its aftermath promoted the study of political cultures and national characters.

6. It should be remembered that until the middle of the nineteenth century, the majority of the US regions where Mexican migrants now live formed part of Mexican territory.

7. This type of statistical information can be found in the volumes of the annual *Inventario Antropológico,* edited since 1995 by the Department of Anthropology of the Autonomous Metropolitan University.

8. For further information, see Marzal 1981, Oehmichen Bazán 1999, Sánchez 1999, and Villoro 1979 [1950]. For a general history of Mexican anthropology, see volumes 1 and 2 of the work coordinated by Carlos García Mora (1987–88); for a brief synthesis, see Krotz 1991.

9. For that reason, Ángel Palerm (1974) devoted one-third of his history of the precursors of ethnology to authors such as Bernardino de Sahagún, Bartolomé de las Casas, José de Acosta, and Francisco Javier Clavijero.

10. This department was originally created as part of the National Poly-technical Institute's School for Biological Sciences, also founded during the Cárdenas years. The Polytechnical Institute was important as a place for the formation of professionals from the popular sectors and for devoting attention to the needs of the majority of the population.

11. See, as an example of this influence and relationship, the case of an educational program for indigenous ethnolinguists (Nakamura 2001).

12. The term "new anthropology" became the name of what is now the country's oldest anthropology journal, founded in 1975.

13. A detailed account of the controversy over the future of the peasantry can be found in Hewitt de Alcántara 1988. On the "Asiatic mode of production," see especially Palerm 1972, 1977, and, for a critique, Medina 1983: 55ff.

14. Medina (1996a: 89–91) has described the existence of a bifurcation in Mexican anthropology, with one line more on ethnological (associated with the Boasian approach) and the other more socioanthropological (associated with the indigenism originated by Gamio).

15. One exception, during the second half of the 1990s, was the discussion of indigenous autonomy, although in part it reminded one of the debates among Marxists in the past by centering on the question of the "true" representation of the interests of the "authentic" indigenous people.

16. However, there has been an attempt to show that "those anthropological approaches practiced by a minority remain healthy, that is, 'they are not in crisis'" (Jáuregui 1997: 52).

17. In this connection, even when Mexican students obtain graduate degrees in foreign countries, they usually conduct the fieldwork for their theses in their places of origin. This situation is not entirely new, but I do

not have room to discuss here either the fact that European anthropology, from its beginnings, concerned itself with its internal others (which led to the bifurcation of an ethnology directed toward the non-European social reality and an ethnology also called folklore, the study of popular cultures, directed at Europe's internal reality) or the interesting reflections on "anthropology at home" in some parts of the anthropology of the North.

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# Part 2

# Power and Hegemony in World Anthropologies

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# How Many Centers and Peripheries in Anthropology?

## A Critical View of France

*Eduardo P. Archetti*

The historian of anthropology George Stocking observed that within the Euro-American tradition in anthropology one could distinguish between "anthropologies of 'empire building' and anthropologies of 'nation building'" (1982: 172). It is usually assumed that the development of anthropology in Great Britain represents the ideal type of empire-building anthropology. There, social anthropology was perceived and defined as the child of colonialism, because its work was carried out in the colonial empire—for example, Malinowski in New Guinea, Radcliffe-Brown in the Andamans, Firth in New Zealand, Tikopia, and Malaysia, Evans-Pritchard in the Sudan, Fortes in the Gold Coast, Richard, Gluckman, and Schapera in British East and South African territories, and Leach in Burma (Goody 1995: 3). In contrast, in other European countries during the nationalist revival of the nineteenth century, a sharp distinction developed between *Volkskunde,* the study of the internal rural population and its folklore, and *Völkerkunde,* the inquiry into more distant others. In other words, anthropologies of nation building could coexist, if not necessarily with empire-building practices, then at least with an internationally oriented discipline that did research in colonial or noncolonial contexts outside of Europe.

Anthropologists had to work under shifting social and economic conditions. These included changes in political contexts and institutional organization; transformations in the sources of financial support; the development of new class structures; the effects of world politics and imperialism; and social conflicts both at home and abroad. Indeed,

national anthropological traditions in both Europe and North America were conditioned by the way these various factors affected the development of the discipline. The increase in anthropological employment during the years of economic depression and war in the United States (1929–45), for example, was related to gaining work in a relief agency or in the Department of Agriculture. According to Thomas Patterson (2001: 81), hundreds of anthropologists "were asked to apply their knowledge to problems confronting the country—unemployment, the conditions of Indian reservations, or the circumstances of small farmers. That social knowledge should have an immediate practical utility was, of course, the dominant viewpoint of the Rockefeller philanthropies, and most anthropologists employed by the government had already participated in Rockefeller-funded projects."

The more visible anthropologists, however, were those "travelers" who did research in the peripheries. Locally employed anthropologists doing applied anthropology and "native" anthropologists from the periphery were less mobile and consequently less visible. In this sense, and from the perspective of the periphery, a kind of unity existed: "international anthropology" was consolidated in different metropolitan centers. In most cases, however, this view was partial, because "nation-building" practices—if perceived at all—were not defined as belonging to the international core of social anthropology.

Different histories of anthropology have tended to reproduce this general pattern. Let me give some examples. In Thomas H. Eriksen and Finn S. Nielsen's book on the history of anthropology (2001), the focus is on the production and circulation of ideas and schools of thought of the intellectual centers, specifically those representing the Euro-American traditions. The anthropologies of the peripheries—Mexico, Brazil, India, and even the Netherlands and Scandinavia—are mentioned as belonging to the expansive moment. But because they never developed into strong and powerful centers, they are seen as contributing to the continuous process of centralization of the discipline (Eriksen and Nielsen 2001: 109).

Similarly, in the case of France, the role given to the founding fathers, Durkheim and Mauss, is central. Arnold van Gennep, who, in addition to his great theoretical contribution to the analysis of rites of passage, also developed the study of French rural communities, is presented as a marginal and innovative scholar who invented "anthropology at home" (Eriksen and Nielsen 2001: 48). The history of this branch of anthropology in France is left outside the scope of the authors' larger historical enterprise—international anthropology—as well as of other

nation-building anthropologies. The international history is, above all, the confluence over time of French sociology, American cultural anthropology, and British social anthropology, and their influences and historical ramifications. What is consistently missing is a more precise analysis of the new center-periphery relationships that developed when anthropology was internationalized.

Similar problems can be found in Robert Layton's (1997) anthropological survey. He sees the development of anthropological theory—from functionalism and structuralism through interaction perspectives, Marxism, socioecology, and postmodernism—as having produced ideas and concepts that impinged upon anthropology, but he does not trace the history of those impingements in full. Durkheim and Mauss are central figures for Layton in shaping French and world anthropology, and of course Lévi-Strauss is a determinant thinker behind structuralism. He does not mention Louis Dumont at all, however, and he notes van Gennep only in connection with Victor Turner's ritual analysis (1997: 205). In other words, the complex institutional histories are again left out. One feels that abstract ideas and concepts have been crystallized into models and, in this way, condition the description of social systems and concrete societies.

Alan Barnard's *History and Theory in Anthropology* (2000) reproduces, in some ways, Layton's outline. Barnard devotes chapters to each school of thought: diffusionism, functionalism, action-centered models, Marxism, structuralism and cognitive science, poststructuralism and postmodernism. Durkheim and Mauss, as well as Lévi-Strauss, are again at the center of theoretical developments and French world influence. Van Gennep is presented as a precursor to both Turner's and the Manchester school's approaches to understanding rituals and social processes. And in this case, Dumont has his place as a producer of a "seminal, regional-structural understanding of social hierarchy in India." Barnard observes that "[Dumont's] work has had its followers, and its critics, in all countries in which the study of the Indian subcontinent is a particular focus" (2000: 136).

In all three books, in discussions of Marxism and its variants, the authors justifiably highlight the roles of French anthropologists such as Godelier and Meillassoux. Thus, with some minor variations, the dominant narrative of the course of French anthropology defines a core of thinkers who, in different historical periods, were able to build theories for understanding culture and society in exotic contexts. Their importance is measured in terms of the force of their ideas and, in some cases, their empirical findings. A number of other relevant French

thinkers and anthropologists, as well as the societies they studied, are not included in the dominant trend. It is interesting that the distinction between "empire building" and "nation building" is not an important variable for these authors.

In the case of France, the narrow emphasis on the ethnographic and theoretical contributions of Durkheim and Mauss, as well as Lévi-Strauss and Dumont, who all conducted fieldwork and research outside the frontiers of the French empire, precludes a systematic consideration of French colonial anthropology. This "tradition" has been unanimously defined as belonging to the international core of the discipline and has extended its influence to other centers and most of the peripheries. The books I have briefly discussed, without explicitly aiming to do so, create not only international centers but also a heterogeneous periphery composed of people and institutions even inside the centers themselves. These books essentially concur with the judgment of Marc Abélès when he writes:

> Until the 1970s the most noticeable contributions to anthropological knowledge in France were produced by anthropologists like Claude Lévi-Strauss, Dumont, and Balandier—an Americanist, a South Asianist, and an Africanist, respectively. The Institute of Ethnology, the first such institution in France, was created before World War I. Until the end of the 1950s, other anthropological research centers did not exist, and anthropology was taught only at the Sorbonne for postgraduate students. The creation of the Laboratoire d'Anthropologie Sociale by Lévi-Strauss, and a few years later of the Laboratory of Ethnology and Comparative Sociology in Nanterre, and the development of cultural area centers in the École des Hautes Études en Sciences Sociales, such as the Center for African Studies and the Center for Indian Studies, have played an important role in the institutionalization of anthropology. (Abélès 1999: 404)

Stocking (1982: 175) remarked that the historical emergence and consolidation of "international anthropology" was essentially Euro-American. According to him, what was missing from this process was a new way of conceiving of the center-periphery relationships that developed "in the very same period that 'international anthropology' emerged." Given that one of the main objectives of the present book is to critically examine the international dissemination of anthropology within and across national power fields and the processes through which this dissemination takes place, in the rest of this chapter I look

at France as a specific case for discussing the historical complexities of the formation of "centers" and "peripheries" in our discipline. French colonial anthropology, French ethnology (the study of France), and Dumont's influence outside the Indian subcontinent constitute "vignettes" presented for the purpose of getting a more nuanced picture of trends, patterns, and power inside France.

## French Colonial Anthropology and the Travels of Michel Leiris

The institutionalization of French anthropology can be dated back to the opening of the Institute of Ethnology at the University of Paris in 1925. Created by Mauss, Rivet, and Lévy-Bruhl, its main aim was to serve the colonial French power, which at that time had a very marginal interest in the discipline. In the text specifying the aims of the new institution, presented by Lucien Lévy-Bruhl to the University of Paris, we read:

> When in a colony there are populations belonging to an inferior civilization or very different from us, good ethnologists can be as necessary as good engineers, good experts in forestry, or good physicians ... In order to extract all the economic value of the colonies in the most complete and efficient way, everyone recognizes that not only capital is needed. It is necessary to have scientists, technicians, capable of producing a methodical inventory of the natural resources, indicating how to exploit them in the best way. The first and most central of the natural resources is the indigenous population, because the other resources are dependent on it, above all in tropical regions. Does not there exist a capital interest in studying it, in a methodical way, in order to get an exact and deep knowledge of its languages, religions, and social organization, which it is not prudent to destroy irresponsibly? (Lévy-Bruhl 1925: 1)

Lévy-Bruhl's arguments were so convincing that, according to Benoît de L'Estoile (2000: 295), the institute began to receive continuous financial support from the colonial administration. Colonization was seen as a marker for entering into a more scientific epoch. Jules Brévié, the governor of French West Africa from 1931 to 1937, played a key role in creating the French Institute of Black Africa in 1937, and then, in 1942, the Bureau of Colonial Scientific Research. This later became ORSTOM and is now the Institute of Development Research. L'Estoile sees this confluence as proof of an alliance between the colonial administration

and the ethnologists working in French Africa. The main aim, from the point of view of the colonial bureaucracy, was the implementation of a rational, scientifically based colonization (L'Estoile 2000: 295).

Among the demands placed upon the ethnologists were to produce territorial identifications of races and to write general descriptions of their cultural patterns. The production of ethnic maps and inventories (languages, places, numbers) was seen as an urgent task. A number of studies were initiated in order to solve practical problems created by the existence of such a diversity of local languages and sociocultural systems. In the 1930s, the colonial administration issued a series of ethnic regional maps of French Africa with the explicit objective of defining the limits of each ethnic group, its material culture, its political organization, and its religious beliefs. These ethnographic and geographic data were of great interest for the politics of the colonial administration (Chombart de Lauwe and Deboudaud 1939). The roles of colonial administrator and ethnographer were in many cases interchangeable.

The lives and works of Maurice Delafosse (1870–1926), Louis Tauxier (1871–1942) and Henri Labouret (1878–1958) are examples of the way ethnographic work could be integrated into both colonial administration and academic life (see Gaillard 1997; L'Estoile 2001). Their scientific practice was defined as an integral part of French national interests as well as of policies regarding colonial domination in Africa. However, as their active participation in the London-based International Africa Institute and the journal *Africa* proves, they were also internationally oriented. Although these men were important actors in the expansive moment of the discipline in the 1920s and 1930s, the role given to them in general histories of anthropology is marginal. In contrast, contemporary British Africanists have a central place. This is due to many factors.

First, the British Africanists codified a British tradition of fieldwork and defined tools such as the genealogical method, household and sibling surveys, marital histories, spatial analysis, analysis of kin terms, and extended-case analysis. We have no French school of fieldwork that can be defined with such precision. The French were practitioners without generating models and concrete techniques. Moreover, British anthropologists articulated ethnography with theoretical preoccupations and models. They developed a special focus on understanding the way societies worked in practice—hence, anthropology became social anthropology. They invented a naturalist and objective narrative; their monographs were archetypal of a modern discipline, and their work contributed to core problems in anthropology—kinship and marriage,

law and religion. Finally, they created departments and university institutions detached from the colonial administration and became international militants traveling to distant places with their message and passion, arguing comparison and holism. The colonial French anthropologists, in contrast, remained important members of a local tradition, in spite of their international links. They were also key figures in increasing ethnographic knowledge, but their "sin" was their lack of general contributions to the discipline.

There were two exceptions: Marcel Griaule (1898–1956) and Michel Leiris (1901–90). Let me briefly explore their contributions to a de-centered anthropology.

Like Lévi-Strauss, Métraux, Dumont, Bastide, and many other anthropologists who followed Mauss's courses at the Institute of Ethnology before World War II, Griaule and Leiris were trained by Mauss. They were not intellectual products of the French colonial administration, and in different ways they became critical of colonial policies. In 1931 Griaule led the famous Dakar-Djibouti expedition, of which Leiris was a member. They belonged to the great tradition of travelers and explorers, taking part in numerous expeditions. Their academic careers in France were accomplished, and they gained great recognition. In 1943 Griaule was appointed the first professor of "general ethnology" at the Sorbonne, and his ethnographic work among the Dogon of Mali had an important effect in France. He created his own school (or tradition) in African studies with people like Germaine Dieterlen and Solange de Ganay. From 1934 Leiris was head of the Department of Black Africa at the Museum of Man, and in 1943 he joined the Centre Nationale de la Recherche Scientifique, later, in 1967, becoming its director of research. But as we saw in the analysis of the three published histories of anthropology and even in the lengthy quotation from Abélès, neither Griaule nor Leiris ever became a central figure or was considered part of the core of international French anthropology, which remained dominated by figures such as Lévi-Strauss and Dumont. Why?

James Clifford (1988), in his discussion of the complex interrelations between ethnography, fieldwork, literature, travel, and art, gave new life to the works of Griaule and Leiris. I agree with the way Clifford characterized Griaule's contribution to anthropology. Referring to Griaule's ethnographic pieces on the Dogon, Clifford wrote: "One hears, as it were, two full chords of a Dogon symphony: a mythic explanation of the cosmos and a native theory of language and expressivity. More than just native explanations or theories, these superb compendiums present themselves as coherent arts of life, sociomythic landscapes of

physiology and personality, symbolic networks incarnate in an infinity of daily details" (1988: 58).

We can accept that Griaule's dialogic method created doubts regarding the validity of his findings. Clifford defined Griaule's masterpieces (*Le renard pâle* and *Ethnologie et langage*) as

> elaborated inventions authored by a variety of subjects, European and African. These compendiums do not represent the way "the Dogon" think: both their enormous complexity and the absence of female informants cast doubt on any such totalizing claim. Nor is their "deep" knowledge an interpretative key to Dogon reality for anyone beyond the ethnographer and a small number of native "intellectuals." To say that these Dogon truths are specific inventions (rather than parts or distortions of "Dogon culture"), however, is to take them seriously as textual constructions, avoiding both celebration and polemic. (1988: 60)

Many of the criticisms of Griaule's work by British social anthropologists have pointed out his extreme dependence on translators, with whom he had life-long relations, and the fact that he gave his total confidence to a small number of key informants (Douglas 1967; Richard 1967). It has been argued that always missing in his ethnography were daily life, a clear definition of social contexts, and a description of politics and power mechanisms in Dogon society. Clifford saw these critiques as pertinent, especially with regard to the problematic role of the Dogon themselves as actors in Griaule's ethnographic project.

The most authoritative critique sprang from Walter van Beek's restudy of the Dogon (1991). He demonstrated that the texts produced by Griaule were impossible to recognize in the field and that Griaule's ethnography neither offered productive insights into Dogon thinking nor provided useful guidance for the reproduction of Dogon culture. The cultural constructs Griaule presented were unrecognizable to both the anthropologist and the Dogon themselves. When van Beek presented his Dogon informants with elements of Griaule's cosmological descriptions, they did not recognize them as a meaningful part of their thinking and way of life. Claude Meillassoux's response to van Beek's article, published with it in *Current Anthropology,* welcomed the critique as much needed in the tradition initiated by Griaule, which emphasized the power of "selected informants through whom a corpus was constructed that seems, on examination, not so much a source of discovery to the researchers as one of surprise to the investigated" (1991: 163).

In contrast to Griaule's work, Leiris's *L'Afrique fantôme* (1934)—a book never translated into English or any other important anthropological

language—can be seen as a central account of the Dakar-Djibouti and, prior to Lévi-Strauss's *Tristes tropiques* (1955), a masterpiece of ethnography, diary, and intimate reflections. A novelist narrates a plot. A poet writes verses. An ethnographer describes practices. In the 1930s Leiris already mixed autobiography with anthropology and obliged his readers and colleagues to think about the special relationship between writing (*écriture*) and ethnography and, perhaps more generally, about the function of writing in the empirical field of social practices. Leiris had one foot in anthropology and another in literature, and throughout his life he maintained close personal relations with artists, philosophers, and other thinkers. Indeed, it was surrealism that brought him to anthropology. Leiris wrote that "it was surrealism, with which I was involved for four years (1925–29) and which represented for me the rebellion against the so-called rationalism of Western society and therefore an intellectual curiosity about peoples who represented more or less what Lévy-Bruhl called at the time *mentalité primitive*. It's quite simple" (Price and Jamin 1988: 158).

He confessed that he wrote *L'Afrique fantôme* to himself in an experimental mood: "I'd had my fill of literature, especially surrealism; I'd had more than I could take of Western civilization. I wanted to see what would result when I forced myself to record virtually everything that happened around me and everything that went through my head; that was essentially the idea behind *L'Afrique fantôme*" (Price and Jamin 1988: 171).

He never showed the proofs for the book to Griaule, because "he was a completely different kind of person from me, and being opposed to the spirit of the book in spite of our camaraderie, he would have asked me to cut it in ways that I wasn't willing to accept. So I decided not to show him the proofs. He was absolutely furious when the book came out; he felt that I had compromised future field studies, and so forth" (Price and Jamin 1988: 171). Even Mauss reacted against Leiris's travelogue approach and reprimanded him, although not as strongly as Griaule. Leiris acknowledged that his relationship with Griaule "was the only one that was spoiled by *L'Afrique fantôme*" (Price and Jamin 1988: 171).

*L'Afrique fantôme* is a powerful book precisely because it is centered on the explicit recognition of the subjectivity of the ethnographer. This fact places Leiris's work in a kind of premonitory position with respect to the discussions and critiques of the traditional naturalist narratives of classical anthropology that took place in the 1980s. This, in some ways, explains Clifford's recuperation of his historical role in

the discipline (see Cogez 2000). What is surprising in reading Leiris's book today are the intermittent flashes of his intense participation with and commitment to the actors he observed and the frequent bouts of tedium that afflicted him during the expedition. But as Michele Richman (1992: 93) correctly observed, he was also "haunted by the formidable ghosts of his own past, and the fear that he [would] never penetrate anything in depth."

Throughout the book, Leiris's presentations of the limitations of an impotent observer and the necessity of observing the rules of detachment imposed on the ethnographer are accompanied by rare moments of intense, emphatic identification with rituals, events, and actors. Leiris equates these moments with a kind of poetic possession. The attraction of *moments parfaits* and a feeling of being unable to overcome them is present in several passages. Clifford stressed the "smooth ethnographic story" that undermines "the assumption that self and other can be gathered in a stable narrative coherence" (1988: 173). However, I agree with the way Marc Blanchard (1992: 111) summarized Leiris's style of writing: "Of all writers in the French language Leiris is one of the more skilled at describing the world of persons and objects in term of practices—actions performed more than once, often every day, in a social context with the purpose of modifying relations between subjects and objects with significant advantage."

Throughout his career Leiris combined literary writing, autobiographical journals, professional monographs, and essays against the Algerian War and colonialism in general. He was a hybrid in a discipline that was becoming increasingly professionalized. He was difficult to classify, he created no school in anthropology, and his theoretical contributions were less evident than his "literary mood." He always believed in the importance of the subjective element, but he did not deny the relevance of the external world or the objectivity of the exterior. He assumed that the most important tool of ethnography was the description of the other and not just of oneself: "You introduce yourself into the scene in order to allow the *calcul de l'erreur* [calculation of error]," he said in an interview (Price and Jamin 1988: 172). He said that subjectivity "always is present, so it's better to recognize it openly than to deal with it secretly . . . I will make a concession to absolute objectivity and state that that is what it would be most desirable to end up with, but it just isn't possible: the subjectivity is always there" (Price and Jamin 1988: 173). In the same interview he acknowledged having been marginal to the different cores of French anthropology during his academic life (Price and Jamin 1988: 171).

Leiris's travel accounts influenced other great travelers in our discipline. Lévi-Strauss's *Tristes tropiques* can be understood better in relation to *L'Afrique fantôme.* This connection is seldom made in the general teaching of anthropology. Leiris's travels and writings resonate in Lévi-Strauss's work because they belong to the same discursive formation: a combination of heterogeneous approaches—philosophy, dreams, personal lives—mixed with the techniques of an anthropologist, overall producing a redefinition of the relation between language and reference. In Lévi-Strauss, considerations of geography and geology are combined with elements of autobiography, whereas in Leiris's work the main references are the objects and events being classified by the author. Nobody would deny that both texts are "classic," but they belong to a late producer of theory, Lévi-Strauss, and to a convinced hybrid and *bricoleur,* Leiris. Lévi-Strauss represents the dominant scientific tradition, a combination of universal assumptions and models demonstrated through ethnographic comparison, and Leiris represents what we might call the literary mood or a poetic approach to social reality. Lévi-Strauss becomes a key figure in the center of anthropology par excellence, the Laboratoire d'Anthropologie Sociale—the most important after World War II—while Leiris maintains his position as a marginal anthropologist in the French academic landscape.

## *La Tarasque* and the Anthropology of France

In the introduction to the second edition of *La Tarasque,* originally published in 1951, Louis Dumont expressed his satisfaction with the re-edition of a book that he called a "*petit ouvrage*" (1988: 3). He gave two reasons why he decided to republish it without changes: first, he believed a monograph must stand as it was conceived, and second, the book could be seen as a contribution from "researchers and amateurs" (*chercheurs et curieux*) doing the ethnography of France to the young generation of anthropologists then doing it in a more professional way (1988: 4). After World War II Dumont worked as a researcher at the Museum of Popular Art and Tradition (Musée des arts et traditions populaires), founded in 1937. His fieldwork in Tarascon, Provence, was guided by the idea of recording a popular religious tradition that was being threatened by modernization and social change. Moreover, as a disciple of Mauss, Dumont adopted "the use of a model of exotic ethnology, making more sociological what until then was 'folklore'" (1988: v). It was in this approach that he saw the reason for the survival of the book.

Thus Dumont found a kind of continuity between the anthropology of France that he was practicing and contemporary studies of French society in which researchers were concerned less with the disappearance of traditions than with the complexities of modernity. In his words, the ethnology of France was transformed into an "anthropology at home" in which systematic sociological theory replaced the dominant folklorist descriptive approach. That van Gennep's important work on the ethnology of France was seen by Lévi-Strauss in 1947 as traditional folklore (1947: 519; see also Cuisinier and Segalen 1986) did not stop Dumont from showing his manuscript to van Gennep in order to get comments and suggestions.

*La Tarasque* is a complex and unorthodox monograph consisting of ethnographic findings based on observations of the ritual of the feast of the dragon in the village of Tarascon, a detailed oral history of its legends, the results of exhaustive historical archival work, and a detailed iconographic presentation. Dumont argued that this way of working sprang from a Maussian perspective, in which social life always has a direction and constitutes a totality that must be scrutinized in great detail. According to him (1988: 15), the study of the triangle of ritual, legends, and iconography made this possible. The confrontation of ritual and legends in the book and Dumont's observations of contradictions and ambivalent meanings are rich. In this sense, Dumont wrote a "modern" ethnography. His conclusions sound familiar: the ritual, La Tarasque, is a profane emblem of the locality, through which the social forces of the community are expressed, but at the same time it is subordinated to the local saint and *patronne,* Sainte Marthe. In this sense, the Tarasconian community reaffirms, through ritual, subordination to an encompassing Christianity. Here we find an echo of the "other" Dumont, the South Asianist, who later developed a theory in which the relations between parts and wholes in terms of subordination, domination, and encompassment were to be central.

My main aim with this brief examination of *La Tarasque* is to contextualize the question of centers and peripheries with a focus on the anthropology of France and its internationalization. It is clear that the rich tradition of studies of France by ethnologists, ethnographers, and folklorists initiated before World War II remained "local" and was not integrated into the creation of an international discipline in which the "more exotic and extreme non-European others" were privileged (see Abélès 1999; Cuisinier and Segalen 1986; Langlois 1999; Rogers 1999, 2001). Dumont became "international" and part of the core of the history of general anthropology only once he left France as his main research concern and became a South Asianist and a theoretician.

The fate of van Gennep's work, too, demonstrates some of the paradoxes of our discipline. As we have seen, Lévi-Strauss (1947) defined him as a folklorist, unaffiliated with the Durkheimian-Maussian school that was the dominant center of sociology in the first three decades of the twentieth century in France. He did recuperate van Gennep's most ethnographic works and his book on *rites de passage* (1909) but left out his vast ethnological (folkloristic) production on France because it was not considered sociology or ethnology. Van Gennep's main "sin" was his lack of theoretical stringency. He had a great deal of intuition, enthusiasm, generosity, and field experience—even including fieldwork in Algeria—but he was unable to develop systematic models or clear conceptual frameworks (see Belmont 1979; Centlivres and Vaucher 1994).

According to Abélès's account of anthropology in France (1999: 404), the Center for French Ethnology, located at the Museum of Popular Art and Tradition, "pursued ethnological work on France very much along the lines of exotic anthropology," but it was marginal. Abélès remembered that in the 1970s, when he entered the "cathedral" of anthropology in France, the Laboratoire d'Anthropologie Sociale, all of the researchers were working on exotic societies, and the work done by French anthropologists on France "was treated more as a curiosity than as something truly serious" (1999: 405). He admitted that this situation began to change at the end of the 1970s, for three reasons:

> First, anthropological studies in France not only provided empirical data but also opened new theoretical perspectives, for example, on what Lévi-Strauss termed "complex structures" of kinship and marriage. Second, there was a close connection between the anthropology of France and new developments in historical scholarship. Historians and anthropologists shared a common interest in studying areas such as kinship and symbolism in rural France. Third, and finally, after 1968, the French public was increasingly interested in questions of identity, history, and memory. Books dealing with rural France, for instance, found a large audience in France. The creation of the Mission du Patrimoine Ethnologique is linked with this expansion of anthropology at home. (Abélès 1999: 405)

All of this created a new situation in France and new dynamics for doing a more legitimate and modern anthropology at home. This time anthropologists were no longer self-made amateurs or dubious folklorists. They were trained in the common tools of the discipline

and did long-term fieldwork in small villages or urban contexts defined in a strict methodological way.

As we know, the creation of institutions with appropriate budgets is behind many miracles in the development of the social sciences. The Mission du Patrimoine Ethnologique, founded in 1980, filled this role. The Mission was placed outside of the Ministry of Culture, and as such, it was guaranteed more than generous ministerial grants (see Langlois 1999: 409). It defined topics of research that had not been thoroughly studied and called for applications for grants from anthropologists interested in working at home. From 1980 to 2000, the Mission financed 450 research projects—an impressive result. The three main areas selected were French kinship and family, the study of urban and industrial contexts—and in these contexts, studies of previously unstudied groups such as members of ethnic minorities and social elites—and the analysis of associations and movements intended to glorify regional culture (regional allegiances and cultural identities) (Langlois 1999: 410). In 1983 it launched a new journal designed to compete with the older and more traditional *Ethnologie française;* it was named *Terrain* (Fieldwork).

Another important tool in the consolidation of anthropology in France was the series of books that the Mission du Patrimoine Ethnologique published together with the Maison des Sciences de l'Homme in the series "Ethnologie de la France." So far the two institutions have published more than twenty volumes, as well as six monographs outside the series (see Mission du Patrimoine Ethnologique 2000). The majority of these texts are of high quality, and some have been translated into English and published by Cambridge University Press in the series "Anthropology of France" (see Le Wita 1994; Vialles 1994; Zonabend 1993).

Many anthropologists turned to the study of France after long experience with non-European societies. This included Abélès himself, who was originally an Africanist, and Christian Bromberger, who had worked in Iran. The research efforts supported by the Mission were without doubt original and are exemplified by the volume edited by Bromberger (1998). The book, *Passions ordinaires,* deals with the ordinary passions of the French, from football matches to national competitions of dictation. The book includes careful analyses of the importance of genealogies and of publicity, the revival of esoteric knowledge, and people's interest in motorcycles, commitment to rock music, and passion for wine. This is far from the construction of the "other" French person as a "peasant" or an "Occitan." Bromberger questioned the assumptions

of authenticity in traditional ethnographies and depicted a world in which new forms of sociality developed in daily life.

The anthropology of France done by French anthropologists was and is clearly a national project financed by the centralized state with centralized institutions such as the Mission (see Rogers 2001). It has coexisted with the international dimension of the discipline, which, as we have seen, belongs to the core of the standard history of anthropology. French anthropologists dealing with non-European populations have felt threatened by this development. As early as 1986, Alain Testart questioned the Research Council's considerable support of anthropologists doing research in France, on the grounds that it was forgetting the "rich past with great theoretical contributions" made when non-Europeans were studied (Testart 1986: 141).

As a national project, this branch of French anthropology is similar to other important national projects, such as those in Brazil, Mexico, India, and Peru, which, by focusing on their own native populations and social problems, have built up "national traditions" (see Lomnitz 2000; Souza Lima 2000). The international dimension of this anthropology of France depends not so much on its theoretical contributions, which are not yet particularly visible, as on its empirical innovations, which have proved important indeed. We might say that anthropological studies of France make up an "area study" constructed through relationships with foreign anthropologists doing fieldwork in France. Just as Peruvian, Mexican, and Brazilian anthropologists have entered into dialogues with foreign colleagues, so have anthropologists in France. French anthropologists have no monopoly over studies of their country, and the growing interest of American anthropologists and others in France has created a field of studies through which foreign and native anthropologists meet (Reed-Danahay and Rogers 1987). This has in turn created a sort of "competition" over ways of understanding French society and culture.

## The Ramifications of Dumont

I return now to Dumont as a theoretician. Dumont went to South India in 1948, and his research among the Pramalai Kallar formed the basis for his doctoral thesis (*doctorat d'état*). He joined the University of Oxford in 1951 and stayed in the Institute of Social Anthropology there until 1955. Back in France he was appointed director of studies at the École Pratique des Hautes Études and, together with Daniel Thorner, created the Center of Indian Studies. He became one of the dominant figures of theoretical French anthropology.

In 1976 Dumont organized a research team, "Équipe de la recherche d'anthropologie sociale: Morphologie, échanges" (ERASME), at the École Pratique des Hautes Études. The team's main aim was to develop theoretical approaches based on comparative, empirical studies of whole societies. Dumont and his followers thus expanded their interest from South Asia to other parts of Asia, Africa, and South America.

After writing *Homo hierarchicus* (1971), Dumont was mainly concerned with comparing ideologies and the ways society and the individual were conceived of. He contrasted modern ideology, in which the individual was felt to be the ultimate value and in which, consequently, society was not regarded as a coherent whole, with holistic ideologies, in which society was the ultimate value and was not separated from nature. The question of the social construction of value was a key element in his comparative work. Moreover, Dumont was always occupied with eliciting hierarchical relations as a way of understanding why one of the elements in an opposition could stand for the whole and therefore encompass its opposite.

Dumont's legacy as a theoretician can be measured only in terms of his influence over the debates concerning India, the caste system, and modern individualism in general (see Galey 1984; see also Visvanathan, this volume). But in a different way of narrating the history of social anthropology, the ramifications of Dumont's ideas and models in unexpected areas and places can reveal the complexity and ubiquity of ideas in a situation of increasing internationalization of theories and models. Mariza Peirano (1995: 36) has called this kind of intellectual exercise *história-teórica* (theoretical history). Two examples, one from Norway and the other from Brazil—countries with anthropologies of two different "sizes"—can help us think about the decentering process.

In the 1980s a group of Norwegian anthropologists based in the Department of Social Anthropology of the University of Oslo entered into close cooperation with ERASME, then headed by Daniel de Coppet after Dumont's retirement. Some of the results were published in a special issue of *Ethnos* in 1990. I comment briefly on some of the Norwegians' readings of Dumont, with an emphasis on their criticism that the role of gender in inequality was missing in his work. Marit Melhuus (1990), for example, critically examined Dumont's theory of modernity and brought up the gender dimension, in terms not only of modern discourse but also of modern practice. She commented on Dumont's overemphasis on the individual in modern society, suggesting that the autonomous agent was instead the couple. She wrote:

In order to understand the notion of the free individual, it is not enough to focus on the individual as a monad. Rather, we should focus our attention on how the notion of the individual is constructed within the framework of the gender relation, i.e. as a dyad. We are suggesting then that it may not be the notion of the individual as an autonomous being which is at the centre of modernity—this would be a false or artificial assumption, to use Dumont's words—but rather the pair, the couple. To phrase the argument yet another way while stretching it to its logical conclusion: the autonomous agent in modern society is the couple. Indicative of this positioning is the idea of romantic love and the very notion of the free autonomous choice of a sexual partner of opposite gender. (Melhuus 1990: 156–57)

Ingrid Rudie (1990) examined Dumont's hypothesis of value hierarchy through the interconnections between gender, kinship, and seniority in rural Malay society. There, the idea of seniority was a prominent value. Rudie described a social process in which a structure of gender-neutral seniority and balanced gender collectivities gave way to a stronger collusion between seniority and maleness, a process that was personified in the husband-wife dyad. Rudie was open to the possibility of a "hierarchising impulse" in cultural processes, but she doubted that the ultimate values would ever fully emerge in social realities, because "society" has no unambiguous delimitation and because organizational and communicative domains shrink and expand over time, so that any dominant value can be challenged through organizational innovations (1990: 197–98).

In a third example, Jon Schackt (1990) tried out the theory of hierarchy and value on the society and culture of the Yukuna Indians of the Colombian Amazonas. Schackt showed that this society could easily be analyzed to fit the Dumontian model of a hierarchical order, but he criticized the notion that such an order could be anchored to an "ultimate value." Rather than stressing the "premodern" aspects of hierarchies, he argued that the cognitive functions implied were expressive of the way the construction of all ideology may rely on the less-than-strictly-logical aspects of human thinking. He suggested that the dichotomy between the "modern" and the "premodern" might itself be an ideological construction.

These three authors, as well as other Norwegian contributors to the volume—including Signe Howell (1990) and Solrun Williksen-Bakker (1990)—looked critically at the Dumontian perspective. The majority of the chapters demonstrate a preoccupation with issues of gender and

gender relations, which provides a novel critique of the questions of hierarchy and value, as well as of premodernity and modernity.

Dumont's influence in Brazil has also been decisive in shaping empirical insights and theoretical debates. One feels that in the consolidation of the teaching of modern anthropology in Brazil, Dumont has been recognized as a central theoretical ancestor (see ABA 1995; Peirano 1991b, 1995). Let me mention the work of three Brazilian anthropologists.

Roberto DaMatta initiated his career as an Amazonist, and his ethnography and interpretations are impregnated with a Lévi-Straussian flavor (see DaMatta 1973). When he turned to the analysis of complex, modern Brazil, DaMatta was clearly inspired by Dumont (see DaMatta 1979, 1984, 1985, 1996). Brazil, according to him, was a society visibly characterized by a sharp division between the "home" and the "street," as well as between the family—a system of hierarchical social relations and persons—and the market and free individuals. For DaMatta these divisions were less about geographical or physical places than they were symbols of moral and ideological universes. Carnival and football were privileged because in them the personalized social world of the home and the impersonal universe of the street were combined in public rituals. Football and Carnival made possible the expression of individual qualities, and thus they were sources of public individualization, much more than they were instruments of collectivization at the personal level. The deep meaning of the rituals was that in them, individuals could experience equality and freedom in hierarchical contexts.

In his analysis of the categories of the person and the individual in Brazil, DaMatta emphasized the concerns of individuals for maintaining order and hierarchy in a world that was imagined as equal: "The world must move in terms of an absolute harmony, the evident consequence of a system dominated by a totality which strong and weak actors agree upon" (1979: 147). DaMatta's Dumontian perspective has been both fertile and controversial in Brazilian anthropology. It has helped create an important field of empirical analysis of the rituals of modernity—sports, dance, games, and plays—and it still provokes theoretical debates (see Gomes, Barbosa, and Drummond 2000; Soares Pechincha 2002).

Gilberto Velho's book (1981) on the complexities of understanding individualism among the Brazilian middle classes was also clearly influenced by Dumont. Velho fully described the tension between family membership and loyalty, on one hand, and individual life projects, on the other. He argued that family and class were hierarchical mechanisms that could be checked by individualism. A proper comprehension of Brazilian society ought to be grasped through a careful ethnography

of life histories, which would demonstrate the existence of cultural and social tensions in the shaping of Brazilian modernity (Velho 1981: 75).

The important work of Luiz Fernando Dias Duarte on the urban working class in Brazil was also inspired by Dumont's theory of modernity. Dias Duarte admitted Dumont's influence in his work and placed his ethnography in relation to the theory of hierarchy and the comparative analysis of holism and individualism (Dias Duarte 1986: 40). He constructed a model in which hierarchy and holism had to be combined with an explicit treatment of individualism and equality—as in DaMatta's work. He saw Brazil as a hybrid social world and never as a pure model dominated by either holism or individualism. Dias Duarte showed that models that emphasized the preeminence of the group over the individual, often used in examining working-class culture, were unable to reveal the entire picture. He maintained that the ideology of individualism was a constitutive element of the working-class cultural world, and in this sense it was both external, defined by social scientists, and internal, experienced by the actors themselves (1986: 141).

Dumont has been very much alive in the tropics, to a great extent through the creativity of Brazilian anthropologists, who have used his theories to understand the complexities of modernity. The development of the anthropology of France, however, and of modernity in general in the Euro-American tradition has not been influenced by Dumontian thoughts and models. We might say that his contributions have been decentered in the center and centered in the periphery—a metaphor for a better way of making comparisons. Both the work of the Norwegian anthropologists whom I mentioned, with their focus on gender equality and hierarchy, and the rich models of Brazilian anthropologists are indications of the intricate ways in which anthropology is made an international discipline.

## By Way of Concluding: Ambiguities and Contradictions

I hope the vignettes I have presented will facilitate a better reading of the developments of anthropology in France. French anthropology has been a central arena in the constitution of international anthropology and a source of inspiration for the discipline as a whole. Generally it is said that "theory" and explicit philosophical thinking have been typical French products, and that exemplary ethnographies were a British trademark. These are, at least, the commonsense or accepted

historical narratives of the way anthropology constituted itself over the last one hundred years. It is time now to carry out a systematic critical analysis of this "tradition," examining some of the ambiguities and contradictions that manifest themselves in institutions, persons, and financial support. I hope I have been able to show, as James Clifford did before, the way travel literature, fiction, and subjectivity, in a kind of *avant la lettre* postmodernist mood, influenced the anthropology of the 1930s and converted some of its practitioners into marginal and controversial figures.

The recent consolidation of the anthropology of France in terms of the high quality of the ethnography it produces, its demographic presence, and the generous public financial help it receives has troubled the conventional practice of the discipline. France has embarked upon, as never before, a national task of studying its own society and culture and, in this process, has led modern social anthropology to incorporate ethnology, folklore, and history. The imperial and the national embrace each other. In the anthropology of modern France, the explicit influence of Dumont is less important than it is in "peripheries" such as Brazil and Norway. The description of travel and of the unexpected ramifications of ideas in distant places and different times is, I believe, a fruitful tool for a better understanding of the way world anthropologies are constituted.

# The Production of Knowledge and the Production of Hegemony

## Anthropological Theory and Political Struggles in Spain

*Susana Narotzky*

> Communication is the process of making unique experience into common experience, and it is, above all, the claim to live. For what we basically say, in any kind of communication is: "I am living in this way because this is my experience." . . . Since our way of seeing things is literally our way of living, the process of communication is in fact the process of community: the sharing of common meanings, and thence common activities and purposes; the offering, reception and comparison of new meanings leading to the tensions and achievements of growth and change.
>
> —Raymond Williams, *The Long Revolution*

The project of creating a world anthropologies network challenges anthropologists to engage not only in worldwide communication but also with knowledge produced in non-academic contexts and in nonscientific realms of experience. The desire to create a new form of communication stems from the will to be alive, to form a community that will allow us to grow and change in unexpected directions. In pursuing this ecumenical objective, however, anthropologists must deal with the awareness that all knowledge is produced in, and seeks to create, particular fields of power, and we are not exempt from this ourselves. The tension in the world anthropologies project is one between "epistemological tolerance," with its paradoxical liberal,

modernist taint, and an epistemological program that has a definite grounding in emancipatory political projects.[1] It is this tension I want to address.

