A New History of Anthropology
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Notes on Contributors

Robert Ackerman ("Anthropology and the Classics") is the author of *J. G. Frazer: His Life and Work* (1987), *Selected Letters of J. G. Frazer* (2005), and *The Myth and Ritual School: J. G. Frazer and the Cambridge Ritualists* (2002), as well as numerous essays on the history of anthropology and the history of classical scholarship. Until 2000, he was Director of Liberal Arts at the University of the Arts in Philadelphia. Now retired and living in Britain, he is a Life Member of Clare Hall, University of Cambridge.

Regna Darnell ("North American Traditions in Anthropology: The Historiographic Baseline") is Professor of Anthropology and Director of First Nations [Native American] Studies at the University of Western Ontario (Canada). She has published extensively on the history of Americanist (including Canadian) anthropology and linguistics, Native North American languages and cultures, and cross-cultural miscommunication. She co-edits a series of Critical Studies in History of Anthropology and the *Histories of Anthropology* annual for the University of Nebraska Press. She chaired the 2002 American Anthropological Association Centennial Commission, is a fellow of the Royal Society of Canada, a member of the American Philosophical Society, and received the American Anthropological Association’s Boas Award in 2005.

Thomas F. Glick ("The Anthropology of Race Across the Darwinian Revolution") is Professor of History at Boston University. He has written extensively on the comparative reception of scientific ideas, particularly in the Spanish-speaking world and Brazil. He is editor or co-editor of *The Comparative Reception of Darwinism*, 2nd edn (1988); *The Reception of Darwinism in the Iberian World* (2001) and *A recepção do Darwinismo no Brasil* (2003) – all volumes to which he also contributed chapters.

Anna Grimshaw ("Visual Anthropology") is Associate Professor in the Institute of the Liberal Arts, Emory University. She is the author of *Servants of the Buddha* (1992) and of *The Ethnographer’s Eye: Ways of Seeing in Modern Anthropology*
With Amanda Ravetz, she is co-editor of *Visualizing Anthropology: experiments in image-based ethnography* (2004). She is currently working on a book, *Rethinking Observational Cinema*.


**Rena Lederman** (“Anthropological Regionalism”) has been on the faculty of Princeton University’s Anthropology Department since 1981. Author of *What Gifts Engender* (1986) and many articles and book chapters on gift exchange, gendered relationships and language, sociopolitical forms, and historical representations in Papua New Guinea, she has also published on research practices, the bureaucratization of “human subjects” ethics regulation, the politics of disciplinarity, and the dynamics of specialist/lay communication (e.g., “Unchosen Ground: Cultivating Cross-subfield Accents for a Public Voice,” in Segal and Yanagisako, eds., *Unwrapping the Sacred Bundle* (2005), and her forthcoming book, *Anthropology Among the Disciplines*).

**Harry Liebersohn** (“Anthropology Before Anthropology”) is Professor of History at the University of Illinois, Urbana-Champaign. He is the author of several books, including *Fate and Utopia in German Sociology, 1870–1923* (1988), *Aristocratic Encounters: European Travelers and North American Indians* (1998), and *The Travelers’ World: Europe to the Pacific* (2006).

**Christer Lindberg** (“Anthropology on the Periphery: The Early Schools of Nordic Anthropology”) is Associate Professor of Anthropology at Lund University in Sweden as well as Associate Professor in Comparative Religion at Turku University in Finland. He has been a fellow at the Swedish Collegium for Advanced Study in the Social Sciences in 2003. He is currently scientific editor of *Acta Americana*, and is on the editorial boards of *European Review of Native American Studies* and of *Anthropologicas*. Lindberg is the author of several books pertaining to the history of anthropology, including a biography of Erland Nordenskiöld as well as three collections of biographical essays. He is currently working on a textbook on the history of anthropology and a comparative study of Native American religions.
Jonathan Marks ("Race Across the Physical–Cultural Divide in American Anthropology") is Professor of Biological Anthropology at the University of North Carolina at Charlotte. His primary area of research is molecular anthropology – the application of genetic data to illuminate our place in the natural order – or more broadly, the area of overlap between (scientific) genetic data and (humanistic) self-comprehension. Nevertheless, his interests are wide-ranging, including evolutionary theory, history, human genetics, and the sociology and philosophy of science. His research has been published in scientific and scholarly journals ranging from *Nature* to the *Journal of Human Evolution* to *History and Philosophy of the Life Sciences*. His books include: *Human Biodiversity: Genes, Race, and History* (1995); *What It Means to Be 98% Chimpanzee* (2002), for which he was awarded the W. W. Howells Prize in Biological Anthropology from the American Anthropological Association; and *Evolutionary Anthropology* (1992), which he wrote with Edward Staski. He also received the 1999 American Anthropological Association/Mayfield Award for Excellence in Undergraduate Teaching, and served as President of the General Anthropology Division of the AAA from 2000 to 2002.


H. Glenn Penny ("Traditions in the German Language") is Assistant Professor of History at the University of Iowa. He is the author of *Objects of Culture: Ethnology and Ethnographic Museums in Imperial Germany* (2002), and editor, together with Matti Bunzl, of *Worldly Provincialism: German Anthropology in the Age of Empire* (2003). He is currently working on a book project about the German love affair with the American Indian, which spans the last two centuries.

Robert N. Proctor ("Temporality as Artifact in Paleoanthropology: How New Ideas of Race, Brutality, Molecular Drift, and the Powers of Time Have Affected Conceptions of Human Origins") is Professor of the History of Science at Stanford University and the author of, most recently, *The Nazi War on Cancer* (1999), which won the Viseltear Prize from the American Public Health Association. His research interests coalesce around the history of scientific and medical controversies; he has also written on cancer and environmental policy, tobacco culture, human
origins, molecular anthropology, racial hygiene, expert witnessing, and the “social construction of ignorance” (agnotology). He has held positions as Senior Scholar in Residence at the US Holocaust Memorial Museum in Washington, DC (1994), and as Visiting Fellow at the Max-Planck-Institute for the History of Science in Berlin (1999–2000). He is now working on a project on human origins (especially changing interpretations of the oldest tools), a book on “Darwin in the History of Life,” a book on figured stones (“Agate Eyes”), and articles on how tobacco hazards came to be recognized.