My goal in this chapter is to explore three probably incompatible discourses that represent, nonetheless, serious attempts to go beyond the easy disqualification of particular nonhegemonic forms of knowledge as "epistemological nativism." The first is the discourse of singularity and autonomous consciousness, which raises the issue of the unavoidable opacities of translation we should contend with if we choose to recognize the "heterotemporality" of plural histories and forms of knowledge (Chakrabarty 2000: 72–96). The second is the discourse of participation in a local political project as part of the production of knowledge, where commitment to a collective struggle against (or for) some form of domination and injustice requires both concrete experience and a certain urgency in identifying historical forces that are seen to be substantial and material. The third is the discourse of ethnographic realism (Terradas 1993), representing an attempt to revive anthropology as a comparative endeavor by developing the explicitness embedded in the realist methodology of early ethnographies. By engaging with these three methodological perspectives, I hope to raise some issues about how a world anthropologies network could provide a real communicative space for fostering growth in anthropological knowledge.

## Andalusia and the Rest

Let me start with a piece of local history—the history of the awareness of a particular form of anthropological knowledge in Spain. In 1973, at the First Conference of Spanish Anthropologists (Primera reunión de antropólogos españoles), held in Seville, Isidoro Moreno (University of Seville) addressed, in a paper about anthropological research in Spain (published in 1975), the production of anthropological knowledge in that country, particularly in Andalusia. He described it as a double colonization. The first colonization was spatial: foreign anthropologists, mainly North Americans, conceived of Spain exclusively as a territory full of informants, as an object of study, and they offered nothing valuable "to the knowledge of Spain, the progress of Spanish anthropology or the development of anthropological theory." The second was theoretical: this was colonization through local anthropologists' mechanical application of concepts and theories developed by Anglophone scholars to deal with other realities (Moreno 1975: 325–26).

In a paper written ten years later, Moreno (1984) developed this early insight more thoroughly and tried to show how two very different ethnographies of Andalusia, one by a British structural-functionalist, J. A. Pitt-Rivers (1971), and the other by an American radical anthropologist, D. Gilmore (1980), both suffered from blatant forms of ignorance stemming from their authors' superficial involvement with the local history, economic realities, political conflicts, and symbolic expressions of Andalusia. He wrote: "In both community studies, once again, Andalusia *provides only the field,* and the excuse, for useless academic polemics that take place in other countries and for obtaining degrees and status for professionals in anthropology who have little real interest in the present and future of the Andalusian people. And this has only one name, that of *anthropological colonialism*" (Moreno 1984: 73, original emphasis).

Moreno's phrases "useless academic polemics," "obtaining degrees and status for professionals in anthropology," and "little real interest in the present and future of the Andalusian people" echo the language of some contemporaneous critiques of the production of anthropological knowledge (Asad 1973; Fabian 1983) as well as path-breaking papers such as F. H. Cardoso's critique of the apolitical "consumption" of dependency theory by US scholars (1977) and works in the earlier phase of subaltern studies (see Pouchepadass 2000). But they were produced without knowledge of those critiques, that is, without the sense of participating in a wider polemic about anthropological knowledge. Rather, Moreno used them in expression of his personal experience as both an anthropologist and an Andalusian nationalist of a Marxian background, strongly engaged in political participation. His critique stemmed from the felt inadequacies of the separation of theory from practice; the reproduction of a structure that validated what counts as anthropological knowledge—that is, the patterns for acquiring professional status at the center; and the lack of personal or political engagement on the part of the outside researcher—that is, the objectification of the anthropological subject. I will come back to this later.

The story, however, has more developments to it. In 1997, in his contribution to the book *Provocations of European Ethnology,* Michael Herzfeld pointed to the

> various responses of Europeans to the sometimes startling discovery that they are already under the dissecting gaze of anthropologists. This is both an intellectual refinement of a covert racism (of the "we are not savages"

variety), at one level, and at another, paradoxically, a late version of the colonialist critique of anthropology. These are not necessarily mutually incompatible stances. Taken together, however, they indicate how powerful and pervasive is the model of occidental superiority and the idea that rational scholars are somehow free of cultural constraints or the messy vagueness of symbolism... Moreover, they reflect the perpetuation of colonialist assumptions even, or especially, within the optimistically named "new Europe." This appears with notable force in the epistemological nativism of certain Spanish anthropologists (e.g., Llobera 1986; Moreno 1984), although rarely those in the national capital [no names or references given], a contrast that shows how easily subnational hierarchies may reproduce international inequalities. (Herzfeld 1997: 714)

What I find revealing in this passage is the way Herzfeld unforgivingly disavows southern European anthropologists as colleagues who might share polemic anthropological ground. I find it revealing, too, that he cites a Catalan, Josep Llobera, and an Andalusian, Isidoro Moreno, both overtly peripheral nationalists, located far from "the national capital" and not particularly Spanish in their self-presentation. Herzfeld points to these scholars' reluctance to be taken as objects of study "under the dissecting gaze of anthropologists," a position he interprets as a paradoxical mix of "the model of occidental superiority" and "a late version of the colonialist critique of anthropology." This creates a breach between reflexive anthropological scholars at the centers, who know that "rational scholars" are not "free from cultural constraints," and other anthropologists, who are immersed in "epistemological nativism." As a consequence, he invalidates their scholarly but obsolete methodological critique of knowledge production in anthropology. Indeed, he negates the possibility of a conversation with these scholars on common professional ground at all.

The problem Herzfeld seems to have with the epistemological nativism of peripheral Spanish anthropologists stems from his view of them as akin to nationalistic folklorists and therefore subsumable under his critique of the methodological "distancing" and conceptual "fixedness" of nineteenth-century folklorists (Herzfeld 1987). As a corollary, this critique is based on his epistemological rejection of the explicit political intent of folklorists' intellectual project. European "anthropologists" who are peripheral nationalists are thus placed in the field as objects of study and are precluded from entering the "a-nativist" (scientific?) epistemological debate in anthropology.

Why is Herzfeld unwilling to engage in a serious epistemological discussion with these anthropologists? Why the arrogant and dismissive tone of his critique? Why is a form of "colonialist critique" that has been voiced by scholars inside and outside the United States and the United Kingdom since at least the late 1960s (Asad 1973; Berreman 1968; Fabian 1983; Gough 1968) considered unacceptable in a southern European location? Is it because it comes from "European" scholars? Because it comes from "Spanish" scholars? Because it comes from (peripheral) nationalist scholars in Europe? Or is it because he feels that there is a competition for field and knowledge production from these local scholars, something that might undermine Anglophone authority in that "area" of study?[2] Is it an appeal to openness or a practice of closure? I will pursue these themes later in the chapter.

## Passionate Epistemologies and the "Dissimulation of Dissimulation in the North"

I want to present another strand of the story of the polemical 1984 article by Moreno. In his critique of Pitt-Rivers's classic of Mediterraneanist anthropology, *The People of the Sierra* (1954), Moreno wrote, referring to the English-language version of the second edition (1971), which was published for the first time in Spanish in the same year:

> When Pitt-Rivers in the preface to his book—which, by the way, was not published in the Spanish version—declares that his objective has been to explain, through an ethnographic example, Georg Simmel's essay about secrecy and the lie, he congratulates himself for not possibly having a better example than Grazalema to prove it, given that—and this is a literal quotation—"Andalusians are the most accomplished liars I have ever encountered ... one never knows what Andalusians think."
>
> What evidence did Pitt-Rivers have to assert this? His two-year experience in Grazalema. How would we characterize the assertion? ... it shows a total ignorance of the meaning of popular Andalusian culture as a *culture of oppression,* in which a series of traits—such as mistrust disguised as sympathy with strangers, English anthropologists included—is a mechanism of defense, the fruit of centuries-old collective experience in the face of that which is external and unknown, which is always something potentially aggressive and a source of possible misfortune... In any case, the aristocracy of British anthropology should have looked in depth into this quality of being great liars that he attributes to us, in order to explain it, instead of presenting it, as he does, as if it were a cultural explanation. (Moreno 1984: 73)

This critique is interesting in the context of a relatively recent publication in US anthropology that has a methodological objective and that uses (among other material) precisely this preface to the second edition of *People of the Sierra*. I am speaking of Michael Taussig's *Defacement: Public Secrecy and the Labor of the Negative* (1999). In this philosophical work, Taussig presents a critique of the use of historical origins and social functions as methods for approaching an understanding of social reality. In his characterization of reality, passion and empathy seem to be better ways into cultural understanding than rational analysis.

But the position of the anthropologist or historian in communicating this reality, or even the need for doing so, remains obscure in his account. Taussig uses Pitt-Rivers's preface (as well as the entire ethnography) to prove a methodological pitfall predicated on the dialectics of revelation and public secrecy. The variable geometries and tensions between reality, performance, and authorial narrative involve anthropological subjects' dealings among themselves (as observed and interpreted by the British anthropologist); the interaction between the anthropologist and his subjects of study; and the interaction between the anthropologist and the reading public (both the scholar, as in Pitt-Rivers's confrontation with Eric Hobsbawm via footnotes, and the English-speaking nonscholar). Thus Taussig points to the unavoidable selective processes of categorization and analysis in the Enlightenment tradition of the social sciences, which obscure and silence certain real-life practices as they enshrine others with central explanatory powers.[3]

Moreover, he stresses the deceptiveness of the methodological pretense that description and explanation are possible at all, because they are based in the "concealment of ideology" (1999: 74), the "charade of scientific detachment" (1999: 75), and the repression of passion (1999: 76). He grounds this deceptiveness concretely in a North-South power relationship: "For what is surely referenced here in this epiphanous encounter between north and south, between the cultivated man of letters from the north and the sun-drenched tillers of the southern soil of untruth, is an uneasy acknowledgment as to a certain secret of the secret in which the south has long had the function of mirroring, in its dishonesty, the dissimulation of dissimulation in the north" (1999: 76–77).

Two things seem to me worth highlighting in regard to Taussig's book. The first is his comfortable unawareness of critiques by local scholars (anthropologists and historians) of Pitt-Rivers's ethnography (Frigolé 1980, 1989; Martínez-Alier 1968; Moreno 1984, 1993; Serrán Pagán 1980). They would have given him some insights into important aspects

of knowledge production, into what the "cultivated men of letters from the south" thought of the encounter, and into the real politics of the production of truth in Mediterranean anthropology through various forms of concealment. How might Taussig have responded, for example, to local scholars' multiple and diverse critiques of Pitt-Rivers's work? How might he have dealt with their methodological perspectives, some more passionate than others, but all of them with a "scientific" pretense and therefore within the social-science Enlightenment tradition? What happens when social scientists, while aiming for description and explanation, do not pretend to be passionless, ideologically neutral, or detached? How does their work speak to the work of those who base their knowledge and its authority on the pretense of detachment? How is it part of a political engagement, locally and nationally?

Reading the work of local scholars would also have given Taussig an additional item of information about public secrecy: the fact that the preface to the second edition of Pitt-Rivers's book was not published in the first Spanish edition (1971), even though it was contemporaneous with the English second edition. Why? Was Pitt-Rivers's conscience uneasy over his calling Andalusians as a whole—as a "culture"—liars? Was it self-protection against possible criticism by local "native" anthropologists such as Moreno? Whatever the case, it provided the turn of the screw for the "dissimulation of dissimulation in the north."

I am more interested, however, in a second aspect of the comparison between Moreno's and Taussig's scholarly exploitations of the "secrecy-and-lie" perspective in Pitt-Rivers's ethnography. Taussig criticized modernist realist pretenses of revealing truth through rational analysis (of functions or origins: e.g., Dunk 2000) and proposed impassioned characterization instead, but from a distance. Moreno, while also criticizing detachment—"professionals of anthropology who have little real interest in the present and future of the Andalusian people"—proposed going into the historical depths of the production of a "culture of oppression" and a meaningful national Andalusian identity that did not eschew class, gender, and race as fields of force (Moreno 1991, 1992, 1993). In Moreno's critique we see that passionate practice is not simply an abstracted idea of concrete participation that becomes an end in itself, for the anthropologist's enjoyment. Rather, it is actively and outspokenly a political project, a desire for change, an emotional engagement aimed at transforming Andalusian lived reality in a particular direction by producing useful knowledge toward that end. As part of a political project, then, it is necessarily part of an abstraction, a process of "fixing" concepts that design and enable particular forms of collective action.

The foregoing can be seen in a contribution by Moreno (2001) to a volume titled *La identidad del pueblo andaluz* (The identity of the Andalusian people), in which he explicitly states his intellectual and political program. It is that of participation in the production of an Andalusian identity in historical, cultural, and political terms in order to empower Andalusian people in the context of increasingly globalized market forces and multilayered structures of governance:

> In no place in the world does *national sovereignty* exist any longer, as it has been understood up to now: our age is now one of "shared sovereignties," in which a web of knots of different sizes and importance is being woven. These knots will define the structure of future relationships between peoples. If Andalusia does not become one of these knots, it will be excluded. If, on the contrary, it manages to occupy one of these positions, this will mean emerging from the present-day periphery and subalternity. And this is not only a problem of juridical definitions, but of an everyday cultural and political leading position. There is no other form, presently, of guaranteeing the survival of a people, in our case the Andalusian people, than through asserting and developing the triple dimension of identity: historical, cultural, and political. (Moreno 2001: 160)

And he adds:

> But the *identidad-resistencia* [identity-resistance] that can be generated today by Andalusian culture should be understood not as an end in itself but as a means, a necessary preliminary stage, toward the construction of an "*identidad-proyecto*" [identity-project] aimed at making possible a less unequal and unjust society than the present one, through a deep transformation of the internal social structure and the termination of external dependency and subalternity. (2001: 170)

To a "scientifically detached" intellectual, this program can be read as an attempt to provide a clear conceptual category, "Andalusian identity," in order to create something similar to what Gramsci called a historical bloc capable of producing an alternative hegemony for revolutionary purposes. Alternatively, it could be read as akin to the nationalistic folklorists' search for "origins" (Herzfeld 1987).[4] For the local scholar involved in the production of this quasi-homogenizing concept of Andalusian cultural values—to be reshaped into tools of

struggle against totalizing market values (Moreno 2001: 162–64)—it is much more than that. Are we prepared to deal with this sort of politically engaged local knowledge production without displacing it from epistemological coevalness? And how would we do so if we eschewed all the unitary frameworks produced by modernity?

## The Production of Knowledge and Forms of Political Engagement

As the foregoing stories highlight, the issue of communicability within fields of knowledge is tied both to the institutionalization of particular regimes of truth and to the involvement of that institutionalization with real-life issues of dominance and exploitation—that is, with the reproduction of particular structures of inequality or, alternatively, the replacement of those structures by others. I am well aware that my own discourse is entrenched in modernist assumptions about history as a continuous and connected (and therefore unique) process tying past and present realities to possible futures. It is not one, however, that incorporates as its foundation a particular teleology of transitions into a particular social, political, or economic future. I remain critical of Foucault's notion of "genealogy," as opposed to "history," for I try to place the concrete analysis of local or regional historical processes within the wider movement of a global, connected history. And I try to follow the threads that create feelings of community and coherence— that create multiple histories enabling political agency—from the raw materials of heterogeneous and contradictory situated experiences.

   If the method of "genealogy," oriented against the power effects of "scientific" discourse, has been a major epistemological breakthrough for the social sciences, it seems to me nevertheless that it has also produced a paradoxically paralyzing effect that Foucault did not intend. The ultimate goal of the genealogy process was, for Foucault, to free historical local knowledges from their subjugation so that they might counter the coercion of a fixed, unitary, scientific theoretical discourse, with the explicit aim of empowering them for struggle. The idea was that of archaeology as method, genealogy as tactic (Foucault 1979 [1976a]: 131). Unlike many of his followers in the social sciences, Foucault *had* a political project explicitly concerned with specific, local struggles. He was deeply engaged in the transformation of social reality as he experienced it. He was interested not just in unveiling or exposing multiple processes of discourse or knowledge; he also wanted to "exercise power through the production of truth" (Foucault 1979 [1976b]: 140).

However—and this is the unresolvable tension in Foucauldian epistemology—the "truth" from which we may "exercise power" in our struggles against diverse forms of domination must eventually be inscribed in some kind of fixed hierarchy, such as a particular concept of justice. It will also have to be inscribed in a geometry of objectives for change and thus acquire a set course, an "orientation," a determined "sense" of and for action. Yet the literal definition of "genealogy" presents an absolute arbitrariness of being, a permanent fluidity of everything: time, space, people, concepts, relations, knowledge (Foucault 1979 [1971]: 13). It is difficult, from this epistemological position, to engage with reality in an attempt to transform it, because there is a break, instead of a dialectical tension, "between 'real history' on the one hand and the historical commentaries and texts of social actors and intellectuals on the other" (Roseberry and O'Brien 1991: 12).

We are left, then, with the question of how to render politically productive the tension between the production of multiple, situated knowledges and concrete political engagements. As part of an interesting debate over the historiography of racisms, Ann Laura Stoler raised the crucial problem of "the politics of epistemologies." Her analysis of anti-racist histories of racisms led her to underscore that "the pursuit of origins that constitutes 'traditional history' is a moral pursuit that is fundamentally ahistoric" (Stoler 1997b: 248). Moreover, "a search for racisms' origins both shapes and is shaped by how we think about race in the present and what we imagine is effective anti-racist scholarship today" (Stoler 1997b: 249). Her point is that the focus on "fixity, permanency, somatics and biology" (Stoler 1997b: 249) as the "original," visible, physical form of racism hides the fundamental ambiguity always present in racisms between "ocular epistemologies" of somatic taxonomies and the fluid plasticity of the intangible qualities that are social and cultural elements of racial political practices.

Stoler concluded that "if racisms have never been based on somatics alone nor on a notion of fixed essence, then progressive scholarship committed to showing the protean features of racial taxonomies does little to subvert the logic of racisms, since that logic itself takes the plasticity and substitutability of racial essences as a defining feature of it" (Stoler 1997b: 252). In short, she concludes that epistemological error deflects anti-racist struggle from the real issues and that contemporary anti-racist political agendas inform histories of racisms' origins (see also Stoler 1997a: 201). If we think of "political rationalities" as an important part of political economy, as Stoler suggests (1997b: 250), we can better situate our knowledge making as part of our own political

agenda (anti- or pro-). As we attempt to analyze or communicate with other forms of knowledge, we need also to be able to gauge the weight of power struggles in the theoretical structures (and teleological histories) they produce.

Consciousness, then—and coherent consciousness as "knowledge"—is a material expression of experience, giving meaning to social relations in real life, and also a material force, exerting pressures leading to change (Thompson 1978: 97, 171, 175–76; Williams 1977: 75–82). It is in light of these discursive practices or political rationalities that I want to approach the issue of knowledge production and political engagement. Johannes Fabian (1983: 152–65) developed the concepts of "allochronic distancing" and "coevalness" in his effort to historicize anthropological practice and find a way out of dominating forms of knowledge production. His insight stresses the unavoidable coevalness of communication not only in the field encounter but also in the encounter with other forms of produced knowledge through polemic. Polemical engagement is the recognition of conceptual co-presence, summoning us to engage with knowledge forms as present realities (and political ones) and not as something enclosed in a past that is no more or is in a realm of other-than-knowledge cultural production (see also Amselle 2000: 211).

I believe we need to know more about the global and local histories that shape a particular order of domination, its material processes and discursive frameworks, and its micro- and macropolitical fields of power. Categories that shape local knowledges should be treated as part and parcel of a historically formed discursive framework during conflictive nationalist, colonial, postcolonial (and so forth) historical times and spaces. We should engage with the fact that these categories take form as part of tensions between different social and political agents at different times, agents who become involved in multiple and heterogeneous relationships as they try to secure differential access to resources and power while forwarding and resisting claims to land, work, and symbols through the production of different discourses that all have a pretense of coherence (Roseberry 1989, 1994).

## Exploring Epistemological Barriers to Real Engagement

At this point I would like to further develop three issues. The first is that of comparison versus incommensurability and the possibility of thinking of any knowledge as beyond communication, as well as the

tension between distancing and participation. The second is the issue of "project" and the break from the epistemologies of "modernity"— that is, the issue of whether postmodern fragmentary epistemologies, in which categories and knowledge are "multisited," endlessly self-reflexive, and permanently unstable, have the capacity to foster change. The third is the issue of markets for knowledge products in relation to the reproduction of power structures, setting the field of forces for "authority" in knowledge, within the academy as well as without it, in local, national, and international arenas. I want to show that political engagement and "project" development are not exclusive prerogatives of the left and that "peripheral" intellectuals are not a homogeneous body of "counterhegemonic" knowledge producers, either.

In order to develop these points in a comparative dimension, I briefly explore the case of postcolonial South Asian scholars, addressing the obstacles for real engagement produced by an abandonment of "realism" and of modernist, unitary epistemological frameworks. These scholars' critique of knowledge production is based on the concepts of power and discursive regimes. The relationship between the two is posed in such a way that their articulation constructs both the object of study and the paradigm (in a Kuhnian sense) or authorial narrative (in a postmodern sense) under which social relationships are explored. There is no fixity (no essentialism?) to the object of study but, instead, constant displacement as the power relations enclosed in the teleological and unitary histories of modernity (colonialist, nationalist, Marxist) seek to produce a particular knowledge in order to perpetuate (or subvert) the existing order (Guha 1983a).

Following a trend that originated in late-1960s Euro-American historiography among the French Annales group, with its "*histoire de la vie privée,*" and among feminist historians—but almost simultaneously among Italian historians doing microhistory, British social historians such as Raphael Samuel and the History Workshop group, and German social historians such as Lüdtke with the Alltageschiste group—the South Asian subaltern studies group aimed to give voice to the "subaltern," a large and heterogeneous category of people. The originality of the subaltern studies group developed as it moved closer to postmodernist or Foucauldian assumptions and away from Marxist social history—that is, when it abandoned realism, when discourse became its only referent of reality, and when its knowledge production became self-referential. As G. Prakash (1990: 406) defined the new postorientalist scholarship: "First, it posits that we can proliferate histories, cultures, and identities arrested by previous essentializations. Second, to the extent that those

made visible by proliferation are also provisional, it refuses the erection of new foundations in history, culture, and knowledge." Moreover, this project was situated as a political one—"an issue of engaging the relations of domination" in which "the power attributed to the knowledge about the past makes historical writing into a political practice and turns the recent post-Orientalists' historical accounts into contestatory acts" (Prakash 1990: 407).

Following other critiques of postcolonial perspectives (Dirlik 2000; O'Hanlon and Washbrook 1992; Pouchepadass 2000; Sarkar 1997, 1999; Subrahmanyam 2000), I want to underline the difficulty this epistemological vision poses to real political engagement. Critics have pointed to the fact that postmodernist perspectives are themselves a "grand narrative" and are inserted in present-day economic and political fields of force (Dirlik 2000: 77; Subrahmanyam 2000: 95). They have pointed out that those producing this postcolonial knowledge are themselves fully a part of the centers of knowledge production, mainly in US universities (Bénéï 2000; Friedmann 2000), and therefore are occupied in academic power struggles within those centers, rather than in subversive action in "subaltern" locations. Some have pointed to the danger that the postcolonialist emphasis on local culture might be used in justifying right-wing nationalist politics locally in India (Bénéï 2000; Pouchepadass 2000: 179).

It is interesting to compare the work of "subalternist" historians in the United States with that of the rest (in Europe and India). S. Kaviraj (SOAS, London), for example, points to the dependency of postcolonial theory on Western knowledge (2000: 75) and seems to propose a detachment from it linguistically and theoretically (2000: 79, 84–85). What he seems to suggest is a vindication of unawareness of Western theory and Western debates (see also Ramanujan and Narayana Rao in Subrahmanyam 2000: 92). On the other hand, a historian such as Sumit Sarkar (University of New Delhi), originally a part of the subaltern studies group and actively involved in the public critique of the fascistization of the Hindu nationalist movement (through his contributions in daily newspapers, his teaching, and his writings in Bengali and English), was unprepared to forgo a Marxian idea of differentiation *within* the framework of a unique history and, significantly, of a realist history. In Sarkar's view, different struggles or localized "histories" are parts of a unique although differentiated process, and there is a distinction between description of a past reality as gathered from documentary information and attempts by a government (or intellectual elite) to construct a particular narrative as well as a particular discourse.

What is striking in Sarkar's presentation of a particular struggle over historical knowledge production is its grounding in reality, that is, in concrete, present-day political struggles in India.[5] Indeed, it is a reality that Sarkar has had serious censorship problems with his account of the history of anti-colonial movements in India, because he stated, through documentary evidence, that right-wing Hindu nationalist movements such as those in power until April 2004 were conspicuously absent from the fight for freedom. What is also striking is his outright disqualification of "old-fashioned, discredited" discourses about history, as opposed to the ecumenical discourse of proliferation of discursive realities and shifting perspectives of postcolonial theory. Sarkar's position is clearly grounded in a "foundational" and "modernist" paradigm that is at work both in the presentation of the past and in the struggle to spread a particular knowledge of the past in the present and *for* present-day struggles—namely, the struggle against the rise of a totalitarian state (Sarkar 1993, 2000).

To my mind, Kaviraj's and Sarkar's intellectual positions represent two different politically engaged possibilities in the struggle against hegemonic control of knowledge production, each with effective subversive power. The former says that we do not have to convince those in power that the particular knowledge we produce is valuable; we simply have to give it value on our own terms and ignore the center's (the West's) unawareness of it. As a corollary, we will be empowered to value nonscientific, nonrational, nonmodern (nonsecular, literary, ritual, etc.) forms of knowledge if we so decide. The results of this project are the absolute incommensurability of forms of knowledge and the "autonomy" of the subaltern consciousness (Pouchepadass 2000: 177–82). Although this sounds radical, I believe it is an expression of the liberal, willful notion that one is free to make history as one wishes.

But what does incommensurability entail? It entails the impossibility of comparison and generalization, thereby impairing the construction of grand narratives, including new emancipatory narratives to replace discredited modernist ones (see Dirlik 2000; Pouchepadass 2000: 181). It entails the impossibility of abstraction across localized, diverse, and often contradictory experiences of knowledge production. It entails the impossibility of a common "project" (is this exclusively a modernist concept as well?) of any sort, because of the endless dynamics of fragmentation (in time and space). How do we deal with propositions that eschew comparison as a foundational principle? With propositions that preclude a unified language of some sort that would make communication, and hence collective action, possible? What are the concrete

political realities of such projects? Theoretically, anthropologists have dealt with the issue of comparison and incommensurability for a long time and have produced concepts such as "emic" and "etic" in an attempt to resolve it. As we all know, these concepts are tricky (because they entail the objectification of anthropological subjects) but useful (because they attempt to bridge the incommensurability of radically different forms of knowledge production and to enable communication).

The case of Sarkar, on the other hand, illustrates the struggle for control of the locations of knowledge production in a concrete situation. Although he underlined, in his view of history, the multiplicity of struggles that converged in the anti-British struggle, he defended a "modern" unitary conception of social history in which different forms of struggle, predicated on different experiences of reality and different (but not autonomous) forms of consciousness, were linked to a unitary movement of history through reference to a "real reality." It is also significant that his particular modernist version of history was related to a modernist political emancipatory project, that of trying to counter the right-wing nationalist Indian government's control of knowledge production.

## Responsibility and Communication in a World Network

The cases I have presented illustrate the different stances taken by social scientists involved in transformative projects of reality. I return now to my initial story, that of the Andalusian anthropologist Isidoro Moreno, his particular way of knowledge production, and how one might deal with it in a world anthropologies network.

Is it possible for a worldwide "scientific" community of anthropologists to be sufficiently open to others' passions to be able to communicate with scholars who are working in their native locations? To achieve this goal, such anthropologists will have to partly renounce the distancing and "objectivity" of the conventional professional anthropologist, a distancing that the coeval experience of fieldwork belies. But is it possible for anthropologists to be open to concepts and paradigms of knowledge that are alien from the one they hold (whatever it may be) or the one that is hegemonic in the academy?[6] How is the participation of anthropologists such as Moreno and historians such as Sarkar in the struggles and debates of their own societies different from the methodological oxymoron of anthropology, "participant observation"? How is the fact of having a project for the transformation of the society

we live in, observe, and study different from "social engineering"? What makes it different? (We should keep in mind that often an "emancipatory" intention or discourse is an important part of the political agendas of both right and left.) Who decides?

The first thing we have to deal with is that anthropologists (and other social scientists) who are engaged in a political project use *stable* concepts and *unitary* laws of movement (whatever they may be). If their objective is to transform reality, then they must have a realist (not just discursive) sense of reality. They will need categories adequate to the political projects they wish to engage in (this has always been the practice among those holding power, as well as among those wanting to become empowered), and they will need a structure of meaning that makes explicit the relationship between those categories and the relationship between categories, analyses of reality, and the transformation of reality—that is, a link through experience that connects consciousness with practice (Dirlik 2000; O'Hanlon and Washbrook 1992). Moreover, they will aim not so much at fragmenting realities as at producing a collective will (Gramsci 1987 [1929–35]: 185).

The knowledge produced by and for political engagement on the ground eliminates distancing and tends to create stable, unitary, and directional frameworks and concepts of the "modernist" type. This type of knowledge is based on a sense of responsibility that clearly establishes the relationship between the anthropologist and those he or she observes, the issues that must be explored to gain better knowledge and set forth some kind of organized, transformative project, and the concepts and models that should be developed for it. Engaged anthropologists in the 1960s, such as Kathleen Gough (1968), represent an earlier expression of this sense of responsibility. But outlining the framework of mutual responsibility between those participating in a coeval reality that is meant to be crystallized as knowledge of some kind is, I contend, the only way in which we can create a real space for communication. Responsibility is what links knowledge production to reality, real people, real suffering, real power. Responsibility is what makes knowledge into a project. "Participant observation," on the contrary, is what creates distancing out of ethnographic fieldwork— an experience that inevitably creates responsibilities while it lasts. But is distancing necessary in order to create some grounds for comparison, some shared discourse across places and types of knowledge? My feeling is that some distancing is necessary if what we aim for is communication and, as Raymond Williams said, through tensions, growth (1961).

But we have other obstacles to deal with if we aim to create this planetary space of anthropological encounter. One, obviously, is language. To be forced to use International English in order to communicate more widely is in itself an aspect of oppression and dependency (Comelles 2002; Kaviraj 2000), but I will not expand on this issue. I am more preoccupied with local political struggles and knowledge legitimation processes and with the frequent unawareness we have of them when approaching the production of our local colleagues. From the distance of another place and a different disciplinary history, the anthropologist often approaches those local knowledge production struggles without any background—as we say in Spanish, *sin conocimiento de causa.*

I have always been surprised by the "errors of judgment" some of my foreign colleagues make about the work and political positioning of some of my Spanish colleagues. I myself make judgments from my particular academic experience, linked to a particular history of the discipline in Spain, and from my political position regarding present-day issues in Spain (and the rest of the world). Yet however biased my appreciation of the knowledge produced by my anthropologist colleagues may be, I can situate that knowledge in concrete practices and particular histories that help me understand what they are *really* saying, sometimes, under the latest conceptual jargon borrowed from abroad (Narotzky 2002). My foreign friends and colleagues with political positions and biases similar to mine are incapable of reading between the lines until they become aware of the local histories and struggles. My question is, How do we build the criteria necessary for understanding the work of colleagues whose disciplinary histories and political positioning with respect to real-world issues we ignore? Is this an unnecessary preoccupation? Is blind openness a "good" per se? Can knowledge, through exchange, appear as detached from its process of production? What sort of vision do we have of knowledge flows as different from the actual "market" of knowledge? Would it be that of a system of generalized reciprocity?

Let me recount another story about Spanish anthropology that shows the differential positioning of native anthropologists in local power struggles and how it affects knowledge production. In February 2000, a pogrom-like event took place against North African immigrants in the town of El Ejido, Almería (Andalusia), a town then governed by the Partido Popular (PP), a party of the political right. Under the eyes of the complacent local police, who failed to intervene, Spanish residents attacked immigrants in an organized way, destroying their property, desecrating their cult space, and driving them to the mountains to seek refuge. For the PP, the event represented a spontaneous burst of

anger from the Spanish residents over the immigrants' nonintegrating practices and criminal behavior (the cause of the pogrom reportedly was the alleged murder of a local girl by a Moroccan immigrant).

What I am interested in exposing in this case is the participation of Spanish anthropologists as "experts" in the production of knowledge about this event and, more generally, issues surrounding immigration policies and multiculturalism. Before the events took place, several anthropologists from Andalusia, such as Emma Martín (University of Seville) and Fernando Checa (University of Granada), had been doing fieldwork in the area of the Poniente Almeriense, where El Ejido is located, trying to evaluate the realities of immigrant workers' lives in intensive hothouse agriculture. Martín's work, part of a larger project including other Spanish regions (Martín, Melis, and Sanz 2001), was being co-financed by the European Union and the Junta de Andalucía (the autonomous government in the hands of the social-democratic Partido Socialista Obrero Español). Their work was mainly empirical but clearly related to a loosely "political economic" framework, highlighting the effects of transnational economic processes and national exclusionary policies. An anthropologist from Madrid, Ubaldo Martínez Veiga (Universidad Autónoma Madrid) was also doing fieldwork in the area (Martínez Veiga 2001). Close to the labor union Comisiones Obreras, on the political left, he was moved to respond to the role of the unions in organizing and defending immigrant workers' rights. The work he produced was very theoretical and framed in a strong political-economy conceptual structure.

After the events, these anthropologists became vocal about the knowledge they had produced, participating in many local forums such as union meetings and meetings of immigrants' associations.[7] All of them told how they experienced forms of intimidation, from censorship to threats against their lives.[8] Some months after the events of El Ejido, on 15 December 2000, the Spanish Senate, with a PP majority, approved a new Ley de Extranjería (law of foreigners), excluding illegal immigrants from basic civil rights such as the rights of association, public meeting, union membership, health, and education. On 4 January 2001, twelve immigrants died in Lorca, Murcia, in a car accident while trying to avoid police controls as they drove to work as agricultural day laborers in the informal economy. In the application of the new law, repression hit the victims: illegal immigrants in the area were expelled from the country in order to apply for legal admission. Local agricultural entrepreneurs in Lorca were exonerated from any legal responsibility for their exploitive and inhuman labor practices regarding immigrants.

Immigrants, for their part, explicitly pointed to the new foreigners law as having prompted the fatal accident.

In this context the PP government created an institution called Foro de la Integración de los Inmigrantes (Forum for the Integration of Immigrants) and named as its president an anthropologist, Mikel Azurmendi, a former member of the Basque nationalist group ETA who is presently in "exile" from the Basque country after allegedly receiving death threats from ETA and who is now close to the PP's views and policies against peripheral nationalism and immigration. Soon Azurmendi produced a book, *Estampas del Ejido* (2001), and numerous newspaper contributions (Azurmendi 2002a, 2002b) in which he explained the events as a conflict between cultures. Immigrants, he wrote, lacked a proper "work culture" and "democratic tradition" and were responsible for upsetting local ways, which justified the government's repressive policies toward them.

Many anthropologists who did not share Azurmendi's views reacted strongly to them. Isidoro Moreno, then president of the Anthropological Associations of the Spanish State (FAAEE), together with Emma Martín, wrote a document protesting Azurmendi's positions and questioning his professional capacity, which was sent to be signed by all other academic anthropologists.[9] The original mailing list consisted of 129 anthropologists tenured in universities; 63 people, including some nontenured faculty, signed the letter—approximately 50 percent of the original list. It is difficult to assess people's reasons for supporting or not supporting such a corporate move, one intended to defend the anthropological profession against an "alien body" (Azurmendi). Many who signed the document would not have written it in the same way but felt that it helped position anthropology in a particular framework of responsibility. Many who did not sign also had corporate reasons, such as not voicing public criticism of a colleague. Others may have declined to sign for pragmatic reasons: government agencies offer a great deal of funding for research on immigration. Still others plainly supported Azurmendi's views.

Where does this leave us in terms of gauging the value of knowledge produced in peripheral locations, or in any other locations, for that matter? It is often the case that we ignore the practices and realities that create a context for understanding the meanings that others produce. The postcolonial critique has often essentialized nonhegemonic locations in the production of knowledge, as it has essentialized Euro-American locations. But as the case I have just presented shows, the production of anthropological knowledge in the peripheries and elsewhere is diverse:

it involves complex power relationships and mutually contradictory projects. It is attached to multiple political agendas spanning both right and left, both the justification and support of governmental policies and criticism of them, and both "institutional" and "alternative" forms of agitation. I am not proposing censorship: I may learn a lot from reading Azurmendi (one has to know the arguments of one's opponents), and I want to know what he has to say *as an anthropologist.* But in order to be able to appreciate his knowledge and make something of it—that is, in order for the communication of that knowledge to be truly possible—I need to know where he stands on real-life issues.

"Scientific" detachment has made us believe that knowledge can flow and be communicated without being grounded, not only with respect to the author but also to the historical context of its production. And it is partly so: that is how we read most of what we read in our Western hegemonic context of knowledge production. But we always have some clues, precisely because the hegemony has produced a small world. We know which sorts of journals publish what; we read an author's acknowledgments and get an idea of his or her personal context; we know about political positioning in the US academy because it is well covered, discussed, and publicized in the many forums open to it. But we do not have the same sort of knowledge about India, or China, or Morocco, or Russia, to name but a few. Would a world anthropologies network provide a space in which not only to access detached forms of knowledge but also to situate them in their production processes?

## World Anthropologies: A Realist Proposal

We return now to the need to communicate our knowledge and to create growth out of communication. How might this be possible within the politicized environment I have described, in which anthropologists who work "at home," who "lack distancing," participate in the very real issues and debates that produce the present? Should we, following Herzfeld, discard "epistemological nativism" as unscientific and engage with it only as an object of study? Should we, following Taussig, opt for self-contained characterizations of passionate experience? Should we, following the lead of Kaviraj, ignore what we do not know? The problem is difficult to solve and is a classical anthropological problem after all, spiced with history and politics and the Damocles sword of postcolonial and Foucauldian critique. It is the problem of incommensurability and comparison, of detachment and participation, of the degree to which knowledge production is political from the outset, and of the need for communication.

As I see it, there is no way out of political positioning in the production of anthropological knowledge. The postmodern critique has made us aware of the profound political implications of seemingly objective forms of knowledge that are nevertheless enmeshed in particular regimes of truth. But should we discard everything we know that was produced from a particular (intended or unintended) political position? Can we learn nothing from Aristotle, Aquinas, Hegel, Arguedas, Rivers, or Malinowski, for example? We undoubtedly can. So how should we proceed? We situate them and their concepts in a historical context, a reality of the past that gives a particular meaning to what they said. Then we proceed analogically, bringing those descriptions of reality, those concepts and structural frameworks, to bear on the present reality we want to explain. We stretch the concepts; we confront them; we create new ones out of a creative synthesis with other concepts from other times and thinkers. We produce a new framework or modify an older one to give meaning to the relations among the concepts we use in reference to the reality we want to understand and change (or support).

In anthropology, moreover, we use ethnographic descriptions (however critical we are about the way in which they were produced) as material for comparison. We adopt in this regard something similar to the suspension of disbelief that realist fiction entails: we must trust that some reference to "real" reality exists in the description. We need to proceed in this way, through conversation with works of very different kinds, in order to grow in our thinking about reality. And to be able to do that in a creative way, we need explicitness—that is, we need to be told (or to know or learn) what the author's political project was. This will empower us to understand his or her work better and to relocate knowledge in reference to a concrete reality. We can learn something from discourses that are alien to our concrete reality and to our theoretical framework only if the author's responsibility in relation to his or her reality is clearly outlined, if an effort is made toward explicitness. Then we will be able to place that knowledge or its critique in our theoretical framework and proceed toward our own project.

Ignasi Terradas (University of Barcelona) proposed something similar with his reappraisal of the methodology of "ethnographic realism":

> To the extent that the ethnography exists, it exists as a *thing* in the Durkheimian sense and in the Marxist sense. Objectivity and alienation are the stereotypes of the failed ethnography. Subjectivity and metonymy are those of a pseudo-ethnography. The realist ethnography moves in between a reality that always exceeds it and a theorization that is an

approximation . . . Judgments about ethnographies must come from their mutual confrontation. If not, we will get not ethnographic knowledge but psychological, stylistic, moral, political, etc. [knowledge] . . . The inter-ethnographic dialogue is what realizes the appropriate and wide meaning of the ethnography. That is its real meaning, that which appears in the context of the flow of ethnographies itself. (Terradas 1993: 120)

For Terradas, it is the relationship between ethnographies that constitutes anthropology as "a scientific (analytic) and artistic (evocative) reality" (1993: 120). And it was the possibility of apprehending the distinction between description and interpretation in ethnographic writing that constituted the epistemological break in anthropology:

Our point of departure now is that realist ethnographic composition and anthropological theory have to come from the total confrontation of several ethnographies . . . the pioneering efforts of anthropologists at the beginning of this [the twentieth] century lay in their forecasting of such a confrontation. That was the reason they made the effort to provide an explicitness without precedent in their descriptions, methods, and theories. The ethnographic text that reaches us after this epistemological break, which we can easily characterize as the revolution of explicitness in anthropology, is what distinguishes ethnographic realism and marks an important stage in the history of the discipline. (Terradas 1993: 121)

Terradas points to the importance of ethnography in our discipline, but it is an ethnography whose aim is to transcend a particular experience through its aprioristic will to communicate with other ethnographies and, through this unending exercise, to try to better understand reality. If it is true that the production of ethnographic knowledge has to be historicized, it is nevertheless our particular link to reality as a social science, and we should relearn to deal with it. It is from within our ethnographic practice (intertwining experience, thinking, and writing) that communication with other anthropologists' work can proceed— but also, unappealingly, the will to communicate has to be part of the life and growth of anthropology from the start. It is from our reference to lived reality—a unitary, contemporaneous, and shared reality—that modes of responsibility can be made explicit. I believe that both distance and participation are necessary for communication to take place and that political projects are an unavoidable reality of the products of social scientists. Only hegemonic forms of knowledge present themselves as apolitical.

# Notes

1. I am indebted to Marisol de la Cadena for the concept of epistemological tolerance and for making me think about this issue.

2. Josep Ramón Llobera was one of the few early "cosmopolitan" anthropologists in Spain. In a letter in response to John Corbin's (1989) reaction to Isidoro Moreno's criticism of foreign anthropologists' double colonization of Spanish anthropology, Llobera, while generally supporting Moreno's critique, dismissed his "emotional and somewhat chauvinistic tirade against" those anthropologists (1989:25). Llobera pointed to the change of position of Spanish anthropologists in the knowledge-production field of power: "Native anthropologists, now not so naïve as before, trained in foreign languages and even in British anthropology as they are, happen to be in a position to discover blatant plagiarisms from Spanish theses, unacknowledged information given by Spanish academics, and other sins… It will take more than nice words to balance decades of asymmetric relationships in which Spanish academics provided anthropological raw material and consumed ready-made foreign theories" (1989:25).

3. We may recall here Arjun Appadurai's "gatekeeper concepts" in area studies (1986) and Michael Herzfeld's analysis of the creation of "fixed" concepts in Mediterranean anthropology (1987).