Emmanuelle Sibeud (“The Metamorphosis of Ethnology in France, 1839–1930”) is maître de conferences (equivalent to assistant professor) in the History Department of The University of Paris VIII. Her main interests are the cultural history of French colonialism and the history of ethnography in the twentieth century. She is the author of Une science impériale pour l’Afrique? La construction des savoirs

**Merrill Singer** ("Applied Anthropology") is the Director of Research at the Center for Community Health Research at the Hispanic Health Council in Hartford, CT, and a Research Affiliate at the Center for Interdisciplinary Research on AIDS at Yale University. Dr. Singer was the 2004 recipient of the Practicing Medical Anthropology Award of the Society for Medical Anthropology, and was recently appointed to the City of Hartford Mayor’s Commission on AIDS. His current applied research projects include a CDC-funded study of sexual risk and communication among inner-city young adults, a CDC-funded study of emergent illicit drug-use behaviors, a NIDA-funded study of unintended messages in oral HIV testing among injection drug users in Rio De Janeiro, and a NIDA-funded study of ethical issues in research among drug using populations. He has conducted anthropological research in the USA, the Virgin Islands, China, Brazil, Haiti, and Israel. He has published extensively on health disparities, public health issues in lower income populations, HIV risk and prevention, factors in illicit drug use and abusive drinking, and health policy issues among Latino populations.

**Hilary A. Smith** ("Using the Past to Serve the Peasant: Chinese Archaeology and the Making of a Historical Science") received her PhD from the Department of History and Sociology of Science at the University of Pennsylvania, where she is currently a lecturer. Her research is on the history of Chinese science, medicine, and society, and her dissertation focused on changing medical understanding of nutritional disorders in Chinese history, with special attention paid to the late imperial and modern periods.

**Nikolai Ssorin-Chaikov** ("Political Fieldwork, Ethnographic Exile and State Theory: Peasant Socialism and Anthropology in Late Nineteenth-Century Russia") is a member of the Department of Social Anthropology at the University of Cambridge. He has carried out field research in northern Siberia on the relation between indigenous peoples and the state. His research interests include comparative colonialism; socialist and post-socialist economic systems; systems of taxation; and anthropological theory. His publications include: "Bear skins and macaroni: The social life of things at the margins of a Siberian state collective," in Paul Seabright, ed., *Barter Networks and Non-Monetary Transactions in Post-Soviet Societies*, Cambridge: Cambridge University Press, 2000; and *The Social Life of the State in Subarctic Siberia*, Stanford: Stanford University Press, 2003.

**Ivan Strenski** ("The Spiritual Dimension") is Holstein Family and Community Professor in Religious Studies at the University of California, Riverside. His most recent
work is a historical approach to theories of religion, *Thinking about Religion* (Blackwell, 2006). Three previous books developed the history of Durkheimian theories of sacrifice in terms of their connection with the political, ideological, and religious contexts of France since the sixteenth century. These are *Theology and the First Theory of Sacrifice* (2003), *Contesting Sacrifice: Religion, Nationalism and Social Thought* (2002) and *Durkheim and the Jews of France* (1997).
Introduction
Henrika Kuklick

This collection will appeal to a range of readers, anthropologists and historians prominent among them. For historians, the value of its essays will be their contextualization of anthropological ideas and practices in specific times and places. Anthropologists will find not only discussions of the discipline's major branches but also analyses of portions of its history that rarely feature in its oral tradition – a tradition highly susceptible to “mythicization,” as George Stocking has noted (1995: xviii).

The classic typology of historians of the human sciences is Stocking's, a dichotomous scheme of ideal types: “presentists” and “historicists.” Presentists, usually practitioners of the discipline they describe, frame their accounts in contemporary terms, often seeking lessons from the past for the present: their tone may be celebratory, as they trace the antecedents of ideas and methods now considered commendable, or mournful, regretting the loss of exemplary practices. Historicists, frequently drawn from other disciplines, are not explicitly concerned with contemporary standards and debates; they show that when we read old texts as if they had just been written, we frequently misunderstand their authors' intended meanings (Stocking 1968: 1–12).

But presentist and historicist approaches are complementary, not mutually exclusive. No matter what their professional training and special interests, historians inevitably ask questions that are important in our age. They know that past concerns were different from our own, but they must also know how contemporary practitioners view their enterprise; the past may appear different in the future, but knowledge of a discipline’s present has some bearing on understanding its history. Thus, today’s anthropologists should be both served by attention to historical matters of contemporary concern and inspired by historicist accounts, which aim to meet anthropology’s time-honored goal of sympathetically reporting distinctive ways of life. And to describe episodes in the development of the human sciences also serves to reveal aspects of the general social orders within which they occurred, addressing questions of interest to all manner of historians.
Remembrance of Things Past

To understand anthropologists’ concerns when they contemplate their discipline’s identity, we must recognize the characteristics of disciplines as such, including disciplines’ struggles to endure in institutionalized forms. Roughly a third of a century ago, many anthropologists feared that their enterprise could survive as such only if it subjected itself to unforgiving introspection (e.g., Hymes 1969). They cherished anthropology’s enduring intellectual and moral commitments – at the very least, to a cross-cultural purview and identification of the many ways that it is possible to be human – but feared that the discipline’s customary boundaries were no longer defensible.

In a recent essay, James Clifford summarized the mood of yesteryear. Practitioners could no longer agree “on (1) an empirical object, (2) a distinctive method, (3) an interpretive paradigm, and (4) a telos, or transcendent object. The object was ‘primitive’ societies; the method was ‘fieldwork’; the paradigm was ‘culture’; the telos was ‘Man’” (Clifford 2005: 37). The object, famously termed the “savage slot” by Michel-Rolfe Trouillot, had a working definition that represented justification for unequal power relationships, such as those engendered by colonialism. Anthropology’s method of (primarily) qualitative analysis based on long-sustained participant observation was no longer distinctive to it. The culture concept had been appropriated by other disciplines. The objective of studying man – i.e., the human species, in modern parlance – in all of our aspects now seemed an unrealizable project, a problem particularly acute for anthropologists in North America, where the traditional four-field organization no longer seemed viable in many departments; separate institutional niches were being found for sociocultural anthropology, linguistic anthropology, biological anthropology (including paleoanthropology, the study of the development of the human species based on fossil records), and archaeology.

How can we make sense of Clifford’s disciplinary obituary? It represents heartfelt sentiments that were widespread but historically inaccurate: disciplines have not been defined by fixed charters. That a cross-cultural purview is at the core of anthropology is undeniable: it has always been a defining characteristic of the field, whereas other disciplines may only occasionally adopt it. In general, however, anthropology is no different from other knowledge-based enterprises. Their pasts and presents are linked through intellectual and professional lines of descent, but all scholarly fields have had fluctuating boundaries. Nevertheless, apparent borderlines – as well as cleavages – can appear extremely important at any given moment.

Anthropologists in Situ: Policing Boundaries; Restructuring Universities

Just as many contemporary sociocultural anthropologists may feel that they have little in common with biological anthropologists, many biological/experimental psychologists have intellectually uneasy relationships with their social psychologist
colleagues and wonder whether psychology’s diverse components should go separate ways. It is not unusual for chemists to worry that their field is dissolving; biologists are seizing possession of one portion of it, while physicists are appropriating the remainder. Sociologists, particularly American ones, have been known to quarrel about whether their discipline is defined by its use of quantitative measures. How should they evaluate sociologists who employ qualitative techniques? Perhaps they are really anthropologists!

Arguments about disciplinary purity tend to proliferate in times of scarce economic resources; relative tolerance prevails when all disciplinary factions have access to funds. Consider American sociology in the decades following World War II. Sociologists’ professional ideology was that they belonged to a united field: their empirical research was informed by their theory. But this was not the case. Postwar sociologists’ so-called “grand theorists,” such as Talcott Parsons, conceptualized societies in organicist terms; sociology’s fundamental unit of analysis was a social group, and individuals’ activities were meaningful because they contributed to the operation of the whole. Meanwhile, many empiricists were nominalists, embracing large-scale survey research, taking the individual as their basic unit of analysis and representing social orders as sums of their individual parts. Sociology’s two sides could be symbiotic: for example, surveys of such subjects as communication networks, mass media, and public opinion done on contract at Columbia University’s Bureau for Applied Social Research represented a financial bulwark for faculty and students. Thus, the discipline was replete with contradictions. But American sociologists could afford to ignore them. They had marketable skills, enjoyed relatively generous funding, and exercised unprecedented international influence.

In no small part, disciplines’ current anxieties are functions of changes in institutions of higher education. Arrangements have varied from one country to another, but universities everywhere have become highly dependent on outside grants to finance their faculties’ research, whether these grants came from private philanthropies, industries, or government agencies. The ability to attract outside funding has become a condition of permanent employment in many disciplines (which means that individuals must learn to make plausible claims that their research fits their patrons’ programmatic goals). Related to funding needs of institutions of higher education were dramatic changes in institutions’ personnel rosters during the last third of the twentieth century: administrators became an enlarged percentage of employees, and their role has been to make higher education economically viable. They help to attract and manage funds, and attempt to make academics more efficient (no matter how they define efficiency). In practice, this means that academic administrators have followed a managerial trend evident in all manner of occupational spheres, reducing the percentage of employees who occupy secure positions and increasingly relying on temporary laborers, who work for relatively low salaries and need not be given benefits; unlike many other types of workers, academics are distinguished by having the possibility of gaining full job security – tenure – but the academic profession is becoming less secure in the aggregate, and academics’ current occupational difficulties are not unique.
Universities are also dependent on income-generating students, and in recent decades have competed for them among fluctuating populations of persons of traditionally appropriate age. The post-World War II baby boom produced its last large cohort of future student bodies in 1964. Boomers’ children constituted the so-called “echo boom,” but the echo boom came to an end, forcing administrators to contemplate a future in which new strategies would have to be formulated in order to appeal to a smaller recruitment base. In essence, university managers’ task is to minimize uncertainty, and the students they seek to attract and retain may be unpredictable in their demands.

In sum, in the managed university, competing for scarce resources, scholars of any persuasion can feel threatened by those whom they see as encroaching on their intellectual domains, as well as by administrators who may cut their departments’ personnel allocations (or even eliminate their departments entirely).²

It is not surprising that all manner of disciplines were beset by crises of identity during the 1970s. At this time, the academy’s economies suffered and new PhD’s employment prospects fell. Moreover, there was also considerable political conflict in the world at large, such as over the Vietnam War – and those academic experts who offered advice in the prosecution thereof were much criticized. Thus, though anthropologists have had distinctive concerns, we must recognize that they have been hardly alone in worrying about the institutional viability and political import of their subject.

Original Sins

For many who problematized anthropology’s identity a few decades ago, the discipline’s fundamental defect was its gestation in the colonial situation. That is, anthropologists’ encounters with exotic peoples became possible through the extension of colonial rule by European and American powers. Certainly, the most vulnerable of aboriginal peoples were those who became minority populations in their own territories, such as those of North America and Australia. But indigenes need not have been official subjects of the states from which Europeans came to witness their behavior, measure their physical traits, and collect specimens of their material culture, which were represented in texts, pictures, and museum displays. The development of the global economic and technological infrastructure that made colonial rule possible had opened new areas for research (see Kuklick 1997). I must stress that visiting Europeans’ attitudes to non-Western peoples were not uniform. In particular, visual representations and museum displays exhibited the full gamut of possible attitudes toward exotic peoples (see especially Grimshaw, Mehos, and Penny in this volume).