4. The project Moreno presents is clearly a search for origins and history, oblivious of the Foucauldian emphasis on genealogies (Foucault 1979 [1971]).

5. During the Wenner-Gren symposium from which this book arose, Shiv Visvanathan remarked to me that Sarkar had been involved in "official" politics and had been pampered by previous governments. He had then supported and excluded particular histories and historians, and his recent harassment had to be understood in the context of these complex, long-term processes in Indian politics and academia. In short, Visvanathan's point was that Sarkar was not an "innocent victim" in a good-guys-bad-guys scenario. His remark lends support to my argument that grounding in scholars' political practices is a crucial element for knowledge communication.

6. It is remarkable how we are prepared to accept the hegemonic discourse—even if only to oppose it—and how in this process we often resort to adopting and adapting a large part of its conceptual tools and driving narratives. Meanwhile, we tend to dismiss without second thought or, more often, to ignore nonhegemonic discourses about society.

7. Another anthropologist from the Universidad Autónoma de Madrid, Carlos Giménez, who worked on immigration but had not done fieldwork directly in that area, was also vocal during an early moment.

8. Martínez Veiga and Martín, personal communications.

9. The document and its signatories appeared in *Página Abierta,* no. 128, year 12, pp. 46–47.

# Anthropology in a Postcolonial Africa
## The Survival Debate

## *Paul Nchoji Nkwi*

African anthropologists grew up in societies that were either colonized or recently decolonized. Westerners initially controlled the production of anthropological knowledge, and the result was functionalist studies that were explicitly ahistorical and often myopic about colonialism. After the colonial period, the new nations of Africa dismissed anthropology both as a cultivation of primitivism and as an apologetic for colonialism. As all states do, the new nations rewarded knowledge production that served state goals, and anthropology simply did not figure into those goals during the early postcolonial years.

While new nations were appearing in Africa, anthropologists in Europe and North America remained mostly committed to the dispassionate study of cultures, looking upon knowledge production as tainted if done on behalf of government or for policy purposes. 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.

These scholars had three possible solutions: declare anthropology dead and try to legitimate themselves as social historians within Africa; export themselves to the United States and Europe, the way many Third World scholars of all disciplines have exported themselves; or change the content of anthropology and search out the information required

by their governments. To most scholars, the first solution was defeatist and unappealing. The second was appealing, but anthropologists were not needed in the West to the same extent that, say, chemists and engineers were, so there were few opportunities for emigration. The third option raised a serious question: would involvement in policy research harm anthropology or make something better of it? In the end, the choice was made for African anthropologists by government funding for research and university posts. Anthropologists would, in fact, serve the research and teaching needs of the state.

In this chapter I examine the ways in which African anthropologists have developed knowledge within a particular set of state needs and within a particular set of power relations. I focus on a particular effort, the Pan African Association of Anthropologists (PAAA). Today, the PAAA is a professional organization accepted by a once hostile community of social scientists. This acceptance has dramatically affected the applied dimensions of anthropological knowledge and the way anthropology is taught and practiced in Africa. I begin with an overview of the anthropology of sub-Saharan Africa and then offer a history of the PAAA. The organization developed at a time when Western-trained African anthropologists were renegotiating their place in the discipline, both in Africa and internationally. The dilemma faced by African anthropologists—how to carry out investigations according to internationally held canons of scientific practice while keeping faithful to the demand for immediately useful research at home—mirrors that of all Third World academics, but the dilemma for African anthropologists is an extreme case because of the discipline's well-documented history as the handmaiden of colonialism.

## Africa's Place in the World System

From the beginning of colonial rule, anthropology in Africa—as the study of human cultures and peoples—largely reflected the outsiders' view of the continent. It would take many decades for Africans to articulate a view of themselves in relation to that outside world. When anthropology emerged as a discipline in the 1860s, Africa was not part of the world economic system. It had been so, four hundred years earlier, but by the mid-nineteenth century scholars in Europe had long forgotten this and saw Africa only as a backwater.

It soon would be part of the world system again. The slave trade had led to the creation of European stations on the African coast for the recruitment of human capital. By the late nineteenth century, despite

the abolition of slavery in most of the world, European nations were jockeying for position and access to the human and other resources of the continent. The treaty of Berlin, in 1878, granted any "civilized state" occupying a coastal African region the right to claim the hinterland. This could be achieved, however, only by occupation (see Ganiage and Héméry 1968: 199), and so the scramble for Africa was on, with a massive outpouring of explorers, travelers, and missionaries who would shape future anthropological work on the continent. Just eight years later, in 1885, the jurisdictional disputes between rival European countries over Africa were settled with the recognition of territorial claims (Sklar 1985: 1). Africa had become an integral part of the world economic system as a supplier of basic resources.

The establishment of the journal *Présence Africaine* in the 1940s was a reaction by African and African American intellectuals against what they saw as a failure by Euro-American intellectuals to recognize adequately Africa's role in world history. Basil Davidson, in his book *The Lost Cities of Africa* (1959), showed that sub-Saharan African history was, in fact, an integral and important part of world history. This reaction later developed into what came to be known as Pan-Africanism, which was to be a powerful influence on many of Africa's early postcolonial leaders and intellectuals.

English-speaking anthropologists dominated anthropology during the colonial period, partly, I think, because of their philosophical doctrine of empiricism, which fostered greater respect for local culture than did the French colonial "mission civilisatrice." Whatever the cause, English-speaking anthropologists served colonial administrators whose directive was to rule through local personnel, and this, in the jargon of postmodernism, produced multivocality and gave anthropologists the opportunity to assert themselves more creatively. The emergence of anthropology as a discipline in the university system in Great Britain during the years between the two world wars led, in 1925, to the creation of a state-sponsored research institute, the International Institute of African Languages and Culture (IIALC). This institute (later known as the International African Institute, or IAI) encouraged the collection of massive quantities of ethnographic data on Africa and further consolidated the discipline.

The institute established the Journal *Africa* in 1928 and, in 1938, under Lord Hailey, published *The African Survey.* It inspired monographs on African politics (*African Political Systems,* edited by Meyer Fortes and E. E. Evans-Pritchard [1940], and *Tribes without Rulers,* edited by John Middleton and David Tait [1958]), cosmology and religion (*African*

*Worlds,* edited by Daryll Forde [1954]), witchcraft (*Witchcraft and Sorcery in East Africa,* edited by John Middleton and E. H. Winter [1963]), and kinship (*African Systems of Kinships and Marriage,* edited by Alfred R. Radcliffe-Brown and Daryll Forde [1956]). Other English-speaking anthropologists of the period included E. E. Evans-Pritchard, Victor Turner, Audrey Richard, and Mary Douglas, all of whom contributed to what Godfrey Lienhardt (1976) called the early theoretical capital of the generation that came into academic seniority after the Second World War. By the time John Beattie and John Middleton edited *Spirit Mediumship and Society in Africa* (1969), social anthropology had captured the imaginations of black Africans, who were turning to the discipline for answers to questions about making development schemes successful in culturally heterogeneous societies. In the 1930s, Jomo Kenyatta, from Kenya, would study under Malinowksi; his *Facing Mount Kenya* was published in 1938. Kofi Busia, from Ghana (1962), and Cheik Anta Diop, from Senegal (1974), who had emerged as defenders of the right of Africans to be part of world history, were also deeply committed to anthropology.

The development of Africanist anthropology in France was largely the work of two key government-sponsored institutes: the Institut Français pour l'Afrique Noire (IFAN)) and the Organisation de Recherche Scientifique et Technique d'Outre Mer (ORSTOM). IFAN was established principally to document, for comparative purposes, the customs and traditions of African "ethnic nations." ORSTOM, on the other hand, had a broad mandate, allowing it to conduct more comprehensive studies in all the French colonies, including those in Africa, by focusing on social, human, mineral, health, and geological research. The creative work of ORSTOM would, like that of its British counterpart, generate massive amounts of ethnographic data.

At the close of the colonial era, Africanist ethnologists such as Georges Balandier (1966) and Jacques Lombard (1967) returned to the French university system. Balandier, one of the leading French political anthropologists, would influence a whole generation of French anthropologists but also drew inspiration from Great Britain. He trained a new group of French anthropologists, including Claude Meillassoux (1968), Marc Augé (1986), and Jean Copans (1990).

In the 1970s, under Meillassoux, Marxist anthropology would again capture the imaginations of Africanists across the world. Anthropology as a discipline was considered "colonial," something that was used to colonize the continent. Marxism, on the other hand—as an ideology, not just as a theory of history—was more sympathetic to the fight

against capitalists and the imperial project of the West than was the empiricist tradition of Great Britain. French anthropologists would leave their anthropological ghetto as French-speaking scholars and explore the rest of the continent. To their surprise, they found a totally different intellectual and academic outlook among British and British-trained anthropologists. They also confronted a huge language barrier between French and British anthropologists.

Ironically, despite their use of anthropologists in the colonial enterprise, officials at the British colonial office were profoundly suspicious of anthropologists, especially those who came from the practical school of anthropology headed by Bronislaw Malinowski. Some colonial administrators accused anthropologists of peddling "tribalism." Nonetheless, under intense pressure from nationalists, Africanist anthropologists from the West withdrew from studies on the continent during the 1960s. They correctly feared that the postcolonial African leaders would endorse neither the old colonial policies of governance nor the scholars who had supported those policies.

## New Nation-States and the University System

At independence, each new nation created its own institution of higher learning with a curriculum based on that of the European universities. State elites assumed that the transfer of scientific knowledge was crucial for development, and each country urgently needed trained manpower, especially in its civil service. For example, at independence one African nation had about sixteen university graduates, of whom twelve were priests and four were laymen. International donors understood this need and supported its fulfillment, so that among the first objectives of the new universities was to produce such manpower. Little attention was paid to the study of African cultures in the new university curricula. Universities recruited African faculty with overseas training to teach alongside expatriates and gradually to replace them. Throughout the 1960s and 1970s, the returning graduates were guaranteed salaries, housing, and even transportation.

The applied dimensions of the discipline would suffer a setback, however, as nationalist movements turned to modernization theory to transform Africa into what they hoped would be an economic power. These nationalist movements continued to regard anthropology as a tool of colonial subjugation and as a discipline of no relevance for the new and modernizing Africa (Nkwi 2000: 21). African and Africanist anthropologists found it difficult to practice their profession openly. At

Makerere University in Kampala, for example, the British had established the Institute of Cultural Anthropology to promote ethnographic research. This once flourishing institute disappeared into the sociology department (Crossman and Devisch 1999: 117).

Caught between a desire to break with the colonialist past and a desire to attain economic and social progress equivalent to that in the former colonial powers, some Marxist-oriented African leaders plunged into an ill-conceived economic development model called "African socialism," or "communalism." The model was an odd mixture of statist and classical development economics. Although many of these leaders claimed African roots for their political ideology, few relied on anthropology to provide the basis for such an ideology. Many spoke of African culture without comprehending what that might mean in practice. Two UNESCO conferences (one in Monrovia in 1979 and another in Yaoundé in 1984) called for the teaching of African languages and cultures, but this simply never happened in most countries. Anthropology could have provided the material for such a curriculum, but the discipline was not taken seriously, carrying the stigma, as it did, of its ties to the colonial past (Crossman and Devisch 1999: 117; cf. Sawadogo 1995).

The first call for a university for West Africa came from three nineteenth-century black intellectuals: Dr. James Africanus Beale Horton (1835–83), Edward Blyden (1832–1912), and Rev. James Johnson (1839–1917). Blyden, for example, called for an indigenous university that would "release Africa from the grip of the despotic mind and restore cultural self-respect among Africans," and Johnson called for "an institution that would leave undisturbed our peculiarities" (Wandira 1978: 39–40; cf. Odumosu 1973).

A century later, while opening the Institute of African Studies at the University of Ghana–Legon, Dr. Kwame Nkrumah, then president of Ghana, invited African scholars to study Africa in all of its complexities and diversity, in order to stimulate respect for the idea of African unity. The study of African cultures and people was not to be limited to conventional and regional boundaries. Nkrumah urged that all investigations "must inevitably lead towards the exploration of the connections between musical forms, the dances, the literature, the plastic arts, the philosophical and religious beliefs, the systems of government, the patterns of trade and economic organization that have been developed here in Ghana and in the cultures of other African peoples and other regions of Africa" (Hays 1958: 10; cf. Hagan 1989). In his book *Africa Must Unite* (Nkrumah 1963), culture is also a dominant theme.

Anthropological studies would be part of the essential programs of the Institute of African Studies.

As it turned out, if African socialism did not work, then neither did the main opposing strategy for development. With profound respect for the scientific principles behind the hugely successful Marshall Plan in postwar Europe, African planners swallowed development theories that targeted investment in industrial development and human capital. Most African leaders in the early decades after independence followed a pattern of industry-first investment and the development of so-called urban growth poles (see Eicher and Staatz 1984). All initiatives were mixed with overbearing state involvement in the management of the economy.

The policies adopted in the 1960s and 1970s permitted African state governments to intervene at all levels, controlling market forces by providing credit and setting prices for commodities. The construction of the postcolonial state saw the disarticulation of economic, cultural, and ethnic differences and the endorsement of arbitrary colonial borders. Economic failure and criticism of state policies would lead to the Structural Adjustment Programs (SAPs) of the 1980s. Those programs demanded the separation of the state from the market economy, reducing public expenditure and empowering the private sector (Coussy 1991:123–39). These adjustments were the price of continued international loans and other supports, but they were detrimental to the masses, who would become poorer and poorer. During the 1990s, the new buzzword was "poverty alleviation"—referring to the poverty that had been created during the 1980s by the poorly executed SAPs of the international financial institutions.

Vitriolic attacks on the discipline by some African intellectuals (Seri 1989) retarded its progress as the "rejectionist syndrome" drove some of our social science colleagues to extremes. In 1991, Ife Amadiume, an African sociologist, recommended abolishing anthropology and turning it into "African social history and sociology of history" (Mafeje 1997: 22). In the 1970s, such criticism led to the emergence of three trends. First, anthropology took cover within African Studies programs. Centers of African Studies emerged in many American universities, and analogous Institutes of African Studies flourished in most Anglophone African universities. Within those institutes, anthropology per se was taught and practiced. Second, the role of Marxist intellectuals in fighting imperialism and colonialism led to the emergence of Marxist anthropology. Marxism, as a philosophy, served as an intellectual cover for many European anthropologists desiring to continue doing

anthropology in Africa without being accused by nationalist movements of being part of the colonial apparatus. Being a Marxist anthropologist was politically correct at the time. Third, anthropology was labeled one of the branches of sociology and was taught within the nascent departments of sociology in African universities.

In South Africa, anthropology continued to function as a formal discipline at the Universities of Cape Town and Witswatersrand, as well as at Rhodes and Natal Universities. These institutions, however, provided little support to departments of anthropology at the so-called bush-colleges (Transkei, Unitra, Durban-Westville, Venda, the North, and so on; see Svawda 1998). These colleges had been established under the apartheid system to provide education to Blacks and the so-called Colored. The *voelkerkunde* tradition, the ideological ingredient of the apartheid system, continued at the Universities of Pretoria, Port Elisabeth, Stellenboesch, and Bloemfontein. In 1996, representatives of the *voelkerkunde* tradition attempted to legitimate their group by joining the PAAA during the association's seventh annual conference in Pretoria. They were not admitted, because of the tradition's association with past racism. In 2000, however, the two traditions merged (Bogopa 2001:2).

Despite the stigma of Africa's colonialist past, many Africanists from Western universities continued to study anthropology in Africa after independence. The Rhodes Livingstone Institute continued to enhance the anthropology of Africa, and the Manchester school, with Africanists such as Clyde Mitchell (1969), continued to publish on African anthropological issues. Other Africanists, such as Elisabeth Colson (1971), Mary Douglas (1963), Audrey Richard (1969), and Ronald Cohen (1971), worked intensively in Africa for decades, even after independence. Kofi Busia, a Ghanaian who studied anthropology and established the Department of Sociology at the University of Ghana-Legon, would even head the Department of Anthropology at Leiden University in the Netherlands. Jomo Kenyatta, a student of Malinowski's, would use his anthropological skills to construct the Mau Mau movement to claim power in Kenya. Leading African anthropologists such as Adam Kuper (1987), Brian du Toit (1974), Archie Mafeje, and Maxwell Owusu (1970) left their countries in search of more conducive environments for serious anthropological work. Others, such as Kwesi Prah (1993), Godwin Nukunya (1969), Harris Memel-Fotê (1980), and Théophile Obenga (1985), remained in Africa to do research and teach anthropology.

In Francophone Africa, as ORSTOM's influence began to diminish in the 1970s, institutes of human sciences were established outside of

the university system. Within the universities, courses on marriage, kinship, African political and social institutions, and other subjects with anthropological content were taught in departments of sociology; those who taught these courses preferred to be called sociologists. These developments coincided with the establishment of professional African studies associations in the United States and Canada and of journals such as the *Journal of African Studies* in 1974. French anthropological research continued in non-university settings, not only in ORSTOM but also in the Centre National de la Recherche Scientifique (CNRS), the Musée de l'Homme, and the École Pratique des Hautes Études. This continued even after independence, perpetuating the colonial legacy of these institutions (see Copans 1990: 32–36). During the 1960s and 1970s, however, the University of Paris X–Nanterre was the only one out of seventy-six French institutions that offered anthropology at the undergraduate level (Copans 1990: 66–70).

These timid efforts within the French university system gained greater impetus under Georges Balandier and Pierre Alexandre. They established the Center for International Relations (later known as the Centre d'Études et de Recherches Internationales, or the Center for Studies and International Research) with a specific focus on Africa, and Marcel Merle and Albert Mabileau set up the Centre for the Study of Black Africa (Centre d'Études d'Afrique Noire) in Bordeaux. After independence, ORSTOM forged a new relationship with new institutes of human sciences throughout the former colonies, and it continued to work under a new umbrella called the Institut de Recherche pour le Développement. In fact, in French-speaking Africa ORSTOM remained, until the 1980s, the only credible institution with the resources to conduct serious anthropological work (including archaeology, linguistics, ethnology, and social anthropology). Even in countries such as Cameroon, where the Social Sciences Institute in the Ministry of Scientific Research collapsed during the Structural Adjustment Programs, ORSTOM continued its research work, though it did so without involving senior local scholars. Despite the slowdown of knowledge production and the reduction in scholarship, the anthropology of Africa continued to contribute to the development of comparative ethnological theory and to the academic debates of the 1960s and 1970s.

## Policy Shifts and Years of Awakening

By the end of the 1970s, mounting evidence showed stagnation or decline in per capita economic growth rates in Africa. The failure of modernization theory to transform nascent modern African economies

led development agencies to rethink their policies. The World Bank committed major resources to developing the "poorest of the poor," which was basically an open admission that macro-level policies had failed to achieve their objectives. As the micro-level approach became a real alternative, some argued for the need for input by anthropologists. This was based on the theory that these fieldworking scholars were in the best position to understand how, for example, food markets worked in Africa, as well as who the participants were.

In the book *Development from Below,* David Pitt (1976) and others showed, through a series of case studies, how development projects failed, specifically when the people whom the projects were intended to help participated in neither the design nor the implementation of those projects. Anthropologists with knowledge of local cultures would have argued for exactly that kind of input—although it might not have been enough to stave off failure. By the 1980s, the demand for anthropological input had strengthened. This had an effect on the small community of African anthropologists who were still operating underground in departments of sociology. These scholars consulted with nongovernmental organizations and other bilateral and multilateral agencies regarding the design of development projects, but they had little involvement in implementation.

Another major policy shift involved the training of applied scientists for rural development. If the emphasis at independence was on producing a critical mass of people to run the postcolonial administration, by the early 1980s the focus had shifted to improving agricultural production and the living standards of the rural poor. This required training Africans in agronomy, animal science, veterinary medicine, soil science, rural economics, rural sociology, and the emerging field of development anthropology. The land grant university system had worked agricultural miracles in the United States, and the US government launched a massive program to help build and staff entire agricultural universities in Africa, based on the land grant model. Science and extension services would be the keys to a new Green Revolution. The Agricultural University of Dschang in Cameroon was one of the beneficiaries of such a policy. However, despite the training of more than forty faculty members, the country-wide agricultural extension model envisioned in the design of the university was never materialized.

Another major impetus for engagement by anthropologists in development programs was the Alma Ata (Kazakhstan) conference of 1978 on health care. The Declaration of Alma Ata called for a new emphasis on primary health care and on the participation of locals in the design

and management of health systems. This shift from hospital-centered to people-centered health care gave medical anthropologists a window of opportunity. The Bamako Initiative—Africa's interpretation of the Alma-Ata declaration—called for "health for all in the year 2000," and this further opened opportunities for medical anthropologists.[1]

Another major policy shift took place at the joint conference of the Economic Commission for Africa (ECA/UN) and the Organization of African Unity (OAU) held in 1984 in Arusha, Tanzania. This conference brought experts together to address Africa's failure to produce economic and social prosperity after two decades of massive foreign assistance. In the final document, ECA and OAU acknowledged that the beneficiaries of development had been marginalized in the process. The document failed to acknowledge that the social sciences had also been left out of the process. Michael Cernea (1982) would also indicate the importance of culture and a people-centered approach in his writings. Of course, the failure to achieve prosperity in Africa was not the result solely of leaving social science out of development; corruption and ethnic violence both had significant roles to play.

## The Cameroon Case

Through its slow and deliberate infiltration of policy making, anthropology has come to be recognized in the intellectual circles of my own country, Cameroon. Anthropologists in Cameroon have been engaged with the state in several capacities. First, as employees of the state—civil servants—university professors have had a duty to teach the subjects they are assigned. Second, some social scientists have become part of the state's policy-making apparatus as members of government ministries, deans of faculties, or even chancellors or vice chancellors of universities.

Cameroon became a German protectorate in 1884. For thirty-two years, until 1916, colonial military forces conducted pacification operations there to quell uprisings by ethnic groups that refused to recognize German sovereignty. During this time, scant ethnographic work was conducted (Nkwi 1989). When the combined forces of British and French troops defeated the Germans in 1915, Cameroon was split into two parts and administered under the League of Nations. France administered almost two-thirds of the original German territory, while Britain took over the rest of it, bordering on Nigeria, and administered it from Lagos. Researchers affiliated with ORSTOM and CNRS worked in the French-speaking part of Cameroon, collecting and analyzing

ethnographic data. The creation of *Études Camérounaise* by these French institutions offered all scholars an opportunity to publish their findings in a unique journal. Some of the well-known anthropologists who worked in Cameroon during this period were Claude Tardits (1960) and Phillipe Laburthe-Tolra (1985), from France, and Peter Geschiere (1982, 1983), from the University of Leiden.

Between 1916 and 1960, while ORSTOM and CNRS conducted anthropological surveys in French Cameroon, anthropologists from Oxford and University College London focused on collecting ethnographic material to give the British colonial administration a better picture of ethnic diversity in the so-called Southern Cameroons. Phyllis Kaberry, from London (1952), and Elisabeth M. Chilver (1966, 1974) and Edwin Ardener, from Oxford, would spend their young adulthoods building the basis of future anthropological work in English Cameroon. A younger generation of anthropologists, including me, would be inspired by the massive quantity of ethnographic data accumulated and sometimes published in the journal *Nigerian Fields*. This generation included Philip Burnham (1996), Michael Rowlands, Jean Pierre Warnier (1993), and Richard Fardon (1990).

In 1973 the government of independent Cameroon decided to reorganize the research, which had remained largely in the hands of French scholars. Of the seven institutes created, one was reserved for the social and human sciences. Within this institute a department of anthropology was established, and the first head of the institute was an anthropologist. The Institute of Human Sciences remained in existence until 1993, when the government shut it down and moved the researchers to various ministries. The reasons for its closure were largely political. With the push for democracy of the 1990s, as well as the political engagement of many of the institute's researchers, the government came under criticism for its mismanagement and for the country's growing economic crisis. At least ten anthropologists accepted transfers to government departments, whereas others refused to accede to government pressure and instead joined various opposition parties.

Earlier, in 1962, the Federal University of Cameroon had been established. Within its Faculty of Social and Human Sciences was a department of sociology, headed by a French anthropologist. In this way, anthropological research and teaching continued at the university for years, although the courses were referred to as sociology. It must be said that the emerging state of Cameroon was not hostile to anthropology, for it continued to invite and deliver research clearance to researchers from Europe, the United States, and Asia (especially Japan).

I joined the Department of Sociology in 1976. Because I was the first faculty member from the English-speaking part of Cameroon, my first assignment, besides teaching basic courses in anthropology, was to assist and counsel English-speaking students. Two other colleagues, who were trained in general ethnology in France, taught courses in the department as well as in the Faculty of Law and Economics.[2] While I identified myself with anthropology, they continued to call themselves sociologists.

Then came the crisis of 1978, when internal reforms eliminated disciplines that were presumed to produce social critics and the unemployed. The university administration convened a meeting of the heads of departments within the Faculty of Arts and Social Sciences. As acting head of the department, I attended the meeting, chaired by the chancellor and vice-chancellor. The dean of the faculty, a historian, presented a report on reforms in the faculty that implied that sociology and anthropology would be phased out of the university curriculum. After a series of meetings involving the departmental staff, university authorities, and the government, the minister of education called off the reforms. Strict instructions were given to the dean to maintain sociology and anthropology in the curriculum, but the reforms were put in place nonetheless. Sociology and anthropology would be taught as part of philosophy, but no BA degrees would be awarded.

When the first continent-wide conference of African anthropologists was held in 1989, the ministry supported it and provided resources. To highlight the importance of anthropology as a teaching subject, the minister of higher education asked the chancellor of the University of Yaoundé to open the conference in the name of the Cameroon government. In welcoming the thirty-five African anthropologists from twenty-one universities, the chancellor called on anthropologists to take their rightful place in the development arena and to show what the "discipline can do to solve some of the problems Africa is facing." From 1993 on we rebuilt the discipline, designing courses for the bachelor's, master's, and PhD degrees in anthropology. Prior to this, the bachelor's degree had not been offered in anthropology. Just eight students declared anthropology as their major in the 1993–94 academic year, but the number grew to over one hundred only a decade later. In the 2002–3 academic year, there were 525 students majoring in anthropology, not to mention the same number of students taking it as their minor.

During this period I witnessed the increased involvement of social scientists in health, agriculture, animal, environmental, and population

research programs funded by the government. There were several reasons for this. First, the proliferation of development programs increased the demand for social science input in general and for anthropology in particular. Second, the university reforms that took place in the 1980s across Africa offered an opportunity for the enhancement of sociology and anthropology teaching programs. For example, in 1985 the University of Nairobi established a full department of anthropology within the Institute of African studies. The University of Yaoundé started a full degree program in anthropology in 1993, giving students access to both undergraduate and graduate degrees in anthropology.

Philip Kilbride (1994: 10) noted that Kenyan anthropologists were "struggling to resurrect anthropology from the ash heap of its colonial associations by advocating anthropology in diverse public and private forums." By 1994, he said, Kenyan anthropology was flourishing "at universities and institutes with research on such issues as overpopulation, polygyny, the status of women, AIDS and sexuality, tourism and children's health." Anthropology had to rediscover itself both as an academic discipline and as a discipline that could help to solve problems. Anthropologists had to show that they were not peddlers of tribalism but that they sought to expand the horizons of human knowledge and to adapt to new areas and the challenges of development (Monteiro 2002: 8).

At the University of Yaoundé, anthropology and sociology remained—for historical reasons—in one department, but they awarded separate degrees at both the undergraduate and graduate levels. In 1993–94, however, with increased resources and the desire to evolve independently, sociology and anthropology went their separate ways. Anthropology expanded its teaching and research and attracted an increasing number of students. The increased demand for consulting work gave anthropologists visibility. This in turn pressured them to assert their identity and to highlight the dual academic and applied approaches of their discipline. Many of us saw application as the best option for the discipline to reclaim its lost glory. Given the strong market for professionals in both national and international development work, I argued that anthropology was a social science discipline ripe for professionalization. Many of us in academia who were already active in consulting knew exactly what was necessary. Targeting critical areas such as general health, reproductive health, population growth, the environment, and agricultural development led to the design of courses in medical anthropology, development anthropology, and environmental impact assessment. Today, the University of Yaoundé–I has one of the most

active and dynamic departments of anthropology in Central Africa, attracting students from the entire region. This department played a vital role in the creation of the Pan African Anthropological Association.

## The PAAA

The formation of the PAAA was one of a series of events in the late 1980s and early 1990s that helped integrate anthropology into the discourse of development in Africa. The first such event was the establishment of CASA, the Council on Sociology and Anthropology in Africa, by CODESRIA, the Council for the Development of Economic and Social Research in Africa, in 1987. UNESCO's Regional Bureau for Social Sciences in Dakar, known by its French acronym, BREDA, endorsed the initiative and provided initial resources to establish the association. In 1988 CASA held its first conference in Abidjan, bringing senior sociologists and anthropologists from throughout Africa together for the first time. The government of Ivory Coast, under then president Felix Houphouët Boigny, provided substantial financial support for the consolidation of the association, but CASA failed to market itself to anthropologists and sociologists across the continent.

The second event was a spontaneous meeting of African anthropologists during the Twelfth International Congress of Anthropological and Ethnological Sciences (ICAES) in Zagreb in 1988. The ICAES is the meeting of the International Union of Anthropological and Ethnological Sciences and has been held every five years since 1934. In 1986, Lake Nyos in Cameroon exploded, killing more than eighteen hundred people. I was studying the disaster and was invited by the Center for Environmental Studies at the University of Leiden, Netherlands, to present a paper at the ICAES on how anthropologists, in collaboration with colleagues from other disciplines, approached the study of disaster mitigation.

After my presentation, which was attended by a few African colleagues, I ran into George Hagan and Albert Awedoba, from Ghana, while they were having coffee with Adam Kuper. Kuper was then the newly elected president of the European Association of Social Anthropology, and he encouraged the three of us—Hagan, Awedoba, and me—to establish an African anthropological association. A chance meeting with H. Russell Bernard from the University of Florida, Gainesville, who was attending the same conference, would also contribute to the PAAA's training programs.

The third important event was a workshop organized in 1991 by CODESRIA in Dakar to review the status of anthropology in Africa. It was an attempt to reassert CODESRIA's determination to revive or establish professional associations. This workshop brought together a small group of established anthropologists of different theoretical and ideological persuasions. Most participants were keenly aware of the fast-paced globalization of science that was under way and were convinced of the need for greater collaboration between anthropology and other social sciences. For example, during that meeting I argued that emphasis should be placed on the reorganization of the discipline rather than on the "deconstruction" of ethnography, and Abdalla Bujra argued for a constructive engagement of anthropology in the development enterprise.

Fourth, and most important, was the discipline's increasing engagement in applied work generally. Although anthropologists must continue to produce knowledge as their primary objective, they cannot remain indifferent to the problems local communities face every day. How many anthropologists confront their governments for failing to improve the quality of life of the people? How many produce ethnographies as their PhD theses, obtain their degrees, and promote their careers while remaining indifferent to the plight of people whom they studied? Of what use is anthropology if we do not listen to people and assist them in finding lasting solutions to their daily problems? Anthropology must and can find ways to survive as a useful discipline without sacrificing scholarship.

It was against this background that a group of African anthropologists sought the assistance of the Wenner-Gren Foundation for Anthropological Research to establish the Pan African Anthropological Association. A few of the Africans who attended the 1988 ICAES got together at that congress and formed a steering committee to organize a meeting of African anthropologists. We sent a letter to vice-chancellors of African universities, asking them to identify anthropologists who might attend the conference. Thirty-five participants from twenty-one universities across Africa attended the first conference, held in September 1989. It was organized around the theme "Teaching and Practice of Anthropology in Africa." Approximately 80 percent of the participants had been trained as anthropologists; the remainder were from sociology, education, and philosophy. Some participants described the conference as a unique occasion for anthropologists to emerge from their "academic bunkers" and practice the discipline openly and with a sense of purpose and pride.

Since 1989, the PAAA has organized twelve annual conferences and a series of training workshops for junior anthropologists. It has also worked hard to bring the discipline closer to other social sciences. The future of anthropology depends, we feel, on how well the discipline integrates with the other social sciences. For anthropology to attract funds, it must take on, and bring a unique perspective to, research problems that are common to other social science disciplines.

The establishment of the PAAA was guided by four motivating needs: the need to break professional isolation; the need to improve teaching and training programs; the need to improve research capacity and enhance publication possibilities; and the need to increase opportunities for African anthropologists to participate in the growing market for consultants and for their effective participation in multidisciplinary development teams. All of these desires were captured in the constitution adopted at the end of the first PAAA conference.

While the PAAA has helped revive anthropology on the continent, Africanists in Europe and the United States have also been reorganizing themselves, seeking greater visibility in the world of scholarship in general. In 1991, French scholars established the Association Euro-Africaine pour l'Anthropologie du Changement Social et du Développement (APAD), mobilizing Africanists in Europe to share information on the anthropology of change. In the United States in the early 1990s, Africanist anthropologists began to lobby for the establishment of an Africanist branch of the American Anthropological Association (AAA). Their efforts were rebuffed at first, but eventually the Association for Africanist Anthropology (AfAA) was established within the AAA. Leaders of both initiatives, AfAA and APAD, declared that they would work with African anthropologists to promote and enhance the discipline on the continent.

Unfortunately, after more than a decade, neither AfAA nor APAD has initiated constructive engagement with the PAAA, the only continent-wide professional association of anthropologists in Africa. While the PAAA has made substantial progress, the problems of running an international organization in Africa are daunting. The constituent national bases of professional anthropologists remain weak, because of lack of resources at the local level. The PAAA counts over 550 colleagues among its members, but few can pay their dues regularly because of the low salaries in African universities. In addition, members cannot finance their own way to meetings. Participants in the PAAA's annual conference expect the organizers to pay all expenses, and this is unlikely to change for some time. On the other hand, few of our Africanist

colleagues in anthropology from wealthier parts of the world—including Africans who have migrated to greener pastures—attend the PAAA's annual conference. Indeed, only one American colleague, Maxwell Owusu (University of Michigan), has consistently attended the PAAA conferences since 1996.

## The Constructive Engagement of PAAA

To assert its presence within the African social science community, the PAAA focused on the training of young professionals and on networking activities. It emphasized applied anthropology as the focus of academic work, in order to rehabilitate a discipline that had been discredited in the postcolonial era. Many colleagues of my generation in Africa stood against those in the West who maligned applied anthropology. The West invented anthropology to study the "other," and it defined the canons. But in developing economies, where resources are scarce, science has to be either useful or be gone. Under such conditions—when the so-called other comes to study itself—disdain for applied anthropology perforce dissipates.

As Conrad Kottak (1997: 254) described it, the ivory-tower approach demands that anthropologists "should avoid practical matters and concentrate on research, publication, and teaching." Most African anthropologists, however, follow what Kottak called the "schizoid approach." It demands that anthropologists "should provide information for policy formulation but should not be part of the implementation process," in order to keep personal value judgments separate from scientific work (1997: 254). Advocacy, however, calls for greater engagement of anthropologists in designing policies that promote well-being or that protect people from harmful development schemes. This approach motivates a large number of African students who want to be part of the anthropological enterprise without being castigated for not doing anthropology.

Precisely because the applied option dominates anthropology in Africa, the need to stay current in method and theory remains critical. During the first PAAA conference in 1989, many participants argued that addressing important human issues, such as the need for health care, the spread of famine, rapid population growth, environmental degradation, discrimination and violence against women, poverty, and ethnic violence would enhance the discipline's tarnished image. These problems, which affect the most vulnerable members of our African communities, could not be addressed without appropriate training in

method and theory. Every brand of anthropology—interpretivist and materialist, qualitative and quantitative, applied and basic—must aim for excellence of scholarship. This was the vision for the PAAA's training program. It seeks to increase the skills of our youngest members so that they can compete successfully with colleagues everywhere for grants, publications in prestigious journals, consulting work, and academic jobs.

The association established professional networks for the exchange of information and experiences in dealing with human suffering and problems. Members of these networks organized training workshops for acquiring skills in writing proposals, publishing the results of research, and using software for data analysis. By addressing contemporary problems, the networks became vehicles for the exchange of ideas and experiences. This, in turn, enhanced the teaching and practice of anthropology.[3]

If the discipline were to survive and make itself visible in Africa, the PAAA had to understand the internal logics of other sciences. Its members' participation in team efforts had to be more than just a token, with an anthropologist drafted into a project simply to fulfill a funding condition. Anthropologists had to offer something of intellectual and practical value. The workshops also attracted other social scientists, enhancing and promoting interdisciplinary collaboration. Over the years, African anthropologists have worked closely with environmental biologists, organic chemists, economists, demographers, health care providers, and others. This experience showed that multidisciplinary work was mutually enriching, because each discipline drew on its unique insights to attain a common goal.

Another area of concern was the shortage of good libraries in African universities. Few institutions can even afford new books, let alone expand their library space. All participants in PAAA workshops received basic training in computing, if they had none, or upgrading of their computer skills, if they already had some. It was our conviction that acquiring such skills would empower young scholars to access libraries abroad electronically and keep them abreast of the latest developments in anthropology. Libraries in African universities could concentrate on collecting materials that were unavailable elsewhere—the sorts of materials that scholars everywhere require for research on African cultures and societies.

One of the problems identified during the first conference was the lack of refereed anthropology journals in Africa. In 1992 we established the journal *African Anthropology,* which became *The African Anthropologist* in

1994. It is a forum for African scholars and Africanists around the world to debate, exchange ideas, and contribute to the social science discourse on issues of importance to the continent. Articles on development issues have dominated the journal, now in its tenth volume. Almost all articles submitted for publication focus on practical issues associated with health, agriculture, politics, the environment, ethnicity, and ethnic conflicts. These articles are often the by-products of consulting work, in which African anthropologists are increasingly involved. As part of its efforts to increase the quality of articles, the PAAA includes writing clinics in its training programs.

Although many of the PAAA's activities have been successful, others have not. One of the most glaring failures has been our inability to incorporate the growing number of African anthropologists who work full-time outside of academia. The link between this large group of applied anthropologists and those teaching continues to be weak. And despite their growing numbers, only a few non-academic anthropologists have either joined or have chosen to participate in PAAA programs or publications. Although a similar problem plagues the mainline anthropology associations in Europe and North America, the implications of this nonparticipation are more serious in Africa, given its profound effects on training and employment.

## Africanists and Their Responsibilities

I have attempted to tell the PAAA's story from the perspective of an African who was strongly affected by the resurgence of African anthropology and sociology. My motive for writing this chapter has been to provide some insights into the ways in which a North-South partnership might be fostered and reinforced.

The European and American traditions of anthropology are distinct, and the discipline surely deserves an African twist as well. It is time for the social sciences, including anthropology, across Africa to regroup and face the challenges that confront us as a continent and as part of the human family: disease, hunger, HIV/AIDS, ethnic wars, poverty. We need to look for answers to these scourges. It will be salutary for Africans to bring their particular perspectives to all the social sciences, including anthropology; but in science, as Russell Bernard (2000: 6) said, anything that is true in London or Paris is also true in Nairobi and Dakar. There is a visceral reaction among many intellectuals in the social sciences today against a scientific, or positivist, perspective.

It is particularly strong in anthropology, but African anthropologists, at least in some circles today, are rejecting this anti-science perspective and taking a leadership role in anthropology and development.

Although most Northern academics acknowledge the critical importance of working with colleagues from Africa, this has occurred only on a case-by-case basis, with the Northern anthropologist almost always taking the lead. That is, their African academic colleagues are seen as key contacts for getting research clearance and background knowledge, without which the Northern colleagues could not function. It is rare, however, that the cash resources brought in by Northern academics are shared appropriately with their Southern partners or partner institutions.

Almost every day, one or another of us in Africa is confronted with the myopic bellyaching of some First World anthropologists about the harsh conditions under which they work in their own country—their lack of funding for graduate assistants, their lack of funding for attending international meetings, and so on—with little reference to the conditions under which their African-based colleagues labor. These small, subtle indignities mirror deeper inequities that are only partially mitigated by the fat per diems that one may occasionally earn by attending a five-day conference in northern Europe or the United States. The $600–1,000 that one might save by eating crackers in one's room rather than dining out is cold comfort when one returns to the everyday realities of a $350-per-month salary, five children, and ne'er-do-well relatives who depend on you.

African academics don't want a handout; they want opportunities to work and earn their way. These opportunities exist and can be expanded and strengthened to benefit all the parties involved, including the First World anthropologists who collaborate with them. To bring this about requires small but doable changes in formal academic training programs, grant administration procedures, and grant requirements, to promote better partnership arrangements. These changes will need to be made in both African and Northern universities as well as in the professional associations. Strengthening the ability of Africans to organize and develop their own professional associations is a way to address all these issues at once. Truly professional associations will link Northern and African anthropologists in a single intellectual, publishing, and teaching endeavor on a more equal footing.

# Notes

1.  As a local anthropologist, I was called upon on several occasions to participate in multidisciplinary team research bringing anthropological insights to bear on health issues. For example, in early 2000, an outbreak of cholera in Madagascar killed over a thousand people within a few months. The World Health Organization's regional headquarters in Harare asked me to join a team of medical experts in Madagascar to evaluate the epidemic. The team, comprising two public health specialists, a physician, an epidemiologist, and an anthropologist, spent four weeks visiting the affected areas, talking to health officials, the military, local people, and politicians. At the end of the visit, after examining the ethnographic information and talking to officials about their prevention strategies, I produced a model that took into account the role of culture in the epidemic, and we recommended a drastic review of prevention strategies taking into account cultural inputs.

2.  These two were Pierre Titi and Joseph Mboui. The latter became a full professor after obtaining his *doctorat d'état*. He later became dean, and then adviser to the prime minister. He ended his career as a minister of national education, after serving as permanent secretary. He is now a Member of Parliament, along with two other anthropologists.

3.  As a matter of record, from 1992 to 1999, the PAAA trained 153 mid-career anthropologists in workshops supported by donations from the Carnegie Corporation ($200,000), the Wenner-Gren Foundation for Anthropological Research ($100,000), the World Bank ($15,000), UNFPA ($35,000), and UNESCO ($30,000). These trainees belonged to the following networks: the Network of African Medical Anthropologists (NAMA), the Network of African Population Anthropologists (NAPA), the Network of African Women Anthropologists (NAWA), the Network of African Environmental Anthropologists (NAEA), the Network of African Students in Anthropology (NASA), and ETHNO-NET AFRICA. This last network grew out of a meeting in Nairobi in 1995, sponsored by the UNESCO-MOST program, on the social problems facing the continent. ETHNO-NET was designed as a network of African social scientists who can work together to collect data and who can serve as an advance warning system for ethnic conflicts.