Moreover, no power relationship can be thoroughly coercive; even prisoners and slaves have developed modes of resistance. The establishment of colonial authority did not protect anthropologists from being regarded as adversarial intruders. Consider, for example, E. E. Evans-Pritchard’s frustration when the barely pacified Nuer repeatedly avoided answering his questions, driving him “crazy,” producing
feelings that he could not resist labeling “Nuerosis” (1940: 13). But anthropologists could turn their imputed association with government powers to their own purposes. When Margaret Mead was working in New Guinea with her then-husband, Reo Fortune, she observed that they were unable to find servants until Fortune “went about from one village to another, unearthed their darkest secrets which they wished kept from the government, and then ordered them to come and carry” (letter of January 15, 1932, in Mead 1970: 308) And therein hangs the tale: anthropologists might elicit subject peoples’ “darkest secrets,” which could prove useful to colonial regimes. Thus, the charge was made that anthropology had been the “handmaiden” of colonialism (e.g., Asad 1973).

Significantly, virtually from the beginning of the Age of Exploration to now, an apparently consistent political attitude has joined predictions about the fates of non-Western peoples: though their societies might have some qualities Europeans could envy, these peoples were destined to become extinct in cultural, if not necessarily physical terms. Thus, their distinctive characteristics must be recorded for posterity. The cause of what is usually termed “salvage ethnography” can be seen in the scientific pursuits of Captain James Cook, who noted that during the interval between his first (1766–1771) and second (1772–1775) sea voyages, New Zealand’s Maoris had suffered from the introduction of European vices, which “disturb the happy tranquility they and their fore Fathers injoy'd [sic]” (quoted in Smith 1992: 99).

Anthropological salvage persists – although I must stress that it does not necessarily have consistent implications. The image of a people untouched by Euroamerican culture excites the imaginations of popular audiences (see, e.g., Marc Lacey, “Remote and Poked, Anthropology’s Dream Tribe,” The New York Times, December 18, 2005). Moreover, non-Western peoples may themselves endorse salvage efforts, such as recording their vanishing languages and preserving their heritage sites.

Among professional anthropologists, however, elegiac tones have long prevailed. Indeed, proto-professional anthropologists of roughly a century ago, such as participants in the 1898 Cambridge anthropological expedition to the islands of Torres Strait (then called Straits), went so far as to instigate re-enactments of ceremonies barely remembered by the oldest islanders. By 1965, Margaret Mead pitied the young anthropologist who might not be able to contribute anything significant to the discipline, unlike the anthropologist of bygone days, who had “the wonderful knowledge that everything he records will be valuable,” since “[a]ll of it is unique, all will vanish, all was and is grist to some fellow anthropologist’s mill” (Mead letter of December 19, 1965, in 1970: 305). Today, of course, the putative cause of cultural extinctions is not the extension of colonial power but the steamroller of globalization – which might be seen as an extension of the status quo ante.

To recognize the political attitude underlying predicted extinctions is to call into question the epistemological status of anthropologists’ observations. That is, it was once a fundamental tenet that anthropologists were able to understand foreign societies because they brought outsiders’ perspectives to their observations; by confronting differences between their ways of life and those of the peoples they studied, anthropologists were able to understand distinctive beliefs and practices of other societies as products of
situational contingencies, rather than as natural phenomena, as the persons who sustained them likely believed. Thus, anthropologists were also primed to recognize their own societies' received wisdom. No later than 1914, W. H. R. Rivers observed

There is nothing which has a greater tendency to interfere with conventional morality than travel, and especially travel among peoples with manners and customs widely different from one's own. It is the change of attitude towards the conventions of one's own society which produces the broadening of ideas and the tolerance which travel brings in its train. (II, 566; and see Gellner 1988)

Thus, anthropology self-consciously adopted the identity of the “uncomfortable discipline, questioning established positions and proclaimed values” (Raymond Firth in 1981, quoted in Wright 1995: 65). There is an echo of this attitude in the standpoint epistemology associated with contemporary feminist theorists, whom Schumaker discusses in this volume (and anthropologists have always known that men and women researchers have had differential access to male and female social spheres, which is one of the reasons that the discipline has been relatively open to women). But standpoint epistemology differs from celebration of outsiders' perspectives insofar as the possibility of perceptual acuity derives from individuals' social positions rather than their choices and achievements. Contrast this view with that of Rivers: loss of faith in the merits of one's own society resulted from experience that might be deliberately undertaken by anyone.

Were anthropologists arguing in thoroughly disinterested fashion when they claimed that their stranger status in exotic places made them acute observers there, and subsequently capable of dispelling conventional wisdom at home? Obviously not: they were legitimating their discipline's erstwhile definition. But this does not mean that we must dismiss their claims – or aims. In the final analysis, if we are incapable of sympathetic understanding of persons unlike ourselves – whether by virtue of their social status, geographical place, or temporal location – the cause of much scholarship, not just of anthropology, is lost. Unfortunately, the celebration of otherness became a political casualty in anthropology: “the other” became a code phrase for the victim of colonized objectification – a vehicle for projective fantasy.

Consider historians by illustrative contrast. Many, if not all, historians distrust accounts of the relatively recent past made by persons who, by definition, share many of the assumptions of their historical subjects; but historians assume that their distance in time from historical actors will allow them to see the conventional wisdom and power structures of past ages with relatively clear eyes. Anthropologists lost faith in a defensible epistemological stance because it was tainted by its association with colonialism; those based in Western institutions felt uncomfortable defining the non-Western world as their main sphere of inquiry. Thus, in 1979, Maurice Freedman mournfully (and presciently) observed a “wholly depressing” trend: “the study of the 'other', and with it the truly international character of anthropology, could die in a welter of national particularism and self-absorption” (15). This unhappy condition was not permanent,
however, not least because anthropology is now practiced in former colonies. And when an Indian anthropologist does research in the Netherlands, she can claim that her perceptual skills are heightened because she does not take for granted many of the assumptions of her subjects (Palriwala 2005).

There are other indications that anthropology has expunged the colonial taint. Those persons who might once have been classified as “primitive” may have spokespeople who are their own, professionally trained “native anthropologists.” And authors of anthropological studies of exotic peoples often feel a moral obligation—or are legally compelled—to submit their accounts for consideration to the persons described therein, as well as to professionals’ scrutiny (see Lederman in this volume). With relative ease, persons remote from centers of power can acquire the means to produce visual images portraying themselves as they wish to be understood (see Grimshaw in this volume). Indigenes now have moral authority and—in varying degrees—legal rights to affect museum exhibits of their ways of life.

Moreover, anthropologists have expanded their substantive purview considerably. No longer confining themselves to what would once have been euphemistically termed “simple societies,” they now consider incontrovertibly complex societies, as well as such ties that bind as transnational associations and relationships within and among complex work organizations—all of which research loci present opportunities to expand our inventory of known behavioral variations (see Lederman in this volume). And they have moved into diverse applied research settings at home as well as abroad, including (but hardly limited to) the school, the corporation, the hospital, the government bureaucracy, and the city street (see Singer in this volume). Moving from the academy into the real world can be dangerous: advice given may not be effective, or it may have unhappy and unanticipated consequences. The spheres of practical anthropological labor may be frivolous as well as important; advising retailers about consumer behavior, say, is quite different from working with the homeless. But the “field” to which the anthropologist takes herself need not be a remote and exotic place (see, e.g., Gupta and Ferguson 1997). Notwithstanding the conservative attitudes preserved in certain anthropology departments, many contemporary anthropologists have taken the entire world as their purview.

“The Past is a Foreign Country”

So begins a novel by L. P. Hartley, The Go-Between, and it is not surprising that this sentence (or a slight variant thereof) has served as the title for a number of historical studies. Confronting anthropology’s history means recognizing significant differences between past and present practitioners. The discipline has roots in virtually every intellectual quarter. But as an enterprise requiring fieldwork, it descends from the natural history sciences. As such, its literary conventions in the late-nineteenth and early-twentieth centuries were very different from what they became. It was once imperative that fieldworkers open their accounts by setting themselves in the scenes of their
inquiries, describing the flora and fauna as well as the diverse persons among whom
they found themselves, in order to convey to the reader that they were authentic
witnesses, whose testimony was reliable – no matter what specific phenomena they
had come to observe (e.g., Haddon 1901; and see Kuklick 1997).

For a number of contributors to this collection, disciplinary discussions about field-
work as such have entailed reconsideration of what used to be a standard trope in
anthropology’s oral tradition: that fieldwork methodology was contrived (or at least
independently invented) by Bronislaw Malinowski as a nearly unintended by-product
of a historical accident. A graduate student at the London School of Economics,
he was visiting Australia to participate in the meetings of the peripatetic British
Association for the Advancement of Science when World War I began; he had
intended to do field research, but might never have spent as much time as he did in
the nearby Trobriand Islands had he not been a citizen of the Austro-Hungarian Empire,
classified as an enemy alien and thus unable to return to Britain until the war’s end.
Malinowski may have publicized his experience effectively, but his was not the first
exercise in sustained field research (see esp. Lindberg, Penny, Sibeud, Ssorin-Chaikov
in this volume).

Malinowski’s most innovative contribution to fieldwork method may have been
the conventions of ethnographic reporting he developed, which made fieldworkers
the lonely heroes of their stories, abstracting them from their social networks – from
their effectively anonymous valued local confidants and support staff, as well as from
other Europeans who were residents in their field sites, ranging from missionaries
to commercial traders, who taught fieldworkers a good deal about local lifeways.
Debunking the legend of the heroic lone fieldworker is more than an exercise in demythol-
ogizing the discipline’s history – though it is that. It expresses as well as facilitates
pedagogic recognition that fieldwork technique is socially embedded practice that
can be taught.

Finding pioneering fieldworkers in many locations also involves questioning
what has been understood to be the international hierarchy of disciplinary practi-
tioners. Until recently, British and American sociocultural anthropology effectively
reigned supreme, and at least “for some purposes” they could together be “regarded
as variants of a single hegemonic anglophone anthropology” (Stocking 1995: xvii).
But this hegemony is itself a historical product. In the early twentieth century, it
was arguably variants of the German-language tradition that were internationally
hegemonic – and these variants included Boasian cultural anthropology, the dom-
inant school in the United States (see Darnell, Lindberg, and Penny in this volume;
see also Gingrich 2005: esp. 111–36).

Discrediting the myth of sustained Anglophone anthropological hegemony serves
the contemporary cause of those who are making anthropology an increasingly
international discipline – as it certainly was in the nineteenth century, as well as, to
a lesser extent, in the twentieth century before World War II (see Kuklick 2006).
It is worth noting that parochialism has been a function of disciplinary specialty as
well as time and place. In the decades immediately following World War II, British
social anthropology was aggressively British, militantly patrolling its boundaries
against foreign invaders (see Kuklick in this volume). By contrast, paleoanthropology has been a virtually international enterprise (see Proctor in this volume).