# Part 3

# Epistemological, Sociological, and Disciplinary Predicaments

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# Generating Nontrivial Knowledge in Awkward Situations

## Anthropology in the United Kingdom

### *Eeva Berglund*

Enlivening anthropology's intellectual project and reassessing its political role require understanding the conditions of power within which culture and difference become matters of justice, if not life and death. It also requires understanding the transformations going on across universities everywhere, such as the intensifying efforts by private capital to penetrate—or swallow up—the production of academic knowledge.

When I was contacted about the symposium "World Anthropologies" in the spring of 2002, both these questions troubled me. I had just decided to resign from my job at one of the liveliest anthropology departments in the United Kingdom—Goldsmiths College of the University of London. I resented its relentless productivism, felt hampered by lack of funds, was frustrated by administrative time wasting, and sensed confusion about pedagogical aims and the discipline's public role. And yet my regrets are massive, not least because so much has recently happened that could strengthen British anthropology. Here I reflect on the changing pressures on our discipline, highlighting, nevertheless, the fact that we already have tools for thinking about the new challenges. For as I considered "world anthropologies" from the perspective of the United Kingdom, I immediately thought about scholars' growing interest there in research on modernity at home and about the potentially beneficial effects of ceasing to worry about what "real" anthropology is and where it should be done. The analysis of people like ourselves—tourists,

activists, administrators—will promote a thorough investigation of our own motives and institutional limitations.

Predictably, some people have misgivings about turning toward the modern at home; some see it as a lazy retreat from anthropology's traditional field, the "margins." However, ecumenical styles of questioning and more inclusive views of appropriate research locations suggest that anthropology's oyster is the whole world. Furthermore, inclusive definitions of anthropology's subject matter may challenge hegemonic views of the world as it is invoked in "world music" or "world markets." There is even hope that turning toward the modern at home will help undo some of the Eurocentrisms that still plague us. Notably, where anthropologists are both analysts and case studies, knowledge cannot be predicated on distinctions such as us versus them or complex versus simple.

## Culture, Information and the Changing Role of Academia

Like all other disciplines, anthropology has adapted to a managerialist style of work, with administrative processes taking up considerable energy. As a relatively junior member of the department, I shouldered a manageable administrative load, but if I had progressed up the career ladder, it could only have gotten worse. Staff members I knew in many departments regularly tried to get around or ignore administrative demands. Humor was an acceptable coping strategy. One tongue-in-cheek suggestion was to mark examination scripts or to review manuscripts by reading only the first and last pages, plus one in the middle—surely enough to give one a sense of a text's caliber. Some genuinely labor-saving practices were implemented. For example, we decided to collate data on student absences at certain intervals only, instead of recording them continuously. But even then we remained annoyed by the very demand that we keep such records. Other tactics I have come across include simply leaving allotted tasks undone. Indeed, a relatively convincing argument for pursuing this method is that if left unattended long enough, forms, letters, and other bits of paper tend to lose their importance anyway. At the same time, normally just enough people elsewhere in the institution will participate in the creation of paper trails for the administration to continue to operate.

The lack of trust that the political establishment shows toward higher education in the United Kingdom, like that toward public sector employees more broadly, is not just causing problems of morale but

taking up scarce resources and staff energies. Proverbially, audits like the Research Assessment Exercise (RAE), along with the Teaching Quality Assessment that departments have to undergo in turn, are the curse of practicing academics across the UK. They are designed to increase public scrutiny of universities and to help funding bodies allocate public resources in order to encourage productivity. But the overwhelming sense is that they are exhausting, even crippling, to the intellectual project of the universities. This is so despite the fact that, anecdotally speaking, many anthropology departments were pleased with the new life that the 1992 RAE breathed into the discipline. But as the pressures continued, a new word crept into the vocabulary of staff hiring: RAE-ability, that is, the capacity to display highly competitive academic results. Put even more bluntly, constant professional auditing means that even moderately ambitious academics must invest huge energies in publishing and being seen to publish, and in securing external funding and being seen to secure it.

Though still earning comfortably above-average wages, academics are unhappy that their pay has fallen behind the overall growth in average earnings in the United Kingdom. Economic conditions in higher education as a whole have been cause for concern ever since the Thatcher administration's assault on social research.[1] The Blair government's questionable goal of increasing student numbers to 50 percent of the age group and the lack of new investments and teaching posts, not to mention funding cuts, are the most obvious causes of complaint.

Anthropology is by no means alone in having to transform its curriculum or its working practices under external pressures. Most academics express nostalgia for an apparently simpler, and certainly less pressured, past. But these changes have not happened completely without reflection and certainly not without resistance, as numerous academic (e.g., Shattock 1992) and news articles (e.g., *Economist* 2002) attest.

In some ways, of course, anthropology's crisis is specific. According to Jonathan Spencer (2000), large departments are coping well and even flourishing, but smaller ones are under constant strain as they adapt to changes in student numbers, course content, administrative structures, and both student and research funding at an exhausting pace. Personal experience and anecdotal evidence suggests that even in the higher-profile departments, administrative changes have taken their toll on academic work, not to mention morale.[2]

The implications of these changes are increasingly being debated in professional publications such as *Anthropology Today* (*AT*), the 2003 issues

of which tackled anthropology's future as, respectively, a professional discipline (Sillitoe 2003), an undergraduate subject (Mills 2003), and a player in the public arena of cultural politics (Eriksen 2003; Kurkiala 2003). Such interventions are absolutely vital, even if they engender "feelings of concern and frustration," as one letter to *AT* put it (Hughes-Freeland 2003). My point is that if cultural identity is now a matter of public urgency as well as anthropology's principal subject matter, then the discipline is being asked to adopt a responsible public role in difficult circumstances.

Hesitations also stem from the way in which anthropology's claim to speak authoritatively about its subject matter has for so long been subject to critique and self-criticism. Yet this has to do with geo-political transformations that inevitably touch not only academia but also governments and other sponsors. Since anthropology became professionalized nearly one hundred years ago, the world has been remade and reclassified in ways that problematize the boundaries of its expertise. The discipline has seen disillusionment with science, post–Cold War hesitations, the growth of cultural studies departments, and the blurring of the boundary between sociology and anthropology, among other things.

The rise of culture as a hegemonic preoccupation draws attention to anthropology but also diminishes its claim to unique expertise. With exemplars such as Malinowski and Lévi-Strauss, anthropology is not hard to criticize as little more than a glorified form of travel, and there remain similarities between being an anthropologist and being just a typically modern traveler (see Augé 1999). By the end of the twentieth century everyone, from tourists to niche marketers to leaders of the so-called free world, seemed to have become interested in cultural difference. Individuals are increasingly preoccupied with constructing identities, but as anthropologists, we know that modernity, as a form of social organization, engenders self-consciousness about cultural identity.

In the United Kingdom, identity politics grew up under Thatcherism, so that the explanatory force of culture in social and political debate has since become a commonplace of politics. A *culture* of mismanagement is singled out as the culprit when public services such as hospitals and schools are failing; the *culture* of an ethnic group is an acceptable explanation for underachievement, violence, and poverty. In short, culture is a cause, not something that invites curiosity and examination. Not the least of the implications of this is that it keeps alive the question at the heart of Thatcherism, "Are you one of us?" (Hall 1993: 356). In today's Britain, this question is as ubiquitous as it was fifteen years ago.

The concept of culture has also helped to aestheticize inequality. Multiculturalism gets packaged as a sales item by governments, as in New Labour's late-1990s slogan "Cool Britannia" (Parekh 1998), and by companies such as Benetton (Lury 2000). The intellectual aim of those with the privilege of academic reflection has to be a critical study of such uses of culture. It was around these themes that cultural studies came into its own, with bookshops, if not university campuses (see note 3), classifying much critical scholarship under that category. But the charge is often made—certainly in corridor talk—that sociologically argued debate over culture quickly implodes into an arcane or showy internal argument.

Fortunately, arguing the case for examining the changing conceptualization of culture and its role in sustaining social relations in a globalized world, Stuart Hall and his colleagues at the Birmingham Centre of Contemporary Cultural Studies generated diffuse but influential work in sociology and cultural studies.[3] Its key contribution has undoubtedly been to highlight race and the legacy of imperialism as building blocks of social life at home. The sophistication with which the complexities of race, history, and power have been treated by authors such as Hall and Paul Gilroy, whose associations are with cultural studies and sociology, means that their publications will probably remain compulsory references for anthropologists who investigate contemporary Britain.

The relationship between cultural studies and anthropology has been checkered (Nugent and Shore 1997). Collaboration flourishes at the research level and is trickling down to undergraduate teaching. Nevertheless, many anthropologists are critical of non-anthropologists' definitions of ethnography and uneasy with the perceived mismatch between cultural studies' claim to political radicalism, on one hand, and the closeness of its subject matter to the preoccupations of the political and economic elites, on the other. Indeed, John Hutnyk (2002) has shown that both cultural studies and what he dubs "post-colonial anthropology" (with the scare quotes) can be caricatured as intellectual fashions, co-opted by capital via the global culture industries.

Before culture was problematized publicly as an important political issue in the 1980s, anthropologists could claim a theoretical canon to help identify the truly anthropological. That canon now seems too diffuse to provide grounds for claims to nontrivial knowledge, and in-depth ethnography now tends to be invoked as the source of anthropological authority. This means that anthropology remains an empirical pursuit at the same time it embraces a humanistic and hermeneutic style.

It is, however, in the interlacing of its practitioners' theoretical and empirical concerns that the strengths of anthropology's tool kit are most apparent.[4] Ethnographic method today engenders self-consciousness about moving between locations as much as it ever did, but ethnographers are increasingly preoccupied with what connects those locations, both to each other and to shared contexts of power (Gupta and Ferguson 1997b). This means that as ethnographers pay attention to the realities with which people have to live, they become co-present with their hosts, bringing together different "worlds." Also, ethnographic process, from fieldwork to text or lecture, forces attention onto the rather obvious point that answers depend on which questions, when and where, are being asked. Thus, emphasizing the empirical does not squeeze out the theoretical, leaving us with specificity only; it simply helps focus it. As Johannes Fabian put it at the "World Anthropologies" symposium, theory should not be reduced to "having a position"; rather, it is about being on the move.

Where anthropology interrogates the culture concept reflexively, in all its myriad contexts, it is likely to remain a key theoretical focus. An increasingly prominent anthropological critique of the uses of culture is that they tend to render exploitation and inequality, which are politico-economic issues, into safe and relatively trivial matters (e.g., Anderson and Berglund 2003). A key example is when "difference" is reduced to exoticism (e.g., Hutnyk 2000). In other contexts, however, anthropology demonstrates that "culture" can also be empowering and can be a resource. My point is that anthropologists can approach culture quite differently from situation to situation. Equally importantly, they can approach it differently from the manner of governments and corporate actors, for whom culture is increasingly reduced to its potential for "wealth creation." In that context, culture is difference, but only within safe limits, and only so long as it does not threaten the status quo.

Culture appears problematic, but knowledge and information also pose problems for critical anthropology. As elsewhere, the promises of the information age in the United Kingdom are part of the discourse of "wealth creation," now the neoliberal state's primary concern. Uses of the word "information" tend toward reducing it (to binary code in many instances) and making it possible to commercialize it (or use it to sell something else). A parallel logic is at work in the way education has become a way of certifying "transferable skills," defined by UK agencies as banalities such as "communication skills, written and oral," or "ability to present knowledge or an argument in a way which is comprehensible to others and which is directed at their concerns" (quoted in Goodland 2002: 401).

Regarding both information and skills, universities and research institutions are center stage, apparently promising the keys to knowledge-intensive wealth and prosperity in the future. State and corporate elites now routinely refer to education as the base or ground upon which economic competitiveness can be built, and they increasingly treat it as the machinery with which to construct the environment necessary for supporting global capitalism. The architects of education policy must know that knowledge is and always has been about complexity and variety, but these things get sidelined in the short-term goals of corporation-friendly government. In short, universities' social function has changed: they are being asked to become direct incubators of wealth creation. Academics have voiced their dissatisfaction with this trend, but they have also acquiesced in it. Outcomes are contingent on circumstances in and around specific departments and on the efforts of individual academics and research students.

Where academic research and teaching are expected to pay for themselves, anthropology is unlikely to become a money spinner. It remains too caught up in tensions that arise from a historiography that is firmly anchored in the rise of modern science and Western imperialism yet has a strong identification with the global South. It tends to lend support to the counterhegemonic rather than to the status quo. Since the "cultural turn" brought geographers, sociologists, scholars of literature and the arts, and historians closer together—some of them anyway—the special privilege of anthropology in the realm of culture has been modified. It now needs to respond to the claims of other professionals, particularly those in cultural studies, to expertise in the area of culture. Departments are anxious about the effects of the proliferation of degree programs in media and cultural studies. Anthropologists watch with concern as government rhetoric of efficiency and transparency casts a favorable light on media studies, because students increasingly enter it believing that they can subsequently apply their degree skills in work beyond academia. That there is disagreement over the academic respectability of media studies alters neither the fiscal situation nor the continuing growth of degree programs in media and cultural studies.

Where disciplinary boundaries are debated, the old question about whether something is or is not anthropology is still being asked. Answers are generally, as Spencer indicated (2000: 17), to be found within the relatively intimate space of the departmental seminar, where negotiations are carried out over how anthropological or otherwise a given topic or style of enquiry is. In an unflattering yet discerning review of current anthropology, John Hutnyk (2002) turned his attention to the

reproduction of anthropology. He highlighted the interests of those not yet caught up in the ongoing internal "crisis" and made the strong claim that any reinvention of anthropology that merely rehearsed old anxieties about fieldwork, without addressing the demands made by students and younger researchers—not to mention the political imperative to put anthropology to work—could be treated only as a bid to sustain the critics (ourselves) in business. That means keeping anthropology so "anthropological" that it will forever remain a clique (Hutnyk 2002: 30–31). I would add that the discipline would also benefit from writing in several different registers and pushing anthropology outward from the academy, for intellectual as well as political reasons.[5]

A broadening of the discipline is, indeed, noticeable as research projects and new PhDs turn to ethnography closer to home. This is, I hope, due not only to world-political and financial constraints but also to intellectual reasons. Though not always problematized as such, the new locations of study also mean new ways of (temporarily) positioning ourselves, that is, of theorizing.

## Ethnography and Persuasiveness

Many anthropologists I know in the United Kingdom say they have too little time for the work they consider to be most important. To some extent, they are also nervous about a loss of authority. Contributing to the recent debate, Tim Ingold (2003: 23) noted that anthropologists "have a huge way to go in training both ourselves and our students to speak with conviction and authority on anthropological matters." Crises of representation and fears of misrepresentation notwithstanding, for anthropology and anthropologists to flourish, this issue has to be tackled. The historical legacy of the modern anthropologist studying the nonmodern Other must be put to the best possible use, not carried along as if it were the original sin to be confessed at suitable intervals.

The authority of modern scientific knowledge relies heavily on erasing its own social and interactive foundations. It is not surprising, therefore, that the claims of a discipline based in ethnographic fieldwork to be scientific are easy to challenge. Yet anthropologists need not acquiesce in a definition of important or nontrivial knowledge that limits it to something modeled on an idealized but inaccurate image of modern science.

That anthropology partakes of both science and humanities is a richness to cherish in itself. Another is that anthropologists' commitment to ethnographic methodology means that they grapple harder with the

ethical and political implications of knowledge than is probably the case in most other disciplines. Indeed, the tensions of ethnographic method are already an explicit part of the anthropological enterprise. As I noted earlier, ethnography promotes a kind of practical philosophy. It draws from real people's real preoccupations in real situations; I include in this teaching and conference work, in which we learn about and from each other. Anthropology and anthropologists, however, follow scripts designed for the ever-changing needs of academic institutions, and these are still modeled on modern criteria of scholarship and the value judgments of our more powerful sponsors. No wonder the ideal of a detached scientific producer of knowledge is invoked, and ethnographic othering reappears, just as it did prior to the publication of *Writing Culture* (Clifford and Marcus 1986), however uncomfortable we may be with it.

Increasingly, anthropology is carried out in situations in which othering is not an option—for instance, if our interest is in scientists, managers, or other powerful groups. In these cases we tend to adopt the role of "documenter," offering transcripts—edited ones for sure—of interviews, rather than blatant objectification (e.g., some the chapters in Downey and Dumit 1997). Our questioning makes our "informants" only more self-conscious than they already were, and so they actually behave like ethnographers of their own lives, explaining events against underlying contexts. But if such "anthropological" self-consciousness is often taken as typical of modernity, this does not entail that others (however defined) are not also seeking to make sense of their collective predicaments.

Because of the explicit tensions of fieldwork, the anthropological study of contemporary modernity at home enhances the potential to reinvigorate, not weaken, anthropology. It is not so much a question of the geographical location of the discipline but a question of the directions in which we take our intellectual project. For everywhere that anthropologists are studying modernity ethnographically, whether among intellectuals in Indonesia, youth workers in Paris, museum curators in Germany, or brain surgeons in London, they render modernity into specificity. This questions the assumptions both that modernity is homogeneity and that modernity is Western. It also shows up the limitations of powerful if familiar knowledge practices.

Already in the early 1970s, Laura Nader (1974) advocated what she called "studying up," noting that it would pose important challenges. The most obvious problems are the constraints imposed by those who have power over anthropologists (Shore and Nugent 2002), as well as

the compromises made as ethnographers move into corporations (Green and Wakeford 2001, *Money Programme* 2002). And yet such situations present problems for all fieldworkers, whatever their research location. Ethnographic process in all cases is shot through with power relations as well as ethical dilemmas. The latter arise not least from the fact that key anthropological knowledge emerges out of what is an intervention, even an intrusion, into the lives of people whose own questions may look nothing like those we want to pose.

Although access has always posed a problem, anthropology has long been done "at home." Even more importantly, giving up pretensions to the neutrality and universality of European modernity as a standard of humanity is a project to which anthropology has contributed for a long time. Yet anthropologists have been reluctant to admit their debts to other disciplines, notably cultural and media studies, that have had an important influence on anthropologists who have turned their ethnographic interests homeward. Certainly the locations for the anthropology of modernity overlap with the locations of cultural and media studies: urban space (Green 1997), public spectacles (Harvey 1999), public institutions (Shore 2000), and domestic interiors (Hirsch 1998). In such situations, cultural studies' insistence on the political nature of contemporary culture and the cultural nature of technology has been brought directly into anthropological discourse, and the writings of non-anthropologists such as Stuart Hall (1993) and Donna Haraway (1997) have become references anthropology cannot do without.

These writers' explicitly postcolonialist and feminist sensibilities invite researchers to encounter their subject matter with a sophistication about the complexity of subject positions that more traditional anthropology was able to sidestep. The us-them setup offered to the metropolitan anthropologist working in the margins is simply unavailable. My own work, first in my PhD dissertation on German environmental activists and subsequently on environmentalists and biotechnology researchers in Finland, has regularly drawn me into debates with "informants" about my relationships with them and about cultural identity in general, but also about the knowledge I can produce about them. Initially, such negotiations revolve around ethics, most bluntly in their question, "Whose side are you on?"—often a pressing concern in environmental conflicts as well as in concerns over science. Gradually, context brings nuance to this question, and different commitments and demands are negotiated, leading to some provisional answers at least, but constantly leading to new domains of inquiry. Ethnography helps to make sense of situations, but it does not explain anything to the point of emptying it.

As my understanding of my ethnographic subjects' work deepens, together with their understanding of mine, the debate tends to shift from identity issues (culture) toward epistemology and, in some cases, to accountability for knowledge claims. For example, in the tiny biotech lab where I most recently conducted research, the power of "the" market is a constant presence in discussions. Its specter looms over the future prospects of the lab itself with each new budget and each proposal for research funds. Lab workers have not (yet) asked me how I intend to represent the lab in relation to the market, but they are concerned that I understand the complex economic and power relations within which they, living in a remote part of Finland, find themselves. I, a researcher identified with the capital city and with an academic affiliation in London, have powers they do not, and they negotiate their relationship to me with care. Identity then becomes a question of how our relationship is mediated. Identity also accrues to the knowledge we produce. From their professional perspective, anthropological knowledge seems to be interesting and worthwhile, though its uses are not immediately obvious. There may not be symmetry between the values placed on biochemistry and anthropology, but the encounter produces moments of mutual interrogation as well as recognition and respect.

What I am suggesting is that one of the unique strengths of ethnographic method is—or could be—its dialogue with "informants," whoever the anthropologist is and whoever the people being studied are. The encounter ensures that whether acknowledged or not, anthropological exegesis proceeds from an "ethnographic moment," one of overlap between the field out there (the lives of "informants") and the field over here (academic discourse), and thus is always in debt to forms of native exegesis (Berglund 2001; Strathern 1988, 1999a, 1999b). Despite this, it seems there has been a fudge in the history of Anglo-American anthropology. In the very act of making its object, and thus of legitimating its specificity, anthropology hid from itself the source of some of its more creative impulses: the voices and questions of the people it used to call informants.

It is the resolutely contextual practice of ethnography that provides grounds on which to resist "grand narratives," but it also gives us room to document and analyze without taking sides or moralizing. Parenthetically speaking, since the launching of the so-called war on terrorism, an added urgency has characterized anthropological questioning of how cultural difference is organized. We have tools for narrating and analyzing "terrorism" or the invasion of Iraq that was being prepared even as we discussed these issues at the symposium. In lecture halls,

for instance, we have plenty with which to work within a conventional anthropological tradition, not least the knowledge that in the places where many of us study, inexplicable disasters are common, or the insight that electronic media are extraordinarily powerful political tools.

Despite the fact that anthropologists know that modernity is not confined to, or even really born of, the West, and despite decades of postcolonial theory, even in anthropology dualistic thinking still pits modernity against indigeneity, science against tradition. Yet the authors of ethnographic studies of technoscience, not to mention critiques of the discipline, cogently argue that European-style "great divides" (see Latour 1993) are not necessary to the task of maintaining intellectual order or technical effectiveness. If our intellectual as well as our moral impulse is to treat as our concern all human relationships, and if our method is to engage with people as capable (as humans are) of narrating their own predicaments, then "great divides" are of more interest to us as symbolic constructs than as analytical aids. And ethnographically speaking, we know that the modern West is neither standard nor uninteresting.

The complexity of the relationship between culture and economics in the neocolonial world suggests that one of our tasks is to reassess what it might mean to produce nontrivial knowledge of culture and society. Another is to communicate to non–social scientists that there is nothing mere or trivial about culture or context. True, the nontrivial is still generally equated with the universal, and increasingly with the calculable, a view that carries substantial authority in many places where anthropologists want to have influence.

The analysis of modernity in all its forms, however, gives anthropology an opportunity to demonstrate that universality and objectivity are themselves historically specific values. We can even demonstrate that natural science and other nontrivial knowledge bends according to the needs of politico-economic power (e.g., Martin 1994), but also that there are limits to such excesses. Alongside the apparent fixity of modern knowledge, modernity and the West/North, including anthropology itself, also operate within a more general Western framework, namely, constructionist understandings of knowledge. Not only anthropology but also a whole range of other discourses increasingly account for context/nature/science by ever further construction, as Marilyn Strathern persuasively argued in *After Nature* (1992). This precipitates a generalized crisis, because if nature is treated as culturally constructed, then it seems to leave nothing solid enough to ground an argument. This is an

uncomfortable predicament for everyone. Here I am concerned with its implications for anthropologists who might want to discriminate, if not adjudicate, between health and pathology, human or environmental. Put differently, in a crisis-ridden world (singular), studying "worlds" (e.g., Augé 1999) would seem to be a relativizing indulgence only the privileged can afford.

## Anthropologizing Ourselves

Penny Harvey's ethnography of Seville's 1992 Universal Exhibition (Harvey 1996, 1999) shows how playing with worlds can become a pastime. It also traces the ways in which modern habits of thought have been changing and staying the same:

> [I]n its late twentieth-century guise this universal fair distinguished itself from the fairs of the previous century in the degree to which it exhibited an awareness of itself, of its own history and its own artifice. The exhibitors mused on the nature of modernity and explicitly addressed the central issues in sociological debates about globalisation such as: multiculturalism and the plural nature of society, the links between the global and the local, the temporal and the universal, the ironic play with similarity and difference, the familiar and the strange, the traditional and the modern, uniqueness and wholeness, discontinuity and continuity (1999: 225).

Visitors to the exhibition, with their self-consciousness and their ethnographic sensibilities, were not unlike professional anthropologists. But they were not contemplating similarity and difference as resources for an academic pursuit, operating simultaneously at several levels of analysis, or paying scholarly attention to the contexts of power within which their activities unfolded. These things are all required of good ethnography, a word that refers less and less to fieldwork method or even to a resulting text and more to a methodology (Berglund 2001), a mode of questioning rather than a method for gleaning answers.[6] Ethnography today incorporates personal experience and long-term, in-depth fieldwork, but also discourse analysis, media analysis, surveys of government documents, and, increasingly, dialogue with experts in fields as distant (or as close) as business management, medicine, engineering, science, and art, and of course dialogue with students and peers.

While culture and difference take on new meanings outside the academy, they operate within the academy, too. So let me return to the preoccupations with which I began, those concerning anthropologists' experiences of disciplinary transformations within systems of power. A volume published in 2000 called *Audit Cultures,* edited by Marilyn Strathern, turns its anthropological attention to precisely these issues.[7]

*Audit Cultures* was the product of the conference of the European Association of Social Anthropologists held in Frankfurt in 1998, and so it reflects the concerns of professional anthropologists beyond the United Kingdom. When its contributors wrote about administrative organizations, which writers often render faceless and soulless and juxtapose against the lively cultural features of other arenas of human action, they brought them to life with faces and actions. Their empirical details revealed many similarities in administrative procedures across national and institutional contexts (a point raised frequently during the Wenner-Gren symposium).

At the same time, we learn that transformations in administrative organizations are inflected through different conditions that cannot be reduced to blanket conceptions of modernity as simply global or, indeed, of modernization itself. Without the empirical element that connects the micro with the macro, the collection would be less compelling, and the argument that small changes matter in the long run, less persuasive. We learn that "like institutional clients elsewhere, scholars have helped to reproduce the bureaucracies they fear" (Amit 2000: 230). We also learn that universities everywhere are increasingly cast as producers of literal exchange value, or at least as the sources of fuel to power the industries of the information society. Although I do not read the volume as a whole as being naïve about the long history of the academy's relationship to capitalism (including state-socialism capitalism), its chapters nevertheless identify something specifically new and worth exploring in the way commerce, state, and academy articulate.

The rhetorical force of the contributions is no doubt heightened by their juxtaposing—implicitly if not explicitly—"their" mythico-ritual world with "our" rational-technical one. "They," however, are not just evil bureaucrats (let alone primitives) but people very much like "us." The contributors demonstrate that to anthropologize the modern is to relativize it, to make it impossible to measure difference as deviation from a standard—which is modernity itself. This does not mean, though, that difference and specificity (peculiarity) are erased or aestheticized in the way I suggested has been the fate of culture—quite the opposite.

The audit, for instance, is presented as an important ritual. It is repetitive and often incomprehensible to its congregation, and yet it is transformative and efficacious. Most bluntly, it can turn numerical representations into tangible realities—for instance, into the allocation of resources. An audit, instantiated in the Research Assessment Exercise, for example, also revises people's understandings of what knowledge might be and of who can use it. The rituals of audit and accountability also make and break communities, enabling and constraining individuals in their efforts to occupy certain positions. They tend to homogenize academic projects, now constrained by narrow and utilitarian definitions of what is worthwhile.

The contributors to *Audit Cultures* play with context and perspective, foregrounding specific relationships. They demonstrate once again a methodological point that Strathern has considered elsewhere—that "anthropologists are adept at dividing the world in order to create fresh explanatory contexts for relationships" (Strathern 1995: 166). By doing so we may, as Strathern implies, be intensifying debates internal to anthropology that leave others unmoved. On the other hand, such an exercise does provide a way to challenge the "metropolitan provincialism" (to borrow an apt phrase from the editors of this volume) that claims its own totalizing vision as universal. The exercise also encourages anthropologists to make relationships appear in which they themselves are accountable. That is, ethnography, like it or not, puts us all into networks of accountability as people, wherever the field may be.

*Audit Cultures* resonates with technoscience in Finland and above all with my experiences of British academia, both of which pursuits are preoccupied with productivism but also identity. The volume goes beyond a critique of a generalized modernity; it takes to task the assumptions of virtue built into anthropology itself, as well as the impotence with which academics have generally responded to demands for financial accountability. It invites a self-critical reading because it acknowledges that academics often experience demands for accountability as vitalizing and, at the same time, potentially destructive.

It might be suggestive to compare the approach taken by contributors to *Audit Cultures* with recent work in sociology, another discipline apparently in crisis (e.g., Beck 2000). Sociology increasingly offers a vision of a world that is unmanageable because of its global scale and because it is constituted as an amalgam of individuals or, at best, networks, society having been proclaimed dead by Britain's Margaret Thatcher herself. The universe that social thinkers are asked to deal with is fragmented,

as opposed to consisting of "society" or "societies," and is horizontally and flexibly networked, as opposed to being vertically integrated. This requires a response from the social sciences. One good response is to "do anthropology," insisting on realities that are based in human relations rather than in Eurocentric "grand narratives" and their crises: "the end of history," "society is dead," and so on.

A key insight presented in another of Strathern's books, *After Nature: English Kinship in the Late Twentieth Century* (1992), was that treating context as grounds for knowledge, including knowledge of nature and science, precipitates epistemological and political crises. We know that European modernity has bequeathed an obsession—namely, the requirement that inside and outside (for example, of a society) be clearly separated. In the constructionist view of knowledge, in which context provides explanation, if "the global" (as in global culture) or, indeed, "the world" (world markets) is the object of analysis, then there is no candidate for the context that provides the (back)ground, that is, the explanatory context. After all, what is more global, more encompassing, than the world, or even the global economic environment? Does society really construct nature? Does culture? Does globalization kill anthropology?

The answer to the last three questions is *no,* but the point is that an ethnography-anthropology that can accommodate a focus on the contemporary modern would not pose them in the first place. Such questions could, however, arise out of imperialist anthropology, because "a" (primitive) society could be imagined as a world unto itself, one that ethnography might capture holistically. Modernity, on the other hand, was always too "complex" even to offer the possibility of being grasped holistically. We strive to make it more manageable by treating only a part of it, an aspect of its complexity. Thus, it is not society or culture as such but "ethics, audit, policy" that increasingly are taken to be "the places to be looking these days if one is looking for *society*" (Strathern 2000: 282). Ethics, audit, and policy are domains that we can name and that enable us to trace networks that effect change and mediate power. Questions must be asked about how they do so, and with what consequences. In doing so, ethnographers are inevitably caught up in relationships of accountability that inform what they can ask and how they represent.

One of the principles that guides work on European modernity such as *Audit Cultures* is that it is unnecessary to discriminate between complex and simple social systems (Harvey 1996). Whatever complexity inheres in anthropological work is in the analysis; home and away are as simple

or as complex as one's questioning allows. And so, in ethnographic perspective, anthropologizing is not at all about contextualizing to the point of making reality vanish. It is more about different ways of making power visible. It is also about giving us the confidence, first, that social power can be analyzed and, second, that we do not have to idealize scientific method as a unitary language from which social scientists are unhappily excluded.

Michel Foucault's work continues to inspire such projects, but more specifically I would note the influence of Bruno Latour and Marilyn Strathern. Their contributions to analyzing Euro-American knowledge practices provide good models for anthropologizing modernity. Constructionist arguments regarding knowledge and power in domains where constructionism used not to apply—specifically, technoscience—resonate and so help to provide new understandings across a broad range of situations, from the uses of information technology (Green and Harvey 1999) to environmentalism (Berglund 1998) and the creation of the European Union (Shore 2000). Importantly, such contributions interrogate the criteria by which they themselves might be judged, but the authors do not let that stop them from trying to be persuasive. Truth claims may be culturally specific, but nowhere are they insignificant. Also, in the supposedly modern West, they are certainly undergoing change (see the previously quoted passage from Harvey 1999).

In the contemporary world, seeing is no longer believing. Harvey's ethnography of the Seville Expo demonstrates the continuing impulse in European modernity to treat knowledge as image (1996: 161). This, too, exacerbates the crisis unleashed by "too much" constructionism, because it risks turning knowledge into opinion. Where images are treated as representations of something else, they are always slightly inaccurate and partial. Yet European modernity, Harvey seems to suggest, provokes less worry over representation than before. Events such as the Seville Expo confront visitors and analysts alike with culture as image only. The context for new knowledge is, then, not nature or ground but more culture—for instance, in a high-tech interactive display of Spanish history (Harvey 1996: 151), where people are moved by difference that is visible. And what moves people has to be real, even if it is virtual.

Reflecting on which realities move people and how societies judge knowledge claims, we know that the modern categories of thought that used to legitimate claims to nontrivial knowledge still operate in some institutions—notably those committed to modernization of one kind or other. We also know that in many places the power of dualistic thought is diminished or superseded. Nevertheless, we cannot

ignore the significance of profits in the way triviality and importance are currently judged. And yet the ethnographic enterprise, whether carried out in situations of extreme inequality or not, constantly brings to the fore the impoverishment that such a conceptualization brings about. As Penny Harvey (personal communication) put it, anthropology is geared less toward knowing more than toward knowing otherwise, something that our quantity-obsessed world desperately needs to recognize.

Thus, as anthropology seeks a viable role, it must have the conviction of its own principle that although ethnographic knowledge is irreducible to calculation, it is nontrivial. Ethnographic dialogue means that culture and society, and their meanings, are themselves treated as dynamic, as well as specific to particular social forms and cultural norms. Indeed, anthropologists over the decades have argued that mechanisms for being persuasive, nontrivial, or even real are hardly universal. At the same time, they have discovered that such mechanisms are also a necessary part of being human, as is existing in relations of power.

Experiences in the academy suggest that the more we have tried to be persuasive by adapting to hegemonic economic credentials and their often numeric indicators, the less we have of something we might consider distinctive or productive. Although anthropologists, too, have submitted to an ideology that pretends to value only demonstrable improvement in results rather than passion or dialogue, we know the significance of the university, and our scholarship is not only that which is accessible to administrative scrutiny.

My personal career decisions notwithstanding, I believe anthropological research, wherever its primary focus, provides precious tools for expanding experience and making sense of the chaos around us. I muse on the possible implications for world anthropologies. Need anthropologists cease to be anthropologists once they resign their posts? Could anthropology be strengthened if the policing of disciplinary boundaries within the anthropological community were treated with the same disdain as the policing of ethnic boundaries beyond it? And further, does anthropological knowledge not already refuse the modern conceit that knowledge that cannot be represented and fixed is trivial? If we insist on the nontriviality of such forms of knowledge, we are already operating against hegemonic leveling devices—numbers and, as I have emphasized, culture as difference.

# Notes

1. The grant-giving state body, the earlier Social Science Research Council, established in 1965, was transformed into the ESRC, the Economic and Social Research Council, in 1983. It was so named in consequence of the government's suspicions over the scientific credentials of social research—not science but research. It also highlighted economic research as the key social science. From the inception of the ESRC, funding for research students was curtailed (Spencer 2000: 11).

2. I can speak only from personal experiences and anecdotes, but I have in mind two of the country's outstanding departments, those at Cambridge and the London School of Economics.

3. Birmingham's Department of Cultural Studies and Sociology, the direct institutional outcome of this now famous research, was forced to close down in the summer of 2002, leaving staff as well as students to seek academic homes in other departments.

4. Michael Herzfeld, in his innovative and collaborative book *Anthropology: Theoretical Practice in Culture and Society* (2001), has even called anthropology a practice of theory.

5. Further, what counts as academic work should include much that is not peer-reviewed articles or monographs, an argument recently made by many non-anthropologists involved in fieldwork, activism, or both (e.g., Mountz 2002).

6. We cannot afford, however, to ignore the fact that for those such as the market researchers recently enthralled by it (*Money Programme* 2002), ethnographic research equates with following people around.

7. Strathern discussed these issues in her 1997 article, "'Improving Ratings': Audit in the British University System," as well.

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# The Production of Other Knowledges and Its Tensions

## From Andeanist Anthropology to *Interculturalidad?*

## *Marisol de la Cadena*

In a recent volume, the anthropologist-politician Carlos Iván Degregori (2000) described anthropologists in Peru, his country and mine, as having developed an inward-looking analytical viewpoint that lacked comparative perspective. This situation, he explained, contrasted with that in the Northern Hemisphere, where access to bibliographic and funding resources provided scholars with a broader view that, nonetheless, featured an inward-looking tradition of its own. Although resources allowed them to compare and contrast anthropological knowledge about Andean countries, they generally did so with information published in English, mostly by US scholars. As an example of this parochialism (which, however, is not generally considered such, given the authority of North America as an academic center), he mentioned an article by a US colleague devoted to an assessment of Andean anthropology in which "out of sixty-two titles mentioned in the bibliography, only two are by Peruvian scholars, and one of them is in English, and written by a Peruvian woman teaching in the US." Yet suggesting the complex geopolitics of knowledge-power relations, Degregori admitted that his own review of Peruvian anthropology excluded, or at the very least subordinated, knowledge produced in provincial universities (Degregori 2000: 17–18).

The hegemony of Euro-American academic anthropology emerges from apparently innocuous disciplinary interactions. As Degregori's self-criticism alerts us, even critical dispositions may prove insufficient

to shelter us from this hegemony; we need, at the very least, to disrupt the silence in which it thrives. Universal in appearance, Western forms of knowledge and their practices are not confined to Europe or the United States—they have exceeded those territories for almost six centuries now. Articulated by a vocation to spread reason, the modern geopolitics of knowledge both established a center (the North Atlantic) and surpassed it, thus constituting regional academic (and intellectual) formations with their own centers, where the institutions of reason accrued, and peripheries, where rational logic had a weaker established presence. These regional formations comprise a complex configuration of multiple, hierarchically organized centers, some of which are "peripheral" in relation to other "more central" ones.

Running through this configuration, layered and many-directional relations of domination and subordination contribute to shaping what eventually is considered universal *knowledge* and what remains local *information*—both worldwide and in specific countries. Indeed, this "universal" and this "local" are also relative within the configuration; how far local knowledge makes it depends—we believe hegemonically—on its "theoretical strength," and this is problematical if by that we mean a knowledge practice that extracts general notions out of local meanings and, in the process, denies their singularity.

To illustrate the hegemony of Euro-American forms of knowledge, and specifically the process through which it is achieved, I attempt in this chapter a genealogical and dialogical discussion of that aspect of Latin American anthropology known as Andeanism. I follow Andeanism as it connected with academic formations in the United States, as well as with political-intellectual discussions within Latin America and Peru—specifically with debates about *mestizaje* and *interculturalidad*.[1] I begin my story early in the twentieth century, when anthropology had not yet coalesced as a discipline. Nevertheless, discussions about "culture" fueled nationalist projects promoted by a regional network of intellectuals who, under the rubric of mestizaje, eventually contributed to the emergence and articulation of Latin America as a geopolitical region of sorts. Significantly, the discussions were also marked by what the sociologist Aníbal Quijano (1993) labeled "the coloniality of power," a historical geopolitical condition that delegitimated non-Western forms of making sense of the world, temporalized them as premodern, and thus set them up for what Johannes Fabian (1983) called noncoeval representations.[2]

Later in the chapter, I describe the emergence of another network, that of indigenous intellectuals. With an identity that would have been oxymoronic at the turn of the twentieth century, when Indians were

unthinkable as rational beings, let alone intellectuals, this network, acting nationally and internationally, rebukes the homogenizing narrative of mestizaje and proposes instead *interculturalidad,* a social relation able to produce a political community that indigenous intellectuals imagine through ethnic-cultural (even ontological) diversity.

Before I describe this network, however, in the second section of the chapter I interrupt what could otherwise have been a sequence—that is, from mestizaje to anti-mestizaje and from traditional to grassroots intellectuals and politicians. In this section I use the life and works of the Peruvian literary writer and anthropologist José María Arguedas to illustrate how Peruvian social scientists contributed to the hegemony of universal knowledge in a peripheral center (Lima) by disqualifying Arguedas's attempts (visionary in the 1960s) to redirect mestizaje into interculturalidad and to promote the diversity that indigenous intellectuals currently champion. Arguedas was a controversial Peruvian intellectual, and his life and works were situated at several highly unusual crossroads. He was both a non-indigenous intellectual and an indigenous Quechua, an ethnographer and a literary writer whose work resists a binary classification as either fiction or ethnography. While this may be commonsensical to postcolonial sensibilities, in the modernizing 1960s Arguedas's life and work defied the limits of certified sociological-anthropological knowledge and the political projects this knowledge sustained.

Arguedas identified himself as "a civilized man who has not stopped being at the core an indigenous Peruvian" (Dorfman 1970: 45). This idea, also personified in the characters of his stories, challenged the nationalist teleology of mestizaje, the idea that Indians would be included in the Peruvian nation as mestizos only once they completed requirements for civilization. Arguedas's self-identification, as well as his work, strove against the "coloniality of power," which supported images of indigenous Andeans as "inferior," and the ideological historicism that legitimated this perception. By historicism I mean the conceptualization of historical time as a measure of the "cultural" distance b-etween coexisting Western and non-Western formations (see Chakrabarty 2000). Intriguingly, and toward the construction of "world anthropologies," Arguedas's work disrupted the silent hegemony of Western forms of knowledge.

## The Inter-American Hub of Peruvian Anthropology

Andeanism, as a set of academic ideas and fieldwork practices, emerged in dialogue with anthropology in the United States and, in an apparent

paradox, with Latin American debates about mestizaje. An important actor in both networks was John Victor Murra, a Romanian by birth who, in the 1970s, while teaching anthropology at Cornell University, was one of Arguedas's most intense interlocutors. Yet Murra's participation in the US–Latin American network predated this friendship. I have traced it back to 1952, when Murra went to Jamaica as a PhD student hired by Sidney Mintz, an anthropologist from the United States who was then working in Puerto Rico under the sponsorship of Julian Steward. From Jamaica, Murra went to Cuba, where he met Fernando Ortiz, the author of *Cuban Counterpoint: Tobacco and Sugar* (1995 [1947]), perhaps the earliest historical ethnography produced by a Latin American intellectual, the first edition of which had a prologue by Bronislaw Malinowski. Ortiz coined the term *transculturación,* with which he rebuked the notion of "acculturation" and joined the discussion of mestizaje, if perhaps only implicitly.

From Cuba, Murra took a boat to Yucatán and then a plane to Mexico City, where he met Ángel Palerm, a Spanish anthropologist who had fled Francisco Franco's fascism (Castro, Aldunate, and Hidalgo 2000: 43). The group later included the Mexican Gonzalo Aguirre Beltrán, a crucial interlocutor in the mestizaje dialogue who had studied anthropology at Northwestern University with Melville Herskovitz and was, like Ortiz, interested in *Africanía*. This dense network of friendships, collegiality, chance, and political passions, connecting at least the United States, Cuba, Mexico, and Spain, underscores the complexity of anthropological conceptual itineraries across the Americas at the time and belies simple unidirectional flows of knowledge from North to South. It also suggests a regional Latin American intellectual formation that crossed national boundaries and connected genealogically with an earlier network, one that existed before the creation of anthropology in Latin America.