Selecting Problems; Confronting Results

In general, examination of anthropology’s past reveals much about peculiarities of intellectual venues – of time, place, and ideology. For example, anthropologists and their forebears were obsessed with the centuries-old problem of defining genuine spirituality, an issue that does not animate contemporary anthropology. Scholars expected to gain understanding of religious belief from comparative cultural analysis. But their conclusions were hardly unanimous. Indeed, many scholars judged that material progress led to spiritual degeneration, rather than that supposedly “civilized” peoples were superior in every way (see Strenski in this volume). The varied ways that Darwinian speculations about race were received in different national contexts tells us much about the political climates of different places (see Glick in this volume). Archaeological inquiries have frequently served political purposes, but these have run the full gamut of available possibilities (see Smith in this volume; see also Kuklick 1991a). And research undertaken for clear ideological reasons has had unintended consequences. The nineteenth-century Russian political radicals who undertook inquiries among Siberian peasants wished to prove that historical trends were inevitably leading to the realization of their ideals. Many of them were enduring forced exile as punishment for their political acts. Alas for the radicals, many of their findings disconfirmed expectations (Ssorin-Chaikov in this volume).

And one must stress that persons apparently joined in a common pursuit have had different motives and effected unrelated ends. The link between anthropology and the classics forged in the late-nineteenth and early-twentieth centuries, described by Ackerman in this volume, was compounded of many elements. There was the straightforward factor of academic politics: relative to the upstart discipline of anthropology, classics had high prestige, some of which might be transferred to the new field. But by the very token of its long establishment, classics was in need of invigoration, and analogizing extant non-Western peoples to those of Ancient Greece and Rome could yield new insights – and also call into question long-cherished beliefs that the Classical Ancients were the intellectual and political progenitors of Western civilization at its best. The histories of ancient empires could also be studied as cautionary tales: rulers of contemporary empires might learn how to avoid the fates of the empires of Greece and Rome.

It is unquestionable that in all of its aspects anthropology had historical connections to colonialism, and many of the contributors to this collection explore these (see esp. Kuklick, Liebersohn, Mehos, Penny, Saunders, Sibeud, and Singer). But the relationships they find are complex and diverse. At one extreme is the relationship between the study of non-Western peoples and political values in the polities that would join to become the German state, where the development of anthropology predated the acquisition of colonial territories and was linked to liberal politics. At the other
extreme is the situation in the Netherlands, in which future colonial officials were
given formal instruction in practical knowledge about subject peoples and museums
were created in which colonies’ commercial value was made evident; but anthro-
pology did not develop in the Netherlands as an esoteric discipline until the end of
the colonial era. Indeed, in no matter what national context, anthropologists’ polit-
ical views have varied considerably at any given moment and over time. Thus, officials
of colonial governments often took exception to anthropologists; officials frequently
suspected that anthropologists were subversives, undercutting government authority
because they identified with those they studied.

It is important to remember that colonies were not worlds apart from the
metropoles; they were joined in a range of social ties. The successes or failures of
colonial ventures had political implications for metropolitan regimes, and colonies
were sites for innovations that might be brought home. The colonies presented
Europeans with a variety of opportunities. Consider the persons A. C. Haddon
encountered in the islands of the Torres Strait, which he first visited as a biologist
in 1888 and to which he returned as the organizer of the Cambridge anthropological
expedition in 1898. Among his contacts were the Bruce brothers, Robert, a barely
educated carpenter and wheelwright, who befriended him during his first visit, and
John, schoolmaster and magistrate of the island of Mer, whose assistance proved invaluable
for his anthropological work; the brothers had come to the islands to escape
straitsened circumstances in Glasgow. The islands were also full of fortune hunters of
many nationalities, including Japanese, hoping to become rich from pearl trading. Among
the careers that could be advanced in the colonies were scientific ones, of which
geology was perhaps the most notable (and profitable); anthropologists were only one
among many types of researchers for whom work in the colonies became vital – both
because the colonies represented distinctive natural worlds, enlarging scientists’ sub-
ject matter, and because the inclusion of positions in the colonies within occupational
job structures literally enlarged individuals’ opportunities to succeed (see Kuklick and
Kohler 1996). But scientific pursuits in colonial places could be quite humble ones,
such as those of Alfred Russel Wallace, who collected natural specimens that might
be sold for display in gentlemen’s houses (for more on Wallace, including his anthro-
pological activities, see Kuklick in this volume). In short, Europeans’ connection to
colonial power could have been nearly incidental, and Europeans’ behavior could range
from reprehensible to admirable.

Knowledge for Whom?

Colonial settings were not the only ones in which early anthropology was seen to have
value; it led to self-understanding and figured in consideration of domestic political
issues (see, e.g., Penny and Kuklick in this volume). But colonial settings were the most
notable locales of applied research. Thus, it is significant, as Schumaker observes in
this volume, that academic institutions in new states have not abandoned anthropol-
ogy because of its colonial taint (though they may choose to call it sociology), and
continue to train students to do fieldwork; but new policy concerns animate anthropological inquiries in former colonies. Moreover, the reward system of anthropology is differently structured in new states; work framed as purely academic (such as that leading to publication in peer-reviewed journals) enhances status in university settings, just as it does in Euroamerican universities, but research with practical value, such as contract labor for non-governmental organizations (NGOs) is far better remunerated than academic employment – and universities may be dependent for daily operating costs on the funds their faculties contribute when they do contract labor (see Holland 2006). Euroamerican anthropologists might note national/regional differences in disciplinary prestige structures and reconsider their colleagues' relative disdain for applied anthropology, which grew in the post-World War II period and was at an especially high point during the Vietnam War era (see Singer in this volume).

That a substantial proportion of credentialed anthropologists now work in non-academic settings has been taken as a sign of the discipline's decline. Certainly, graduate students in anthropology, like graduate students in other human sciences – economics, political science, psychology, and sociology – must recognize their mentors' view that academic careers are best. But the possibility of diverse types of anthropological careers is really a sign of the discipline's strength. Other human sciences also present students with evidence that should (in various ways and to various ends) provoke introspection and self-understanding, just as anthropology does in its humanistic incarnation. But their viability as disciplines rests in no small part on demonstration that their insights can be useful. And as a substantial literature on the history of knowledge-based occupations has shown, professional workers of every type have greater freedom of thought and action if they have many clients, and need not be the creatures of a single sort of patron (the classic text is Johnson 1972). Psychology has not been exiled from the university because the majority of its PhDs work in applied settings. The human science with the greatest prestige, economics, is that with the greatest perceived practical value – and palpable influence, supplying industries, governments and international agencies with advisors at the highest level. Indeed, it is notable that economists are now competing with anthropologists for a sector of applied research turf: developing strategies to reform self-destructive and antisocial human behavior. When anthropologists' methods prove successful in solving social problems, their discipline's prestige is enhanced. Moreover, anthropology's considerable value as, in Firth's previously quoted words, the "uncomfortable discipline, questioning established positions and proclaimed values," is not incommensurable with its practical application. Anthropologists formally employed to solve social problems may be more effective advocates for their clients than those who have themselves embraced advocacy (see Singer in this volume).

Indeed, the prestige differential between abstract scholarly inquiry and popular recognition is a false one in the real world, though it may seem genuine within the academy. Respect gained in the world at large legitimates disciplines. Consider the career of Franz Boas: he both appealed to popular audiences and became a (indeed, for long
Figure 1  A literal illustration of Margaret Mead’s popular recognition, a paper doll included in Tom Tierney, Notable American Women. Paper Dolls in Full Color (1989). The other heroines of the collection are Emily Dickinson, Isabella Stewart Gardner, Mary Cassatt, Emma Lazarus, Jane Addams, Juliette Gordon Low, Helena Rubenstein, Willa Cather, Helen Keller, Margaret Sanger, Clare Boothe Luce, Georgia O’Keefe, Margaret Bourke White, Babe Didrikson Zaharias, and Lorraine Hansberry. Reproduced with the artist’s permission.
the) dominant figure in the discipline, populating anthropology departments with his students. When he retired, *Time* magazine made his career its cover story, which emphasized the success of his lifelong project to discredit scientific racism (May 11, 1936). Indeed, sociocultural anthropology’s popular prestige in the United States surely hit a high point during and after World War II in consequence of the involvement of a substantial proportion of its practitioners in the war effort, participating in activities ranging from setting foreign policy to (there is no ignoring it) the management of Japanese internment camps (see Singer in this volume). Perhaps no anthropological analysis has had a greater policy impact than Ruth Benedict’s *The Chrysanthemum and the Sword* (1946), which guided the post-World War II reconstruction of Japan by victorious occupation forces. And certainly no sociocultural anthropologist has been a more prominent public intellectual than Margaret Mead, whose status as such developed before the war but was enhanced by her activities during it. She became an expert witness before Congressional committees, a television personality, and a columnist for the mass-circulation magazine *Redbook*, in which she broadcast her opinions on all manner of subjects; as she herself was aware, her academic colleagues were somewhat disdainful of her because she wrote popular works (she tried to raise their opinion of her by also writing works for strictly professional audiences), but by gaining widespread recognition she served to enhance the value of her discipline in the public eye.

**Academic Structures; Public Responsibilities**

Rena Lederman suggests that anthropology cannot define its boundaries and mission without attention to the public’s understanding of its role: “anthropology’s comparative, first-hand point of view is expected to offer answers to questions other scholars and laypeople have about fundamental ‘human nature’” (2005: 56). The specific issue she is addressing is whether there is justification for perpetuation of American anthropology’s traditional four-field disciplinary structure, and her argument is that intellectually productive exchanges among subfield practitioners are results of such a structure – not just that a four-field department matches popular understandings of anthropology.

The arguments made for subfield fission or fusion fit genres hardly original to our time, invoking essentialist definitions dating to the nineteenth century. At Stanford University, for example, one department split into two in 1998: the Department of Anthropological Sciences, structured along canonical four-field lines but defining the discipline by its *method* rather than its *content*; and the Department of Cultural and Social Anthropology (including archaeology), grounded in the assumption that anthropology’s substantive content defies formulation of law-like generalizations, requiring interpretative, humanistic analyses. Stanford's administration ordered the two departments to reunite in 2007. Sociocultural anthropologists who feel that their work belongs among the humanities and biological anthropologists who feel close affinities with population geneticists are reasoning along the lines that divided the Stanford
anthropologists. But though arguments about the nature of different types of knowledge systems are fruitful material for the flourishing new specialty of the anthropology of science, I will say no more about them as such.

Nevertheless, the question of what contemporary biological anthropologists and sociocultural anthropologists can say to one another has to be taken seriously. That is because we must consider anthropology’s historical contribution to the pseudo-science of scientific racism, which justified such unfortunate policies as slavery and eugenic measures ranging from forced sterilization to attempts to exterminate whole populations. But anthropologists’ arguments about race more frequently took liberal forms—including opposition to all of the aforementioned policies. Indeed, anthropologists’ most constructive contribution to public discourse was long considered to have been discrediting for all time the “dangerous myth” of significant differences among types of humankind, or “races” (see, e.g., Darnell, Glick, Marks, Penny, and Proctor in this volume; see also Barkan 1992).

Unfortunately, however, the argument that biological and behavioral variation are linked has again become respectable on many fronts. It is an argument that appeals to popular audiences as well as to various types of academics—illustrating the general phenomenon that the line between specialized knowledge and popular belief is always somewhat blurred. Indeed, there are social developments that could not have occurred absent scientific advances—but did not follow from science per se. How else, for example, is one to understand why DNA testing that revealed their Jewish ancestry has led a number of members of a Catholic population of Spanish descent living in the American southwest to either convert to Judaism or to incorporate Jewish elements in their Catholic religious practices (reported in The New York Times, October 29, 2005)? Consider the phenomenon of the link between commercialized DNA testing and tourism: travel plans to specific parts of Africa may be based on testing of tissue samples that supposedly reveal persons’ geographic origins. And racism of the sort once considered thoroughly discredited is gaining scientific sanction with such recent developments as the recent approval of a drug, BiDil, judged to be especially effective in treatment of heart disease among African-Americans; one assumes that physicians’ assignment of African-American identity to persons who will then receive BiDil is likely to be based on such superficial characteristics as skin color, whereas, in fact, DNA testing often reveals to persons that in genetic terms they are not what they appear to be. Of course, persons outside the academy as well as inside it understand races as social constructions rather than natural population units, but that does not mean that racist judgments have not lately gained in respectability (see, e.g., unsigned editorial in The New York Times, “Debunking the Concept of ‘Race’,” July 30, 2005).