Articulated by a regionalist-cum-nationalist political drive, this network had, since the late nineteenth century, grouped intellectuals around the idea of Indo-America, a subcontinental community that intellectuals imagined emerging from their common pre-Columbian and Hispanic cultural pasts.[3] Witnessing, participating in, or opposing political events such as the Mexican Revolution and the increasingly expansionist ventures of the United States in Latin America, particularly the Marine invasion of Nicaragua in the 1920s, the leaders of Indo-America knew of each other, and some even worked together.[4] Generally, Indo-Americanists (commonly known as Indigenistas) were provincial intellectuals (mostly lawyers) familiar with their surroundings: archaeological remains, folklore, colonial writings, vernacular languages, and indigenous ways of living.

As anthropology consolidated in the United States, Indigenistas traveled north, both to share their local knowledge with their US counterparts and to have it academically certified. Julio C. Tello, an archaeologist from Peru, acquired an honorary degree at Harvard in the early 1920s, and the Mexican Manuel Gamio obtained his degree at Columbia, where he was one of Franz Boas's students. Luis E. Valcárcel, head of the Museum of History in Lima (created in 1930), toured several universities in the United States, where he was "impressed with the Boasian, Smithsonian, and Harvard institutions" (Salomon 1985: 89; Valcárcel 1981). The US academy, however, did not exhaust Indigenistas' intellectual interests, for Indo-Americanism was also a political doctrine— and anti-imperialist at that. Mexico was an important ideological hub in the network, the site of a successful revolution and a source of ideas about mestizaje.

Mestizaje was a demographic policy, a population-making tool, that promised to uplift the indigenous groups by shaking off their backwardness. It represented Latin America's potential as a future equal of its northern neighbors while accepting the inferiority of the region in its current stage of evolution. Navigating the political-academic network that connected the two Americas, Latin American nationalist discussants of mestizaje encountered the concept of "acculturation"; indeed, they might even have influenced it, as Ralph Beals (1953) seems to have suggested.[5] Resuming Paul Radin's (1913) discussions of the influence of whites on indigenous cultures in the United States, in 1936 the American Anthropological Association included "acculturation studies" as a legitimate field for anthropological work and defined it as "the investigation of the cultures of natives that participate in civilized life."[6] A year earlier, the Social Science Research Council (SSRC) had established a subcommittee to promote "acculturation studies" (Beals 1953; Patterson 2001; Sartori 1998), and the American Council of Learned Societies (ACLS) created a Committee on Latin American Studies that years later became an ACLS-SSRC joint committee. These associations were to coordinate research and resources with policy needs of the US government as indicated by the Office of Inter-American Affairs, the coordinator of which was Nelson Rockefeller. With funds from this institution, the North American John Collier joined Mexican anthropologists in founding, in the 1940s, the Instituto Indigenista Interamericano. Its mission was "to carry out research on 'Indian problems' in countries in the Western Hemisphere" (Patterson 2001: 95). Through these and other connections, "acculturation" entered the Indo-Americanist network, where it met both adherents and opponents.[7]

Starting in this period, funds for collaborative research (particularly from the United States) became a crucial component of Latin American(ist) anthropology and its politics.[8] The *Handbook of South American Indians* (1947–59) is an icon of such collaboration. Produced under the auspices of the Office of Inter-American Affairs and the leadership of the archaeologist Wendell Bennet and the material-ecologist Julian Steward, the *Handbook* resulted from a collaboration between Southerners and Northerners that must have been fraught with the tensions of academic hierarchies. "The North American creators of the *Handbook* and the French ethnologists of the Instituto Francés de Estudios Andinos took as apprentices a large number of Peruvian students," wrote Frank Salomon (1985: 90). Yet the "Peruvian students" were prominent Indigenistas, salient participants in the regional mestizaje network and influential "local" intellectual-politicians and lawmakers in Peru. Their "apprenticeship" was specific to the discipline of anthropology then emerging from the Indo-American network—a group that was politically influential in the South yet academically subordinated to North Atlantic centers of knowledge, particularly to the United States and (to a lesser degree) France.

Concerned with the creation of Peru as a modern nation, enmeshed in official politics, and boasting an Inca heritage, Peruvian anthropologists chose past and present Andean "indigenous cultures" as their object of study and political representation. The first anthropological institutions to be established in Peru, in the 1940s and under state sponsorship, were museums, the Instituto de Etnología y Arqueología, and the Peruvian chapter of the Instituto Indigenista Interamericano, which had its main office in Mexico City. Over the next fifteen years, anthropology became an established discipline in Peru, and as the epicenter of a "culture area" of its own, Peru became the center of US Andean anthropology, rivaling Mexico and shadowing the development of Andeanism in neighboring Bolivia, Ecuador, Chile, Argentina, and Colombia. In contrast to the situation in Mexico, the Peruvian state's economic support for anthropology weakened by the 1960s, and the discipline came to depend almost entirely on public and private funds from the United States and Europe. As in the rest of the world, the historical linear narrative proposed by modernization theory—in both its rightist and leftist versions—weighed heavily on Peru during this period.

In Peru, the prevalence of modernization paradigms meant reinforcing the teleology of mestizaje. The earlier Indigenista culture-history nationalist rhetoric, however, was replaced by an economistic discourse colored by the polarized political ideologies then prevalent. Conservative

proposals envisioned Indians becoming "farmers" or normalized as urban mestizos. From the other end of the spectrum, revolutionary projects required "peasants" or "wage earners" rather than superstitious Indians immersed in subsistence economies. Proponents of "dependency theory" shared this view. "Dependentistas," as they were known, represented a left-inclined conceptual alternative to modernization theories that emerged in Latin America and held that the region's lack of industrial development was a result of historical colonial relations of domination and contemporary capitalist economic exploitation. From this viewpoint, the influential Peruvian sociologist Aníbal Quijano proposed in the 1960s that mestizaje be conceptualized as *cholificación*—the transformation of the rural *indio* into the urban *cholo*.

Thoroughly interdisciplinary and immersed in politics, anthropology thrived in Peru in the 1960s as discursive fields such as "peasants" and "the countryside" proliferated in intellectual discussions in connection with relatively successful rural social mobilizations. Accordingly, social scientists evaluated and accepted or rejected foreign theoretical influences using value scales stemming from their ongoing political projects. For example, anthropologists working with the state welcomed "applied anthropology"; adherents of dependency theory followed the work of Eric Wolf and Maurice Godelier; and Clifford Geertz and Claude Lévi-Strauss had little impact. "Culture" became the concern of a few marginal anthropologists (whom modernizing Marxists usually considered conservatives) under the leadership of John Murra. In dialogue with José María Arguedas, Murra popularized the term *lo Andino,* a notion swiftly adopted by members of the Peruvian Indigenista network. In years to come, this notion would spur an interesting controversy in the United States. It was stimulated by the criticism of Orin Starn, a US anthropologist who accused Andeanists of political blindness because they had "missed the revolution" that the Shining Path activists organized even in the villages where some of the Andeanists worked (Starn 1991).

Although the discussion surrounding US Andeanism was not prominent in Peru, controversy over Arguedas's work has long survived his death in 1968. He was identified as the instigator of *lo Andino*—a notion that many leading intellectuals interpreted as "culturalist." His anthropological production had little influence and is currently ignored. His literary work, however, continues to be contentious among social scientists and politicians alike.

### *All the Bloods:* Arguedas as an Unthinkable Epistemological Revolution

The controversy over Arguedas's work came to fruition around his 1964 novel *Todas las sangres* (All the bloods). In the late 1960s, at the Instituto de Estudios Peruanos, a renowned think tank in Lima, a group of prominent social scientists and literary critics gathered around a table and discussed the novel for many hours.[9] After a bitter discussion—which was taped, transcribed, and published as a booklet in the 1980s—they arrived at the conclusion that the novel proposed an unfeasible political project, one that could even be harmful to the country. The roundtable meeting has become legendary in Peruvian academic mythology: it represents a foundational moment of *lo Andino* and of its scientific rejection.

The publication of the novel coincided with a period of intense conflict in Peru between large landowners and indigenous agriculturalists, known as "peasants" or "Indians." Inspired by a combination of orthodox Marxism, dependency theory, and indigenous politics, members of the peasant movement were successfully seizing hacienda land.[10] *Todas las Sangres,* although sympathetic to the rural struggle, contradicted the leftist intellectual-politicians' script. The script, common to Marxist insurgencies in Latin America in the 1960s, said that the teachings of political activists (the revolutionary vanguard)—as well as activism itself—would transform Indians into peasants. Illuminated by "class consciousness," these peasants would leave superstition and Indianness behind and become *compañeros,* modern partners in history. In *Todas las sangres,* Arguedas disputed this transformation and thereby touched a highly sensitive political nerve among progressive intellectuals. Even more significantly (and unacceptably), his novel posed an epistemological challenge to the hegemony of the singular modern subject proposed by leftist and conservative projects alike.

Set in the Andean highlands, the novel describes a bitter dispute between two brothers, don Fermín and don Bruno Aragón de Peralta, supreme lords of an Andean region. Fermín incarnates capitalism, progress, and reason and wants to modernize Peru. His regional project is to develop a mine. Bruno, instead, is a traditional *hacendado;* in Arguedas's words, "he considers modernization to be a danger to the sanctity of the spirit" (1996 [1965]: 15).

Flanked by the two brothers stands Demetrio Rendón Willka, a supervisor of the Indian workers in the novel and the core of the controversy at the roundtable. An Indian recently returned from several

years in Lima, this character should have been purged of his superstitious beliefs—following the dominant mestizaje-acculturation script—and become an ex-Indian, an urbanized cholo, scornful of things indigenous. Yet Willka belies the script. Formal education and urbanization have not transformed him (as proposed by nationalist projects and state policies); he alternates between urban and rural Indian garb with ease and self-identifies as "a *literate* comunero; yet *always* a comunero" (Arguedas 1964: 33). Willka's urban experience has taught him about the power of modern technology, but he also acknowledges the might of the sun. Rather than the typical hybrid on his way toward modernity, Willka personifies an oxymoronic hybridity that refuses consistency and is able to think and act in both modern and nonmodern terms—much the way Arguedas himself revealed he did.

By the end of the novel, Willka's inconsistency crosses the tolerable threshold as he enters the political sphere to organize an unprecedented group of indigenous leaders who, like himself, recognize the power of mountains and rivers. Together they lead a successful insurrection, moved by magic and reason alike. It is reminiscent of the 1855 Santal rebellion in India as Ranajit Guha (1983b) has represented it. Ultimately, *Todas las sangres* proposes an alternative indigenous social movement, a critical ally of the modern left but with an amodern hybrid logic of its own. Literacy and modern politics are important, but they have to be selectively used and translated, rather than eradicating indigenous ways. For example:

> In jail one learns a lot. There is a school there. You have to listen to the politicians [political prisoners]. The world is very big. But you do not have to follow what the politicians say. We have to learn what they teach according to our understanding—*nuestra conciencia.* They are different. Nobody knows us. You will see!! They are going to take you to prison… You already know how to sign. In jail you will learn to read. Let them take you to Lima! (1964: 307)

In his analysis of the Haitian revolution, the historian Michel Rolph Trouillot explained that at the time of the event the idea of black slaves fighting for the independence of Haiti was unthinkable: the idea of black people (let alone slaves) defying power—and on their own terms—exceeded historically defined conceptual and political categories (Trouillot 1995). Similarly, the minds of central Limeño intellectuals—many of them earnest socialists and prominent proponents of dependency theory—in the 1960s held no conceptual or political

place for Rendón Willka. Aníbal Quijano's eloquence in this respect has become legendary in Peruvian social science circles. About Rendón Willka he wrote that

> this character is extremely equivocal. I had the impression that he returned from Lima totally *cholificado,* and that he was going to proceed, in a supremely astute and Machiavellian way, to assume political leadership in the process of peasant insurrection, and therefore he appeared a little in disguise among his own. But the next impression, particularly at the end of the novel, suggests that Rendón reintegrates—not totally, not in a fully conscious way, but in some sense he reintegrates—back into the indigenous traditional [world]. (Quijano in Rochabrún 2000: 59)

The indigenous world and its animated landscape were not the secular arenas that modern political organization required. In an apparent paradox, then, the analysis that Quijano (and many other socialists) proposed worked as a "prose of counterinsurgency" (see Guha 1988), for even as rural upheavals took place under the leadership of indigenous politicians (probably much like Rendón Willka), they were deemed not to be *indigenous political* movements; they were only a subordinate aspect of the revolutionary struggle led by urban politicians. Had not Eric Hobsbawm (1959), whose analytical sample included Peruvian rural movements, defined peasants as pre-political actors? The notion of "change" encompassed by modernizing premises (including those of dependency theory and class analysis) was specific: it moved forward from past to future, from "superstition" to "historical consciousness." Untamed by this narrative, Willka represented the "Indianization of politics," a historical impossibility for sociologists, who imagined a different kind of leader:

> I am currently working in research on peasant leadership, and last year I traveled to several areas affected by the peasant movement. In all the peasant unions I have visited, I have found only one indigenous leader. *Indigenous leadership does not exist today* within the peasant movement; it appears as an exception and in isolated fashion; the Indian leader is himself going through a process of *cholificación*. Thus, I do not think that an indigenous solution to the peasant problem would be feasible. (Quijano in Rochabrún 2000: 59–60)

These words—again, Aníbal Quijano's—were the last ones transcribed from the recording of the bitter roundtable session. Albeit simplified—given the tension of the session—they refer to a more complex argument

published in the same year as *Todas las sangres* and soon to become famous as *cholificación.* It described the transformation of Indians into cholos, their de-Indianization, and their incomplete integration into Western ways of being and knowing.[11] Notwithstanding, cholos represented a hopeful national future. They indicated, according to Quijano, "the emergence of an incipient mestizo culture, the embryo of the future Peruvian nation if the tendency continues" (quoted in Rochabrún, 2000: 103; my italics).[12]

Even a cursory contextualization of the debate makes it clear that Quijano's position was not unique, although he might have been Arguedas's most articulate and vocal opponent at the roundtable. They were friends and intense mutual interlocutors, so the discussion was embedded in previous unresolved conversations, the details of which I am unaware of.[13] This does not cancel, however, the conspicuously historicist lexicon Quijano used to define cholos—notice the future-oriented words in the last phrase quoted: *emergence, incipient, embryo, the future, the tendency*—a lexicon that prevailed over the academic and political logic of the period.[14] It saturated the imagination to the point of seducing brilliant intellectuals to irrational historical oblivion: they disregarded that cholos (albeit with different labels) had existed (historically "in between" rather than "moving forward") for almost five hundred years, from the Spanish invasion of the Andes to the 1960s. From the historicist perspective, Demetrio Rendón Willka was not only a contradiction, he was impossible. He emerged from the genealogy of mestizaje only to belie its teleology as his character proposed that indigenous ways of being, rather than assuming the forward-moving history of modernity or simply "persisting," had a historicity of their own, the undeniable power of industrial capitalism notwithstanding.

More significantly, Willka's political leadership implied the inclusion of indigenous forms of knowledge in nationwide projects and thus challenged the knowledge-power premise of socialism, which, as secular communalism, required the "cooperation of rational beings emancipated from gods and magic."[15] Socialist liberating politics required the supremacy of reason, and *Todas las sangres,* perhaps prematurely, opposed this fundamentalism. Arguedas explained that "socialist theory gave a course to my whole future, to all my energy; it gave me a destiny and charged it with might by the direction it gave it. How much did I understand socialism? I do not really know. *But it did not kill the magic in me* [Pero no mató en mí lo mágico]" (1971: 283).

Arguedas's effort, I believe, paralleled, from some thirty years earlier, Dipesh Chakrabarty's project of "provincializing Europe" (Chakrabarty

2000). Suggesting that European thought was indispensable yet inadequate for exploring questions of political modernity in the Third World, Chakrabarty examined the possibilities of renewing and transforming currently hegemonic forms of knowing from the margins of modernity. Similarly, in his literary work and testimonials, Arguedas proposed an alternative politics of knowledge, one that saw the necessity of Western reason and its simultaneous incapacity to translate, let alone capture or replace, Andean ways of being. Rather than reading Arguedas as proposing a multiculturalism tolerant of "all bloods"[16]—as his politics has been interpreted (Karp 2003)—I want to read him as proposing multi-ontologism and a nationalism capable of being both general and singular, articulated by both reason and magic on equal standing, and socialist at that.[17] Beyond prevalent economistic explanations, he exposed the way capitalism derived its power from the will of proponents of modern epistemologies to replace non-Western ontologies with modern forms of consciousness. Thus he unveiled what Quijano (perhaps moved by this encounter, yet almost thirty years after it happened) theorized as "the coloniality of power," a notion that undermines the teleological logic needed to sustain *cholificación* (see Quijano 1993*)*. In the late 1960s, however, with only one exception (a linguist named Alberto Escobar), all participants in the *mesa redonda* derided Arguedas's project.

The author of *Todas las sangres* was as complex as the characters he created—he was like Rendón Willka, he disclosed to one of his colleagues[18]—and as "unthinkable" (in Trouillot's terms) for his intellectual interlocutors of the sixties and seventies. The son of a provincial lawyer, and prey of a wicked stepmother, Arguedas was raised by indigenous men and women (Arguedas 1996 [1965]). In 1969 he told Ariel Dorfman: "For someone who first learned how to speak in Quechua [as he had], there is nothing that is not a part of the self." And this ontology equipped him with a way of knowing, he continued in the same interview:

> I was purely Quechua until my adolescence. I will probably never be able to let go of ... my initial conceptualizations of the world. For a monolingual Quechua speaker, the world is alive; there is not much difference between a mountain, an insect, a huge stone, and a human being. There are, therefore, no boundaries between the "marvelous" and the "real." ... there is neither much difference between the religious, the magical, and the objective worlds. A mountain is god, a river is god, and centipedes have supernatural virtues. (Dorfman 1970: 45)

Rebuking the directionality of mestizaje on a different occasion, he declared: "I am not acculturated," and he reiterated his pleasure at

being indigenous and non-indigenous simultaneously: "I am a Peruvian who proudly, like a joyous devil, speaks in Christian and in Indian, in Spanish and in Quechua" (Arguedas 1971: 282). This speech has become famous among Latin American(ist) literary critics, who usually see in it a confession of the author's dramatically singular life trajectory, even an explanation of his death by suicide, evidence of the impossibility of his way of being.

Contained within literature—up until the debate about *Todas las sangres* at least—the writer's ideas were considered "magical realism," the literary genre in which the uncanny ceases to be such and becomes ordinary.[19] And in Arguedas's life the uncanny was ordinary—not quite an object of study, but part of his subjectivity. "I know Peru through life," he used to say (1996 [1965]: 50). With life as a source of knowledge and literature as his expressive genre, he blurred the distinction between reality and fiction. He described the stories he heard and used as inspiration as "absolutely true and absolutely imagined. Flesh and bones and pure illusion" (1971: 22). I suspect that canonical practitioners of the social sciences had a hard time with Arguedas's assertions. Even anthropologists would have disagreed: the animated landscape and "magical" insects belonged to the realm of indigenous beliefs, and as such they were distant objects of study, and vanishing at that. The discipline was politically at odds with Arguedas's views. He wrote in a letter to John Murra on 3 November 1967:

> Development projects to integrate the indigenous population have become instruments that aim to categorically uproot Indians from their own traditions … famous anthropologists … preach with scientific terminology about … the inexistence of a Quechua culture; they say that Peru is not bicultural and that indigenous communities have a subculture that will be difficult to uplift to the level of national culture. (Murra and López Baralt 1996: 162).

Amid the modernizing will and the rigid political-economic positions that colored the controversial roundtable and continued to characterize academic thought in the following decades, scholars' concerns for Andean singularities gradually became labeled *lo Andino,* confined (many times scornfully) to anthropology and ethnohistory, the sciences of the past; sociologists and economists—scholars of "social change"—devoted themselves to the study of the present. As notions of *lo Andino* circulated in the United States and became Andeanism, Arguedas's political suggestion for an alternative form of knowing—which he phrased as a demand for "magic" to be considered on a par with reason

and for "informants" to become subjects of knowledge—disappeared. Through a combination of French structuralism, British functionalism, and US Andean ethnohistory, indigenous knowledge eventually became "Andean thought," the object of theoretical explanations that translated the singularities of Andean ways of being into the universal languages of "structures" and "systems." The label described a type of anthropology interested in the cultural specificities of the region, the genealogy of which connects with A. L. Kroeber's notion of "culture areas" and Indigenista political views. Controversial since its inception, *lo Andino* also connected with the preexisting inter-American mestizaje network, inasmuch as it endorsed Indo-America as a peculiar cultural-political entity (Rama 1982). Additionally, it promoted a specifically regional formation that interlocked anthropologies from Ecuador, Colombia, Bolivia, and northern Chile and Argentina.

## Indigenous Politics and the End of Mestizaje: *Interculturalidad*, or Knowledge as Dialogic Relationship

I have been told that the roundtable discussion of *Todas las sangres* had no immediate repercussions; the tapes were lost and not unearthed until several years later, during a cleaning spree at the Instituto de Estudios Peruanos.[20] Yet the tense and at times heated dispute was not an ephemeral and isolated incident. Once the transcription was published as a pamphlet (which has had several editions), the event became a topic of conversation in Peruvian and international academic circles.

The controversy featured a double, intertwined disagreement.[21] Epistemologically, the discussion expressed the tension between a widespread analytical tradition that "tends to evacuate the local by assimilating it to some abstract universal; and a hermeneutic tradition that finds thought intimately tied to places and to particular forms of life" (Chakrabarty 2000: 18). Politically, the discussions in the *mesa redonda* were a prelude to the intense disputes that pitted "campesinista" (or "clasista") political leaders against their "indianista" counterparts, which took place all over Latin America in the last decades of the twentieth century (Hale 1994; Yashar 1998). These were part of a process that some have labeled "the return of the Indian" (Albó 1991; Ramón 1993; Wearne 1996), a reference to the increasing political significance of social movements that articulated their demands around indigenous rights and ethnic claims—and that in one way or another challenged simplistic, universalizing analytical viewpoints.

Emerging in the early 1970s, organizations such as the Consejo Regional Indígena del Cauca (CRIC) in Colombia, ECUARUNARI in Ecuador, AIDESEP in Peru, and the Movimiento Revolucionario Tupac Katari in Bolivia surged into the political pictures of their countries, demanding and enacting indigenous citizenship. Since their inception the movements have proposed projects that defy the teleology of mestizaje. Accordingly, by the 1980s (albeit, like any other political organization, pervaded by internal ideological conflicts) they managed to install a new nationalist (yet highly heteroglossic) vocabulary. Words such as *pluriethnic, pluricultural,* and *plurinational* reflected their demands for respect of their ethnic singularities. More significantly, the new terminology, in its very heteroglossia, challenged the homogeneity that sustained nationalist ideals and the state formation that implemented them. Indigenous political organizations gradually acquired prominence and jumped to center stage in the 1990s, coinciding with the quincentenary of Columbus's landfall in the Americas. Perhaps the most unexpected and spectacular event in this respect was the Ecuadorian *levantamiento indígena* (indigenous uprising), which shook the country and saw the occupation of the capital, Quito, in June 1990. According to the Ecuadorian historian Galo Ramón (1993: 2), the *levantamiento* "removed the dam that the dominant project for a national state had created since 1830."

Predictably (although surprisingly and still inadmissibly to some), the political mobilization—the return of the Indian—also meant an "uprising of knowledges" (see Foucault 1980: 81–87), the insurrection of ways of knowing defined by science as local, disqualified, and illegitimate. Reminiscent of Arguedas's character Rendón Willka, the original leaders of the movement were indigenous persons who combined rural and urban experience, as did the movement as it deftly appropriated modern practices and transformed their logic. Illustrative of this, and from the very beginning, the movement's political demonstrations boasted Andean ritual iconography and enactments, thus desecularizing politics, as in Arguedas's novel. Intended as "acts of memory" (see Bal 1999), the desecularized political rituals also defied official nationalist histories, introducing into the political pantheon the presence and ideas of indigenous activists.

In Bolivia, for example, as the memory of Tupac Katari was revitalized and politicized, his phrase "I will return transformed into thousands" became central to the indigenous social movement. Tupac Katari was an indigenous insurgent who led an anticolonial struggle at the end of the eighteenth century; his very memory demanded the restoration

of indigenous actions and knowledges in history—the decolonization of history. Urged on by this need, the social movements produced their own organic intellectuals, and indigenous university students and professors decided to "recover and reelaborate the indigenous past and its forms of historical knowledge" (Ticona 2000: 12). They also established nongovernmental organizations such as the Taller de Historia Oral Andina (THOA), which has functioned in La Paz, Bolivia, since 1983–84 and works to "investigate, disseminate, and revitalize the culture, history, and identity of indigenous peoples."[22]

The process of rewriting indigenous histories and transforming the political habitus in Andean countries is no panacea. Like any other political process, this one has been fraught with power struggles and expressed in essentialisms, factionalisms, and the production of universalizing metanarratives of its own (Albó 1994; Ticona 2000; Van Cott 2000; Warren 1998). However, it has certainly burst open the evolutionary narratives of indigeneity and advanced a politics of indigenous heterogeneity. Within this novel narrative, though in reference to the Maya area and not the Andes, the Guatemalan-Maya historian Edgar Esquit (2000:4) explained that "Mayaness is what Mayas do, provided that other Mayas recognize it as such."

More importantly, the public (and at times highly influential) presence of indigenous intellectuals has made obvious the possibility for an epistemic borderland (see Mignolo 2000) where, at ease or awkwardly, rational knowledge cohabits with nonrational knowledge. Organized in social movements, this blend sustains political projects that have as an important ambition the transformation of the modern state. The most widespread expression of this attempt is currently phrased as *interculturalidad,* a political project through which the indigenous social movement in Ecuador, for example, proposes to create "a plurinational state that recognizes the diversity of its peoples" (Yumbay 2001: 14).

Sustained and produced by political organizations frequently opposed to the neoliberal policies that Latin American states have implemented since the 1980s (Selverston-Scher 2001), *interculturalidad* belongs to the genealogy of mestizaje yet works against the coloniality of power-knowledge and the "stageist" narrative of history that sustains it. Like mestizaje, *interculturalidad* produces and is produced by a dialogic, academic-political, intellectual Latin American network, but the current network (enhanced by the World Wide Web) includes indigenous intellectual-politicians and global institutions ranging from funding agencies such as Oxfam America to multilateral organizations such as

the World Bank. Emerging in the 1970s from discussions about bilingual education programs for elementary schools in Peru, Ecuador, and Bolivia, *interculturalidad* (again, like mestizaje) is a highly heteroglossic notion. The most widespread Peruvian version is a state project defined as a "dialogue among cultures" (Godenzzi 1996), which continues to function as a biopolitical attempt to "improve Indians" on the basis of notions of bilingual education (Quechua and Spanish) that emphasize Western literacy techniques. In Bolivia, the PROEIB Andes, a college for bilingual education teachers in Cochabamba, has featured a similar mission since it was established in 1996. In both countries, the main activities are administered and funded by the state through the Ministry of Education, and the participation of indigenous organizations is marginal.

Yet *interculturalidad* also has an ambitious, even radical, version that aims at forging a world characterized by "pacific cohabitation among peoples and cultures, based on justice and equality for all" (Menchú 1998: 13). Toward that goal, in Ecuador "the indigenous movement has had as one of its main political and ideological objectives the construction of *interculturalidad* as a principle that articulates demands to a monocultural state and that aims at transforming the very conceptualization of the state itself" (Walsh 2002: 115). The greatest challenge for *interculturalidad* is to become *a new social relationship* that, along with feminisms, environmentalisms, and indigenous social movements, can confront former social hierarchies of reason, property, gender, and sexuality and produce a democratic state that "does not hold cultural renunciation as a condition for citizenship" (Tubino 2002).

Seemingly, then, in one of its most consequential versions, *interculturalidad* is both a novel (and, I would say, deeply subversive) state-making technology and an epistemological site for the production of a different kind of knowledge. Related to this (as well as to the urgency of rewriting national history and producing histories), the creation of alternative centers of knowledge has been a central concern of indigenous social movements. In Ecuador, the Universidad Intercultural represents such an effort. A document stating its goals describes it as a plural space (that is, not exclusively indigenous or for the production of "indigenous knowledge") designed "for the creation of novel conceptual and analytical frameworks, able to produce new categories and notions that have 'interculturalidad' as their epistemological framework" (Instituto Científico de Culturas Indígenas 2000). This editorial document criticizes modern science as having emerged from a monologue and having built self-referential categories "that did not allow

the inclusion of the 'strange' and 'different' within the borders of knowledge." Intriguingly, it concludes with a series of questions:

> If modern science has been monologic, and if the conditions for knowing are always implicated in the conditions of power, then how can we generate the conditions for a dialogue? How do we articulate intercultularidad *within the limits of epistemology* and the conditions of knowledge production? How do we contribute to the adventure of knowledge from different sources? [¿Como aportar a la aventura del conocimiento desde nuevas fuentes?] (Instituto Científico de Culturas Indígenas 2000)

I want to bring these stimulating questions to the arena of anthropology—which the Universidad Intercultural rightly criticizes as having constituted itself by creating and maintaining indigenous peoples as others and, moreover, by excluding their possibility for self-understanding. I want to use the questions as an opportunity to call for an anthropology (most specifically for an ethnographic production) articulated by what I call "relational epistemologies." Inspired by Arturo Yumbay, an Ecuadorian politician who described the role of anthropologists who work with indigenous social movements as that of *acompañantes* (companions, in a dialogic sense; see Yumbay 2001), I see relational epistemologies as a situated knowledge position (see Haraway 1991). That position assumes the historical contingency of universal categories and uses them in dialogical process with local thought, while paying relentless and critical attention to processes of translation between the two, thus rendering local knowledge visible. Relational epistemologies work to cancel subject-object positions, and upon interacting with their others as selves who speak, think, and know (Salmond 1995), they have the potential to create the conditions for the emergence of anthropologies in the plural—skilled enough to overcome Western singularity and become, collectively, a worldwide discipline. Eventually, beyond its disciplinary boundaries, a world anthropologies could communicate between Western disciplines and other knowledges, considered such in their own right.

## Concluding Remarks

At the beginning of this chapter I said I would use José María Arguedas to illustrate the politics of knowledge production as it emerged within the Peruvian intellectual-political community. Yet I did not mean to present a polarized situation, with Arguedas on one side and recalcitrant

rationalists on the other. This is not the way hegemony works—and the hegemony of Western knowledge practices is also apparent in Arguedas's work. For in spite of the epistemological challenge that his literature represented, the process through which he crafted his anthropology was full of intriguing tensions that reveal his compliance with reason, science, and the social-academic hierarchies that structured Latin American society in the 1960s and that linger today. In his correspondence with anthropologists, he repeatedly regretted his "ignorance of theory" and subordinated local anthropology to metropolitan centers of knowledge. "Only those who have been seriously trained abroad can teach here, can conduct scholarly institutions . . . The rest, like me, can do a little in art, but in the sciences we're pathetically dead, and some of us accept remaining in our positions because there is no one better yet," he wrote in a letter in 1966.[23]

This opinion belongs to the genealogy of knowledge against which the proponents of *interculturalidad* have rebelled. Yet the dynamics and hierarchies of hegemonic knowledge continue to pervade the production of *interculturalidad*. Pamela Calla, a Bolivian anthropologist, has described some conflicts at the Bolivian PROEIB College where she teaches. Students, she says, have coined labels that attest to different forms of being indigenous, which, however, highlight the tensions of being "inferior" in a modern sense—that is, less educated or less masculine. For example, on one occasion the students classified themselves into "academics" and "fundamentalists." Not surprisingly, the academics positioned themselves as a superior group and were challenged by the fundamentalists' self-identification as "more indigenous" and therefore more masculine (Calla 2002). Although the latter interpretation challenges dominant stereotypes, whereby "women are more Indian" (de la Cadena 1991), it conforms to modern gender hierarchies. Similarly, pressures to be both modern and indigenous are complex—as illustrated in the following passage, by an indigenous leader whom I will keep anonymous: "Sometimes I feel I am going crazy because I cannot think like an Indian anymore. I fight for Indians among whites, and therefore I have to think like them. I represent indigenous interests within state institutions, but I have not been back in my village for three years. I travel all over the place, and I know I am an Indian. But what kind of an Indian?" (Oliart 2002).

*Interculturalidad* is not a smooth, let alone automatically successful, process. For one, it has not eliminated the images of indigenous timelessness that academic Andeanism created in the region. One consequential example illustrates the way such images still thrive among the powerful

in Peru. In 1984, caught in war between the Shining Path and the Peruvian Army, indigenous peasants from the village of Uchuraccay (in the Ayacucho region, the epicenter of the violence) collectively killed six journalists who were investigating another massacre that had taken place weeks earlier nearby. Reactions to the event included colonial anti-Indian fears as well as paternalistic pro-Indian attitudes. The government responded by nominating a commission to investigate the massacre. Because the assassins were Indians, not modern Peruvian citizens, the key members of the group—led by the internationally famous Mario Vargas Llosa—were two anthropologists, rather than lawyers as they would have been in a criminal investigation. Removing the killers from history, the anthropologists explained that the Indians had been moved to kill the journalists by a combination of ancestral fears and cultural principles.[24]

The anthropologists who wrote the report are currently key advisors in a governmental effort to transform Peru into a multicultural nation compatible with the economic mission of neoliberalism. From this perspective, Andeanist multiculturalism continues the legacy of earlier acculturation theories. Indians can successfully become modernized cholos. The current president, Alejandro Toledo—commonly called "el Cholo Toledo" in Peru—represents this possibility, for he is "an ex-Indian with no complexes and the cool calculating mind of a Stanford and Harvard academic," who has the ability to "understand life from a viewpoint rooted in analytic rigor and scientific information." It may be only a coincidence, but the author of the quoted lines is Álvaro Vargas Llosa (2000: 20), the son of Mario Vargas Llosa, the authority in the aforementioned report. Mario Vargas Llosa is also the author of a book titled *La utopía arcaica* (1996), in which he discusses Arguedas's work as an anachronistic desire, a reversal of history—and thus not only a utopia, but an archaic one at that.

In the 1960s and 1970s, historicist class analysis worked as a "prose of counterinsurgency" that excluded indigenous revolts from the academically defined field of politics. At the turn of the twenty-first century, liberal multiculturalism can work as an "antipolitics machine" (see Ferguson 1990) by including within the hegemony of liberalism—or neoliberalism, in this case—circumstances that might reveal and thus politicize everyday narratives of "cultural" or "ethnic" exclusion. The inclusive yet depoliticizing work of multiculturalism operates by normalizing education. In Peru, for example, the scandal that might otherwise arise from having a cholo as president of the country is canceled, or at least calmed, by references to Toledo's training in the

centers of reason, an indication of his adequacy as a modern politician. Arguedas, through his intricately fictional Rendón Willka—and through his own life—questioned normalization through education. He thus rejected the everyday habits of thought of his peers and provoked an intellectual-political scandal that the counterinsurgent prose of modernity could not control.

Similarly scandalous are discussions of *interculturalidad* and the presence of indigenous intellectuals in countries such as Guatemala, Ecuador, and Peru. Siding with the scandalous (for it challenges the simplicity of modernity) and inspired by Arguedas, I want to propose that inasmuch as indigenous social movements articulate an alternative to modern politics—and the nation-states they sustain—they have the potential to transform the liberal empirical notion of "diversity" currently tolerated in liberal multiculturalisms into political demands for the citizenship of plural ontologies and their forms of knowledge. As a Western social science enabled by non-Western locations, anthropology is in a position to contribute to the visibility of other forms of knowledge. In order to do that, an awareness of anthropological knowledge as a dialogical process of translation—between the local and the universal, between histories and History, between the singular and the general—is in order.

# Notes

1. I use Bakhtin's (1981) notion of dialogue together with Foucault's (1980) genealogical perspective in order to avoid the linear historical narrative that naturalizes the current geopolitics of knowledge.

2. To formulate this notion, Quijano (1993) explained that an entwinement existed between Eurocentric forms of knowledge and current forms of domination throughout the world. The roots of this power formation could be traced back to the sixteenth century, when beliefs in the superiority of Christianity over "paganism" enabled Europe to constitute itself as the epicenter of modernity, allegedly the most advanced historical moment of humanity. Supported by a Eurocentered notion of linear time, the power that supported the conquest of the Americas and connected the "new" and "old" worlds conditioned a production of knowledge according to which Americans occupied the past and lacked what Europeans had: civilization and reason. Installed in the discipline of history, this conceptual alchemy,

which relentlessly and pervasively reproduced the image that Europe was the future of non-European populations, survived decolonizing movements and continued to inform dominant ways of knowing.

3. Influenced by readings of Spengler's *The Decline of the West*—which reached Latin American readers through the Spaniard Ortega y Gasset's *Revista de Occidente* (Valcárcel 1981)—Indo-Americanists proposed that their "ideological and philosophical liberation from trans-Atlantic domination" was to be epistemologically inspired by "a spiritual attitude sympathetic to the past" (García 1937: 33).

4. The most prominent proponent of this regional-cum-nationalist community was José Vasconcelos, credited with having invented the Raza Cósmica—the leading slogan of the Mexican nation-building project known as mestizaje. The Peruvian Víctor Raúl Haya de la Torre founded the Acción Revolucionaria Americana (later to become APRA, an important populist Peruvian party) in 1924 while in Mexico, where he worked as a personal aid to Vasconcelos, then minister of education. In turn, Haya de la Torre was a conspicuous supporter of the anti-imperial struggles of César Augusto Sandino in Nicaragua, and both subscribed to Vasconcelos's brainchild, *Indoamérica*. Similarly, from the other end of the continent, the Argentinean Ricardo Rojas crafted the image of "Eurindia," suggesting a regional identity built from the encounter between indigenous American and European traditions, imported to Argentina by colonial Spaniards and by Italian, Spanish, and English immigrants in the early twentieth century.

5. According to Beals (1953), Robert Redfield, then at the University of Chicago, coined the term *acculturation* after his visits to Mexico in the 1920s. Similarly, Melville Herskovitz, another of Boas's students and, like him, interested in African Americans, used the term upon returning from fieldwork in Surinam, where he might have come in contact with Caribbean notions of *métissage* and negritude. He was working with Redfield at Chicago at that time (Beals 1953).

6. Also in 1936, Redfield, Herskovitz, and Ralph Linton wrote "A Memorandum for the Study of Acculturation" (Beals 1953).

7. Among the first to contest the notion was Fernando Ortiz. Acculturation, he opined, simplified the complex cultural give and take that had characterized Latin American society since the arrival of the Spaniards. The mixture was *transcultural*—it operated in multiple directions as the Latin American indigenous, Spanish, and black cultures changed interdependently (Coronil 1995; Ortiz 1995 [1947]; Rama 1982). Although some literary critics used the notion of *transculturación* to conceptualize Arguedas's position, Ortiz's concept maintained "the notion of levels of cultural development" (Coronil 1995: xix) that Arguedas's experience and writings opposed.

8. Also as a consequence of the popularity of "culture areas" (and illustrative of the notion's international influence), the Instituto Francés de Estudios Andinos was founded in 1948, with Alfred Métraux as an important authority.

9. Created in the early 1960s by a group of elite sociologists, anthropologists, historians, philosophers, and economists, the Instituto de Estudios Peruanos was among the first institutions to seek and receive private funding. It was peculiar in that it combined the legacy of Indigenismo with cutting-edge dependency theory. The elite social position of its members, along with their leftist penchant, made the institute an influential organization, central in the development of the social sciences in Peru. Luis E. Valcárcel, John Murra, and José Matos Mar—all figures related to the Mexican hub of inter-American anthropology—were members of the institute.

10. To control the turmoil—and modernize the countryside—the state responded with development plans to "integrate the indigenous population," in which anthropologists, foreign and local, participated profusely. The best-known efforts were the Cornell-Vicos project and the Plan de Integración de la Población Aborígen. With the participation of anthropologists from the United States and Peru, these projects functioned in the 1950s and 1960s.

11. See Quijano 1980. The Peruvian sociologist Guillermo Rochabrún, in a recent analysis of the roundtable discussion, mentioned that Quijano's notion of *cholificación* was first published in 1964, under the title "La emergencia del grupo cholo y sus implicaciones en la sociedad peruana (esquema de enfoque aproximativo)" (see Rochabrún 2000: 99–100).

12. According to Guillermo Rochabrún, the quoted passage appeared in "El movimiento campesino del Perú y sus líderes," which was published in 1965. The same article was published later in Quijano 1979.

13. Aníbal Quijano, personal conversation, August 2003.

14. This is not an isolated perspective. For a similar evolutionary mindset, see Bonilla 1981.

15. The words belong to Enrique Bravo Bresani, an engineer who attended the *mesa redonda* discussion of *Todas las sangres*. He soon became an ideologue of the revolutionary military government that in 1968 issued an agrarian reform law aimed at halting the rural turmoil.

16. Among the critics who have commented on the phrase "all bloods" are Rowe, Escajadillo, Cornejo Polar, Escobar, Lienhard, Spitta, Rama, Larsen, Lambright, Moreiras, and Devine.

17. The Uruguayan Angel Rama, for example, has likened Arguedas's denial of acculturation to the Cuban Ortiz's earlier "transculturation." But Arguedas's testimonial suggestions transcend the bidirectional cultural mixture that Ortiz defined as transculturation. While Ortiz's notion altered the linearity of

acculturation and argued for the cultural specificity of Cuba, it yielded to the superiority of Western civilization. Moreover, it was conceived from a Western way of being and knowing.

18. He told the poet Tomás Escajadillo this in 1965 (see Escajadillo 1970).

19. In being more at ease writing literature than anthropology, José María Arguedas and Zora Neale Hurston were similar intellectual personalities.

20. David Sobrevilla, personal communication, August 2003.

21. According to Carmen María Pinilla (1994), the discussants were prey to "a scientificist" position that prevented them from offering a "more open" viewpoint and attitude. Arguedas's two most prominent opponents were considered to be among the "most serious" representatives of the nascent social sciences (Pinilla 1994: 107).

22. This passage is quoted from the website http://www.aymaranet.org/thoa7.html.

23. The letter was addressed to his dear friend Alejandro Ortiz Rescaniere, who was studying in Paris under the direction of Claude Lévi-Strauss, an almost unknown figure in Peruvian anthropology circles in the 1960s (Ortiz Rescaniere 1996: 209).

24. That these "timeless Indians" were seasonal laborers on coffee plantations, that they made weekly trips to nearby towns to purchase rice, sugar, kerosene, and cigarettes, that their sons and daughters were servants in the city, and that they were unfortunate actors in the war between the state and the Shining Path were absent in the report.

# A Time and Place beyond and of the Center

## Australian Anthropologies in the Process of Becoming

### *Sandy Toussaint*

"American anthropology's obviously the best, it's so big!" exclaimed an Australian postgraduate student fresh from attending an American Anthropological Association conference. While his observation made explicit (among other things) the privileging of quantity over quality, his enthusiasm also endorsed a view that anthropology was represented by, and measured primarily against, a powerful matrix or "center." The meta-message was that anthropologies elsewhere or otherwise, including those in postcolonial Australia, were unevenly positioned. In this chapter I contemplate the problems involved in circumventing such a claim, and I consider (at what might be described as the other end of the spectrum) the use of anthropological knowledge beyond "the center" of its own production. The example of an Australian native title land claim helps to illuminate an analysis that rests on the hybridity of Australian anthropologies in the process of becoming.[1]

## Anthropology in a Time and Place beyond "the Center"

Without wishing to privilege structural or oppositional analyses, but keen to provide a contextual beginning, I suggest that Australia can be broadly understood as both urban and remote, dry and waterlogged, densely and sparsely populated, and culturally plural and singular.

Depending on one's vantage point and knowledge, Australia may also be described as both resource rich and impoverished, socially heterogeneous and homogeneous, politically conservative and liberal, and touched by and distant from the connections, disconnections, and disturbances evoked by globalization.[2] Within these broad descriptions, Australia is, of course, also many shades in between.

Australian anthropology replicates the parallels, dislocations, and betweenness evident in descriptions of Australia's people and their cultural landscape. Although anthropologists share epistemological, methodological, theoretical, political, and ethnographic interests, they also differ on a range of issues.[3] Reflected in their differences and tensions are the uneven attention they pay to reflexivity in anthropology, their debates over the discipline's authority and purpose, and the increasing pressure they feel to develop the commercial arm of anthropology.

In spite of Australia's geographical proximity to the Pacific and Southeast Asia, successive Australian governments have tended to be biased toward the policies, ideologies, media, cultures, and governance of the United States and the United Kingdom.[4] Anthropology was, and still is to some extent, heavily influenced by theoretical developments in those countries (Keen 1999a). When understood from this perspective, Australian anthropology, like the anthropologies of Europe, Latin America, Asia, and the Middle East, can be interpreted as nonmetropolitan, or distant from the "center" (see Eriksen and Nielsen 2001: 158), while also heavily influenced by it.[5]

In Australia as elsewhere, the past twenty-five years have witnessed significant challenges to the purpose, style, intellectual trends, and claims of anthropology. The critique emerged from four broadly significant (but neither homogeneous nor necessarily exclusive) positions: those of colonized groups and those of scholars in the fields of feminism, postmodernism, and cultural studies, respectively. A key point of inquiry focused on the relationship between indigenous groups and anthropologists, particularly on the ways in which Aboriginal and Islander women and men have been represented and by whom (Bell 1993; Langton 1981, 1993; Muecke 1992; Peace 1990; Toussaint 1999).

Perhaps because of Australian anthropology's nonmetropolitan status, its tempered response to the crisis of representation, and Australia's shifting political landscape regarding reconciliation over the wounds of colonialism, certain fields of anthropology are thriving in Australia. In a discipline of many parts, anthropologists now work as academic, applied, and consultant anthropologists. None of these categories is self-contained. A number of academic anthropologists do

consultancy work, and some consultant anthropologists are employed as academics on a fixed-term basis in Australian universities (*Practicing Anthropology* 2001). Almost all compete for work within a private or public marketplace. Anthropologists conduct research, teach, or both in a range of settings, such as universities, community organizations, government and nongovernment agencies, and sectors of industry. Depending on context, most of them employ research methods that rely on the technologies of globalization, such as personal computers for recording and analyzing data, the internet for literature searches, and email exchanges with colleagues near and far.

Largely as a result of government policies aimed at commercializing the tertiary sector, academic anthropologists have been drawn into increasingly intense competition for research grants and consultancies. As Stephen Hill and Tim Turpin showed (1995: 137–38), a "new enterprise culture" now influences the administrative structures of Australian universities, and "grass-roots involvement and faculty debate have become increasingly marginalized in favor of managerial efficiency." The effect on disciplines such as anthropology has been profound. Under already demanding circumstances, academics are now required to write time-consuming applications for government and industry research grants or to undertake short- or long-term consultancies.

Consultancies, which Andrew Strathern and Pamela Stewart described as resulting in the "contemporary transformation of applied anthropology" (2001:3; see also Gardner and Lewis 1996), characterize much of contemporary anthropology in Australia, especially in the field of native title land claims. Although at one level this situation has improved employment prospects for graduates in anthropology and taken the discipline's expertise beyond the academy, the growth in consultant anthropology has also generated a certain disquiet about the discipline's ethos. Reasons for this unease stem largely from anthropology's colonial history, the awkward relationship between applied and academic or theoretical anthropology, and ongoing considerations about anthropology's use and purpose (Hamilton 2002; Keen 1999a; Stead 2002; Strathern and Stewart 2001). Although encompassed in well-worn debates, many of which have been raised in the books *Reinventing Anthropology* (Hymes 1974), *Recapturing Anthropology* (Fox 1991), and *Anthropology beyond Culture* (Fox and King 2002), these important issues continue to require examination in new circumstances, especially in a globalized world where "the pace of life is speeding up" and "the time taken to do things is becoming progressively shorter" (Inda and Rosaldo 2002: 7).

Jim Birckhead (1999) and Philip Moore (1999) each showed the effects of commercialization and time frames in restricting the quality of anthropological practice. Drawing on the work of Erve Chambers (1991) and John van Willigen (1991), Birckhead concentrated on the process of doing "rapid ethnography" consultancies in Aboriginal Australia. He observed that even though brief field research may be adequate if the work is "done well, and in a critically informed way," especially in the arena of project evaluation and assessment (1999: 221), such an approach has clearly resulted in an "epistemology of brief encounters" (1999: 198).

Moore presented a slightly different view. Mindful of the shortage of considered research time in the kinds of brief encounters Birckhead described, Moore concluded from his experience as a consultant in Aboriginal cultural heritage that competition among anthropologists for research contracts tended to drive the process. In such circumstances, "less time remains for critical reflection on anthropological practice" (Moore 1999: 249). Inspired by Arturo Escobar (1991, 1995), Moore invited other anthropologists to "subject the [consultancy] process to critical examination to identify and expose how this development encounter shapes anthropological practices and representations of Aboriginal heritage" (1999: 250). In a similar vein, and also referring to Australia, Ian Keen (1999a: 54) lamented, "What kind of information an anthropologist can and should produce for particular purposes or what effect it may have is seldom discussed in depth."

Writing as Australian anthropologists employed by AusAID to work on "development" projects in Southeast Asia, Jocelyn Grace (1999: 124–40) and Jim Taylor (1999: 141–61) expressed similar anxieties.[6] Taylor's work in northern Thailand led him to offer a critique of development discourse, especially with respect to projects that "ignore local conditions and historical forces" (1999: 154). Calling for anthropologists to devote attention to alternative interventionist processes, Taylor revealed a clear need for more "localised or micro ethnographic studies concerned with development discourses and practices" (1999: 157). Grace, who focused on health projects on Lombok, Indonesia, echoed similar themes, but she also posited that "some development projects benefit local people regardless of how flawed the epistemological ground from which they have grown" (1999: 138). Referring to her work as a consultant on an AusAID "child survival project," where the women among whom she worked "took" what they wanted from the project and "ignored" or "rejected' what they did not want (1999: 139), Grace claimed that anthropologists who act as consultants in development are continually

affected by the "damned if you do, damned if you don't" dilemma (1999: 124).

The views expressed by Birckhead, Keen, and Moore about Australia, in conjunction with those of Grace and Taylor as Australian anthropologists engaged in sponsored development projects in Indonesia and Thailand—and others, such as Hill and Turpin and Strathern and Stewart, commenting generally on anthropology as a commercial enterprise (via consultancies, research grants, and competitive tenders)—represent significant contemporary themes. These themes might be conceptualized under four headings: the driving of research by powerful economic networks; the limitations of "brief encounters"; the overlooking of local knowledge and regional histories in research projects; and the lack of time to reflect on fieldwork findings and their consequences. It also appears that, in some projects at least, communities, individuals, or families may benefit as a result of long-term or delayed anthropological research and that "rapid ethnography" suits some topics better than others (Birckhead 1999). These themes reflect a spectrum of anthropological practices, theories, and knowledges and represent the different contexts, times, and places in which anthropological engagements occur.

Moore, Taylor, and others whom I have not mentioned participate in a public critique of anthropology, especially with respect to ambiguous or contradictory outcomes for the persons and communities among whom they work. But much of this critique occurs within established anthropological frameworks and networks, such as in texts, via the internet, and in conference venues where anthropologists converse among themselves. These productive exchanges are vital to the discursive health of the discipline. At the same time, it is clear that anthropological knowledge is not always produced for the sole benefit of anthropologists or for study within the discipline. One of the most challenging issues that continues to affect anthropologists in Australia is the question of how to translate different forms of knowledge from one relational context to another.

## The Anthropology of Native Title

While globalization and pressure to commercialize the discipline have had their effects, increasing interest in studying anthropology and working as an anthropologist have also emerged in response to changes in Australia's legal and moral history. The most significant of these dates to 1992, when the Australian High Court handed down a decision generally known as *Mabo v. Commonwealth (No. 2)*. The Mabo decision

acknowledged prior Aboriginal and Islander rights and interests in land, or "native title." Focused primarily on a small island in the Torres Straits, the decision established the principles whereby future native title claims on the Australian mainland might be made, and it rejected the legal fiction of *terra nullius,* which had underpinned Australia's colonial history for more than two centuries (Bartlett 1993; Fingleton and Finlayson 1995; Henderson and Nash 2002; Keon-Cohen 2001; Mantziaris and Martin 2000; Sutton 1998).

The Mabo decision represented the first time that a form of land title had been constructed around indigenous notions of property ownership (Langton 2000). Indigenous peoples, with supporting evidence from anthropological data, now have to prove that they have a right to claim native title. That an individual or group can show a continuing connection with the land consistent with traditional indigenous use, or that rights in land have been conferred by customary law, is essential to the requirements of native title law.

Broadly translated into legislation in 1993 as the Native Title Act, the Mabo decision ultimately generated the establishment of a mediating body, the National Native Title Tribunal (NNTT), to hear Australia-wide claims.[7] Hundreds of mainland native title claims have been lodged with the NNTT, and some have been mediated or directed to the Federal Court or the High Court in Canberra, Australia's capital city. Only thirty-one claims have been either wholly or partially successful, and amendments made to the act in 1998 have weakened its potential.[8] According to Keen (1999b: 2), under the Native Title Act, claimants must show that native title "is held by some kind of community or group; the group must have genealogical connections with the community or group which occupied the country [meaning "land" in an Australian context] in question at the time of the establishment of British sovereignty and have substantially maintained its connection with the country; [and] the laws and customs through which title is framed must constitute a tradition, if a changing one."

By recognizing indigenous land tenure laws and by leading to a tribunal in which native title claims could be mediated, the High Court's 1992 decision also enhanced employment opportunities for anthropologists. Cross-disciplinary research has flourished, and anthropologists, historians, and linguists often undertake fine-grained ethnographic, linguistic, and historical research for the common purpose of claim preparation and presentation in native title proceedings (Toussaint 2004). Promoted also by the tertiary sector's "push" to compete for research grants, consultancies, and competitive tenders, cross-disciplinary work

is not without its problems, especially when the ethical codes and methods of certain disciplines conflict.

Anthropological involvement includes ethnographic research with indigenous traditional owners, cultural mapping, the recording of genealogies, and the preparation of detailed reports that will be lodged with the tribunal or other relevant court. Associated activity includes peer-reviewing reports, giving expert witness testimony, and advising indigenous, industry, and government organizations. Anthropologists may also be instrumental in the way a report is reviewed by tribunal personnel or members of the judiciary. As key actors in native title discourse, anthropologists have developed extensive knowledge of indigenous land tenure systems throughout Australia and provided important policy advice to indigenous and other organizations.

Anthropologists may also be employed full-time or as consultants by organizations that oppose a native title or cultural heritage claim. Such organizations include those related to sectors of the mining industry, government departments, the fishing industry, farming, and developers.

That the anthropology of native title claims has generated advantages for some anthropologists is without question. It is less clear that the discipline as a whole or native title claimants generally have been advantaged. Proving that native title has not been extinguished, for example, and that cultural connections to land and sites have been maintained and reproduced over time has been difficult for those claimants on whom colonization and dispossession have taken their greatest toll. Consideration of the Yorta Yorta native title claim helps to explain this point.

## The Yorta Yorta Claim

The *Yorta Yorta Aboriginal Community v. Victoria* native title claim was lodged in the Victoria Division of the Federal Court of Australia and heard by Justice Olney in 1998.[9] Olney ruled that the claimants, anthropologists, and linguists had produced insufficient evidence to show that the Yorta Yorta continued to hold customary rights in accordance with the Native Title Act. Claiming that "the tide of history had washed away any real acknowledgement by the Yorta Yorta of their traditional laws and customs," Olney made minimal use of anthropological and linguistic evidence and appeared skeptical of indigenous testimony (Auty and Patten 2001; Bowe 2002; Case 1999).

One criticism of Olney's judgment was that he privileged as evidence the writings of an amateur historian and squatter, E. M. Curr, in a record titled "Recollections of Squatting in Victoria," first published in 1883. In Olney's view, present-day Yorta Yorta cultural beliefs and practices should have conformed to those described by E. M. Curr in the nineteenth century.

Aware of the precedent that Olney's ruling set, and devastated by his failure to recognize their native title claim, the Yorta Yorta, in conjunction with anthropologists and lawyers, appealed the decision. They did so on the grounds that Olney had erred in law by applying a biased test for deciding whether or not native title existed and by finding against the Yorta Yorta primarily on the basis of Curr's written record. The appeal also contested Olney's view that the Yorta Yorta were required to show that they and each generation of their ancestors since colonization had observed the same lifestyle, beliefs, and behaviors, because such a view did not accord with the spirit of the Native Title Act and anthropological analyses of cultural continuities within the context of change.

The Australian High Court heard the Yorta Yorta's appeal against Olney's decision but dismissed it in 2002, by a five-to-two vote. The judges concluded: "As their Honours found that the [Yorta Yorta] society that had once observed traditional laws and customs had ceased to do so, it was held that it no longer constituted the society out of which the traditional laws and customs sprang. Therefore, any claim by the Yorta Yorta people that they continued to observe laws and customs which they, and their ancestors, had continuously observed since sovereignty must be rejected" (Australian High Court 2002).

Elsewhere in the judgment (2002: 96), the court found that "the forebears of the claimants had ceased to occupy their lands in accordance with traditional laws and customs; and there was no evidence that they continued to acknowledge and observe those laws and customs." Despite Yorta Yorta knowledge, beliefs, and practices, and despite anthropological and other "expert" testimony in support of their claim, the court's decision, based on the evidence that came before it, was that native title no longer existed.

Preparation of the claim, followed by the original judgment and the appeal, generated substantial anxiety over the possible fulfillment of long-term land rights expectations for the Yorta Yorta claimants. At the same time, a huge amount of work was generated for anthropologists, linguists, historians, and lawyers. Much of the anthropological and cross-disciplinary research on the Yorta Yorta claim was demanding, politically intense, and undertaken in adversarial circumstances (Bowe

2002; see also Choo 2004 and Christensen 2004 with respect to the Miriuwung Gajerrong claim in Western Australia and Stead 2002 on claims in the Northern Territory).

Anthropologists researching native title claims also faced unique and critical questions, including questions about interpretations of continuity in cultural beliefs and practices; indigenous land tenure laws; evidentiary matters; meanings attached to "tradition"; the veracity of genealogical connections; and assertions about the effects of sovereignty. The Yorta Yorta claim, like other native title claims, was therefore not only about reconciliation and the recognition of indigenous knowledge and land tenure laws; it was also about generating work and rich sources of data for anthropologists. Somewhat ironically in a postmodern world resistant to certain orthodox methodologies, native title discourse has also served as a catalyst for the reintroduction of university courses on the construction of kinship classifications, genealogies, and so on. In this regard, anthropological and other research in the native title field can be seen to have enhanced the enterprise aspirations of Australian universities.

The discursive and pragmatic benefits of native title research for anthropology are obvious. But while many anthropologists expressed disappointment over Olney's decision, most had another intellectual space, another place, another claim, another time, and another inquiry with which to displace that disappointment. The benefits of native title research for the Yorta Yorta are less clear. Claimant Rochelle Patten made clear the "fixity" of Yorta Yorta attachments to persons and places:

> My name is Rochelle Patten and I am a Yorta Yorta woman from Ulupna through my grandmother. My totem and that of the Yorta Yorta people is the long-necked turtle. My personal totem is the crow. The crow guides me. I was born at Mooroopna Base Hospital which is on the Goulburn River in Yorta Yorta territory. I have seen a copy of the map which shows the Yorta Yorta land claimed in this native title claim and I say that this is the traditional Yorta Yorta land. I know this because my mother told me. Her father told her and his father told him. My relationship with the land goes back to my grandfather and further. The land is part of me. My responsibility to the land is to look after the land like a mother in the way the elders looked after it. This is the responsibility of all Yorta Yorta people. (Quoted in Auty and Patten 2001:6)

The connectedness to land and family that Patten expressed, and the disaffection she and other indigenous claimants felt, represents a kind of disjuncture between the native title claimants and the anthropologists.

But it also reveals a more powerful cultural and political rupture: the fissure between anthropology, indigenous groups, and Australian courts of law.

The native title situation in Australia makes it plain that although a critique can be made of unitary anthropological thought and practice, in order to make room for its plural—anthropologies—there are powerful places in which anthropological knowledge in its singular (let alone plural) form continues to struggle for recognition outside the milieu of its own creation and reproduction. The anthropologist David Trigger, writing about the Croker Island native title claim (also known as *Yarmirr v. Northern Territory*), encapsulated the problem this way:

> [T]he different worlds of discourse of law and anthropology are evident [in cases such as Croker Island]. For lawyers, their pleadings are statements that are "assertions" to be proved or supported by something called "evidence"; the difficulty is that … anthropology projects such "assertions" as conclusions that are already based upon interpretations of action and speech, and … the way one justifies the conclusion is to provide illustrative exemplary material. Our most general challenge … lies in better explanation of the nature of research methodologies and theories in anthropology … amidst legal colleagues whose own training is both tantalizingly familiar and frustratingly distant from our own. (Trigger 2004: 33)

## Anthropologies in the Process of Becoming

The anthropology of native title is an example of a strong field of engagement that has generated significant intellectual interest and employment for Australian anthropologists in recent years. That anthropologists (including some Aboriginal people, who increasingly train and work as anthropologists) continue to be engaged in native title claims, despite their lack of success, reflects the continuation of the land rights struggle in Australia as much as it reflects the difficulty anthropological knowledge has in being heard beyond its own "center."

My analysis is not meant to imply that the issues will disappear, that native title discourse is beyond critique, or that Australian anthropologists should resist challenges to the reproduction of hegemonic anthropology. But it does suggest that, at one end of the spectrum at least, anthropologists must continue to work hard to convey complex material to proponents of "other" forms of knowledge, including people given power and authority by a sometimes hostile state.

Many forms of anthropological knowledge and avenues for its production exist in postcolonial Australia, where emergent anthropologies might be described as exhibiting "a turning of boundaries and limits into the in-between spaces through which the meanings of cultural and political authority are negotiated" (Bhabha 1990: 4). Australian anthropology now seems to sit somewhat precariously, with several avenues to explore. One of these might be to disturb a process that emphasizes an uncritical, unitary anthropology; another is to respond to the pressures of economic demands and the danger of diminished time for reflection; and still another is to integrate anthropology into multidisciplinary formulations, a pathway that might lead to broader public debate and visibility. Of course a combination of these three avenues and other reconfigurations are also possible.

Closure on debates surrounding internal, interstitial, or external anthropological knowledges is unlikely as anthropologists of and beyond "the center" endeavor to develop theories and practices that incorporate knowledge from other places in addition to developing an anthropology of their own. I hope that in the process, a more self-conscious Australian anthropology will discard aspirations to be the "biggest" or "the best," so that it can productively engage with the contradictions, ambiguities, and complexities embodied in a globalized, world-anthropologies ethos. Mikhail Bakhtin (1981), a critic of attempts to ideologically frame the meanings of words, ideas, and actions, concentrated on the significance of contextual and relational analyses. For Bakhtin, the dialogic approach allowed consideration of the ways in which meaning and interpretation changed over time. This sort of analysis is important for understanding reformulations of Australian anthropologies.

The multiple knowledges presented in the context of native title claims are rarely the kind of "evidence-knowledge" that is persuasive to judges deliberating in Australian courts of law. While anthropologists in Australia may be increasingly open to deconstructing hegemonic anthropology and receptive to its transformations, we need also to be mindful that such knowledge is often incomprehensible (perhaps for a variety of intentional and unintentional reasons) to those outside the construction of that knowledge, including the groups with whom anthropologists work—such as the Yorta Yorta, who continue to explore ways to achieve the return of their land.

The sorts of multiple knowledges with which anthropologists engage do not crystallize and mutate in a vacuum. In the case of native title claims, where the emphasis is on mediation and litigation, anthropological and other knowledges must be both accessible and

persuasive to adherents of a "different" body of knowledge and law. That anthropologists conduct research across disciplinary boundaries as they negotiate the complex demands of indigenous groups and the state represents a slightly different yet no less important problem than that of disturbing "the center" and contesting the power and authority of an inherited periphery-center divide.

# Notes

1. My experience as an applied and academic anthropologist concerned mainly with Aboriginal Australia and my strong interest in epistemological and ethical issues are reflected in this chapter. Although many of my colleagues conduct research in ethnographic settings outside of Australia, particularly in Southeast Asia but also in Venezuela, Madagascar, and the Middle East, anthropological work on Australian indigenous cultures remains a significant field of inquiry. An increasing interest in doing "anthropology at home" and some rapprochement with cultural studies is also evident among Australian anthropologists, as is a resurgence in sociological inquiry. Current topics of research in my department at the University of Western Australia include attachments to place, environmental issues, tourism, health, medicine, genetics, ethnicity, migration, refugees, urbanization, globalization, cultures of consumption, psychological and evolutionary anthropology, and legal cultures. A spectrum of approaches embraces postcolonialism, poststructuralism, and more orthodox ethnographic and theoretical frameworks. Any of these emphases might lead to a slightly different interpretation of Australian anthropology from the one I present here.

2. Though not focused on Australia, Bhabha (1990), Featherstone (1995), Appadurai (2002), and Ferguson (2002) are among those who have explored the complex interplay (connections and disconnections) between local conditions and globalization, including the hybridization that emerges when the two collide. The extent to which indigenous groups in Australia have been affected by globalization is unclear, although Michaels (2002) discussed how the desert-living Warlpiri of Central Australia related to Hollywood videos in a way that rendered the videos both analogous and meaningful to their own history and circumstances.

3. For an example of divergent views among Australian anthropologists, see Brunton 1995 and 1996, Tonkinson 1997, and Bell 1998, works that present

a range of anthropological positions on the "Hindmarsh Island dispute" in South Australia. The website http://www.library.adelaide.edu.au/gen/H_Island contains findings from the 1995 South Australian Royal Commission.

4. Paul Keating, Australia's former Labor prime minister, was unusual in stressing Australia's interest in developing stronger social, cultural, and political ties with parts of Asia and the Pacific, in addition to trade-based ties. As Watson (2002: 77) explained, "Keating wanted to keep the US in the neighbourhood, but he did not want Australia to be either a spoke of American policy or a cheerleader. He wanted collective dialogue in the [Asia-Pacific] region, and he wanted Australia involved in it." Keating and the Labor Party lost power to a conservative government in 1996. The Liberal-Coalition, led by Prime Minister Howard, has not developed the close regional ties encouraged under Keating.

5. Taking relative population sizes into account, the demographics of the discipline are revealing. The professional body of the Australian Anthropological Society (AAS) consists of around 290 members in the categories of fellows and ordinary, unsalaried, and retired members, in comparison with 10,000 members of the American Anthropological Association (AAA), 2,000 members of the European Association of Social Anthropologists, and 1,000 members of the British Association of Social Anthropologists (AAS Secretariat 2002; Eriksen and Nielsen 2001: 158).

6. The term *development anthropology* is rarely used in Australian anthropology. Sometimes it is rejected on the basis of its associations with colonialism and in response to critiques of the term. Most anthropologists working in Australia identify themselves as doing "applied" or "practical" work.

7. Before passage of the Native Title Act, the land entitlements of Australian indigenous groups had not been recognized at a national level, despite decades of agitation (including some by anthropologists). Several state- or territory-based pieces of legislation existed, the most significant being the Northern Territory's Aboriginal Land Rights Act of 1976.

8. The NNTT was unable to provide figures for the number of claims lodged since its inception. The figures presented here refer only to native title claims in which the NNTT found that native title continued to exist and to have meaning for indigenous claimants. The areas in question varied markedly in size, and some decisions are currently under appeal.

9. The Yorta Yorta native title claim is one of a number of claims on which indigenous claimants, anthropologists, lawyers, historians, and linguists have worked together. Other claims include the Wik claim (1996), in which the Australian High Court ruled that the grant of a nonexclusive pastoral lease in Queensland did not extinguish native title, and both titles could co-exist— although the rights of the pastoralist would prevail if a conflict of interest

emerged; the Croker Island claim (2001), in which the High Court ruled that native title extended to the sea but did not grant indigenous claimants exclusive possession of sea areas; the Miriuwung Gajerrong claim (1998–2002), in which the High Court ruled for partial extinguishment of native title; and the De Rose Hill claim (2002), in which native title was found to be extinguished (the website www.nntt.gov.au provides details).

# Official Hegemony and Contesting Pluralisms

## *Shiv Visvanathan*

**I**

In their current discussion of center-periphery models in anthropology, researchers must be sensitive to problems of intellectual production. Center and periphery are no longer frozen geographies but have been rendered fluid by the protean nature of diasporic flows. Yet a scholar working in the periphery might be caught in a time warp. When world revolutions are announced at the center, he feels like Rip Van Winkle, a belated entrant into an already established issue. If you mention world anthropology in India nowadays, you are referred to Veena Das's monumental *Oxford Companion to Sociology and Social Anthropology* (2003). It is a fascinating effort to compile a set of texts in order to create a textbook and evolve a consensus, both Indian and diasporic, about social anthropology. It captures the normal science of anthropology at its best. But the trouble with monuments is that they quickly become statues. In criticizing them, one feels like an unofficial sparrow, especially when what one conceives of as balletlike movements are caught in frozen positions.

Moving from discourse to institutions, we have Partha Chatterjee's slim but equally magisterial survey of the social sciences in India, sponsored by the Social Science Research Council in New York (Chatterjee 2002). Chatterjee's orchestrated effort to understand the institutional structure of social science is a superbly cosmopolitan work and is bound to affect policy and syllabi in years to come.

Yet the problem of center and periphery is reflected in the nature of these two works. Das teaches at Johns Hopkins University; Chatterjee

serves every year at Columbia University. Both are sensitive people, yet neither meditates on her or his location or particular mode of intellectual production. Chatterjee's article could easily be entitled "Key Clubs in the Social Sciences," so closely interconnected were his sources of information.

The irony of the center-periphery reflection is that this very problematic might itself be inherited, and even marginal reflections might actually represent the virtual marginality of floating professionals in a globalized world. What do you do when your problem and your problematic are themselves creations of the center? What do you add when influential reflections are already in print, one insisting it is a handbook and the other occupying space as a policy statement? One cannot disparage these efforts. These are outstanding intellectual maps, but in speaking of the territories of the mind, one can invent a different geography, a wishful space closer to one's own autobiography.

Anthropology is in many ways the product of a dissenting and eccentric imagination, a subject perpetually quarreling with itself. What I hope to do in this chapter is to look at "world anthropology" and "the politics of center and periphery" through dissenting lenses, but in relation to official views. From such a perspective, center and periphery become not parts of a reified anthropology of preemptive futures but cards in a continuously shuffled intellectual pack, or performers in a circus of epistemologies. What one senses then is not the hegemony of imperial thought but the restlessness of the anthropological mind. I want to try to link the marginal to the radical, dissenting, and eccentric imaginations to redraw this cognitive geography.

Anthropology in India can be read as a series of shifting scenarios. The debate moves over a variety of axes, including the colonial, the civilizational, that of the nation-state, that of civil society, and the global. Anthropology, in this context, becomes not only an official discourse summoning and inventing certain forms of "Foucauldian practice"—from the census and the survey to that great colonial creation, the gazetteer—but also a compendium of alternative dreams. Anthropology becomes a way not only of panopticonizing the other but of inventing a variety of playful others. Juxtaposing colonialisms as frames for anthropology, we have other colonialisms.

Anthropology in India is confident enough to go beyond the *ressentiment* of orientalism to pluralize the colonizing perspective. One must include within the standard narratives of colonialism the insights of people who might be dubbed "the other colonialists." They were not conventional imperialists reading the colony as a plantation or even

a culture to be preserved in museums. These other colonialists were comparative sociologists at heart. They discerned trends the colonizer had made recessive within himself and treated the colony as a site for the reinvention and recovery of intellectual possibilities in education, town planning, science, and politics. Not all British officials saw in India a site to be surveyed and ruled. For many it was a theater for alternative knowledges, experiments that had failed in the West. One thinks of Patrick Geddes, the first professor of sociology at Bombay University, who saw in India the possibilities of a post-Germanic university, and Albert Howard, who saw in his anthropology of agriculture a theory of a society based on a different attitude toward soils. One might even invoke theosophist anthropology, with its dreams of childhood, which worked toward a new reading of the Boy Scout, and of the occult child, which drove new notions of pedagogy for disabled children.

For many it was a theater for alternative knowledges, experiments that had failed or turned recessive in the West. I emphasize two separate arguments here. First, the discourse of the other colonialisms modified and created worlds parallel to the official space of colonial anthropology. While official anthropology mimicked the orientalist enterprise, participants in the other colonialism engaged in dialogue with the sociological ideas of the Indian national movement. For every Risley and Hutton there was a Patrick Geddes and an Annie Besant.

Second, the affable orientalism of Blavatsky, Besant, and Allan Octavian Hume was met by the hospitality of Indian nationalism. The anthropological confidence of the nationalist movement must be emphasized, especially when the nation-state today functions like an intellectual corset. Indian nationalists, even while attempting to overthrow the British, were always open to eccentric and dissenting imaginations. Indeed, Indian nationalism itself was a fascinating anthropology of the other, an imagining that extended right up to the debates of the constituent assembly functioning under the shadow of the Partition. India was like a compost heap, perpetually chewing ideas, in which nothing was lost or eternally defeated. Indian nationalism can be seen as a dialogic framework for world anthropology, for in it the Gandhian anthropology of the village contended with the traditionalist aesthetic imaginings of Ananda Coomaraswamy and E. B. Havell or quarreled with the occult anthropology of the theosophists and the Leninist vision of scientists dreaming of a society based on the scientific method (Nandy and Visvanathan 1997; Visvanathan 2001).

Intellectual historians have often presented these approaches as separate intellectual grids, when instead they were the warps and wefts

of a complex intellectual debate. It was a debate over questions such as, What is the role of modern Western science in Indian civilization? Can a dialogue between medical systems provide a different frame for policy? Can India construct a post-Germanic university that embodies a different idea of knowledge and not only reflects an Indian style but also makes a contribution to world knowledge? Bernard Cohn (1996) and other writers have emphasized the invention of colonial knowledge, from Hinduism to colonial law. They have ignored the alternative circuses of debate that created other possibilities for science, urbanism, tradition, technology, architecture, and agriculture. To use more recent terms, if colonialism pretended to be a world system, then Indian nationalism projected the possibilities of a world anthropology with its ideas of pluralism, diversity, and dissent. To sense the power and confidence of this dream, one need not go as far as Pannikar did when he requested that we allow pockets of Goa and Pondicherry to remain colonial so that we might continuously study the West in us. To understand what anthropology is, one must not follow the official "dictionary" level of discourse but must see the anthropological act as a shifter—something that changes meanings as it moves from context to context.

With the coming of independence in India, the epistemic circus of anthropology became more domesticated, more disciplinary, yet it was still full of memories of another world of debates.

## II

The debates that began with independence centered on a set of simple questions. Is sociology a discourse of, and oriented toward, the national state, or can it mediate between civilization and nation? Is sociology possible in a civilizational sense? Can one create a universalist sociology, or is sociology a particularistic exercise tied to certain unique institutions? Are sociological categories unique, universal, and translatable?

These questions drew three sets of responses, each a fascinating way of redefining the problem. Each response hyphenated sociology to a different subject, suggesting that the answer to the question might lie in the hybrid so created. The first and the most publicized answer was that of the French Indologist Louis Dumont, who still stands larger than life on the Indian scene. Dumont argued that the possibility of a sociology of India lay in a closer cooperation between Indology and social anthropology. The Dumontian problematic dominated Indian sociology and triggered one of the most cosmopolitan debates over

the issue of a "sociology of India" (Bailey 1959). The journal Dumont established with David Pocock, *Contributions to Indian Sociology,* became the dominant journal, displacing both the more quotidian *Sociological Bulletin* and the more regional *Eastern Anthropologist.*

The debates over a sociology of India constitute one of the most fascinating archives on the possibility of a world anthropology, but the terms of the debate and its "official" history marginalize two other fascinating answers to the fundamental questions. Both came from the most heavily intellectualist school of the time, and both have been marginalized or forgotten. The style and debates of the Lucknow school must be recovered, and I shall do so in two stages. First I outline the works of D. P. Mukherjee and Radhakamal Mukherjee, and then I elaborate the arguments of A. K. Saran. While the first two sociologists attempted to purify and translate the economic vision of Marx as a sociological enterprise, the third carried out a guerilla war against modernity and the dreams of a universal Western sociology (Gupta 1974; Joshi 1986; Madan 1994).

Members of the Lucknow school of economics and sociology took a holistic view of sociology. Indeed, the very boundaries between today's economics, sociology, and political science have confined them to oblivion, for their sociology constituted, to use Albert Hirschman's term (1981), virtually "essays in trespassing." The Lucknow school is remembered today for its three musketeers: Radhakamal Mukherjee, D. P. Mukherjee, and D. N. Majumdar. Saran, younger than they, could be seen as the spiritual D'Artagnan.

The roots of the Lucknow school lay deep in the anticolonial national awakening that exploded out of the Bengal renaissance as a literary and political outpouring. The Lucknow vision saw social science as a theater and a site for imagining a society struggling for national emancipation and against backwardness and poverty. Tacitly, it was concerned with the way Indian civilization and community responded to the nationalist project of planned development. Its founder, Radhakamal Mukherjee, noted:

> History was prized by me at the beginning of my educational career as a systematic study for the necessity of the glory of India, but face-to-face contact with the misery, squalor and degradation of the slums of Calcutta decided my future interest in Economics and Sociology… Ricardo, Mill, Marshall, Walker, Carver were not concerned with the problems of poverty at all, but did not these current textbooks of economics formulate certain problems that required understanding and

interpretation in order to analyze and alleviate Indian poverty? (Quoted in Joshi 1986: 8)

What Mukherjee tried to create at Lucknow was sociology as an institutional economics appropriate to Indian society. As a sociologist of Indian culture, he argued that "the postulates of Western economics were entirely different from those that could be deduced from a realistic study of Indian economic life" (Joshi 1986: 11). Mahatma Gandhi approved of Mukherjee's ideas as early as 1917. Citing Mukherjee with approval, Gandhi observed "that the principles of Western economics could not be applied to Indian conditions in the same way [that] the rules of grammar and syntax of one language could not be applicable to another language" (Joshi 1986: 11).

But the Indian nation-state was committed to planned development. Mukherjee articulated a sociology that provided a civilizational ecological view, an antidote to the Eurocentric approach to Indian economics. Embedded in the strategy of the Lucknow school were two approaches to sociology as a reinventing of economics. The first was what P. C. Joshi, in his fond memoir, called an "Asian Exceptionalism," a conviction that "we can no more alter the economic institution of a country than its language and thoughts" (Joshi 1986: 16). The second was a vision of socialist transformation oriented to Asian conditions. The latter struggle was articulated in the life and ideas of D. P. Mukherjee, R. K. Mukherjee's colleague. Joshi mentioned the difference in the two men's styles. Whereas Radhakamal Mukherjee was austere and simple, D. P. Mukherjee was a pleasure-loving cosmopolitan who loved food, cigarettes, and ideas, a coffee-house intellectual who was an authority on music.

D. P. Mukherjee was a quintessential Indian intellectual who saw in independence a challenge for intellectuals. He observed that "the French dared in 1789, the English in 1683, the Germans in 1848 and the Russians in 1917. For the first time in several centuries, India has a chance to dare" (Joshi 1986: 20). For D. P. Mukherjee, the issue was how a civilization dares to change, with its blends of tradition and modernity. Sociology for him was a collective biography of this exercise in change, a challenge made particularly acute because of sociology's affinity to Marxism. Marxism was a dream of a world-transformative anthropology. The question was, Could it blend with Indian civilization?

D. P. Mukherjee felt that Indian Marxists must induce a creative encounter with Indian civilization; indeed, he quoted Marx as saying that "the deeper you go down to the roots, the more radical you become." In his "Man and Plan in India," he wrote:

Marxism has to creatively mediate between Western values and Indian tradition. Thus it is that two systems of data are to be worked out. One is the plan, with its basic Western values in experimentation, rationalism, social accounting and in further Western values centering in or emerging out of bureaucratization, industrialization, technology and increasing urbanization. The other is not so much the Indian traditions as India's forces of conservation and powers of assimilation. At present they are not sharply posed. If anything, the first datum is gradually becoming ascendant... The second requisite is social action to push on with the plan and to push it consciously, deliberately, collectively into the next historical phase. The value of Indian traditions lies in the ability of their conserving forces to put a brake on hasty passage. Adjustment is the end product of the dialectical connection between the two. (Joshi 1986: 22)

D. P. Mukherjee recognized that Marxism was the most powerful critique of capitalism and exploitation but believed it was incomplete as a theory of value. What Marxism needed was a "spiritual restlessness," a framework that Gandhi provided. Gandhi was opposed to modern technological civilization because he saw it as a theory of both greed and need. He was convinced that the increasing and large-scale use of machinery was an engine of exploitation, and he opposed technology because it represented the negation of normal social order, which in Gandhi's view was based on the principles of wantlessness and unpossessiveness. Mukherjee believed it was this that Marxism lacked, because it rejected spiritual norms based on wantlessness and a code of conduct founded on self-control and prayer.

Mukherjee grappled with another profound difficulty that he could not solve. It dealt with time, and as critics such as Saran and Dumont have pointed out, it was something he struggled against futilely. Mukherjee pleaded poetically for "a concept of time that will not move along one direction, [but] that will not be cyclical. It will be neither Greenwich time nor the twinkle of Brahma's eye. He hoped that with this change from transcendental to human time, philosophy would become one with history." But as the ruthless Saran noted, "the new concept of time he proposed is a little too eclectic, not to say elastic." It was difficult to imagine how D. P. Mukherjee visualized a nondirectional time that was also noncyclical. The imperious Dumont noted this as the unresolved problem in Mukherjee's sociology. He pointed out that "one's recognition of the absence of the individual in traditional India obliges one to admit with others that India has no history, for history and the individual are inseparable; it follows that 'Indian civilization is unhistorical by definition'" (quoted in Madan 1994: 16).

D. P. Mukherjee's life, like his anthropology, remained a series of reluctances. Saran noted that the three world notes that beckoned to him—Vedanta, Western liberalism, and Marxism—did not mix. T. N. Madan, in his book *Pathways* (1994: 23), wondered what Mukherjee's autobiography would have looked like. I wonder what his prospective sociology would have looked like.

The Lucknow school lost out to the dreams of planned development. It was the London School, not the Lucknow school, that dominated independent India. D. P. Mukherjee and other sociologists like him were seen as dreamers, coffee-house aficionados in the world of technocrats. I shall explore A. K. Saran's ideas in section IV while discussing the sociology of modernity; space does not permit me to discuss the intellectual styles of N. K. Bose and G. S. Ghurye. What I need to confront is the sociological manifesto that marginalized their sociologies. We come now to the vision of sociology that Louis Dumont invented.

## III

If Max Mueller haunts Indology and the ghosts of Risley and Hutton still stalk colonial anthropology, then Louis Dumont is the specter that haunts Indian sociology. His *Homo Hierarchicus* (1980) is the greatest masterpiece of modern Indian sociology. The magazine he inaugurated, *Contribution,* determined the professional quality and the style of Indian sociology. As critic, irritant, foil, opponent, benchmark, and ancestor, Dumont triggered the fascinating "for a sociology of India" debate. But Indian sociologists, unfortunately, often read only half of Dumont. They often failed to complement his reading of India with his studies of the West. So while Dumont was often deaf to Indian critiques, Indians were equally one-sided in understanding his attempts to create what would be called a comparative sociology along the lines of Weber.

Louis Dumont initiated his studies of India as a mature scholar already respected by Claude Lévi-Strauss (Madan 1982: 405–18). Dumont wanted to go beyond an empirical, eclectic, or clerical India; he wanted an India that could be understood as a theoretical orientation. He also wanted to create an India that was an intellectual, civilizational other of the West. Indeed, he viewed India in civilizational rather than societal terms. Civilizationally, one could deal with wholes at the level of values, and such a perspective provided for comparisons. It was a vision of world anthropology that escaped ethnocentrisms.

One can understand this in terms of his magnum opus, *Homo Hierarchicus,* which was essentially a study of the caste system and its

implications. It was a thought experiment on a grand intellectual scale; it was not an ethnographic exercise or a historical study. For Dumont, ethnography and history were only instruments of elucidation. He complained that Western scholars saw the caste system through Western ideologies and spectacles. As a result, the study of caste became a victim of Western ethnocentricity. Caste was about inequality, but to oppose it mechanically to the Western idea of equality was, though perhaps politically correct, effete. To understand caste, one had to exorcise oneself of egalitarianism, individualism, and political economy. One had to go beyond the simplicity of equality to the grand complexity of hierarchy.

I cannot go into the critique of *Homo Hierarchicus* but must now present Dumont's idea of a sociology of India. The essence of the approach, as T. N. Madan noted (1982), was presented in his inaugural lecture at the École Pratique des Hautes Études in 1955 and in an abridged version of the lecture, with David Pocock playing Charles Lamb, in *Contributions to Indian Sociology* (Dumont and Pocock 1957). The two claimed that "a sociology of India lies at the confluence of sociology and Indology." Such an approach would allow one to apprehend the unity of India in a civilizational sense. That unity had to be theoretical and structural. It should not be diverted by an emphasis on isolated features or stumble on the phenotypical diversity that haunts superficial studies of India. Once we grasp India foundationally, "the empirical diversity recedes into the background and an almost monotonous similarity springs forth."