Contemporary developments are likely to remind practitioners of science studies of the oft-told story of the acceptance of Gregor Mendel’s articles demonstrating laws of heredity unaffected by environmental factors (acquired characteristics were not inherited), which were published in the mid-1860s. But Mendel’s laws were ignored until 1900, when they were simultaneously—and independently—rediscovered by three scientists, one in Germany, one in the Netherlands and one in Austria, after which
knowledge of them spread throughout the scientific community. Late-nineteenth century scientific work had favored their acceptance. Equally important, there were many outside the world of science whose attitudes were consistent with hereditarianism, representing extravagant expectations that the quality of the human species could be elevated with the application of scientific knowledge. This is to say that hereditarian views became mainstream, although they were not universally accepted. (The famous British polymath, Herbert Spencer, for example, refused to abandon belief in the heritability of acquired characteristics, arguing that human progress was impossible in the absence of this mechanism.)

The moral of this story is not simply that scientific findings will not be accepted until widespread social attitudes favor their recognition. Scientific findings can also inspire social movements – including public opposition to scientific judgments. Consider contemporary campaigns to portray Darwinian evolution in school textbooks as merely a theory, to which “intelligent design” (“creationism” in disguise) should be considered a viable alternative. Using such techniques as those of molecular genetics, scientists have powerful tools for establishing relationships of descent and divergence among species – defining these in ways that challenge many persons’ religious beliefs.

In short, popular attitudes and professional developments are together serving to oblige contemporary anthropologists to explicate the relationship between biological and cultural phenomena, rehearsing arguments that once seemed firmly settled. Old controversies, such as whether varieties of humans are members of one species (the doctrine of “monogenism”) or are truly different species (the doctrine of “polygenism”), which seemed resolved in favor of the former position more than a century ago, are surfacing in euphemized form in such disciplinary specialties as paleoanthropology – which may best exemplify the relationship between popular and esoteric spheres, since its findings both require mastery of highly specialized knowledge and techniques and are of interest to an enormous popular audience (see Proctor in this volume).

It is the very mixture of enterprises that anthropology may join that allows its practitioners to provide distinctly sophisticated answers to revitalized questions. In this volume, Jonathan Marks argues that sociocultural and biological anthropologists are as intellectually close as (if not closer than) they have ever been, and it is notable that relationships between the subfields are now being forged outside the United States (see Kuklick in this volume). That is, a nuanced understanding of recent work in the biological sciences complements sociocultural anthropologists’ recognition of the extraordinary range of possible ways that humans may live – the plasticity of “fundamental ‘human nature’.” Research indicates that individuals’ development both in utero and after birth is shaped by interactions between their biological potentials and environmental factors. That is, humans have survived over time under highly diverse conditions because our brains evolved to facilitate adaptation – sufficiently flexible to be accommodating and innovative. The cross-cultural perspective that has consistently been at the core of anthropology does not permit reductive notions. It is ignorance of societal variation that allows evolutionary psychologists, say, to argue that contemporary humans have an internal
programming dating to the evolution of the species, naturally preferring to mate with persons of specific body types in order to maximize the number of their offspring. Likewise, ignorance allows some biologists to argue that persons are genetically programmed to specific types of behavior. In short, subfield practitioners need one another now as much as they ever have in order to make the sorts of arguments that are dear to each.

Conclusion

Some of the contributors to this collection are anthropologists, while others are historians, and some essays in the collection may have special appeal for contributors’ disciplinary colleagues. But many essays should have broader appeal – although different intellectual types will read them for different reasons. All readers should appreciate the value of placing ideas and practices in historical contexts. But historians will be pleased to read studies that reveal previously unexposed dimensions of significant phenomena – the political philosophy of John Locke, say, or arguments about slavery, the perennial question of Home Rule for Ireland, the crisis of German liberalism, and that odd creature the intellectual French colonial official. Anthropologists will learn of neglected ancestors and bring a historical perspective to contemporary debates.

Notes

1 For various forms of assistance to me as I produced this collection, I would like to thank Orit Abuhav, Robert Ackerman, Ana Alonso, Rita Barnard, Glenn Bowman, Joshua Berson, Paul Burnett, Brian Daniels, Joseph Farrell, Sarah Fee, Edgardo Krebs, Maneesha Lal, Rena Lederman, Jonathan Marks, David Mills, Howard Morphy, Joy Rohde, Anne Rothenberger, Brent Shaw, Mark Turin, Nathan Sivin, Susanna Trnka, Peter Wade, and Maxim Waldstein.

2 For just two specimens of histories of the university as an institution, see Rüegg 2004 and Graham and Diamond 1997. One should note that the consequences of administrators’ efforts to maintain or enlarge the size of their institutions’ student populations have not been all negative. Campaigns to enroll persons beyond normal student age and with backgrounds different from institutions’ usual recruitment bases create more stimulating learning environments. And consider the coeducation movements of the late nineteenth century and the end of the twentieth century. At both times, all-male institutions opened their doors to women because the size of the cohorts of males available for recruitment as students were relatively small; decisions made for economic reasons contributed to the growth of women graduates’ demands for greater recognition of their talents and accomplishments.

3 The movement to improve the human species – eugenics – was not invariably linked to immoral policies, since it included advocacy of public health measures, which would facilitate expression of individuals’ inherent potential. The classic work on the subject is Kevles 1985.