For Dumont, a sociology of India needed both the stranger and the native. The stranger, as outsider, looks at social facts as things. Yet he realizes that social facts are both things and not things and advocates a dualistic study of Indian society from within and without. Duality led to dialectics, and Dumont studied caste along with anti-caste, kingship alongside the renouncer.

The Dumontian project was seminal in its fruitfulness and the diversity of critiques it generated. Indeed, the sociology-of-India debate continues to this day, though it has narrowed in focus and fury. What began as a debate over the politics of knowledge and the need to construct India as something beyond an epistemic other for the West has sometimes degenerated into a sociology of the profession in India. At its best it produced the writings of Uberoi, Kantowsky, Madan, Saran, Khare, and Marriot (see particularly Kantowsky 1984; Khare 1990; Uberoi 1968). It was open, dialogic, and hospitable. The first critiques of Dumont's work held that his was a Hindu-centric sociology that had no place for the Christian or the Muslim. In their search for a theoretical unity,

Dumont and Pocock ignored the fact that the sociology of India must also be a sociology of Muslims, Christians, Parsees, and Jews. Part of this oversight stemmed from the fact that Dumont and Pocock posited unity as a theoretical imperative rather than as an empirical fact. They wrote that it was "essential that this unity be postulated from the outset," contending that "while this complicates our methods, it simplifies our principles and our objectives." Without this, they warned, "there will be no sociology of India except in a vague geographic sense."

There was a second sense in which critics saw the Dumontian project as partial. They saw it as "ahistorical," as emphasizing the concerns of a traditional rather than a modern India. They also considered that Dumont and Pocock, in their preoccupation with traditional India, completely ignored the growing importance of an increasingly developed modern India. Their vision of a sociology of India as a sociology of a traditional Indian society made them choose Indology rather than history as a partner of sociology.

Three other critiques were even more theoretically powerful. The first, by J. P. S. Uberoi (1968), raised the necessity of a reciprocal sociology. He talked of an Indian thought freed from its colonial influence—a *swaraj*-ist, or "self-rule," thought—and simultaneously advocated an Indian reading of the West, not just as an example of countercoloniality but as the beginning of a more playful universalism.

D. Kantowsky (1984), while supporting Uberoi's project of a *swaraj*-ist science, offered a critique from a different angle. He supported the need for a national school of thought to reformulate imported questions and concepts. He examined the need for an Indian sociology by leapfrogging through the *Contributions* debate. He asked rhetorically whether advocacy for an Indian sociology meant that there was also, for example, an Indian chemistry.

F. G. Bailey (1959) raised the same issue in his critique of Dumont. Criticizing what he dubbed Dumont's insistence on the uniqueness of Indian society, he held that "the unique is scientifically incomprehensible. There can be no Indian sociology except in a vague geographic sense and [no] more than there are distinctively Indian principles in chemistry or biology" (Bailey 1959: 88–101). Bailey, a sociologist, was of course unaware of the work of J. B. S. Haldane or C. V. Seshadri (1993), but that is another essay.

What Kantowsky challenged was Dumont's idea of unity. He observed that Dumont maintained that the unity of mankind manifested itself in a unity of thinking and that Hindus, like all other people, thought through distinctive opposites. He then cited Satish Saberwal's work

to show how the cellular universe, constituted around the normative autonomy of the *varnas* (castes), appeared to accept a both-and rather than an either-or logic (Saberwal 1983: 301–15). It was exactly this capacity of Indian society to absorb layer upon layer of new codes, and its corresponding ahistoric both-and logic, that Weber found difficult to understand and tried to fit into the either-or logic of his Western-type historical thinking and its Protestant model. A. K. Ramanujan offered a playful version of this issue in his classic article, "Is There an Indian Way of Thinking" (1999: 41–55).

Finally, the Dumontian project was criticized from a different angle—from the world of modernity. As André Béteille remarked (1993), "Dumont in fact has tried to vindicate a world that Indians have left behind, not one they were trying to create."

## IV

The writings of A. K. Saran provide an entry into the sociology of modernization in India. Saran's work was an imperious, unforgiving, and lonely critique of the sociological encounter with modernity. One can follow him best through K. P. Gupta's exegesis in his "Sociology of Tradition and the Tradition of Indian Sociology" (1974). Saran began by asking whether the indigenous system of tradition could produce, support, or co-exist with completely alien systems of modernity. He was specifically interested in understanding whether Western influence and the attendant internalization of the West could lead to the modernization of the Hindu tradition.

In broad terms, for contemporary Indian sociology, the answer was in the affirmative. Indian sociology rarely expressed an unqualified allegiance to Hinduism or indulged in a tactless celebration of Westernization. As Gupta remarked (1974: 34), "in the modernized consciousness of the Indian sociologist, tradition is fully compatible with modernity. It is strictly within this context that the synthesis and the hybrid have become the two most popular components of modernization theory in India."

Saran dubbed this tactic "a false consciousness," because it instrumentalized tradition as a tool for modernity and because it trivialized tradition by employing modernistic criteria to evaluate and understand tradition. Gupta added that the tactic had fatal consequences at an empirical and methodological level. He explained that a sociologist was restricted to fragments, morsels, or residues of tradition such as religious fairs and fasts. At a slightly deeper level, an Indian social scientist might express an

irrepressible joy upon seeing a fatalist Hindu peasant accepting fertilizers and pesticides. This act in itself seems to confirm all his predispositions about modernization of Hindu tradition. "At a methodological level, Hinduism is delinked from its institutional base and is studied only as a congeries of disparate acts and beliefs which can be used and abused in the process of modernization" (Gupta 1974: 34).

For Saran, Hindu religion and society were inseparable, but Muslim conquest and colonial rule had artificially bifurcated them. As a result, one saw evidence of apologetic patterns of "synthesis" and "adaptation." Saran argued that the only anthropologist who resisted this was Gandhi. But Gandhian anthropology was rejected in post-independence modernization and socialism, which accommodated it only as a humanistic counterforce to the transfer-of-technology regime. With a weakened tradition and a superficial modernity, India easily adopted colonial models of change, even when they were outdated.

Saran's scattered writings offered one of the original challenges to social science research in India. He established clear links between imperialism and social science research and was among the first to warn of India's pseudo-autonomy in the sphere of economic development and modernization. And he made one last crucial contribution. He saw center and periphery not as colonized and colonizing spaces but as hegemonies of time. He claimed that "modernity is a time-word, and with the idea of progress, 'life has to be lived on an upward slope.'" The hegemony of time was what marked the sociology of development. He complained:

> Modernity cannot be used as a quality-word without freezing a portion of history; and this absolutization of a fragment is so patently anti-historical that it is a miracle that no serious attention has been paid to this problem. If there have to be any absolute values validated by history, time must have an end; and time, for our modern sociologists, does have an end. To see this, one must know that though the sociology of modernization and social change pretends to be universal in validity and global in scope, it is meant exclusively for the low development countries (LDCs), and does not apply at all to North America and Europe. For them there may be problems of post-industrial, post-modern, or post-Christian society, but these are radically different from the problems and dilemmas of modernization in the low development countries. For European and American sociologists think in terms of alternative utopias, futurology, coping with future shock, constructing and coping with a cybernetic society, beating the ecological crisis (zero growth rate),

the death-of-God theology, counterculture, consciousness III and the greening of America. None of this has any real kinship or even affinity with the modernization of the under-developed countries of Asia and Africa except in honorific senses. (Saran 1975: 104)

But sadly, although Saran's was a brilliant challenge to modernity, he failed to build a sociology of tradition. As he became a lone voice, what dominated sociology was the rhetoric of "the modernity of tradition." Modernity, sociology, and nation building went happily hand in hand in the discourse of Indian sociology in the 1960s and 1970s. This happy but modest consciousness is what I want to discuss in the next section.

# V

The debates I have described so far painted sociology as an intellectual exercise meditating on civilizations and metaphysical issues of time, modernity, and epistemology. The 1960s saw an overall change in style. Sociology became more pragmatic, more professional, and more everyday. It exploded from being a marginal exercise performed by a few university dons into an experiment of nationwide proportions. This explosion is best understood by following the narratives of outstanding sociologists of the period such M. N. Srinivas and André Béteille. Both were superb storytellers.

In his various reminiscences, M. N. Srinivas recounted that in the colonial period sociology was barely tolerated as a subject in India. And even that bias, he explained, was a colonial inheritance:

To British academics, sociology was a foreign subject; its origins were in Europe and it was also associated with socialism. The Indian elites, educated in British universities, accepted these prejudices as a matter of course. It was significant that in Britain, sociology was established as an academic discipline outside Oxbridge, in LSE, founded by the Fabians, Sidney and Beatrice Webb. The first professor of sociology in LSE was Edward Westermarck, a Finn. (Srinivas 1994: 10)

He added that the change in Indian scholars' attitude toward sociology came with a switch in their models of excellence. From the 1960s onward, the United States became the country of academic excellence and a strong source of intellectual influence. Srinivas noted that "it was the Ford Foundation under Douglas Ensminger which sold the

whole idea of community development to Prime Minister Nehru. A few rural sociologists from the US visited India to advise the Planning Commission on how to promote development in Indian villages" (1994: 11).

The post-independence years saw the establishment of programs on South Asia at Chicago, Cornell, Pennsylvania, Columbia, Wisconsin, Duke, and California. Srinivas described the moment as an epochal one "which marked the discovery of India by American scholars, if we except a few odd but important figures like Norman Brown and David Mandelbaum" (1994: 11). These years also witnessed a substantial funding of Indian studies by the Ford and Rockefeller Foundations. In addition, a substantial quantum of research was done in the social sciences thanks to US development aid given under Public Law 480.

Of course Srinivas was quick to add that sociology did not develop as a result of external initiatives alone. Indian society, he emphasized, had established reservations for scheduled castes and tribes, an affirmative action plan that affected over 400 million people. The very fact of this process of social change and the need to make sense of it contributed to the popularity of sociology. Srinivas established the dominant department of the time at the Delhi School of Economics at the University of Delhi. The question one must ask is, What kind of sociology grew in this and other departments?

André Béteille (1993: 291–304), one of the first lecturers in sociology at the University of Delhi, explained that the goal was to create a comparative sociology—and sociology had to be comparative, or it was nothing. A sociology that abandoned a comparative approach tended to be either abstract and philosophical or narrow and eccentric. He said it was well known that the latter bias was found in American textbooks, and Indian students suffered for it. For Béteille, committed always to steering between extremes, "a comparative sociology is the best safeguard against excessive narrowness."

A comparative sociology, he believed, was also an intellectual and pragmatic challenge to colonialism, which had created an artificial barrier between "primitive" and advanced societies and a parallel split between social anthropology and sociology. He contended that students of society and culture had a special role to play in examining how far this separation was justified. An Indian seeking to understand his own society, he wrote, encountered a variety of social formations, from the simplest to the most complex. Given this, the division of labor between sociology and social anthropology was both colonial and constricting. And this division was most marked in US

universities, which inevitably consigned a social anthropologist to an anthropology department.

To Béteille's vision of a comparative social anthropology, Srinivas added an obsession with fieldwork. For Srinivas, sociology was an empirical discipline, and he saw fieldwork as integral to it. He combined this view with a structural-functional approach, the possibilities of which became known through an analysis of simple village studies. What structural-functionalism did as a method was to render unfashionable the explanation of Indian institutions such as caste and family by reference to the scriptures.

The house of sociology that Srinivas built was a happy consciousness rendered clearer by a tacit boundary maintenance between sociology and economics. In the early years, social anthropologists stayed away from policy and planning. The arrogant confidence of the economists at the Delhi School of Economics, especially Sen and Chakrabarty, reinforced their tacit acts of boundary maintenance. Finally, as Srinivas observed, the apparent relevance of sociology rose because of the popular misconception that it was a kind of social work.

Sujata Patel (1998), in her study of Srinivas, observed that his auto-biography, *Indian Society through Personal Writings* (1996), reflected a typically Brahman, middle-class approach to sociology. She noted the absence of politics in the work, remarking that "obviously the power of the colonial state or the protest against the nation-state that had emerged in and through the national movement had no influence on the family's fortunes" (Patel 1998: 41–69). As we meet Srinivas the fieldworker, we see someone who embodies the hopes and desires of the elites of a newly independent country. Changes are taking place in village India and are being praised for their benefits. As a fieldworker, Srinivas recorded "new techniques entering the village, oil mills being set up, bus routes being started. He [was] proud of the fact that one of his respondents was later able to buy a car and even gave him a lift as he was walking down the street." In a strangely predictable way, social anthropology became a collection of cautiously celebratory narratives describing economic development and emphasizing the resilience of caste. The village Srinivas described in his 1976 book, *The Remembered Village* was, like the Malgudi of the novelist R. K. Narayan, a transparently happy village. The gods were in heaven, Nehru was in command, and all was almost right with the world of village India.

It was this self-satisfaction with a professionalized social anthropology that came tumbling down when Indira Gandhi imposed the Emergency, a period of constitutional dictatorship, in 1975.

# VI

The sociology of the Emergency and its aftermath is not something one finds in official Indian sociology books. It does not appear in *Contributions* or the *Sociological Bulletin,* except for a brief mention by Srinivas claiming it was an aberration. The Emergency rendered effete the major institutions of Indian society, from trade unions and courts to the Indian university. Strangely, no major sociological reflection on it exists. Indeed, the finest study of the Emergency is still that of the Shah commission, a commission that was to investigate the "excesses" of the Emergency. Whatever competent sociology was done at the time was the work of journalists such as Arun Shouries, Kuldeep Nayar, and Ashok Mitra. Yet even they seemed haunted by the ghosts of the Frankfurt school; they constructed the Emergency through a theoretical miming of Adorno and Horkheimer. Indira Gandhi, however, was no Hitler, and India's Central Bureau of Investigation no Gestapo.

Sociology could offer little toward understanding the Emergency, but the reciprocal effect was devastating. The Emergency shattered the happy world of functional sociology and sociologists' odes to Parsons, Merton, and Levy in a way no Marxist critique could have done. It met the innocence and naïveté of an Indian social science bereft of any theory of dictatorship, institution building, or evil. It proved that sociologists, like most other Indian citizens, loved Indian democracy but had little knowledge of it beyond a few election studies. A society too preoccupied with development, modernity, and caste suddenly appeared illiterate about violence and democracy.

The shock of the Emergency forced social scientists to reexamine their Nehruvian commitment to modernity, nation building, and science. What emerged was a sociology that challenged the social contract between science, the nation-state, security, and development.

The sociology created by the Emergency and its aftermath emerged not in *Contributions* but in grassroots journals such as *Lokayan Bulletin.* The new sociological problematic was constructed out of the battles of human rights activists and feminist groups. From the Emergency and its commitment to the big science of the nation-state arose one of the finest critiques of science in the world, exemplified in the writings of Vandana Shiva, Ashis Nandy, Claude Alvares, and C. V. Seshadri. In an attempt to reinvent democracy, these critics emphasized, first, that the standard notions of citizenship were not enough to protect marginal people from the degradations of modern development. They highlighted the paradox that India had more refugees from development than from all the wars it had fought. Second, they recognized that a new sociology

of human rights was needed that went beyond the universalism of the UN charter. Third, they emphasized that nature and technology had to be reworked in a sociology of India. And finally, the critics stressed the new relations of science and democracy, which meant new demands on both. Democracy needed to encompass more than just participation, and science needed to exceed the mere diffusion of technology within India.

Strangely, this vision of sociology was captured best in the writings of a political scientist, Rajni Kothari, and a psychoanalyst, Ashis Nandy (Kothari 1989a, 1989b, 1989c; Nandy 1980, 1988). At the Center for the Study of Developing Societies in Delhi, the two created a new sociology in which they attempted to reinvent a vision of democracy, publishing primarily in journals such as *Seminar, Alternatives,* and *Lokayan Bulletin.* Marxists, structuralists, and functionalists had little to say about the violence of the Green Revolution or the new colonialism of development. Oddly, the debates over a sociology of India had little to add. This arena for sociology became the tacit prerogative of little magazines and grassroots movements.

One of the outstanding critiques of the Emergency was a new sociology of science that bypassed the world of Merton, Shils, and even Bernal. What emerged was a critique of epistemology as a value-neutral enterprise. Science as a method had to be linked to ideas of life, lifestyle, livelihood, life cycle, and life chances. This critique triggered an exploration of alternatives that created a more cosmopolitan sociology than the sociology of modernization, which was mirrored in the center-periphery model. A search for new paradigms and exemplars was on as Parsons and Merton gave way to Gandhi and Illich. It brought about two powerful readings of history. First, there was the search for an alternative science based on the works of Alvares (1992), Dharampal (1971), and even the old orientalist documents. This new reading of Indology and of the colonial documents met its counterpart in a new generation of historians called the "subalterns." Unfortunately, no real encounter or dialogue has taken place between the subalterns' reading of colonialism and the alternative histories of the critique-of-science movement. The subalterns, in a later move, did produce a series of studies in science, but the work of Gyan Prakash (2000) and David Arnold (2000) remains antiseptic next to the muscular subalternism of Alvares and Sunil Suhasrabudhey. An encounter between the two remains one of the major tasks of world anthropology.

Sadly, the dissenting imaginations of the post-Emergency period were easily domesticated. The possibility of an alternative science and

the critique by the feminists were absorbed, in a reductive sense, by the UN discourse on sustainability and the World Bank's and United Nations' discourses on development. There is a split-level sociology here that we must understand. In fact it was the economists, seeking to absorb the critique and humanize economics, who performed the exercise. One thinks in particular of Mahbub ul Haq and Amartaya Sen. Their idea of entitlements as social capital blended with the notion of sustainability and created the possibility of a better world for women and children in terms of education, nutrition, and quality of life. But such UN discourses ignored the wider plea of grassroots sociologists to incorporate the ideas of alternatives, the commons, or even cognitive justice and representation into the new charters of development. Generally, though, Sen and M. S. Swaminathan rested comfortably with Nandy and Kothari. The official and the dissenting, the state and the nongovernmental organizations, were easy fellows in consensus around the idea of sustainable development.

## VII

The celebration of civil society and alternative science that marked the 1980s and 1990s slowly came unstuck with the emergence of globalization. Globalization caught the sociology of India flat-footed. The writings of Nandy, Kothari, Das, and Madan have little to say about it. Partly it might be too protean a text for the postcolonialists to decode. The sociology of globalization in India has to rely on diasporic intellectuals such as Arjun Appadurai, whose studies of globalization have acquired a textbook imprimatur. Indeed, the position of Appadurai and scholars such as Dipesh Chakrabarty, Homi Bhabha, and Veena Das demands that we understand the diaspora both theoretically and in terms of case studies. These scholars' writings have helped create a postcolonial understanding of India, but it is still an understanding at a remove from India. It is an exercise created in Chicago, Sydney, or Sussex and then mimicked in Delhi and Bombay. In an ironic way, postcolonialism threatens to be a diasporic exercise, at least in terms of its creative centers.

But this only emphasizes the need for a more sophisticated study of the diaspora. Sociologically, the diasporic being, as citizen, must be differentiated from the exile, the migrant, and the refugee, who dominated the problematic of twentieth-century sociology. One must also study the long-distance nationalism of the diaspora, which not only fueled the violence of Punjab and Sri Lanka but also became a form of

consumption. A new generation of diasporics from India will soon be studying India on American campuses in Berkeley and New York. How will they construct India? Will the diaspora create a new orientalism subtler than those described by Edward Said? One already notices the technocratic fundamentalism that stems from Silicon Valley and fills the coffers of the Visva Hindu Parishad (VHP) and Bhartiya Janta Party (BJP). Or can the diaspora help create a civic internationalism transcending the compulsions of the nation-state? Where do members of the diaspora stand on a sociology-anthropology for India?

The sociological imagination as it exists in the global age thrives not in *Contributions* or even in *Economic and Political Weekly.* It exists at four levels and as four sets of possibilities. First, and for the first time, there is a regional South Asian imagination. One can move at least from the sociology of India to the sociology of South Asia, a change impelled by three forces. The first is an intellectual appreciation of the fact that Sri Lankan anthropologists have produced better and more sophisticated studies of violence and ethnicity than the almost mechanical narratives that Indians have produced about the Partition. Second, and similarly, the Nepalese sociology of water is a powerful challenge to its Indian neighbors. The third force is that Indian sociology, after years of obsession with the United Kingdom and the United States, suddenly senses dynamism and a diversity of ideas in South Asia. Its dissenting imaginations seem happily fraternal. There is a sense that ideas appear more playful closer to home. All this has led to repeated pleas for a South Asian university.

The second, and surrogate, imagination is a cinematic one. The anthropological imagination for once is no longer restricted to print. Cinema, which was always in advance of social science, has become a powerful index of the global mind. The works of Prakash Jha, Meera Nair, Mahesh Bhatt, Aparna Sen, and Gurpreet Chaddha create a new anthropological consciousness that sociology in print finds hard to match, except as a secondary act of deconstruction.

The third imagination is a literary one. Indian writings in English have become powerful anthropological imaginings, adding to the earlier analyses of R. K. Narayan, V. S. Naipaul, and Nirad C. Chaoudhary. The works of Salman Rushdie, Amitav Ghosh, Vikram Seth, and Rohinton Mistry provide a better understanding of the sociology of the middle class, the Emergency, and the new biculturalism that celebrates globalism than does the formal textbook sociology of globalism.

Finally, there is the new Dalit sociology, tired of the indifference of Srinivas and Dumont toward issues of violence and atrocity. It challenges

the ethnocentric definition of caste enshrined in Dumontian sociology by claiming tactically that caste is a form of race and that official India can be seen as engaging in apartheid. Debates in Durban, South Africa, where caste was presented as a form of racism, showed the power and imagination of the movement. But Dalit sociology still needs a more powerful critique of the modern and the postmodern.

Surrounded by these four imaginations is the new academic sociology that is struggling to understand terrorism, multinational corporations, ecology, the network society, the plight of marginals, and the sociology of disasters. One suddenly realizes that a whole generation has either retired or turned diasporic. The new sociologists have yet to emerge as clear exemplars or with new paradigms. Sociology suddenly appears silent or strangely imitative. Oddly, at a time when the sociological imagination floats freely across sectors, academic sociology in India appears mute or helpless. The twenty-first century arrived early in India, but its concepts seem sleepily outdated. One senses the need for a new D. P. Mukherjee or an A. K. Saran; indeed, one longs for a new conversation on a sociology for India. Until then, globalization is going to be a discourse conducted by diasporics and the World Bank about a strangely silent India. There is an emptiness here that we need to understand and confront.

# Part 4

# From Anthropology Today to World Anthropologies

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# The Pictographics of *Tristesse*
## An Anthropology of Nation Building in the Tropics and Its Aftermath

*Otávio Velho*

In 1982, the Swedish journal *Ethnos* dedicated a special number to the subject of peripheral anthropologies and the building of national anthropologies. In an evaluative epilogue, titled "A View from the Center," George Stocking Jr., the renowned historian of anthropology, commented: "Indeed, on the basis of what is presented here, anthropology at the periphery seems neither so nationally varied nor so sharply divergent from that of the center as the conception of 'the shaping of national anthropologies' might have implied" (1982: 180). Farther along, Stocking mentioned the failure of peripheral anthropologies to sharply "differentiate themselves or to present radical alternatives to 'international anthropologies'" (1982: 185). He concluded the article by saying:

> While such problems may be viewed as temporary aspects of the shedding of dependency, these resonances of the sense of malaise at the center suggest that the identification with "nation-building" has not enabled peripheral anthropologies entirely to escape involvement in the post-colonial "crisis of anthropology." What the outcome of that involvement may be is beyond the scope of these comments… What does seem likely is that institutional inertia will carry on a certain "business as usual" until the year 2000—at which point those of us who are still around may judge for ourselves. (Stocking 1982: 186)

Congratulating ourselves for still being around, now that the time has come it seems appropriate to make the effort to judge the situation,

as Stocking suggested. His article is a good place to start. Although it offers many points for discussion, I want to concentrate on Stocking's (frustrated) expectations, returning to his text after a brief digression.

In an article about the Brazilian philosopher of law Roberto Mangabeira Unger and his proposals for social and political reform, published in 1991—less than ten years after Stocking's article appeared—Richard Rorty compared Unger to none other than Walt Whitman. According to Rorty, the encouragement Whitman sent from the New World to Europe in the 1880s was comparable to what Unger was sending from Brazil, a Third World country (despite the fact that he taught at Harvard), to the rich democracies of the Northern Hemisphere at the end of the twentieth century. Rorty expressed the wish: "We hope to Heaven these imaginary institutions [proposed by Unger] do sell in Brazil; if they should actually *work* there, maybe then we could sell them here. The Southern Hemisphere might conceivably, a generation hence, come to the rescue of the Northern" (1991: 181).

Unger used the expression "Alexandrian figure" to refer to the kind of American intellectual Rorty himself identified with, that is, someone "still trying to be a liberal, but unable to repress his excitement over the rumors about the barbarians" (Rorty 1991: 184). For Rorty, such intellectuals must recognize that their familiar language games have turned into "frozen politics," serving to legitimate the forms of social life from which they "desperately hope to break free" (1991: 189).

Toward the end of his essay, Rorty insisted that "if there's hope, it lies in the imagination of the Third World" (1991: 192). My argument is that although Rorty expressed hope regarding what might be expected from the "romanticism" of Third World intellectuality, and Stocking expressed disappointment, even if momentary, both men's attitudes were "Alexandrian." Indeed, to Stocking's disappointment could be added Rorty's if he had recognized that, contrary to his own belief, Unger found his main audience not in Brazil but in the academic circles of the United States. We might say that besides being Alexandrian, both Stocking's and Rorty's attitudes were exoticizing and orientalizing ones, although always in a benevolent or even messianic way.

Today, we are apparently witnessing moments in center-periphery relations when the domination that calls for mimetic reproduction is being replaced by the agonistic demand for an "other" who can provide difference. At least this seems to be occurring in certain fields, among which is anthropology. Yet the communication of this change seems incomplete; it finds more resonance among intellectuals who are closer to the center and who take on the paradoxical role of self-proclaimed

rebels, as has already been pointed out in postcolonialist debates. In Brazil, however, this communication seems not to have been incorporated by the immense majority of intellectuals generally or by anthropologists in particular. What *their* expectations are in this case is another issue.

# I

But maybe this situation is not restricted to Brazil at all. At least this is what I understand from an article by Mona Abaza and Georg Stauth (1990) about the relationship between Islam and the West, the implications of which for debates over the Middle East and Southeast Asia can be put aside for now. Abaza and Stauth contest current approaches that see Islamic fundamentalism as today's functional equivalent to what Calvinism, according to Weber, was to the West. Such approaches have the revisionist purpose of admitting the possibility of a non-Western capitalist development. In contrast, the authors provocatively invert the positions, opposing the religious fundamentalisms of Western modernity and the secular fundamentals of modern tendencies in Islam. They identify, in the perspective they criticize, a tendency toward "going native" among both Western and local academics, in the name of a reductionist Foucauldian discourse that demanded (at the time) an "indigenization" of the social sciences in the Middle East and Southeast Asia.

   The important point for our discussion is that according to these authors, criticism of traditional orientalism contributed, paradoxically, to creating a new form of orientalism, triggering an attack against secular Third World intellectuals. Abaza and Stauth therefore inverted the Weberian interpretations (both orthodox and revisionist), according to which the West appears secular while the East remains religiously inspired. Fundamentalism, for them, is actually a product of mass culture, an oriental version of Western imagery of projected "religious spirituality" and an aestheticizing of the "Orient." Those who demand "indigenization" ignore the fact that the "local knowledge" with which they intend to construct an alternative has long been a part of global structures, participating in a "global game which itself calls for the 'essentialization' of local truth" (Abaza and Stauth 1990: 213).

   The authors question whether modernity in the West did in fact lead to a complete process of secularization. They also question to what extent Islamic fundamentalism might be, rather than a reaction to an excess of modernization and secularism, a reaction to "an incomplete and false transposition of religious language into the language of 'modernity'"

(1990: 216)—the language of a negated Christian fundament—that took place in the West. One might say that Islamic fundamentalism, with an intellectual finesse rarely appreciated, harbors a point of view that in the West—in contrast with the discourses of modernity and of Weber himself, together with those of a great majority of social scientists—would probably have been endorsed by none other than Friedrich Nietzsche in his critique of the secular masks that perpetuated the (Christian) negation of life, secular man being the "last Christian."

On the other hand, "native" anthropology did not produce alternatives to Western methodology, as Stocking concluded more generally when he mentioned the "resonances of the sense of malaise at the center" (1982: 186). Such native anthropology would have found itself too ingrained in the epistemological, methodological, and politico-ideological criticisms of Western science: "the 'indigenisation perspective' falls into the very trap of cultural globalisation against which it wants to stand up: the claim for cultural and scientific authenticity in local traditions is in itself a production of modernity" (Abaza and Stauth 1990: 219).

## II

If one takes the expression "orientalism" in a nonterritorial sense—as is becoming current—one can say that Brazil, too, has been the object of an orientalism. Indeed, Brazil experienced a peculiar second-degree orientalism, because the Portuguese colonizer himself was often taken as a kind of oriental vis-à-vis the actual West. The best-known Brazilian social scientist, Gilberto Freyre (1900–1987) may be pointed out, especially for foreign audiences, as an iconic figure in this matter. Freyre (1946) insisted on the Portuguese capacity of adjustment to a new environment and believed that the Portuguese did not sustain a typically European stance. But perhaps partially in reaction to this orientalism and the structures that supported it—together with other, general factors—an institutional apparatus that was meant to be a monument to modernity was built throughout Brazil from mid-1960s onward. It prolonged, though it gave a new turn to, the modernizing efforts already being carried out since the thirties at the University of São Paulo, which had led to a temporary (as we shall see) neglect of scholars such as Freyre.

This new apparatus—contrary to São Paulo's—was backed by the creation of a postgraduate system modeled on the North American one. It forced anthropology into a university environment based on a departmental structure. As a by-product, anthropology's ties with museums

weakened (even when departments were nominally connected with them) to such an extent that the new situation has been entirely naturalized, especially by the younger generations. One important consequence has been the avoidance of a confrontation with the issues involved, for good or for ill, in this arranged marriage with the university.

A large part of the generation that was responsible for this institution building graduated in the United States, Great Britain, or France. To make their project possible, they could count on the military governments established from 1964 onward, with their dreams of national grandeur channeled toward the development of science and technology. (This situation contrasts with what happened at about the same time in Argentina.) But such development also relied greatly on support from the United States (coming mostly through the Ford Foundation), which often was activated in the name of liberal ideals to counterbalance the military regime itself. In order to make the best of this peculiar, double-bind combination, a good deal of political ability and institutional engineering was required of anthropologists, which in a way heralded a double discourse to which I return later.

All of this resulted in an impressive intellectual and institutional apparatus, including scientific associations such as the earlier-established Brazilian Association of Anthropology (ABA) and the National Association of Graduate Studies and Research in the Social Sciences (ANPOCS). This development included, as well, a notable devotion to identities such as that of the anthropologist and to the discipline's classics.[1] It was this anthropology to which Stocking reacted in his 1982 article, evincing another of the double binds in which Brazilian "modern" anthropology has found itself. The discipline is at once modeled after central anthropologies and expected to provide solutions to the dilemmas of those central anthropologies.

We could, inspired by Foucault, consider in detail the questions of power involved in this double bind, probably following a more refined line of thought than the one Abaza and Stauth criticized. But even then we might not go much beyond what today is already common sense, risking an excess of complacency toward those who are supposedly dominated. Such complacency might end up revealing, in Nietzsche's terms, a spirit of resentment of the part of the supposedly dominated against those who supposedly dominate. It might be preferable to be more provocative, and I suggest that the irony of this situation (for both sides) should not be overlooked, because being provocative might offer greater food for thought than being complacent.

# III

The burlesque image of a colonized elite taking its tea at five is provocative, especially when the ritual was slowly being abandoned by those who created it. Brazil is certainly exemplary in this respect. We need only recall the powerful social and political influence among Brazilians of Comtean positivism, which lasted far beyond the time when, in France, it was reduced to the existence of a single museum (sponsored, it seems, by Brazilian funds). Roberto Schwarz (1977) coined the expression "ideas out of place" to describe the deep shifts in meaning that result from such transpositions of context, although the expression does not necessarily convey the organicity that these meanings attain in new contexts.

Would it make sense to view Brazil's universalist "modern" anthropology in such terms? If so, what would be the way out if we took into account Abaza and Stauth's remarks about the predicaments of "native" anthropology? These are difficult but crucial questions for a Brazilian anthropologist to consider. I highlight a few points in an effort to prevent the discussion from being abandoned or put aside, which still seems to be the dominant tendency.

Brazilian anthropology (or "anthropology in Brazil," should we prefer to emphasize universalism) has attained considerable social prestige (and size) since the 1960s. Its public influence in Brazil might seem unimaginable to colleagues from the "center." This influence pervades the media, the educational system, and even state policy—as in the case of the legalization of the use of some hallucinogenic substances when taken in ritual contexts. A popular saying in Brazil warns that "one should make no changes when a team is winning." But maybe this is exactly where we should begin, by examining more closely the success of the anthropological "team."

Abaza and Stauth resorted to Bourdieu to explain a going-native attitude on the part of Arab intellectuals. According to them, competition with Western colleagues in the 1970s and 1980s consisted of "establishing [Arab intellectuals'] own competence in a position of bargaining for the 'real'" (Abaza and Stauth 1990: 220). Access to information resources became important in this competition. The same thing happened (and still happens) in Brazil, but there, to a certain extent, the competition became a frozen battle, *faute de combattants*. This was due to geopolitical changes—to which the Middle East was, in contrasting ways, of course also susceptible—that should not be ignored. In Brazil the construction of the "modern" apparatus of anthropology in the 1960s coincided

with the great international attention paid to (and the tensions over) Latin America, especially Cuba. This attention prolonged an interest in Latin America, including Brazil, on the part of European and especially American scholars that had been growing since the eve of the Second World War. Foreign Brazilianists were salient characters in "anthropology in Brazil," serving mostly as role models. Today, however, attention to Latin America has decreased considerably—with some variation from country to country—not only in terms of foreign affairs but also, revealingly, in US academic circles. Instead of becoming competitors, Brazilianists on the whole simply vanished, particularly the seniors among them.

My point is that the Brazilianists' absence contributed decisively to preventing the construction of a nativistic anthropology—analogous to the kind of social sciences Abaza and Stauth mention—as an internal reaction. I also think, however, that the ghostly presence of the Brazilianists contributed to a more subtle development, which a Brazilian anthropologist might describe by saying, "We are loyal to 'universal' anthropology, but at the same time, as self-proclaimed natives, we insist on having a special knowledge and sensibility from which to consider and deal with Brazil."

This development was associated with an anthropology done almost exclusively within the country, an "anthropology at home," *avant la lettre,* which made possible the perpetuation of this ambiguous position, also a sort of double bind. "Comparison" was a key notion in establishing Brazil's necessarily contrasting position, but comparisons were usually constructed in binary and oppositional terms, with no systematic research carried out abroad to support them. Their purpose was basically only the creation of a counterpoint—a kind of "occidentalism" (Latour 2000b: 207). This produced a methodological conundrum that was overlooked and resulted in abstract—although in some cases very suggestive—generalizations and a neglect of possible convergences or symmetries.

## IV

I would like to illustrate Brazilian anthropologists' ambivalence by commenting on a text of questionable taste published in 1992 by the American anthropologist Paul Rabinow. He wrote it after a stay, in 1987, as a Fulbright visiting professor at the Museu Nacional (my institution) in Rio de Janeiro. In his article, Rabinow conveyed a certain orientalizing view, although disguised by a postmodern style that allowed him to

make commonplace observations from an apparent, but not very convincing, distance.

It is interesting to imagine whether today, after what has since happened in his own country, Rabinow would still, upon arrival at the airport, consider strange the attitude of Brazilian customs agents, heirs to our dictatorship. At times his orientalizing seems to be a product of misinformation on his part, and at times it seems to be a product of ignorance or a desire on the part of his informants—anthropologists included—to attend to his expectations. When he writes of "one of the many formerly coffee-covered hills punctuating the topography around which modern Rio has expanded and in whose newly wooded groves the syncretic *candomblé* cult is practised nightly" (1992: 250), he manages in a single sentence to exaggerate the number of hills in Rio that were covered by coffee plantations in the nineteenth century; guess wrongly that the woods on them represent recent reforestations; confuse *candomblé* from Bahia with other, less internationally known African-Brazilian cults; and ignore that at the time, those cults were in large part being replaced in the region by Evangelical (Baptist and Pentecostal) churches (although, in fairness, this was not yet much talked about). All of these orientalizing mistakes seem to legitimate the suspicions of Brazilian anthropologists regarding the competence of North American colleagues to understand Brazil.

But Rabinow also made other comments.[2] He described, for example, a visit he witnessed of four African social scientists to the Museu Nacional. He mentioned their attack on Brazil's supposedly harmonious racial reality and their denunciation of the dearth of black students and teachers in Brazilian universities (our postgraduate department included). He pointed out the defensive stance of Brazilian anthropologists on the racial issue, which to them was either a strictly socioeconomic problem or something of such cultural complexity and subtlety that generalization was impossible without previous consideration of the contexts involved. Rabinow also recalled the lack of interest on the part of anthropologists at the Museu regarding the Ford Foundation's suggestion that they organize a commemorative exhibit on the centennial of the abolition of slavery. He mentioned another episode—similar to one recounted by Lévi-Strauss in *Tristes tropiques*—when, during a dinner party in São Paulo, extremely sophisticated and cosmopolitan social scientists demonstrated nostalgia for paternalistic relations with their maids and performed the "sociologically objective absurdity of intelligent people lamenting their future maid-less state when millions upon millions of Brazilians are living near or below subsistence" (1992: 258–59).

Rabinow's comments could well be discarded as purely anecdotal and, once again, indicative of the foreign observer's lack of sensibility. This is what has usually happened, I myself having read his article only recently. Really, one need not be an anthropologist to make such remarks, and Rabinow himself admitted that he did not write an ethnography—as indeed the mention of an "objective absurdity" seems to confirm. In Brazil, incidentally, being an anthropologist can even get in the way when it comes to such matters.

It could also be pointed out that the African-Brazilian movement for equal rights did not attribute, at least at the time, much validity to the centennial of the abolition of slavery. But the truth is that Rabinow's commentary might be considered nothing more than simplistic if it did not touch upon some taboos. The text's lack of delicacy may have been a good pretext on which to ignore it, plunging it into great silence.

## V

Without intending to reach definitive conclusions, I suggest that Rabinow's outsider's point of view reveals the extent to which anthropology in Brazil created a *neo*-orientalism, despite its intention of being part of the production of universal knowledge. An interesting symptom of this is the way Brazilian anthropologists have rediscovered and celebrated Gilberto Freyre as an ancestor, after a period of ostracizing him for being insufficiently modern and universalist—a period that was probably necessary for the establishment of a new hegemony and during which "modernity" seemed to be an absolute criterion. Although the notion of nation building was originally cast in a universalist mood (Peirano 1980; Stocking 1982), such neo-orientalism has as its touchstone an evident connection with nation building, which is often presented in culturalist terms.[3] The anthropologists involved have class interests as well—much less admitted than their interests in nation building. Such class interests are reflected in a certain mannerism in which politeness and etiquette attain unprecedented importance, constituting what from an outsider's viewpoint might be described as a *triste* anthropology, like the tropics.

When Lévi-Strauss mentioned *tristesse,* his main reference was to the conditions of indigenous people's lives. To use the adjective *triste* to describe intellectuals could be a revealing extension—already implied by Lévi-Strauss—itself part of the confusion over who the "real" natives are. If we were to consider anthropologists, who are supposed to have special resources for dealing with the country, as members of the Brazilian

elite, we would have to decide what kinds of natives they are and what this dealing implies. Such ambiguity is so complex as to include the observers themselves. There is no doubt that, in a certain sense, colonial elites can appear more modern and secular than metropolitan ones, as Abaza and Stauth proposed. This is especially so if we consider that the colonial administrative apparatus in Brazil often found among us less resistance to putting into practice a modern social engineering project than was the case in that project's ports of origin. This is an important but often neglected point. Moreover, "ideas out of place" are so precisely because they constitute a radicalizing of the original models, making the colonial elites "more royalist than the royals."[4] Brazilian anthropologists, for example, espouse such strict affirmations of the universalism of the law and the separation of social realms that affirmative action appears to them a transgression of both principles, which in academic life are supposed to be represented by the absolute primacy of merit. At the same time, they combine universalism in a curious way with a neo-orientalism that denounces such actions as being (culturally) bred in the North.

An interesting contrast exists between the situation on which Abaza and Stauth commented and that of Brazil. In many Islamic countries, commentators denounce the incompleteness of Western modernity, which smuggles in its own intrinsic, nonmodern values and thus corroborates Bruno Latour's claim (1991) that "we [in the West] have never been modern." In Brazil, such contraband is not denounced, and whatever there is of the nonmodern in Western models is ignored. Such unrecognized nonmodern elements constitute true blind spots, for if we Brazilians acknowledged them, then we could not invoke Western models to legitimate our modernity. Our response is the opposite of that in Islamic countries, although the Islamic response may be a reaction to a similar tendency (as shown by prerevolutionary Iran). That many Western countries are monarchies or have state churches, for example, or that politics in the United States are permeated by religion, is little explored in Brazil. We end up searching for an abstract model of modernity with the ideological tenacity of a born-again Christian and without any pragmatism.

This is what happens, as I mentioned before, when Brazilian anthropologists embrace the "modern" principle of equality before the law. From that stance, they cannot take minority rights into consideration or recognize collective subjects. They ignore, in the name of universalism, the possibilities of affirmative action and positive discrimination, even when someone like Rabinow calls this a "sociologically objective

absurdity" in a country that has one of the worst income distributions in the world and where—to use data relevant to the theme of this chapter—the average salary of blacks with undergraduate diplomas (themselves a minority) is 64 percent of what is paid to whites of equivalent education. Despite Rorty's wishes, language games turn into frozen politics in Brazil, too. And even more alarming is that, like adults who learn a second language, we never really attain the necessary familiarity to manipulate such games or to assess the way others manipulate them, in order to face a world in crisis and undergoing a rapid change of civilizational models.

## VI

Anthropologists in Brazil seem to resist recognizing their complex relationship to modernity, probably in part because of the previously mentioned class interests, which make it difficult to objectify this profession of objectifiers—as Bourdieu proposed and Abaza and Stauth recalled. But this difficulty of Brazilian anthropology also results from the role that has been attributed to it, by its own practitioners as well as others, a role associated with nation building. This attribution takes place even in cases when anthropology's quasi-systemic connection with nation building is neither intended nor made explicit. It is the way anthropology is *received* in Brazil—which feeds back into its production—that is responsible for the connection.

The anthropological version of nation building made of itself a speciality—in various ways and with great performative effect—by valorizing native discourses to an extreme that set anthropology apart from political science, sociology, and economics. In the variant of nation building dominant in Brazil, in contrast with other cases (such as that of Argentina), group formation often takes place independently of the state or of political institutions in general. This opens a strategic space for anthropology. I believe, however, that in occupying that space, Brazilian anthropology did not proceed in the crucial and positive direction of simply letting grassroots phenomena lived by "informants" affect anthropologists. Although it took a populist turn (in the sense of the Russian *narodnik*s), it tended to freeze and reify grassroots discourses, to the detriment of analysis. This "going to the people" thus actually represents the paradoxical threat of impoverishing ethnography, and all the more so with ethnographers' recourse to technologies such as tape recorders and cameras (not to mention the internet)—easy mimetic solutions, but only in appearance.

The next stage in the development of Brazilian anthropology, following the populist turn, has been the (paradoxical) abandonment of the field itself, which ends up being replaced by general formulations in the name of "culture," this being connected to a certain fading of the "referent." Populism now proceeds—and this, too, is a crucial but paradoxical twist—to assume its much less attractive Latin American meaning (which may have always been there), in which anthropologists take the place of "real" natives.[5] These general formulations tend to reaffirm images according to which social groups perceive themselves, somewhat the way, in the United States, the picto(graphic) art of Norman Rockwell "portrayed Americans as Americans chose to see themselves" (Finch 1994: 5). Divergent discourses are transformed into variants of a single discourse, but still in the name of diversity. This occurs even when it means a step-by-step reduction to this single discourse, which does not lack sophistication. It is as if in the name of the nation we proclaimed its tutorship, and in the name of diversity we ended up domesticating the "other."

The transformation of the entire world into a homogeneous field, subject to the scrutiny of ethnography, is another aspect of an exacerbated modernism that should not be overlooked. Here the distinctions between the language of words (discourses) and the language of the body and (other) acts are granted little importance, with consequences for the purportedly valued "thick" descriptions. A skill is developed for the construction of the text that permits a double reading through a pattern (and an aesthetics) that increasingly leads to less ethnography and more interpretation, though the latter may be disguised as the native's point of view. No loose threads seem to be allowed, although these are typical of actual, ongoing research.

On the other hand, in the name of models that supposedly replicate those of the First World, government agencies impose upon researchers bureaucratic and time limitations inspired by the "hard" sciences. These limitations are almost unequaled anywhere else. And since this is done particularly through the postgraduate system, the problems involved in the arranged marriage with the university thus reemerge for anthropology, which, in Brazil, is a graduate degree. This is a good example of a domestic attitude of trying to be "more royalist than the royals," in this case through the replication of an "audit culture" (Strathern 2000) and a reified science (Latour 2000a) that find remarkable possibilities of combination with our own old-style bureaucratic tradition, which goes back to colonial times.

## VII

It is precisely because anthropology in Brazil reached a high level of organization that we should not be complacent toward it. It is evident that anthropology has contributed to the self-knowledge of a vast and complex country. The anthropologist, in this case, has become an informant to the public about its own society. This trend reinforces the production of digestible, mass-media anthropology. Here we distance ourselves from the "esotericism" of scientific communities noted by Thomas Kuhn (1970): the objective is a self-recognition of society that at the same time stands for its (re)construction. In this way the anthropologist becomes part of a great national project and is often envied by colleagues in other disciplines.[6] Anthropology in Brazil is almost a mass phenomenon, closer to North American models than to European ones, contrary to what Stocking supposed about its scope and to what Rabinow said about the predominance of a French influence in the country, which in fact has rapidly receded. At the same time, the coincidence of its reorganization and growth in scale with the development of a postgraduate system closely surveyed by the state has allowed for the maintenance of a high level of organizational and intellectual homogeneity.

The other side of the picture is that it is difficult to find in this anthropology a moment for elaborations of greater complexity. If space were made for such considerations, it would put us in the privileged position of discussing serious contemporary issues and dilemmas in dialogue with other groups and individuals who deal with them. But contrary to Stocking's belief, Brazilian anthropologists do not face the "malaise" and the crisis of anthropology. The critique of textual representation of the 1980s and 1990s, for instance, and further developments related to it, encountered strong resistance among us.[7] It did so not because we found other solutions but because recognizing problems and obstacles that might undermine the public authority of the discipline is (like the systematic use of literature from outside the discipline) not a part of our anthropology. Some of the critique was eventually absorbed, but it was relieved of its cutting edge in a "doing prose without acknowledgment" sort of way. It is difficult to find in this "Norman-Rockwellian" anthropology a place for that which might threaten it. It is revealing that people's sensibilities regarding such an enterprise are very acute, if not paranoid. The exercise that has sometimes been suggested elsewhere—that of not taking ourselves too seriously—is in this case entirely out of context.

But none of this is outwardly evident, for both public success and the legitimacy of the discipline depend on its projecting an image of strict observance of scientific objectivity. Such observance is often identified with a ritualistic reference to (and reverence for) the classics, which is the means through which anthropologists seek to distinguish themselves from their colleagues in other disciplines, as well as from media professionals.[8] Such rituals constitute our "tea at five," when, for example, we refuse to recognize the discussions—elsewhere very lively today—of the Eurocentrism of Weberianism. And it is this subtle combination of universalism and neo-orientalism that seems to confuse observers like Stocking, who complain of a lack of originality. It is necessary to underscore that the public of Brazilian anthropology is limited almost exclusively to the country but not necessarily to the discipline: hence the extreme importance of publishing books and appearing in the media. This inward orientation has, incidentally, served to create an autonomous system of evaluation of the national anthropological production and to minimize the domination of English as a lingua franca in Brazil in a manner that is difficult to recognize from the "center." The production of anthropological knowledge directed outside the country—but in this case restricted to the discipline—seldom constitutes more than an eventual by-product.

Perhaps this other side of nation building could emerge only in these contradictory times, when the myth of globalization assaults us, together with concrete entities such as transnational bodies and networks that constitute shared references that impinge even on what were until recently "local" issues, such as Indian land claims (Velho 2000). Only now has the cosmos of nation building—which certainly constitutes a vital force—begun to reveal more clearly its restrictions, which menace the previously mentioned success of the "team." The normative presuppositions of our activity now emerge disguised as scholarly common sense much more than in the case of the explicit search for the "good society" that political scientists inherited from philosophy. Room is made for other, vaster horizons.

Part of Brazilian ethnology—which is synonymous with the study of Indian groups—seems already to have escaped the constraints that bind anthropology to nation building and to the combination of universalism with neo-orientalism. This is significant because, although only a minority of Brazilian anthropologists actually engages in ethnological research, Indians have great iconic meaning for the country and for the discipline itself. Indigenous land claims have become, as in Australia, a key issue. Thanks to these new developments, Indian groups have

reemerged all over the country—not only in the Amazon but even in the urbanized South-Southeast and in the Northeast—and the Indian population (contrary to nation-building expectations), instead of disappearing, is increasing faster than the population as a whole. The same may be said of the group of anthropologists who study religion, most of whom are well aware—despite the "disenchantment" argument—of religion's persistent power of conversion and public presence and of its increasing diversity, contrary to scholarly common sense and also to many nation-building expectations (Velho 2000).

On the other hand, whereas for practitioners of empire-building anthropologies, doing "anthropology at home" may be a way of exorcising empire building, for nation-building anthropologies it is not necessarily liberating. It may become compulsively repetitious and self-referential when "home" is interpreted in a restricted sense. Not to mention that "home" can become a tricky notion in a country as vast, varied, and unequal as Brazil. After all is said and done, carrying the same imaginary passport is no guarantee of being closer to the "natives." In any case, the number of studies done outside the country by Brazilian anthropologists has begun to increase. There has been an equal increase in the number of anthropologists working outside academic circles (in nongovernmental organizations, for instance), who are exposed to other influences and networks. As perceptions change within society (and in politics), it is fair to expect that Minerva's owl will finally ruffle its feathers, even if, alas, in a reactive manner.

What can be substituted for this pervasive atmosphere of nation building that, being quasi-systemic, goes far beyond individual intentions or accomplishments? The answer, again, will most likely reflect geopolitics to an extent not usually recognized, as, for example, in Brazil's recently intensified relationship with Argentina and the rest of South America. Contrary to what Rabinow observed, Buenos Aires no longer seems more distant than Paris (Velho 1997). But one should also recognize that the "pure" science of anthropology in Brazil did develop thanks to the "impurity" of nation building, something even the military regimes (1964–85) did not overlook. Basic anthropological research was a sui generis kind of "application" (as in "applied" anthropology), paradoxically and uniquely inherited from more traditional intellectuals and essayists, such as Freyre. It is most notably unique in comparison with "applications" from other sciences, even though the term itself—"applied" anthropology—is still close to anathema among Brazilian anthropologists, as if to maintain a distance from "technology."

Brazilian anthropology competes more easily, however, with what comes from outside the country, giving anthropologists a surprising, although limited, advantage vis-à-vis colleagues from harder sciences. Yet in contrast with similar cases of the combination of purity and impurity examined by Bruno Latour (1987), in Brazil the combination made specialization difficult and ideologized the discipline's routine, for the "object" that was created by its activity was itself a rhetoric of the discipline, immediately communicable in the name of science and at the service of nation building. Thus, besides being a practice, this rhetoric also constitutes a real second discourse, although this discourse is directed toward a different public and is employed in a different context from the discourse of pure science and does not necessarily have the same agents as its main exponents. Through different channels and brokers it even constitutes a kind of "pop anthropology" as a strategic part of a "culture." Also in contrast to Latour's cases vis-à-vis the discourse of pure science, it is closer to *splitting* (as in Gregory Bateson's use of double-bind theory) than to *concealment*—which is exactly what permits it to gain a discursive form.

What can replace nation building? Human rights? The empowerment of subordinate groups? Concern with the environment or awareness of our part in it? Cosmopolitics? Technology? Global justice? A mix of some or all of these? Something else? And what will be the outcome of this replacement? Whatever the answers to these questions, they may not be a matter of choice, for one thing seems sure: the increasing spread of democracy makes it increasingly difficult for intellectuals to act as privileged spokespersons (indeed, in many cases, strictly spokes*men*) for societies that presume to formulate the relevant issues and disregard those that go against their set agendas. Not only might nation building be replaced, but whatever comes in will probably not occupy the same place and will probably be less mimetic of what Latour (2001) called "regimes of enunciation," which have to do primarily with religion and revelation or with group formation. It will refuse to mimic such regimes not in order to become "purely" scientific but, on the contrary, simply in order to be better able—albeit weakly from the viewpoint of science—to display the networks of which it (and the anthropologist) is a part and which in fact reach far beyond academia and are not restricted to the channeling of undistorted information.

Will we then maintain a splitting of the discourses of pure and impure sciences? Or will we take a turn toward the concealment of a second discourse, transformed into a muted practice? Perhaps the two discourses have been feeding on each other all along—lending, for

instance, political authority to science—so that we will finally have a single discourse, a middle path close to actual research practice, that supersedes not only the discourse of nation building but also that of a reified Science. This single discourse will not exclude all sorts of mixtures and the full recognition—at last!—of its connection to a collective in the wider sense of the term and the implications of the collective nature of its production for claims to authorship, individual or corporate.

Be that as it may, it is part of another story—a story that could conduct Brazilian anthropologists beyond the circle of chalk that has circumscribed us for so long, so that our double binds might prove more productive and provide practical, concrete answers to today's questions. The trick will consist, therefore, in revising the models of reference not only among us but, above all, at their main source. We might in this way cease being exclusively narcissistic specialists in our own society, a convenient role in which, it sometimes seems, some of our colleagues from the North also prefer to see us cast, as long as they have the last disciplinary word on the supposedly "neutral" forms of presentation. Instead of creating, once more, an orientalist distance from the central countries, we will be distancing ourselves from the dominant images we have of them. Such images have in fact prevented genuine proximity. For this we should adopt a stance of attentiveness and affordance (Ingold 2000) in place of a simultaneously nihilist and omnipotent social constructionist one, which purports that we can inscribe meaning in a world otherwise devoid of it.

At the same time, inspired by C. S. Peirce and Sherlock Holmes, we should substitute a cult of pictography for a symptomatology, a decryptography that collects evidence horizontally (or across the board, so to speak), thus disrespecting limits (Eco and Sebeok 1988). For this it will be crucial to observe other possibilities in the world to help break the hypnotic effect of the "occidentalist" models. Just as the opposition between "world religions" and "local religions" tends to become virtually obsolete in times of globalization, the same may happen with "world anthropologies"—taken in a restrictive and nongeneralizable way—versus "local anthropologies." So-called South-South relations—not necessarily in a spatial sense, for they could include, say, Siberia—might, it is hoped, have a vivifying and strategic (even therapeutic) effect in this respect. The change from nation building might also help overcome the risks of incommensurability. And all of this while taking full advantage—before we ourselves become Alexandrian—of the knowledge and human resources we have been able to accumulate.

Because I have already mentioned *tristesse,* I can recall what Spinoza described in the *Ethics* as affections of sorrow. Who knows whether henceforth we might not make a fuller transition from such affections to those of joy, which increase our power of action in the context of wider and richer attachments and mediations?

# Notes

1. Stocking, symptomatically, called attention to the fact that I capitalized "Anthropology" in an article of the same issue of *Ethnos* (Velho 1982), suggesting that although I mentioned a conflict between "local demands" and "Anthropology as a science," my capitalization assumed "the ultimate indivisibility of the latter" (Stocking 1982: 181).

2. Besides making indelicate remarks about the Brazilian colleagues who hosted him in the country, Rabinow meant his paper to be a *post scriptum* to Lévi-Strauss's *Tristes tropiques.* But the lack of finesse on his part (which includes the confession of his intentions) shows the limitations of his apprenticeship.

3. In his 1982 article in *Ethnos,* Stocking referred to the "anthropology of nation building," in contrast to that of empire building, a classification that has since become canonical.

4. This seems to be the case when, in countries such as Brazil, the politics of free commerce are pursued in a way that seems more purist than that of the central countries themselves. For instance, when outsiders transform *favelas* (shantytowns) into international tourist attractions, the Brazilian elites are caught off balance. They cannot understand how people from the First World might find value in *favelas,* which for them are a shameful sign of poverty and backwardness—again, the double bind.

5. Elsewhere (Velho 1982) I have discussed what I call "anthropological populism."

6. Parodying a colonial writer who discussed blacks, Indians, and whites, a political scientist commented that "Brazil is the political scientists' hell, the sociologists' purgatory and the anthropologists' paradise."

7. I think a creative appraisal of anthropology as a whole should reincorporate Gregory Bateson's work (1904–80), especially if we intend to make a nonregressive critique of culturalism. In this chapter I have only hinted at my appreciation of Bateson through my frequent references to his double-bind theory, suggesting that its application might be a way to pursue some of the topics here developed.

8. Anthropologists also attempt to use certain key "scientific" concepts, such as "culture," to distinguish themselves, but the very success of the discipline makes this increasingly difficult. Anthropologists lose control of (and the monopoly over) such concepts when segments of the general public incorporate and reinterpret them. In a recent television documentary, for example, the host asked an indigenous man the name of a dance that had just been performed and got the following answer: "This dance is called The Ritual."

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# World Anthropologies
## Questions

## *Johannes Fabian*

Could the topic of this book, and the symposium on which it is based, be reformulated as a series of questions? Do we need to think about world anthropologies instead of, or in addition to, world anthropology? Can and should the centers of the discipline give up power or avoid creating hegemonic structures that prevent world anthropologies from emerging? Should institutions that are located and individuals who are employed in presumed centers of the discipline be taken to represent world anthropology (in the singular), and if so, can they nevertheless be expected to further the cause of world anthropologies (in the plural)? Are there perhaps already kinds of world anthropologies being practiced to which disciplinary recognition should not be denied, although they define themselves largely against the dominant discourse and practice of anthropology?

If you—we—were to agree that these questions fairly sum up what anthropologists need to discuss, I would be bothered. Not because my catalogue of questions may be incomplete (and perhaps inaccurate),[1] but rather because it seems unlikely that the organizers, the sponsor, or the participants in the symposium could have responded to these questions negatively. Such positivity does not bode well for a critical debate.

I must confess that the organizers' statement about the symposium, because it formulated a program I could not but support, at first left me paralyzed; I wanted to opt out. Only after much agonizing did I begin to see the shape of a possible contribution. It occurred to me that our discussion could profit from our asking a number of seemingly simple,

yet fundamental, questions about anthropology. They are questions that cannot be answered yes or no, because they are not about whether this or that action should be taken or whether we think this or that development has occurred. Rather, they are intended to encourage examination of what usually goes without saying as each of us toils at his or her work.

## Who?

Who is (belongs to, represents) anthropology? A century ago a list of names, short enough to be remembered by those included, would have been a possible answer. Today, that list having grown enormously, it might still be possible to name names, but the result, interesting as it might be for, say, commercial promotion or governmental surveillance—for those who want to sell us something or keep an eye on us—would hardly be considered meaningful by those who are on the list. Not an accumulation of certified practitioners but rather agency is queried when we ask the "who?" question, which must be asked whenever we get together, as we did during the symposium, to discuss courses of action.

Who acts when anthropology is practiced? The history and theory of science seem to hold one ready answer: Agency in anthropology is located in its organization and recognition as an academic discipline. True, anthropologists have always found it difficult to recognize themselves in Thomas Kuhn's vision of "normal science," but on the whole we play along when we present ourselves collectively as a discipline. Critical reflection has repeatedly denounced as hypocritical not only the grand assertion that is named in the organizers' statement for the symposium—the "claim to be a universal discipline in spite of its Western foundations"—but also the fact that we have been living with fundamental contradictions between discourse and research practice.[2]

After decades of such critique, we are prepared to envisage another, more radical answer to the question of agency in anthropology. Although we may assume that to have been imperial is a predicament of all "Western" science, our discipline undoubtedly acquired a special role and status in this constellation. Historically and theoretically, our subject matter (an object *made* at least as much as found) has been peoples we represented as the Other. We may regret and lament the fact, as I do, that this crucial insight is in constant danger of evaporating in clouds of fashionable talk about "othering," but this should not make

us abandon a vision of agency that is in essence dialectical. Anthropology may be what anthropologists do (as someone from my teachers' generation once defined the discipline), but anthropologists do what they do by doing it with, and perhaps sometimes to, others. (Why this is not invalidated by "native anthropology" or by an "anthropology of the West" I will argue later.)

Empirically, we base our claims to validity on fieldwork, on direct interaction with those whom we study. Ethnographic authority may be said to rest on "having been there," that is, on our presence. But for what would our presence count if it were not matched by the presence of those whom we study? Neither presence, ours nor theirs, is a natural, physical fact (nor is intersubjectivity as a condition of communicative interaction); it must be achieved, and it is always precarious.

More recently, fundamental changes have been taking place. Literacy and modernity no longer function as demarcation lines between Us and Them. Take the example of history, which has occupied me in much of my work in the Congo. At one time, the project of African history might have hinged on data and methods that were considered to follow the rules and meet the standards of academic historiography. Now we have a situation not only in which anthropology draws on history as a complementary field (and vice versa) but in which both academic disciplines face popular history as a competing practice (Fabian 2001: ch. 4). Similar situations exist in other fields, notably in the study of religion, of the visual arts, theater, and dance, and of healing and medicine. All these one-time objects of anthropology are being recognized as subjects, as co-producers of knowledge. If demarcation lines between those who know and those who are known crumble, what happens to the "who?" that stands for agency? The very least that can be said is that any project to practice anthropology must take into account these transformations that have already taken place. There is no safe academic ground to which we can retreat to ponder our problems.

Which adds another twist to the "who?" question. More than once my own ethnographic work has led me to make assertions such as the following: "Who are we to 'help' them? We need critique (exposure of imperialist lies, of the workings of capitalism, of the misguided ideas of scientism, and all the rest) to *help ourselves*. The catch is, of course, that 'ourselves' ought to be them as well as us" (Fabian 1991: 264). This is why everything we practice concretely and specifically must be conceived dialectically as universal. It is in this sense that we should continue to uphold the ideal of anthropology as a universal science of mankind, undeterred by the sad fate that an abstractly conceived

universality may have had as an ideological construct in the course of our discipline's history. Such a view makes it legitimate to consider a seemingly contradictory notion, that of "world anthropologies," as an anthropological project.

## When?

Another assertion I would like to make is that the farthest-reaching transformation to affect anthropology in recent years has been its "temporalization." To say that this is something new may cause disbelief, given that anthropology's academic beginnings were defined by the paradigms of evolutionism and diffusionism—natural history and cultural history, respectively. But as I tried to show in *Time and the Other* (Fabian 2002a), evolutionism and diffusionism, as well as their successors—let's call them functionalism, structuralism, and cult-uralism—were suffused with spatial thought. Difference was conceived of as distance, and identity as systemic and "boundary maintaining" and hence somehow territorial. This went together with territorial conceptions of our practices of research as fieldwork, and it was all mirrored in genres of writing such as the monograph.

Among the outer, and obvious, signs of temporalization have been, apart from the professionalization of the historiography of anthropology, its historization.[3] This was initiated and accompanied (after timid beginnings with "ethnohistory") by cross-fertilization between the disciplines of anthropology and history and by proposals, first inspired by Marxist thought, to replace or complement our guiding concept of culture with that of practice.

Temporalization also occurred in the sense of anthropologists' devot-ing increased attention to time and timing, both in performances of culture and in the study of such performances (a connection that, at least in my mind, imposes itself by the need to establish coevalness, co-temporaneity, in research).[4] Even approaches that use spatial terms as their tags—James Clifford's "routes" (1997) and George Marcus's "multi-sited ethnography" (1995)—are symptoms of temporalization in that they advocate abandoning the single-site territory as the principal or only target of ethnographic research.

On the level of discourse, specifically regarding uses of theory and the never-ending debate over the meaning of culture, temporalization is adumbrated—though by no means worked out—in arguments that we should ask the "when?" question, and not only the usual "what?" and "where?" questions. Two statements will illustrate this. The first,

regarding theory, I formulated in an introductory talk at a workshop called "The Point of Theory," as follows:

> The question of the point of theory is almost always heard as a question of the *place* of theory... In hierarchical conceptions of knowledge, be they interpretive or explanatory, hermeneutic or classificatory, pleading for theory is a matter of claiming a place, usually above or upstream from that which becomes the object of knowledge. This positional quality of theorizing is too obvious to have escaped critique. Theory is then denounced as a token of power relations, of elitist, Western, male, and undoubtedly other forms of dominance. But if theory belongs to the things we do in the real world, then we must take a further step: Theory has no place unless it has time. In the real world *theory happens*...
>
> [We need] to reflect not so much on theory's place as on its time, that is, on moments in the production of knowledge leading from research to writing in which we must take positions; moments that determine how we get from one statement to another, from one story to another, indeed, from one sentence to another. (Fabian 2001: 4–5, 6–7)

The second statement I found in the introduction to a collective essay titled "Conversation about Culture" in a recent issue of *American Anthropologist:* "For decades now, culture has been a topic anthropologists argue about: *What* it does or does not mean; *if* it should or should not constitute a central concept of the discipline. This essay steps outside these arguments to rephrase the issue and our approach to it. It explores *when* it makes sense to use the cultural concept" (Borofski et al. 2001: 432).

The coincidence is striking. As far as I can see, however, Borofski's co-authors, though united by an anti-essentialist view of culture, do not explicitly address temporality in theorizing, except perhaps in suggesting that the turn away from the *what* may have been inspired by American pragmatist philosophy (2001: 441–42). None of the contributors seems to remember that a praxis-oriented conception of culture had been advocated decades earlier in a seminal but largely forgotten book by Zygmunt Bauman (1973), who went even one step further when he insisted on the rhetorical (I would add: and political) nature of debates about culture.

As I see it, one role that "world anthropologies" can play is to continue challenging our discipline's "central concept." Their chances to make a difference in such debates will increase as they keep asking "when?" questions about performative and historical timing. Of course it will

then have to be understood that our interest in world anthropologies cannot be in their becoming a "state of affairs."

## Where?

There was a time when the answer to the "where?" question, addressed to anthropological practice, would have been: elsewhere. If you were a student of anthropology, you would have been told to do your anthropology elsewhere; if you were a subject studied by anthropology, the knowledge gained from you would have been stored and used elsewhere. Much of this has changed; doing anthropology "here" has become acceptable. An increasing portion of work in our discipline is being done in contemporary societies by native researchers (see Jacobs-Huey 2002). Of course the ambiguous connotations of "native" make one suspect that being native to a Western society and being native to societies that used to be the principal targets of anthropology are far from the same thing.[5] This leads us to questions that touch upon the core of concern with world anthropologies.

Practicing anthropology can be a heroic task for, say, our underpaid African colleagues, working at ill-equipped institutions, having to make do with scarce funds, if any, for research or travel. In this and many other examples of academic life away from the metropolitan centers we may see inequality determined by location; this is certainly the way it is often expressed. Yet it is politics and economics, not location, that cause inequality. There is no such thing as a natural, geographical periphery. And who would doubt that inequality, perhaps in different forms or degrees, can—on the basis of gender, race, ethnicity, and so forth—be inflicted and experienced in places that count as central? Of course, pessimistically one may still fear that notions whose political natures hide behind quasi-cosmological façades (for example, concepts such as the world system and globalization) will continue to operate wherever anthropology reproduces itself. Suppose oppression and exploitation were to cease: would this necessarily make us stop thinking in terms of centers and peripheries?

Or take the generally accepted idea that posits the nation as a given and seeks to understand anthropology as consisting of a multitude of "national traditions" or "styles." The insight that, like every other scientific practice, our discipline works under cultural conditions and cultural constraints may count as a critical achievement. At the same time, it carries with it all the problems of a culture concept whose history (this is another insight formulated by Zygmunt Bauman) has

been fatefully tied to the nation-state as a political reality and as a provider of collective identity. Working toward "world anthropologies" we will have to face these questions. It will inevitably be caught up in a political struggle whose global prospects may give us cause for gloom and pessimism.

Matters look different with regard to nation and individual identity. What I see, or like to believe, is that anthropology has succeeded in making many of its practitioners into transnationals, that is, into scientists whose frame of mind is no longer set by an unquestioned national identity. Of course being transnational is an individual achievement inasmuch as it goes together with such things as accidents of biography, multilingual and multicultural competences, varied work experiences, and networks of close professional cooperation and lasting friendship. Still, individual transnationalism that reaches a "critical mass" is bound to have a collective impact, and that would be the "when?" of its significance for world anthropologies.[6]

The idea of a transnational subject (or agent) harks back to the "who?" question. It could also be relevant to the "where?" of anthropology. It opens up the prospect of alternatives to existing professional groupings and associations. We could envisage, or dream of, anthropological scenes, shifting but vital consociations resembling artistic or literary scenes that spawn creativity. We could begin to see ourselves practicing anthropology not so much in "fields" or professional territories as in *arenas* of agonistic intellectual work and play. Arena, at first glance a concept of space, is always also a concept of time: of events that take place, of moments of intensity not easily reached outside an arena. You may point out that arenas already exist in the form of symposia such as the one that prompted this book, and we all know how important professional meetings are for experiencing our discipline as real and for keeping it alive. As something that deserves to be promoted, scenes and arenas should open a perspective for world anthropologies. Should world anthropologies perhaps be envisioned as events and rallies more than as institutions or organizations?[7]

Finally, if a decade or two ago discussions of anthropology's space would have concerned themselves with territories, fields, and center-periphery relations, today we must come to grips with the virtual space that has been opened up by the internet. As yet, scholarly attitudes toward the new medium remain ambiguous. The internet has been celebrated as the dawn of an age when information and knowledge will be democratized; it has been condemned as the end of literate intellectual life as we have known it through the ages. But here as

elsewhere the proof is in the pie. What can we *do* with the internet? My own practical experience—and many others will probably recognize this as their story, too—began with word-processing, a great advancement for someone who had never been able to think at a typewriter. It proceeded to email, more by the force of circumstances than by choice; regular mail had suddenly dwindled to publishers' catalogues and administrative ephemera from my university. Then I graduated to search engines and online library catalogues (I still spend very little time surfing and none at all chatting). It was not until a colleague and I claimed a place in virtual space—a website—that I began to see some of the ways in which the internet bears on the "where?" of anthropology, practically as well as theoretically.

Certain practical consequences of the internet are obvious; we need only consider how making contacts and circulating information worked for our "World Anthropologies" conference. We can be there for each other wherever we are at the moment. (This, though, should not make us ignore some less obvious consequences. Who is excluded, or at least disadvantaged, without access to the internet? And are speed and facility of communication unequivocally positive for intellectual work that takes time?)

Less obvious but more exciting are possibilities offered by the internet that are bound to affect, for instance, our ethnographic practices. I am not referring to the data collection and storage made possible by the use of a laptop in the field or the application of programs for data processing and analysis at home, none of which is necessarily dependent on the internet. What I would like briefly to report on is a discovery I made in the course of setting up a virtual archive of texts on a website devoted to "Language and Popular Culture" (www2.fmg.uva.nl/lpca), a project under construction and still embryonic. The project has special significance for language- and text-centered anthropology, but I believe it is more generally relevant, given that ethnography, irrespective of the approach taken, is mediated by textual material. My discovery is that a virtual archive creates a new and different kind of *presence* of texts—the "where?" question again—that changes conditions for a practice we are all involved in: ethnographic writing and publishing. Reflecting on such changed conditions has led me to predict that an-age old genre, the commentary, is likely to emerge in a new form (see Fabian 2002b).

The presence of ethnographic texts on the internet is of course not limited to "presence for us" (the virtual archive is in principle accessible to anyone); many of those whom we study have claimed their place on the internet in websites and chat groups and are now producing corpora

of texts, and we begin to see dissertations based on ethnographic data of this sort (for instance, Franklin 2001). Anthropology's place (or places) in virtual space is far from well understood (especially if we consider that, besides texts, virtual archives can also accommodate audio and video recordings), and it is certain that it will need our constant critical attention. But it can hardly be doubted that this new kind of "where?" will have to be considered in a project of world anthropologies.

## What?

As questions accumulate, it becomes increasingly difficult to keep them separate. If our observations on changes in anthropology that appear when we reflect on "who?" "when?" and "where?" have any merit, and if the questions are indeed connected such that many of the straight answers we have been accustomed to are no longer valid, then we should be prepared for the unexpected when we ask for the "what?" of anthropology.

It has become difficult to put oneself back into the frame of mind of our predecessors, who were certain that anthropology had an object that it had found rather than made: primitive (later traditional, premodern, developing) society/culture. That societies labeled primitive were also the targets of imperialist expansion made anthropological knowledge desirable at first and suspect later, when the same processes that had been set in motion to colonize primitive peoples resulted in the end of direct colonization. Conditions for fieldwork changed, and so did the object, or objects, of research. The "when?" began to play a decisive role in determining the "what?" when recognition of the co-temporaneity of all societies or cultures dawned on us (for epistemological reasons) or was forced on us (by technological and political developments). I would even maintain that recognition of co-temporaneity, gained as anthropology was decolonized, enabled our discipline to turn its attention to modern society: "Studying our own societies" is the telling phrase describing this turn. "Telling" because, though it may be Eurocentric most of the time, it could also be a sign of a thoroughly changed understanding of the "who?" of anthropology (remember what I said earlier about "us being them").

Radical as some of these transformations may have been, they seem to have left the agenda of traditional ethnographic subjects (another aspect of the "what?") untouched, notwithstanding important additions such as gender, literacy, material culture, the media, and ecology. Myths and cosmologies, religion and ritual, magic and witchcraft, chiefship

and clans, kinship and alliances, gifts and exchange—all these persist or are rediscovered as unexpected practices of modernity. Does that mean that anthropology is unreformed in maintaining a metropolitan gaze, an Enlightenment perspective inspired by what G. Gusdorf called the "myth-history of reason?"

Reflecting on my own work, which has included studies of language, religion, work, visual and performing arts, and collective memory, I sense a shift that may be of interest in a debate over world anthropologies. Our discipline, which started out as the study of diversity and similarities of human *life* (hence the emphasis on custom, tradition, structures, and systems), has become a science of forms of human *survival*. This is why practice seems more appropriate as a key concept than culture; why concepts such as strategies and projects seem to fit the actions we observe better than habits and schemes; why resistance often describes collective action better than conformity; why production and innovation interest us more than reproduction and tradition, politics more than aesthetics, hybridity more than purity or authenticity, interaction spheres more than territories and boundaries. I do not want this to be misunderstood. Hammering on one side in these oppositions can be as much a bore as hammering on the other. Culture, tradition, identity, authenticity, and so forth are bound to remain perennial subjects of inquiry, if only because we need them to maintain a dialectic tension without which anthropological inquiry into the most timely subjects would relapse into positivist routine.

Nor is all this only gratuitous juggling with possible oppositions. There is a time (a "when?") that, to return to an example from my own work, may require arguing for a concept of *popular* culture against culture *tout court,* because the former makes us discover cultural creations to which the latter left us blind (Fabian 1998). After all, it is indisputable that, as ethnographers of African cultures, we once paid little or no attention to contemporary African music, theater, or painting, and that we do so now. I am sure that most readers could come up with other examples.

World anthropologies, as they were envisaged at our symposium, seem to be predestined for the study of human life as survival. Social and political commitment almost dictate this change in perspective. Commitment is to the individual anthropologist's credit and may have effects, in his or her sphere of action, on the way anthropology is done. Still, we should at the same time have in mind changes that may be farther reaching because, and to the extent that, they redefine the "what?" of anthropology.

# How?

To introduce my last (well, next-to-last) question, it is perhaps useful to remember that, historically, the "how?" question was among the first to be posed. This happened at the beginning of a period of reflection in our discipline that eventually brought us together to think about world anthropologies. A radical critique of the ethics and politics of anthropology may have been the most immediate and widely shared response to its postcolonial predicament. The most lasting achievement, however, has been the insight that we could not hope for changes in the "what?" unless we considered and changed established views of the "how?" of anthropological practice.

In my view, this was the moment when our discipline stopped taking free rides on theories and methods developed in other fields (or just clinging to confused ideas such as that of participant observation as our distinctive "method"). We became serious about epistemology, that is, about the specific conditions of producing anthropological knowledge based on empirical work that we call, not quite appropriately, ethnography. This is not the place to recount in any detail what has happened in this respect since the late 1960s. Let me just mention a point of departure that was crucial and then briefly outline a series of steps or turns that led to the present state as I see it.

The beginning I spoke of came when we realized that in the kind of empirical work we had come to see as distinctive of, and fundamental to, our discipline—field research—we never simply collected information. Ethnography is a product of interaction, with speaking as its major, though not only, medium; it is dialogical. What we take away from research as "data" is only sometimes found; most often it is made. As documents of communicative events, our material is never simply grist for analytical mills. Documents must be interpreted, placed in context, and weighed according to the circumstances and conditions under which they were produced.

This must suffice as a brief reminder of the moment when we took our distance from positivism and scientism. Other steps followed. Emphasis on communication and language-in-action made us realize how much of cultural knowledge and hence of ethnography is performative; what we learn often does not come as responses to our questions but is enacted in, and mediated by, events that we may trigger but cannot really control (every simple interview is such a performative event). Similarly, concern with interpretation and hermeneutics made us aware of positivist naïveté regarding the relations between research and writing

in the production of ethnographic knowledge. At its best, this "literary turn" made us more scientific because it sharpened our awareness of the epistemological significance of presentation and representation. In sum, paying attention to the "how?" question helped us to see more clearly than ever before the "when?" of our work—its historical contingency—as well as the "who?"—that is, the roles authority and power play when we pronounce our discourse about those whom we study.

The epistemological revolution I just sketched in its barest outlines has at least one important practical consequence for the project of world anthropologies. Though this may meet with reluctance and opposition (see the sustained debate over the "scientific" nature of anthropology in *Anthropology News,* for instance), we must admit now that the "how?" of our discipline is no longer guaranteed by a unified "scientific method." Or, if there is a method, it is no longer conceivable as ahistorical or apolitical. And that means that hegemonic, metropolitan interests can no longer wield an instrument that was all the more insidious because it could be paraded as neutral and objective. The epistemological revolution has given us spaces of freedom in which we can move innovatively, productively, and even playfully (something, however, that we should not confuse with the pseudo-liberal arbitrariness that goes under the name of postmodernism).

## So?

The conveners of our symposium ended their programmatic statement with half a dozen questions to which they asked participants to reply. I responded to their questions with mine because I had no ready answers to theirs. To say something nontrivial about world anthropologies, I first needed to recall and rehearse what I know about, and how I experience, anthropology. I now would like to add a few autobiographical remarks.

After having been socialized in Austro-German ethnology, I became a survivor of North American four-field anthropological training in the 1960s. Then followed more than a decade divided, albeit unevenly, between teaching in decidedly metropolitan institutions in the United States and in a department of the National University of Zaire, which was then in full sway of decolonization. When, after another five years at an elite college in the United States, I moved to the Netherlands, I encountered for the first time a situation in which Marxism was the orthodoxy. To me this was so unusual that, initially, I failed to recognize

that it was the case. This was good because it allowed me to stay aloof of ephemeral doctrinal disputes. Aloofness continued to be the most practical attitude throughout my years in the Netherlands. Neither my American training nor my German citizenship endeared me to those of my colleagues who searched for, or had found, their intellectual home in a distinctly national anthropology (understandably so; after all, Dutch anthropologists, though not under that label, had been among the avant-garde of our discipline when it still had close links to the colonial enterprise). But I should also say that I never encountered the slightest obstacle against orienting my work internationally—though I do not know whether that was due to tolerance or to indifference.

What made me embark on this retrospective of practicing my profession in the center, at the periphery, and (as I remember putting it) in a center of the periphery was to prepare an answer to one of the organizers' questions: No, I do not think the concept of a matrix adequately describes the current state of world anthropology. Not only is it inadequate, it is inappropriate. National and international, peripheral, central, and intermediate practices of anthropology do not relate to each other taxonomically (which the concept of a matrix implies to me); they do not make up, nor do they fit into, a system or paradigm. Anthropological practices happen in events and movements. They acquire their collective identity not by subscribing to a single discourse but by having to face a common predicament: they must let themselves be constituted by facing a world that is non-anthropology.

This is not the same as what we mean when we discuss anthropology's "object(s)." I deeply believe that a realistic view of our discipline must acknowledge that our kind of science is practiced in the presence of other kinds of knowledge production. These other kinds of knowledge production are not limited to other academic disciplines; they include the discursive, performative, aesthetic, and political practices of those whom we study. What enables us to communicate with and represent other practices is not (only) our command of contents, which count as data, or our findings, which count as the results of research; it is our ability to converse as knowers. And that conversation includes confronting each other, arguing with each other, negotiating agreements, and stating disagreements, as well as conceiving common projects.

Let me, before I end, attempt to clarify the idea of world anthropologies conceptually. Like the symposium conveners, I did not start out with a definition; I joined an ongoing discussion. Such a break with rules of argumentation ("define your concepts before you use them") may raise objections or at least give cause for concern. Of course, one may point

out that such "classical rules" have never ruled natural conversation, nor have they been adhered to in post-Kantian philosophy. Still, even when attempting a generally accepted definition of a concept is a hopeless task (the debate over "culture" in our own discipline offers a vivid example), it makes sense to strive for a common understanding of a concept in a given debate. Here is what I suggest.

We should take "world anthropologies" as a floating concept, one that cannot be anchored in a system; if it could, it would lose its interest and usefulness. Why this is so and must be so has to do with the fact that the only conceivable function of a concept such as "world anthropologies" is to conceptualize practices. As practices, world anthropologies are themselves floating. Everything I believe to have understood about them suggests that we use the concept in order to formulate a vision of anthropology that makes *place*—be it geographical or geopolitical location, rank in a hierarchy, position in theoretical and methodological schools, occupancy of fields and territories—if not irrelevant then at least questionable, and hence a target for resistance as well as a source of innovation.

If you are still disturbed by the notion of a floating concept—one could point out that ill-defined concepts are the tools of demagogues— let me try one last tack. First, floating is not the same as ill-defined; it is in contrast to something like fixed or grounded. Of course we have the responsibility to be as clear as possible about what we mean when we speak of "world anthropologies," but clarity should not be sought by recognizing a single instance (such as "science") that might relieve us of the burden of critical reflection by giving us context- and power-free rules. At any rate, it is always legitimate to adopt strategies that make virtues out of vices; perhaps the term *floating* has undesirable connotations and we should adopt a less provocative label, such as "mobile." Perhaps—as long as this does not make us lose sight of our aim, which is to understand and promote practices that contest hegemonic claims based on power rather than authority (claims that always confuse *their* world with *the* world).

That "planetary" anthropology should be realized through anthropologies whose relations with each other are neither hierarchical nor hegemonic could appear a utopian project, something that by definition cannot be reached. "World anthropologies" describes a state that already exists on some levels of discourse and practice. That it does not—and cannot—encompass other levels as long as power, prestige, and control of funds are the predominant motives for making alliances should not discourage us. I remain optimistic because I have seen that our discipline

has been open (or, if you prefer, vulnerable) to critique and because, so far, I have reasons to believe that critical anthropology has proved that it can be productive.

# Notes

1. What is the role of existing national and supranational associations in all this? I decided this question should be added to the list after I had the opportunity to observe a recent meeting of presidents of national and international anthropological associations (sponsored by the Wenner-Gren Foundation) in Recife, Brazil, in June 2004.

2. I tried to demonstrate this point in *Time and the Other* (Fabian 2002a).

3. In January 1986 a Wenner-Gren conference met in Fez, Morocco, to consider what historization would do to the concept of symbol. The results were edited and introduced by Emiko Ohnuki-Tierney (1990), who also took stock of the historization of the culture concept (2001). This is not the place to argue about why temporalization and historization are not the same thing, except to say that the former is, in my view, more directly relevant to the topic we discussed at the "World Anthropologies" symposium.

4. For an example of how this increased attention to time and timing has also reached textbooks in anthropologically oriented linguistics, see Hanks 1996.

5. Being a native of "Western society" exemplifies a predicament that may be shared, though not in every respect, by others (Japanese, Chinese, Indians, Latin Americans—the list should be at least as long as there were nationalities represented at the Wenner-Gren symposium). And that leads one to question whether the term "native" is of much use, if any, in debating "world anthropologies" unless it is concretized, as I try to do in what follows.

6. After writing this I discovered that there is a professorship in transnational anthropology at Oxford University, held by Steven Vertovec. See his recent book on cosmopolitanism (2002), which I have not yet been able to consult. My hunch is that this new specialization is concerned more with transnationalism as an object of study than with transnationalism as a characteristic of our discipline.

7. This vision differs from, or at least complements, proposals to "institutionalize" international anthropology that were discussed at a forum during the 2001 meetings of the American Anthropological Association, as reported by June Nash (2002).

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